# The Persistence of Power: How Family Origins Shape Political Representation and Policy*

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May 1, 2026

**Abstract**

In the United States, long hailed as the land of opportunity, is access to political office truly open across society, or do the most privileged children disproportionately rise to enter political life? This question speaks to a longstanding concern that elite families may entrench themselves in positions of power, reproducing a form of hereditary privilege within a democratic system. We study the family backgrounds of U.S. politicians over the late nineteenth and early twentieth centuries and show that children from wealthy and privileged households have been substantially overrepresented in elected office. This imbalance has changed little over time and, at the highest levels of office, varies little across political parties. To test whether political access depends on family resources, we exploit the sudden economic shock caused by the end of slavery. Despite the large and concentrated losses at the top of the wealth distribution, the children of slaveholders continued to enter government at high rates. Finally, we examine whether politicians' socioeconomic origins shape policy by constructing a new sample of close elections linked to detailed information on U.S. House candidates' family backgrounds. Comparing otherwise similar districts in which a candidate from a high socioeconomic status family narrowly wins rather than loses, we find that districts represented by higher status candidates are less likely to support pro-tax positions in roll-call voting. Together, the evidence across our analyses shows that family background strongly predicts entry into political office and has measurable consequences for policy choices.

**JEL Codes:** H10, H70, J45, P16, J62

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## 1 Introduction

Are elected officials primarily drawn from privileged families, or is access to political office broadly open across social backgrounds? This question goes beyond the well-recognized finding that politicians are often economically and professionally advantaged as adults (Carnes and Lupu, 2023). If officeholders are disproportionately drawn from affluent households, electoral outcomes may in practice reproduce a form of hereditary privilege. Political theorists such as Mosca (1939) have emphasized that ruling classes tend to become hereditary, raising the prospect that political power persists across generations. This concern also appears in early American political thinking, as Thomas Jefferson warned that democracies must guard against an "artificial aristocracy, founded on wealth and birth, without either virtue or talents" (Jefferson, 2010, p. 563). Such a pattern would bear directly on debates over whether elected representatives can effectively serve citizens from across the social spectrum (Mansbridge, 1999), and on broader concerns about intergenerational mobility in a society where many continue to believe in the possibility of rising "from rags to riches" (Alesina, Stantcheva and Teso, 2018).

This paper provides the most comprehensive evidence to date on the family origins of U.S. politicians and the consequences of these backgrounds for political decision-making. We assemble new large-scale samples that rely on recently digitized historical records and original data collections. First, we study representativeness by identifying all local, state, and federal officeholders in the full count decennial census waves from 1860 to 1940 and linking them to their childhood households using the Census Tree (Buckles et al., 2023), a recent advance in historical record linkage that achieves substantially higher match rates than earlier approaches. To characterize family background, we construct a measure of socioeconomic status (SES) based on each father's wealth percentile imputed using detailed occupation, state of residence, and age. This approach, which is similar to widely-used occupational

income scores (e.g., Abramitzky et al., 2024), yields an omnibus measure of family social and economic background. Second, we make progress studying factors driving political entry by examining whether elite political access is insulated from sharp shocks to family financial resources. Using recently digitized full slave schedules (Hacker et al., 2025), we identify children from slaveholding and non-slaveholding families and follow them to later political outcomes in the post-bellum period. Our test centers on estimating whether the loss of family financial resources generated by emancipation limited political entry among children of elite slaveholding families relative to children of comparably wealthy non-slaveholders. Third, we study whether family background shapes behavior in office by constructing a novel dataset of close U.S. House elections and linking candidates to measures of family socioeconomic background and subsequent roll-call voting. We then implement a regression discontinuity design that compares otherwise similar districts in which a higher-SES candidate narrowly wins rather than loses, helping isolate the effect of family origins from broader electoral incentives.

The main finding from our descriptive analysis is that children of high SES families are substantially overrepresented among elected politicians. Children from the top quintile of the family SES distribution account for 30.7 percent of elected officials, compared with 11.5 percent from the bottom quintile, and this imbalance is even more pronounced at the highest levels of office, where the share from the most advantaged families rises sharply. These results stand in notable contrast to evidence from Sweden, where Dal Bó et al. (2017) show that representation of family background, as measured by parental income and occupation, is remarkably even among elected politicians. The overrepresentation of children from elite families in the U.S. has remained largely unchanged across the long historical period we study. Notably, this overrepresentation is also evident among members of Congress, and it is strikingly similar across Democrats and Republicans, suggesting that unequal access to political office is not a distinctly partisan phenomenon.

The overrepresentation of elected officials from high SES families could stem from a variety of factors, including access to financial wealth, influential social networks, or inherited skills. Wealth and networks, in particular, are central in theories of political selection and entry (e.g., Cruz, Labonne and Querubin, 2017; Corvalan, Querubín and Vicente, 2020). In general, it is difficult to disentangle these factors, but a distinctive feature of our setting is that our data span a long period that encompasses major economic and social transformations, such as the Civil War and Reconstruction. The postbellum period provides a particularly compelling test case because emancipation delivered a massive shock to the wealth of elite slaveholding families (Ager, Boustan and Eriksson, 2021). We exploit this natural experiment by comparing political entry among slaveholding and non-slaveholding families, using emancipation as a unique opportunity to assess whether the loss of financial resources constrained the ability of elite families to send their children into political life.

Strikingly, we find that sons of slaveowners are more likely to enter elected office than children from similarly wealthy non-slaveholding backgrounds. These results highlight the persistence of elite family power even after significant economic shocks and stand in contrast with prior studies that demonstrate the importance of family resources (e.g., Dahl and Lochner, 2012; Aizer et al., 2016). Notably, we estimate that sons of slaveowners are more likely to enter political office even in census years when slaveholding families' economic standing was depressed (Ager, Boustan and Eriksson, 2021). Our evidence suggests that, for these wealthy families, access to elected office arose from a broad range of social and economic factors, not financial wealth alone.

In the final part of the paper, we examine how family background shapes behavior once politicians hold office. Whether inequalities in political entry matter ultimately depends on whether family origins also influence how elected officials exercise power. This question bears directly on longstanding concerns about whether governments drawn from a narrow social or economic stratum can credibly represent a broader public. Politicians make consequential

choices over taxation, spending, and other core areas of public policy. If politicians from affluent households systematically support different policies than those from less affluent backgrounds, unequal access to political office may shape not only who governs, but also whose interests policy serves.

We study this question by focusing on the U.S. Congress, where roll-call votes provide a transparent measure of legislative behavior, and focus in particular on tax policy. By the early twentieth century, a series of major House tax votes centered on revenue acts and tax bills that altered the burden on high incomes, estates, and corporate profits. These were among the clearest distributional conflicts of the era and thus constitute a natural setting for testing whether politicians' socioeconomic origins shape the policies that they support (Brownlee, 2004).

A challenge in studying how family background shapes legislative voting is the selective nature of political representation. The legislators who reach Congress are not a random sample of either candidates or the districts that they represent. Importantly, districts that elect politicians from affluent households may differ systematically in their preferences over redistribution and the role of government, and candidates from different backgrounds may also differ in other politically relevant characteristics, including education or social ties. Simple comparisons across legislators therefore cannot cleanly distinguish the influence of family background from the effects of constituency demand, electoral selection, and other characteristics correlated with elite origins.

To make progress on this question, we construct a new sample of close U.S. House elections with detailed information on candidates' family socioeconomic backgrounds and implement a regression discontinuity design. Our approach compares otherwise similar districts in which a candidate from a higher-SES family narrowly wins rather than narrowly loses, creating a contrast in the family-SES background of the representative who takes office. At the cutoff, this design holds constant district-level political conditions, helping separate the influence

of family origins from differences in the constituencies that elect affluent-background candidates.

Using this approach, we find that districts that narrowly elect candidates from higher-status backgrounds are subsequently represented by legislators who are 27.8 percentage points less likely to support higher tax positions on major tax bills, a reduction of roughly 38 percent relative to the sample mean for lower SES elected officials who are to the left of the election cutoff. This pattern coincides with a sharp first-stage shift in the family-SES background of the representative who takes office. At the same time, we find no detectable discontinuities at the cutoff in key observable candidate characteristics, most notably pre-congress occupational standing, measured using standard occupation scores. This balance is important because it indicates that the RD comparison is not simply identifying the well-documented finding that politicians tend to come from higher-status occupations. We also find no detectable discontinuities in educational background or age at entry into Congress. Although this evidence does not rule out all remaining candidate differences, it suggests that the close-election design isolates variation in family origins more cleanly than standard comparisons across legislators.

Our paper contributes to several strands of research on the social origins of political elites and unequal access to office. First, it relates to work on descriptive representation and the backgrounds of officeholders. Existing work shows that politicians are often advantaged in terms of their own education and occupations (Carnes, 2012; Gulzar, 2021; Carnes and Lupu, 2023), while qualitative histories have long emphasized how wealthy families create enduring political dynasties, though this evidence is largely confined to a narrow set of especially prominent families. We build on this literature by providing systematic large-scale evidence on the family backgrounds of U.S. elected officials over a long historical period. Parallel recent work documents similar socioeconomic gradients in entry and advancement in science and academia (Bell et al., 2019; Abramitzky et al., 2024; Airoldi and Moser, 2024),

suggesting that unequal access to elite positions extends well beyond politics. More broadly, our findings speak directly to longstanding concerns that democratic politics may reproduce a form of hereditary privilege rather than open access to office.

Second, we contribute to the literature on political selection and entry. A growing body of work studies who becomes a politician and how resources, networks, and other advantages shape access to office (Carnes and Lupu, 2016; Folke and Rickne, 2016; Fisman et al., 2020). We extend this literature by bringing new historical evidence to bear on a central but difficult question: whether family resources are important for promoting political office holding of future generations. Our analysis of emancipation provides a rare test of this issue. Despite the large wealth shock imposed on slaveholding families, their children continued to enter politics at high rates. This finding suggests that elite political persistence does not depend narrowly on contemporaneous wealth alone and that access to office may be more deeply rooted in family status and related forms of advantage, consistent with recent evidence for post-revolutionary China that elite status can re-emerge even after radical efforts to eliminate wealth and educational inequality (Alesina et al., 2020).

Third, our paper contributes to research on whether the social and economic backgrounds of officeholders shape policy choices once they govern. Recent work shows that lawmakers' personal backgrounds can matter for legislative behavior (e.g., Carnes, 2012). Closest to our analysis is Feigenbaum, Palmer and Schneer (2025), who study how family immigration history shapes congressional voting and find that those descended from immigrants are more supportive of permissive immigration legislation. In contrast, we study the consequences of socioeconomic origins for taxation, one of the clearest distributional conflicts of the early twentieth century. Using a new sample of close U.S. House elections linked to candidate family backgrounds, we show that districts that narrowly elect candidates from higher-SES families are subsequently represented by legislators who are less likely to support pro-tax positions. These findings suggest that unequal access to political office may shape not only

who governs, but also whose interests public policy serves.

## 2 Historical and Institutional Background

### *2.1 The Political Class, Elite Status, and Family Origins*

An important literature shows that politicians typically enter office having attained economically and socially advantaged positions in adulthood. For instance, Carnes (2013) finds that lawyers and business owners make up roughly 10 percent of the general population, but at least half of both chambers of the U.S. Congress. Similarly, Bonica (2020) shows that lawyers enter U.S. congressional politics at notably high rates, and conditional on running, win at twice the rate of candidates from other backgrounds. These patterns make clear that officeholders are an unusually privileged group in terms of their careers and achievements.

Yet, this evidence does not by itself tell us whether political advantage is intergenerationally rooted. A political class can be highly selected on education, occupation, or earnings in adulthood even if access to office is broadly open across family backgrounds. Dal Bó et al. (2017), for example, show that Swedish politicians are positively selected on their own traits, while their parents' social class and earnings remain broadly representative of the population. Their evidence highlights a central conceptual distinction for our paper: politicians may be socially distinctive as adults without being disproportionately drawn from high SES families.

Whether U.S. politicians tend to come from advantaged families remains an open question. Much of the evidence to date has taken the form of qualitative accounts of prominent families rather than systematic evidence on the broader political class. Biographical accounts of the Roosevelt family, for instance, trace how its nineteenth-century elite economic standing helped shape the political rise of Theodore and Franklin Roosevelt (Ward, 1985; Dalton, 2002). A more quantitative strand of research has studied political dynasties in the U.S. Congress—families in which multiple members have held this level of elected office—and finds that dynastic legislators enjoy measurable electoral advantages over their non-dynastic

counterparts (Dal Bó, Dal Bó and Snyder, 2009; Feinstein, 2010). However, this work faces two limitations as a window into the relationship between family socioeconomic background and political access. First, it is by design restricted to families already embedded in political life, and so cannot speak to whether family background shapes entry and representation across the full population of candidates. Second, dynastic legislators have constituted only a small fraction of Congress, roughly 10 percent for much of the span of U.S. history (Dal Bó, Dal Bó and Snyder, 2009), meaning that the large majority of the political class falls outside the scope of this research. In sum, what remains missing is comprehensive evidence on the family socioeconomic origins of the broader U.S. political class, beyond a small set of notable families and congressional dynasties. We also lack evidence on how family origins are related to political representation across the wider hierarchy of U.S. officeholding, from local government to state and federal politics.

### *2.2 Slaveowners, Emancipation, and Wealth*

The end of the Civil War and the emancipation of slaves provide a sharp test of whether elite political access depends on family financial resources. The scale of the shock was enormous and highly concentrated. About half of antebellum Southern wealth was held in enslaved persons, while slaveholding itself was concentrated among the upper tail of the white Southern wealth distribution: only 21 percent of white Southern households owned any slaves in 1860, and fewer than 0.5 percent owned more than 50 (Soltow, 1975; Goldin, 1973). Emancipation abruptly eliminated this central asset of the Southern elite. Between 1860 and 1870, wealth in the South fell at every percentile, with the largest declines among the rich, producing a sharp compression of the Southern wealth distribution (Ager, Boustan and Eriksson, 2021).

Whether this shock permanently displaced elite families, however, remains debated. A classic view of the postbellum South holds that emancipation was a major rupture for the

Southern ruling class. Woodward (1951), for example, argued that no elite in American history had been so completely stripped of its economic foundations, and pointed to evidence that much of the South's later industrial wealth came from non-slaveholding backgrounds. By contrast, revisionist work, often drawing on small-scale studies of particular locations, emphasizes continuity and adaptation rather than collapse. Wiener (1978)'s study of wealthy Alabama planters, for instance, concludes that what followed was not the "downfall" or "destruction" of the old planter class, but rather its persistence and metamorphosis into planter-merchants and other locally dominant elites. Related work likewise suggests continuity in planter influence in a specific Reconstruction-era political setting: Ager (2013) shows that delegates to Alabama and Mississippi constitutional conventions between 1865 and 1890 often had direct household connections to antebellum slaveholders or wealthy planters. While slaveowning families eventually regained their economic standing (Ager, Boustan and Eriksson, 2021), what remains less clear is whether the destruction of slave wealth in the immediate postbellum years reduced these families' ability to send their children into elected office.

### *2.3 Identity, Legislators, and Tax Policy*

Do elected officials' family backgrounds matter once in office? One traditional view suggests that they should matter little: if legislators are closely constrained by electoral accountability and constituency preferences, then policy should primarily reflect voter demands rather than the personal characteristics of officeholders (Miller and Stokes, 1963; Barro, 1973). An alternative view is that elected officials retain meaningful discretion, so that their own backgrounds can shape how they interpret interests and exercise power (Mills, 1956). The distinction is central for our purposes. If the former view is correct, unequal access to office may have limited policy significance. If the latter is correct, then the family backgrounds of those who govern may directly affect representation and public policy.

A growing empirical literature suggests that legislators' backgrounds can shape behavior in office, but clean causal evidence remains limited. A central challenge is that lawmakers are not randomly assigned to constituencies: the same forces that shape who gets elected may also shape how they govern once in office. Legislators' backgrounds are often entangled with party affiliation, district composition, and electoral incentives, making it difficult to isolate whether legislative behavior reflects constituency demands or the personal characteristics of officeholders. Moreover, most existing studies focus on characteristics such as race, gender, or adult socioeconomic position (e.g., Canon, 2020; Fridkin and Kenney, 2014; Carnes, 2012), although some work considers parental background, including Grumbach (2015)'s study of parental occupational background in recent Congresses. Closest to our analysis, Feigenbaum, Palmer and Schneer (2025) show that family immigration history shapes congressional voting on immigration policy, while emphasizing the difficulty of separating personal background from constituency and party forces. These considerations point toward the value of studying family origins in a policy domain where redistributive stakes are especially high. Tax policy is a natural setting because classic models of distributive politics link individuals' preferred tax rates to their positions in the distribution of capital and labor income (Alesina and Rodrik, 1994). If family origins carry persistent class-linked interests, networks, or worldviews into office, they should be especially visible on major tax legislation.

The early twentieth-century U.S. House offers a particularly useful setting for this analysis. The ratification of the Sixteenth Amendment in 1913 and the wartime revenue acts that followed transformed federal taxation, shifting national politics away from its earlier reliance on tariffs and consumption taxes and making progressive income taxation a central arena of distributional conflict in American politics. Brownlee (2004) argues that this transition "raised the stakes of conflict over tax policy," generating a sequence of controversial House revenue votes across multiple Congresses. Because revenue bills originated in the House, it stood at the center of national tax politics. This combination of substantive importance and

repeated legislative debates makes it a particularly valuable context for examining whether family socioeconomic origins shaped legislative behavior.

## 3 Data and Measurement

This section describes the data and measurement underlying our three empirical analyses. We begin by constructing a linked-census sample that identifies elected officials and recovers the family backgrounds of the households in which they grew up, which forms the basis for our descriptive analysis of representation. We then describe the linked intergenerational sample used to study whether the elimination of slave wealth after emancipation altered political entry among the children of slaveholders. Finally, we introduce the congressional data used to examine whether politicians' socioeconomic origins shaped legislative behavior, including our linked sample of Congress members, roll-call voting data, and the close-election sample used in the regression discontinuity analysis.

### *3.1 Identifying Elected Officials and Their Family Origins*

Our descriptive analysis of representation relies on a linked-census sample that identifies elected officials and recovers the family backgrounds of the households in which they grew up. Using the full-count decennial U.S. censuses from 1850 to 1940, we first restrict attention to individuals employed in public administration, excluding armed-forces occupations, and then classify as elected officials those whose occupation strings correspond to offices such as governor, mayor, senator, representative, legislator, alderman, or councilman. To account for common spelling variants and transcription errors in the historical census record, we combine exact and fuzzy string-matching procedures.[1] We code an individual as an elected official if either the exact- or fuzzy-matching procedure identifies the record.

[1]The fuzzy-matching procedure compares each occupation string to a fixed set of elected-office titles using Jaro-Winkler distance, which measures string similarity while placing additional weight on agreement at the beginning of words. Intuitively, this helps recover cases in which census enumerators or later transcribers recorded office titles imperfectly but the intended office remains clear.

We next recover the family backgrounds of these elected officials by linking each adult officeholder to earlier censuses in which he appears as a child. To do so, we use the Census Tree Project (Buckles et al., 2023), a large-scale record-linking data product that provides person-level links across decennial census waves. The Census Tree is a major breakthrough in historical record linkage because it combines genealogy-based links built with private family information with machine-learning expansion, producing over 700 million links and far higher match rates than earlier approaches.[2] In our build, we use these cross-census links to identify childhood records in which future officeholders are observed as sons living with their fathers, and we then merge in information on the father from the same household. The resulting linked sample connects each adult officeholder to the household in which he was raised and allows us to observe the socioeconomic characteristics of his father during childhood.[3] To improve match quality, we restrict attention to linked records with age profiles consistent across census waves and focus on white male officeholders for whom reliable childhood links can be constructed.

To measure family socioeconomic background, we rely on the father's predicted rank in the wealth distribution during the politician's childhood. Our approach is similar in spirit to Abramitzky et al. (2024), who proxy academics' backgrounds using the percentile rank of fathers' predicted income—constructed using the estimated relationship between income and occupation, state, age, and race in the 1940 census and then applied across historical censuses. Because direct wealth is not available on a consistent basis across all decennial census waves, we instead construct a comparable wealth-based measure by estimating, in the 1860 census, the relationship between observed wealth rank and a father's detailed occupation, state of residence, age, and broader occupation-by-region profile, and then applying

[2]The share of links in the Census Tree that are correct, as determined by agreement with human assessments based on full census records, is also very high (Buckles et al., 2023).

[3]When there are multiple observations of a son in his father's household, we construct the average of a father's predicted wealth across census years.

the resulting coefficients to fathers in all childhood census waves. We convert the fitted values into percentile ranks within each census year and use the father's nationwide predicted wealth percentile as our baseline measure in the descriptive analysis. This variable is best interpreted as a summary measure of broader family socioeconomic background, rather than as a narrow measure of material resources alone.

Our final representation sample consists of 5,369 elected officials linked to their childhood households. Appendix Table A1 reports matching rates and statistics from our approach to linking. Our coding procedure identifies 8,810 elected officials in the census files who can be potentially linked, implying an overall link-back rate of 60.9 percent.[4] The sample of elected officials begins in 1860, which is necessary to have at least one census for linking backward. The link-back rate is lower for the earliest elected officials identified, but this pattern is expected because politicians observed in the mid-19th century have a more limited set of eligible childhood census records. Reassuringly, linked and unlinked elected officials look broadly similar on basic observable characteristics in both the pooled sample and within broad periods. We provide additional discussion and details on the data construction and sample in Appendix Section B.[5]

As an additional component of our descriptive analysis, we also construct a separate linked sample of U.S. Congress members to examine whether representation differs across the two major political parties. For this exercise, we begin with congressional biography records containing party affiliation (McKibbin, 1992) and use standard name matching and manual matching methods to link members to full-count decennial census waves. We then use the same Census Tree links to identify childhood households and recover the information

[4]This overall link-back rate compares favorably to Abramitzky et al. (2024), who link 40.4 percent of their main faculty-roster sample to a childhood census overall, although there are important differences in sample construction and data sources.

[5]Appendix Figure B2 reports the distribution of elected offices in our sample. Although the sample is composed primarily of mayors (42%), it also includes a substantial number of federal officeholders, including U.S. Senators (4%) and U.S. Representatives (14%).

needed to measure family background using the father's predicted wealth rank during the politician's childhood. The resulting linked Congress sample contains 2,286 members with congressional service overlapping the 1850–1940 period and provides the basis for our party-level analysis of representation; Appendix Table A2 reports additional diagnostics on the sample construction and link rates.

Finally, complementing our basic comparisons of linked and unlinked individuals, Appendix Table A3 reports additional diagnostics of whether adults who are linked back to their childhood homes differ systematically in terms of other measures of socioeconomic status. If politicians from more advantaged families are systematically easier or harder to link backward, the linked samples could misstate the degree of class representation. For the main representation sample, the key diagnostic uses a standard surname-based SES measure that can be constructed regardless of whether an elected official links to a childhood household.[6] In the full pool of potentially linkable officeholders with available surname information, the average surname-based family SES measure is very similar: 51.2 for those who are linked back to their childhood home and 51.4 for others. As an additional test, the 1940 Census provides measures of schooling and income for adult officeholders themselves; in that sample, contemporaneous measures of SES are also very similar for linked and unlinked individuals. The same table reports analogous diagnostics for the Congress member sample, where linkage status is essentially unrelated both to surname-based SES and to pre-congress occupational standing. Taken together, these patterns suggest that differential linkage by observable socioeconomic status is limited.

[6]We construct our surname SES proxy from the full-count censuses as a surname-level socioeconomic percentile based on the average predicted wealth rank of white men with children in the same surname-year cell. More broadly, this approach is related to recent work using surname information to proxy otherwise unobserved family background; see, e.g., Feigenbaum, Palmer and Schneer (2025). For each elected official observed as an adult in the census, we determine the earliest feasible childhood census year (with implied child age 0—18), and then merge the surname SES measure using that childhood year and surname.

### *3.2 Wealth and Slaveholding Legacies*

A core feature of our analysis is our ability to study whether a large wealth shock affected political entry by comparing the later officeholding outcomes of children from slaveholding and non-slaveholding families. For this analysis, we construct new longitudinal samples of white male sons age 18 or younger who are observed living with their fathers in the full-count decennial U.S. censuses of 1850 and 1860, the pre-emancipation census waves in which fathers' slaveholding status can be measured. Because the wealth shock of emancipation was concentrated in the slaveholding South, the analysis is restricted to sons observed in households living in the South.[7] Using the digitized slave schedules from Hacker et al. (2025), we identify whether each linked father owned slaves. We then use the Census Tree links to follow sons forward to later census waves through 1930, allowing us to observe their subsequent occupational status.

Appendix Table A4 shows that the 1850 baseline cohort contains 1.39 million Southern white male sons and the 1860 baseline cohort contains 1.75 million. Of these, 856,949 sons from 1850 and 1,238,322 sons from 1860 link to at least one follow-up census, implying link rates of 62 percent and 71 percent, respectively. These linkage rates are high by current standards of historical census-linking work. For comparison, Ager, Boustan and Eriksson (2021) use a traditional automated name-based census-linking approach rather than genealogy-enhanced Census Tree links and report a 16.8 percent match rate when linking sons from 1860 to 1900. In terms of selection in our sample, son age is nearly identical across linked and unlinked individuals, and differences in father's age are also very small. Linked sons are somewhat more likely to have slaveholding fathers and somewhat higher baseline wealth percentiles.

The resulting linked sample allows us to test whether the large loss of slaveowners' wealth

[7] We include in the South all slaveholding states and Washington, DC (where slavery was legal in 1860).

after emancipation reduced the later political entry of children from these families. We merge onto each linked son information on the father from the baseline household, including age, occupation, wealth, and region of residence. Because our goal is to study political entry rather than the contemporaneous composition of officeholders, the unit of analysis is a linked son observed in childhood and then re-observed in adulthood. As in the representation analysis, we focus on white male links with age profiles that are consistent across census waves.

In addition to measuring slaveholding status in 1850 and 1860, we record the father's observed wealth in the baseline census, which allows the analysis to condition directly on the father's position in the prewar wealth distribution. We further harmonize fathers' occupations to common occupation codes and construct a parallel indicator for whether the father himself held elected office. To verify the immediate impacts of emancipation in the postbellum period, baseline fathers in the sons of slaveholders analytic sample are also linked forward to the 1870 census records which contain information on reported wealth.[8]

For sons, we measure later political entry using the same occupational coding procedures described above. Specifically, in each later census we first identify individuals employed in public administration, excluding armed forces, and then classify elected officials using a combination of exact and fuzzy occupation-string matching. The resulting stacked sample pools linked sons from 1850 and 1860 households and follows them to later censuses from 1870 to 1930.[9] We designate the final follow-up year as 1930, when the resulting linked sample remains sizable and sons of slaveholders are still observed at ages for which occupational status is informative about political entry.

[8]Appendix Table A5 reports linkage diagnostics for the father wealth-shock sample. Requiring a unique, age-consistent 1870 link with nonmissing 1870 wealth yields final linkage rates of 38.5% for the 1850 father cohort (192,123/498,659) and 50.5% for the 1860 cohort (335,892/664,905). The linkage rates for fathers are lower than the corresponding son follow-up linkage rates (62% and 71%) because we link fathers to only 1870 but link sons to censuses from 1870 to 1930. Some observable differences between linked and non-linked fathers are present but moderate in magnitude.

[9]We restrict to links with age profiles consistent across censuses and adult follow-up ages (20–79).

### *3.3 Congressional Voting and Close-Elections*

To study whether family background shapes legislative behavior, we construct a new sample of close U.S. House elections. We begin by using the Our Campaigns historical House candidates database covering elections that overlap with the years covered in our descriptive analysis and identify races in which the top two candidates are separated by no more than five percentage points (Our Campaigns, 2025). These close elections are useful for identification, but our analysis requires a measure of candidate socioeconomic background that is not directly observed in historical election data. We therefore impute family socioeconomic status using surname-based measures from the full-count censuses, indexed by a candidate's birth year. A key challenge is that birth year is not systematically recorded for losing candidates. We address this by constructing a new birth-year dataset for close-election losers using a combination of automated searches and manual web research, followed by a structured reconciliation process that resolves disagreements across sources.[10] This procedure supplies birth years for 97.3 percent of winners and runners-up in the close-election sample, allowing us to construct a consistent background measure for nearly all close-election candidates. With birth year in hand, we harmonize surnames and merge each candidate to a surname-by-cohort SES measure derived from the census (assigning candidates to the nearest available census year).[11] This yields a unified family SES measure for both narrowly elected and narrowly defeated candidates, enabling the close-election analysis.[12]

Our main outcome for this analysis is legislative voting on major federal tax legislation. We obtain roll-call voting records for members of the U.S. House from VoteView (Lewis

[10]The manual review process compares sources (e.g., Wikipedia, FindAGrave, and historical newspaper archives) and selects the birth year that best matches the candidate's name, race, and electoral context. See Appendix B for detailed discussion.

[11]We use a surname-based measure of family socioeconomic status for the close election analysis because attempting to link candidates to the census and Census Tree would result in a substantially smaller sample.

[12]In Section 6.1, we provide evidence that this surname-based measure is an informative proxy for family background in linked elected official samples where father-based SES can also be observed.

et al., 2025), which provides standardized identifiers, bill numbers, roll-call counts, and vote codes. We focus on major House tax votes between 1916 and 1940 and construct a set of roll calls on the principal revenue acts of the period. Appendix Section B lists the ten bill-level roll calls used in our analysis. These bills focus on the major revenue measures enacted after the ratification of the Sixteenth Amendment, including politically contentious episodes such as the Revenue Act of 1935, popularly known as the "Soak the Rich" tax, and the Revenue Act of 1932, which broadly raised estate and corporate taxes.[13] For each roll call, we code a binary indicator for whether the legislator cast a pro-tax vote, defining the coding so that a value of one always corresponds to support for the higher-tax position of the legislation. This produces a harmonized measure of legislative behavior across tax increases and tax reductions.

We merge the close-election sample to the roll-call voting data and restrict attention to House members who cast votes on our sample of tax bills. The resulting dataset links close electoral competition, candidate SES background, and subsequent legislative behavior. Because close elections can involve candidates with similar backgrounds, we define two RD estimation samples that require a meaningful family SES contrast between the top two candidates, using pre-specified cutoffs in the surname-based SES distribution. Appendix Table A6 reports how these restrictions shape sample size and the composition of the estimation sample.

## 4 The Family Origins of U.S. Elected Political Elites

A central question in this paper is whether elected office is disproportionately held by individuals raised in wealthy households. Our analysis relies on the linked-census sample

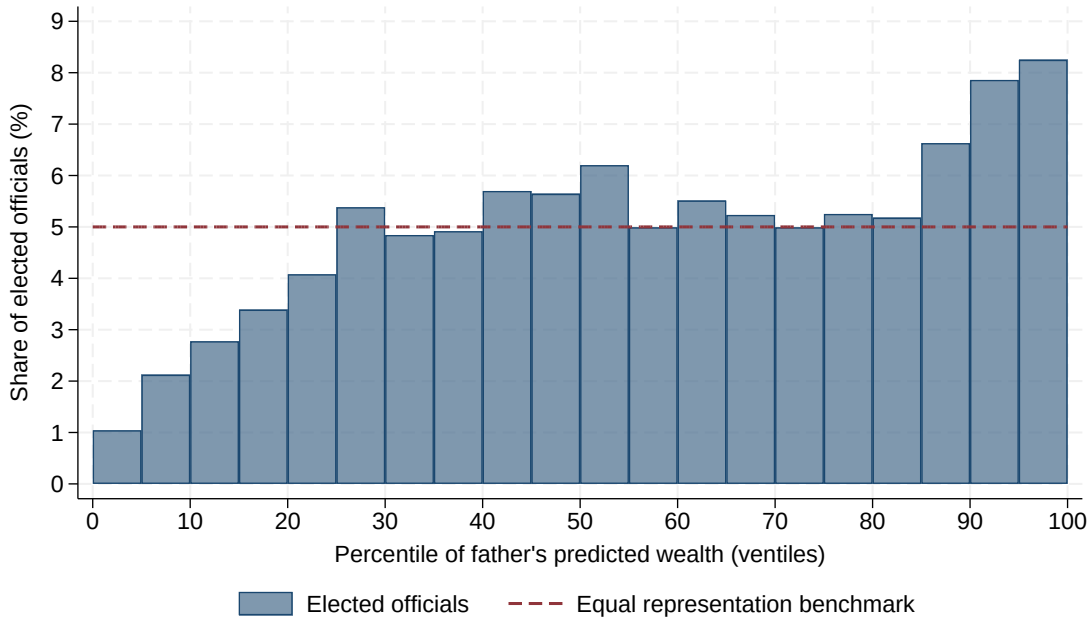
[13] We exclude several revenue bills that were either uncontested or difficult to code along a single pro-tax dimension. The Revenue Act of 1937 passed 173–0 and was enacted as a technical anti-avoidance measure, leaving no meaningful vote variation from which to infer legislators' underlying policy preferences. The Revenue Act of 1924 illustrates the second concern: it reduced income-tax rates while raising estate-tax rates and introducing the gift tax.

described above to compare the childhood socioeconomic backgrounds of elected officials to those of the broader population. Throughout, we measure family background using the father's predicted wealth rank during childhood, which should be interpreted as a summary measure of socioeconomic status, and benchmark representation against the distribution that would prevail under equal representation across family backgrounds.

Figure 1 visualizes the share of elected officials that come from each 5-percent bin of the father's predicted wealth distribution. The results show a clear skew toward more advantaged origins. If family background were unrelated to political representation, each bin would account for 5 percent of officeholders, as indicated by the dashed equal-representation benchmark. Instead, elected officials from lower socioeconomic backgrounds are underrepresented, representation is roughly proportional through much of the middle of the distribution, and then rises sharply in the upper tail. The richest ventile alone accounts for 8.3 percent of elected officials, compared with only 1.0 percent from the poorest ventile. Taken together, the figure indicates that elected office was disproportionately occupied by children from affluent households.

Our natural next questions are whether these representation gaps vary across levels of office or over the long period of time covered by our data. On the former, Panel (a) of Figure 2 splits the pooled sample of elected officials according to the level of government associated with the office in the census record, distinguishing local, state, and federal positions. A key finding is that the skew toward advantaged family origins appears at every level of the political hierarchy, indicating that the pattern in Figure 1 is not driven solely by a narrow set of especially elite posts. That said, the imbalance is most pronounced in federal office. Children from the top quintile of the family socioeconomic score distribution account for 33.7 percent of federal elected officials, compared with 26.9 percent of local elected officials and 27.9 percent in the pooled sample. Thus, advantaged family origins characterize elected office throughout the political hierarchy, but they are especially concentrated at the federal

**Figure 1:** Representation in Elected Office by Family Socioeconomic Status



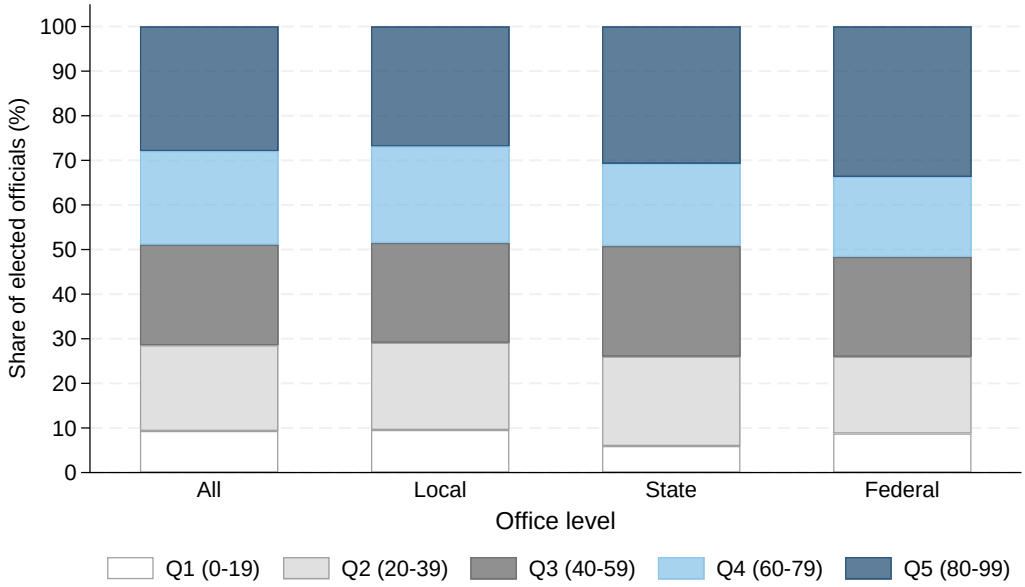

*Notes:* The figure shows the share of elected officials by ventiles of father's predicted wealth percentile (5-percentile bins). Bars report each ventile's share among elected officials. The dashed horizontal line marks the equal-representation benchmark of 5 percent per ventile. The sample consists of linked elected officials with non-missing father wealth rank in the analysis sample.

level.

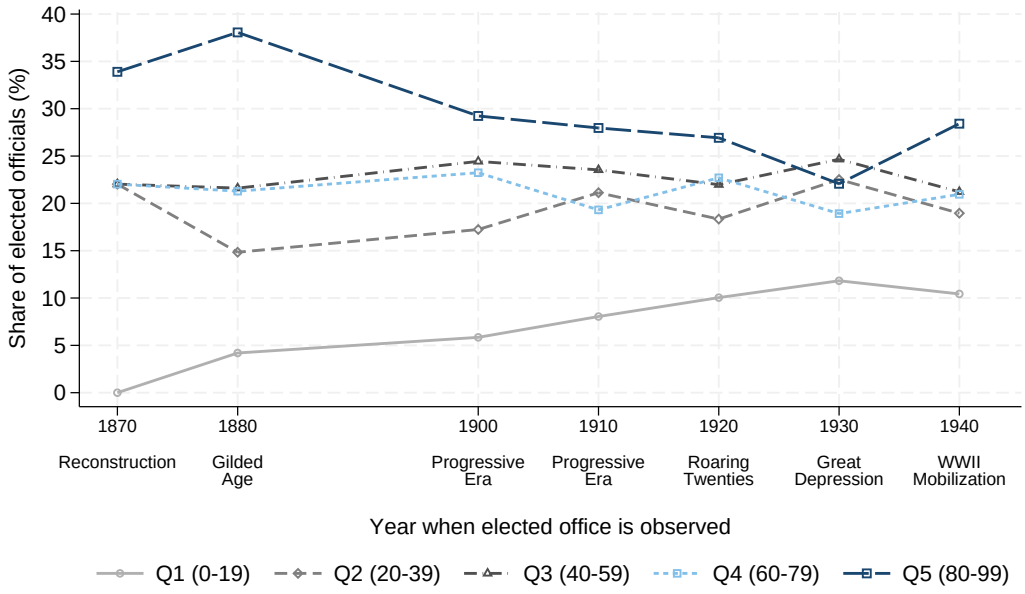
On the latter question, Panel (b) documents how these representation gaps have varied over time. The figure plots, by census year of observation, the share of elected officials drawn from each quintile of the father's predicted wealth distribution. The broad pattern is one of persistence rather than convergence. In every year, the bottom quintile remains well below the 20 percent share that would prevail under equal representation, and the top quintile consistently exceeds the bottom quintile (and is the largest quintile in every year except one). There is some movement across years, especially in the early part of the sample where the number of observed elected officials is limited, but the data show no sustained shift toward parity across family backgrounds. Even in 1940, 28.4 percent of elected officials came from the top quintile, compared with only 10.4 percent from the bottom quintile. The

**Figure 2:** Representation Across Office Levels and Over Time

**(a)** Representation Across Office Levels




**(b)** Representation Over Time in Elected Office

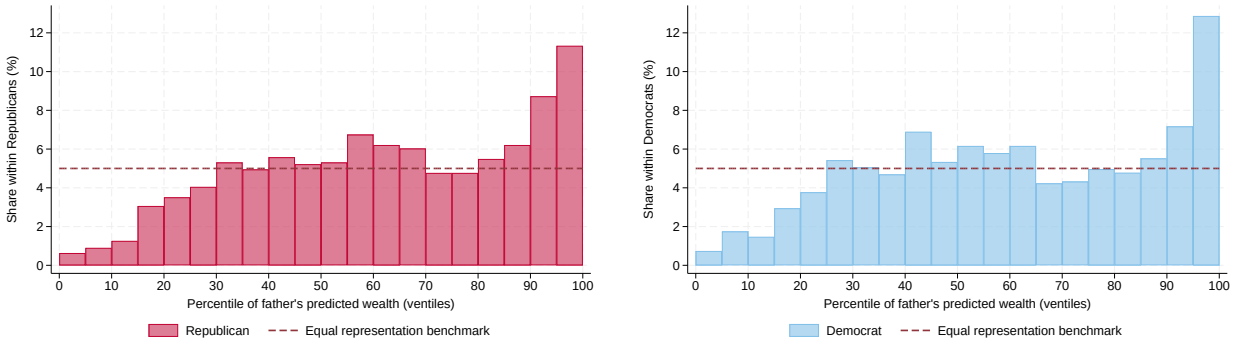



*Notes:* Panel (a) reports the composition of elected officials by father wealth quintile for four office categories: all elected offices, local offices, state offices, and federal offices. Office levels are identified by census industry codes. Each stacked bar sums to 100 percent within each office category. Panel (b) plots, by Census year of observation, the share of elected officials from each father-wealth quintile. Shares in panel (b) sum to 100 percent within each year. Quintiles are defined from father's predicted wealth percentile: Q1 = 0–19, Q2 = 20–39, Q3 = 40–59, Q4 = 60–79, and Q5 = 80–99.

overrepresentation of children from advantaged households was therefore not confined to a particular historical moment; it remained a durable feature of elected office throughout the period we study.

Finally, we examine whether the skew toward advantaged family origins differs across the two major U.S. political parties. For this analysis, we rely on our sample of linked Congress members, for whom party affiliation can be observed. Figure 3 shows that the pattern is strikingly similar across Democrats and Republicans, particularly at the upper tail.[14] Politicians from families in the top 10 percent of the family SES distribution account for about 20 percent of both Democratic and Republican membership, while the bottom 10 percent accounts for only 2.5 and 1.5 percent. In sum, the overrepresentation of children from advantaged households does not appear to be a distinctly partisan phenomenon in congressional officeholding.

**Figure 3:** Representation Across Father Predicted Wealth Ventiles by Party




**(a)** Republicans **(b)** Democrats

*Notes:* This figure shows the distribution of linked Congress members across ventiles of father's predicted wealth, separately by party. Panel A reports Republicans and Panel B reports Democrats. The dashed horizontal line marks the equal-representation benchmark (5 percent in each ventile).

[14] Appendix Figure A1 reports the analogous ventile distribution after pooling Democratic and Republican members of Congress.

## 5 Wealth Shocks and the Persistence of Elite Political Entry

Section 4 shows that children from advantaged households have been persistently over-represented in U.S. elected office. In this section, we provide new evidence on the durability of this pattern by studying the effects of a sharp shock to one component of the bundle of advantages that affluent families typically transmit. Family socioeconomic background includes financial resources alongside social networks, local standing, political traditions, and family-transmitted know-how. If political access depends exclusively on the financial component, a large destruction of elite material wealth should reduce the ability of privileged families to place their children into political life. If political access depends instead on the broader bundle, descendants of elite families should continue to enter office at elevated rates (and may even accelerate to compensate) when financial wealth is sharply reduced. We examine this question using the emancipation of slaves as a sharp historical test. Enslaved people constituted a large share of Southern elite wealth, and emancipation abruptly eliminated slave ownership. Therefore, the postbellum period provides a rare opportunity to test whether a major decline in elite material resources limited political entry among the children of slaveholding families relative to comparably wealthy families that did not hold slaves.

Before turning to sons' political entry, we first verify that the linked families in our sample experienced the expected post-emancipation wealth shock. Specifically, we estimate regressions of fathers' 1870 wealth on an indicator for baseline slaveholding:

$$f\big(\mathit{Wealth}_i^{1870}\big) = \beta\, \mathit{FatherSlaveholder}_i + \alpha_{b(i)} + \eta_{p(i)} + \mu_{o(i)} + \delta_{a(i)} + \varepsilon_i, \tag{1}$$

where $f(\mathit{Wealth}_i^{1870})$ is a function of father $i$'s wealth in 1870, $\mathit{FatherSlaveholder}_i$ indicates whether the father owned slaves in the baseline census, $\alpha_{b(i)}$ are baseline census year fixed effects for $b(i) \in \{1850, 1860\}$, $\eta_{p(i)}$ are fixed effects for the father's baseline wealth percentile, $\mu_{o(i)}$ are fixed effects for the father's baseline occupation, and $\delta_{a(i)}$ are fixed effects for the

father's age in 1870.[15] We estimate this regression only among fathers living in the South in the baseline year. Although most fathers have positive wealth in 1870, a non-negligible share are at zero; we therefore report results when the dependent variable is $\ln(Wealth)$, which drops observations with zero wealth in 1870, and alternative transformations that retain the full sample—$\ln(1 + Wealth)$, $\text{asinh}(Wealth)$, and a Poisson specification where the dependent variable is *Wealth* and the explanatory variables are modeled using an exponential conditional mean function.

Appendix Table A7 shows a clear first-stage wealth shock among slaveholding fathers in the South. When focusing on the 88 percent of fathers with non-zero wealth in 1870, slaveholders have wealth that is 7 log points lower than non-slaveholders who were comparable at baseline (columns 1 and 2). When also considering extensive margin changes, which are captured in the various specifications in columns 3–8, the decrease in wealth is even larger, ranging from 12–16 log points. As a whole, the magnitudes are broadly consistent with prior evidence on the immediate wealth shock generated by emancipation: in a comparison of equally wealthy Southern households, Ager, Boustan and Eriksson (2021) show that households with up to eight slaves in 1860 held 8–25 log points less wealth by 1870 relative to one-slave households, with even larger losses among the largest slaveholders. Our estimate for baseline slaveowners is therefore of a similar order of magnitude, while naturally smaller than the largest intensive-margin effects in prior literature. Taken together, these estimates confirm a sizable immediate loss of material resources in the same families used for the intergenerational analysis. Having established this father-level wealth shock, we next ask whether it translated into lower political entry among sons.

To study this margin, we turn to the slaveholding sample introduced in Section 3.2, which links sons observed in 1850 and 1860 households living in the South to later censuses

[15] Because measures of wealth are available in the 1850 and 1860 census, we do not need to use predicted wealth to measure SES for this analysis.

and records whether their fathers were slaveowners. Because the representation patterns in Section 4 are shaped by which families are able to send children into office, political entry is the natural outcome for this analysis. We therefore estimate whether sons of slaveowners were less likely to be observed later in life as elected officials than sons from non-slaveholding families with similar baseline circumstances. Our specifications progressively condition on whether the father himself held elected office, the father's position in the antebellum wealth distribution, his occupation, and census-year fixed effects. This design is closely related to Ager, Boustan and Eriksson (2021), who use emancipation to study intergenerational economic recovery. Our focus, however, is on whether this sharp loss of wealth altered access to elected office in the next generation.

Formally, we estimate regressions of sons' later entry into elected office on an indicator for whether their father was a slaveowner:

$$Elected_i = \beta\, FatherSlaveholder_i + \gamma\, FatherElected_i + \alpha_{b(i)} + \lambda_{t(i)} + \eta_{p(i)} + \delta_{a(i)} + \mu_{o(i)} + \varepsilon_i, \quad (2)$$

where $Elected_i$ is an indicator equal to one if son $i$, observed with his father in baseline census year $b(i) \in \{1850, 1860\}$ with father's baseline wealth percentile $p(i)$, is later observed as an elected official. The term $FatherSlaveholder_i$ indicates whether the father owned slaves, $FatherElected_i$ indicates whether the father himself held elected office in the baseline year, $\alpha_{b(i)}$ are baseline-year fixed effects, $\lambda_{t(i)}$ are fixed effects for the post-childhood census year in which the son is observed, $\eta_{p(i)}$ are fixed effects for the father's baseline wealth percentile, $\delta_{a(i)}$ are fixed effects for bins of the age of the son in census year $t(i)$, and $\mu_{o(i)}$ are fixed effects for the father's baseline occupation.[16]

Table 1 reports the main results. Across both specifications, the estimated relationship between having a slaveholding father and later entry into elected office is positive and pre-

[16]For many sons, we have multiple linked observations between 1870 and 1930. We keep all available observations for these sons and cluster standard errors by father.

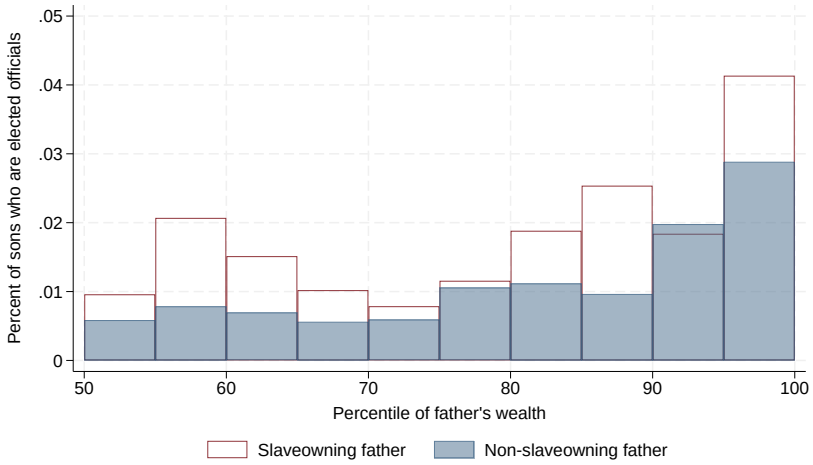
**Table 1:** Slaveholding and Son Entry into Elected Office

| | (1) | (2) |
|---|---|---|
| Father slaveholder | 0.008*** | 0.007*** |
| | (0.002) | (0.002) |
| Father elected official | 0.003 | 0.003 |
| | (0.004) | (0.004) |
| Observations | 5,354,198 | 5,354,196 |
| Mean dep. var. | 0.011 | 0.011 |
| Baseline and follow-up year FE | Yes | Yes |
| Father baseline wealth percentile FE | Yes | Yes |
| Son age-bin FE | Yes | Yes |
| Father baseline occupation FE | No | Yes |

*Notes:* This table estimates political entry among sons of slaveholding and non-slaveholding fathers in the South. The sample consists of white male sons observed with fathers in the 1850 or 1860 census and linked forward to adult census observations through 1930. The dependent variable is an indicator for being an elected official in the follow-up census year. The key regressor is an indicator that the father was a slaveowner in the baseline census. Columns differ by included fixed effects, as indicated in the table. There are separate son age bins for each five-year interval. The regression sample contains 2,095,271 unique sons; because sons can appear in multiple follow-up census years, observations are at the son-by-baseline-year-by-follow-up-year level and therefore exceed the number of unique sons. Standard errors are clustered at the father level and reported in parentheses. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

cisely estimated. In column (1), which conditions on baseline and follow-up year fixed effects, the father's prewar wealth percentile, son age fixed effects, and whether the father himself held elected office, sons of slaveowners are 0.8 percentage points more likely to be observed later as elected officials. In column (2), after adding father-occupation fixed effects, the estimate is very similar. These magnitudes are sizable relative to the underlying rate of political entry: the dependent-variable mean is 1.1 percent, so the estimates imply that sons of slaveowners were about 64 percent more likely to enter elected office, even after conditioning on rich measures of prewar status. Moreover, the estimated relationship with father's slaveowner status is over twice as large as the relationship with father's elected official status, which is not a statistically significant predictor of sons' entry into elected office in these specifications. Rather than showing that the destruction of slave wealth reduced political entry, Table 1 indicates that sons of slaveowners were substantially more likely to enter elected office.

**Figure 4:** The Relationship between Elected Office and Father Slaveownership, by Wealth Bin and Census Year




**(a)** By Father Wealth Bin


**(b)** By Census Year

*Notes:* Panel (a) plots the probability that a son is later observed as an elected official, separately by whether his father was a slaveowner. The sample is restricted to sons whose fathers fall between the 50th and 100th percentiles of the baseline wealth distribution, grouped into 5-percent bins. The non-slaveowner series is the raw bin mean within this sample. The slaveowner series is interaction-adjusted: for each wealth bin, it equals the non-slaveowner bin mean plus the estimated bin-specific coefficient on Father slaveowner from an interacted model that regresses the indicator for elected office on wealth-bin indicators interacted with $FatherSlaveholder_i$, controlling for whether the father himself held elected office, father baseline wealth percentile fixed effects, baseline-year fixed effects, follow-up-year fixed effects, son age-bin fixed effects, and father-occupation fixed effects. The sample pools sons observed in 1850 and 1860 households and follows them to later censuses from 1870 through 1930. Panel (b) reports estimates from re-estimating the wealth-fixed-effects specification separately by the later census year in which sons are observed. Each point is the coefficient on $FatherSlaveholder_i$ from a regression of sons' later entry into elected office on an indicator for whether the father was a slaveowner, controlling for whether the father himself held elected office, fixed effects for the father's baseline wealth percentile, father-occupation, the baseline census year, and son age-bin.

Panel (a) of Figure 4 explores whether these effects are apparent throughout the distribution of family wealth. The figure plots average rates of elected officeholding for sons of non-slaveowning fathers in blue. For sons of slaveowning fathers, we plot the sum of these means and the estimated impact of having a slaveowning father for each ventile of the wealth distribution.[17] Within nearly every 5-percent bin of the father's baseline wealth distribution in the upper half of the sample, sons of slaveowners are more likely to be observed later as elected officials than sons of non-slaveholding fathers. This pattern shows that the positive coefficients in Table 1 capture a broad response across the distribution of wealthy families. Consistent with the main interpretation of this section, the destruction of slave wealth did not sever elite families' access to elected office, even at the top of the prewar wealth distribution where the shock to financial resources was largest.

The dynamics of these estimates also reinforce the interpretation of our results so far: political entry appears to have been remarkably insulated from the destruction of slave wealth. Panel (b) of Figure 4 reports estimates from re-estimating our wealth-fixed-effects specification separately by the later census year in which sons are observed. The coefficient on $FatherSlaveholder_i$ is positive in every postbellum census year a child is observed in adulthood and statistically distinguishable from zero in 1870 and 1880. This pattern contrasts with Ager, Boustan and Eriksson (2021), whose central result is that emancipation generated a large immediate wealth shock for slaveholding families, followed by substantial economic recovery across subsequent generations. Though their findings are consistent with our first stage, we do not see an analogous initial postbellum collapse in political entry. If anything, our estimates suggest that access to elected office was even more insulated from the emancipation shock than economic status itself: descendants of slaveowners retained elevated entry into

[17] These estimates come from a model that regresses the indicator for elected office on wealth-bin indicators interacted with father slaveownership, controlling for whether the father himself held elected office, baseline-year fixed effects, follow-up-census-year fixed effects, father-occupation fixed effects, father baseline wealth percentile, and son age-bin fixed effects. Appendix Figure A2 shows that unadjusted results are nearly identical.

elected office from the first postbellum census onward.

Overall, these results suggest that the loss of financial wealth experienced by slaveholding families did not limit their children's ability to enter elected office. Instead, the elevated rates of office holding among slaveowners' sons suggest that elite families drew on other forms of advantage—including social networks, local standing, and political connections—to sustain access to political life even as their material resources were sharply reduced, consistent with theoretical accounts of elite persistence through de facto political power (Acemoglu and Robinson, 2008). An important caveat is that we cannot fully rule out the possibility that children of slaveowners were especially motivated to enter elected office after the Civil War. Even allowing for this possibility, our evidence suggests that financial wealth is not the sole factor explaining why children of affluent families are overrepresented in elected office.

**Robustness Checks:** Appendix Table A8 shows that the relationship between father slaveownership and sons' later entry into elected office is stable across a range of alternative specifications. First, we consider exercises aimed at the construction of the stacked son-follow-up sample. Weighting each son-follow-up observation by the inverse number of total follow-up observations for the son (so that each son gets equal weight) and, alternatively, restricting the sample to each son's first observed adult follow-up census year both yield estimates that remain positive, statistically significant, and close to the benchmark specification. Second, we show that the result is likewise robust to adding baseline state fixed effects and baseline father age-bin fixed effects. The coefficient on having a slaveowning father changes little relative to the benchmark, remaining tightly clustered across specifications. Third, estimates are also similar when running separate regressions for sons who are observed in the 1850 or 1860 census. Taken together, these results reinforce the conclusion that the elevated political entry of slaveholders' sons is not an artifact of panel construction, broad cross-state differences, or simple differences in fathers' lifecycle stage at baseline.

## 6 Legislative Voting, Close Elections, and the Causal Effect of Family Origins

So far, the results demonstrate that children from advantaged households were persistently overrepresented in U.S. elected office and that this advantage survived even a large destruction of elite wealth. We now ask whether these inequalities in political entry also shaped how officeholders exercised power once in office. This question is central to the broader significance of our results, because unequal access to elected office matters most if politicians from different family backgrounds make systematically different policy choices. We examine this issue by studying major tax legislation in the U.S. House, where roll-call voting provides a transparent measure of legislative behavior.

Our analysis relies on a close-election approach that compares otherwise similar districts in which a candidate from a more advantaged family background narrowly wins rather than narrowly loses a U.S. House election. This design is useful because the main empirical challenge in studying whether family background shapes legislative behavior is the selective nature of political representation. Legislators from more advantaged families are not randomly assigned to the districts they represent, and those districts may themselves differ systematically in their preferences over redistribution and the role of government. Candidates from different family backgrounds may also differ along other politically relevant dimensions, making simple comparisons across legislators difficult to interpret. Our close election approach helps isolate variation in the family background of the representative from broader differences in constituency demand and electoral selection.

As previewed in Section 3, we construct the close election sample using the Our Campaigns universe of historical U.S. House races, focusing on elections in which the top two candidates were separated by no more than five percentage points. Birth year is already available for most candidates in these close races, but missing information for some narrowly defeated candidates required additional data collection. To fill this gap, we assembled

a new birth-year dataset from multiple independent source streams and reconciled discrepancies across them in a structured way. As a result, birth year is observed for 97.3 percent of candidate-election observations in the close-election sample. Once birth year is in hand, we assign each candidate to a surname-by-cohort socioeconomic-status wealth score derived from the full-count censuses, which places narrowly elected and narrowly defeated candidates on a common background scale and allows us to classify candidate family wealth consistently within close-election contests.

To sharpen the design, we restrict attention to close elections in which the top two candidates differ meaningfully in surname-based family wealth. We implement this restriction using two pre-specified contrast definitions that generate distinct samples of close elections. Our preferred approach defines the higher-SES candidate as having a surname-based SES score above the 60th percentile and the lower-SES candidate as being below the 40th percentile. As an alternative, we use a tercile split that defines the higher-SES candidate as above the 66th percentile and the lower-SES candidate as below the 33rd percentile. Elections that do not satisfy a given contrast rule are excluded from the corresponding estimation sample. This step sharpens the interpretation of the design by ensuring that crossing the electoral threshold corresponds to a discrete shift in representative background, while also making clear the tradeoff between a sharper first stage and sample size. Appendix Table A6 reports the resulting implications for the election level samples.

The final estimation sample is formed by carrying the winners from these close-election contests forward to their subsequent House tax votes. For each election in the estimation sample, we retain the winning candidate and merge that representative to roll-call votes on the ten major tax bills in our sample.[18] This produces a representative-by-bill panel in which the outcome records whether the elected legislator cast a pro-tax vote. The running variable

[18] As discussed in Section 3, we focus on major House tax votes between 1916 and 1940 after the ratification of the Sixteenth Amendment, which transformed federal taxation by reducing its reliance on tariffs and consumption taxes and increasing the role of income taxes.

is the high-family-SES candidate's vote share in the underlying House race, and treatment occurs when this high-SES candidate narrowly wins rather than loses. The design therefore compares districts on either side of the zero-margin threshold that are similar in electoral conditions but differ discretely in the socioeconomic background of the representative whose subsequent voting is observed.

Formally, we use the representative-by-bill sample and estimate a standard regression discontinuity specification:

$$ProTaxVote_{ib} = \alpha + \theta\, HighSESWin_{e(i)} + f(Margin_{e(i)}) + \varepsilon_{ib}, \tag{3}$$

where $i$ indexes winning representatives drawn from the close-election sample, $b$ indexes the major House tax bills in our sample, and $e(i)$ denotes the election in which representative $i$ won. The outcome, $ProTaxVote_{ib}$, is an indicator equal to one if representative $i$ casts a pro-tax vote on bill $b$. $HighSESWin_{e(i)}$ equals one if the higher-SES candidate won the underlying House election, while $Margin_{e(i)}$ denotes that candidate's vote margin over the lower-SES opponent, centered at the zero threshold that determines electoral victory. The function $f(Margin_{e(i)})$ allows the relationship between the running variable and voting behavior to vary flexibly on either side of the cutoff. The coefficient of interest, $\theta$, therefore captures the discontinuous change in pro-tax voting at the threshold where the higher-SES candidate narrowly wins rather than narrowly loses. Given the bill-panel structure, we cluster standard errors at the representative level.

Before turning to the results, it is useful to clarify what effects our close-election RD design can identify. At the cutoff where the higher-family-SES candidate narrowly wins rather than loses, the socioeconomic background of the winning representative shifts sharply by construction. In our preferred sample, defined by elections in which the higher-SES candidate lies above the 60th percentile of the surname-based SES distribution and the lower-SES

candidate lies below the 40th percentile, the SES of the winning candidate increases discontinuously. At the same time, this design does not necessarily compare two politicians who differ only in family background. If candidates from more and less advantaged family origins also differ along other personal or political dimensions, the treatment may in principle bundle a shift in family-origin SES with shifts in other characteristics of the winning representative. Panel A of Appendix Table A9, however, shows little evidence of such bundled discontinuities in the observed personal characteristics we examine. Age at first Congress, college degree attainment, Ivy League school attendance, pre-congress occupation scores, and Southern birth all remain balanced across the cutoff, and the joint tests for Panel A fail to reject balance in both the main and restricted windows. The balance on pre-congress occupational scores is especially important given that prior work shows that politicians' adult occupations and class backgrounds differ sharply from those of the general population and can predict policy behavior (e.g., Carnes, 2012, 2013). Our RD result is therefore not simply a comparison between representatives from different occupational strata. Instead, within close elections among candidates with similar observed occupational standing, the discontinuity is in the socioeconomic background of the family from which the representative comes. As a whole, the balance results suggest that threshold crossing primarily changes the family-origin SES of the winner, rather than generating a broader discontinuous shift in the observed personal profile of the elected representative.[19]

A separate question is whether elections on either side of the cutoff appear locally comparable. Standard RD diagnostic checks support that interpretation. Appendix Figure A3

[19]One political characteristic that merits particular caution is party affiliation of the winner. In the main 5 percentage-point window, we do *not* detect a statistically distinguishable discontinuity in whether the winning candidate is a Democrat, though in the tighter 4 percentage-point window the estimates are negative and statistically significant (see Appendix Table A10). This pattern suggests that partisan identity may be one component of the treatment bundle in some specifications. However, the results in Appendix Table A11 show that controlling for whether the winner is a Democrat attenuates the estimated effect of a high-SES victory on pro-tax voting but does not eliminate it. We therefore interpret the RD as identifying the effect of electing a representative from a higher-SES background, while recognizing that party may also shift in some specifications.

shows that the distribution of the running variable is smooth through the threshold, and robust McCrary density tests fail to reject continuity at the cutoff, with $p$-values of 0.989 in the preferred sample and 0.579 in the tercile sample. Panel B of Appendix Table A9 likewise shows no evidence of discontinuities in the election- and district-level characteristics we examine. The mean predicted family wealth of the two candidates, the number of tax bills linked to the race, district population, Black population, foreign-born population, demographic shares, and Southern location are all balanced across the threshold, and the joint tests for Panel B and for all covariates combined are far from conventional significance levels for both bandwidth choices. These patterns are consistent with the identifying assumption that close races just above and below the threshold are similar in observed electoral and constituency conditions, while differing discretely in the family background of the representative whose subsequent tax voting is observed.

**Table 2:** RD Estimates: SES Gap and Pro-Tax Voting

| | SES Gap | | Pro-Tax Vote (Bill Level) | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High SES win | 22.5*** | 23.7*** | -0.278** | -0.369*** | -0.282*** | -0.390*** |
| | (3.9) | (4.1) | (0.108) | (0.120) | (0.107) | (0.119) |
| Mean dep. var. | -11.9 | -12.5 | 0.731 | 0.720 | 0.731 | 0.720 |
| N | 448 | 363 | 448 | 363 | 448 | 363 |
| Model | Linear | Linear | Linear | Linear | Linear | Linear |
| Bandwidth | Within 5 pp | Within 4 pp | Within 5 pp | Within 4 pp | Within 5 pp | Within 4 pp |
| Controls | No | No | No | No | Yes | Yes |

*Notes:* This table reports linear RD estimates for the predicted family wealth percentile winner-loser gap (first stage) and pro-tax voting outcomes in the bill-level sample. Reported coefficients are threshold-crossing effects of a high-SES candidate victory. Columns (1)–(2) use SES gap as the dependent variable; columns (3)–(6) use pro-tax voting. Columns (1), (3), and (5) are estimated within 5 percentage points of the threshold, while columns (2), (4), and (6) use the restricted bandwidth within 4 percentage points. Columns (1)–(4) are estimated without additional covariates; columns (5)–(6) add winner-level controls: age at first Congress, no-college indicator, Ivy indicator, pre-congress occupation score, and an indicator for being born in the South. The Mean dep. var. row reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Standard errors are clustered at the representative level. The sample is restricted to close House elections in which the higher-SES candidate has a surname-based family predicted wealth above the 60th percentile and the lower-SES candidate is below the 40th percentile.

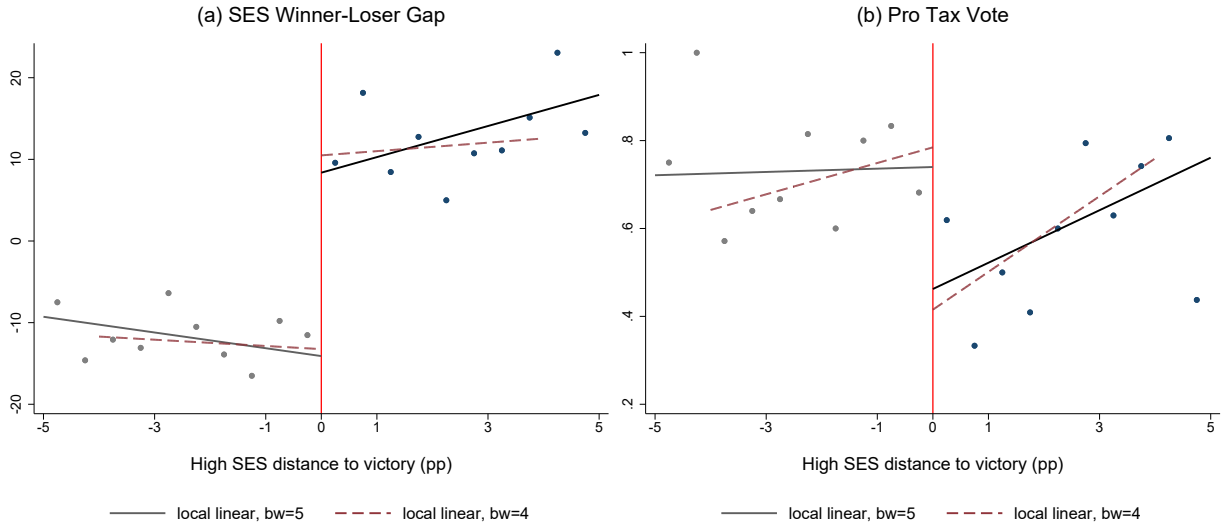
Table 2 reports the main regression discontinuity estimates from equation (3). Columns

(1) and (2) reveal a strong first stage: when the higher-SES candidate narrowly wins rather than loses, the SES gap between the winning and losing candidate rises discontinuously by 22.5 to 23.7 percentile points at the cutoff. Columns (3) and (4) then show the corresponding reduced-form effect on legislative behavior. In the initial specifications, districts that narrowly elect the higher-SES candidate are 27.8 to 36.9 percentage points *less* likely to cast a pro-tax vote on the major House tax bills in our sample. These effects are economically large relative to the mean pro-tax voting rate in the estimation sample, which ranges from 73 to 72 percent. The results are also stable when we add the observed winner characteristics discussed above. Columns (5) and (6) show that including controls for age at first Congress, college background, Ivy attendance, pre-congress occupational status, and Southern birth leaves the estimates essentially unchanged. Taken together, Table 2 indicates that close victories by higher-family-SES candidates generate a sharp shift in representative background and are associated with substantially lower support for the higher tax positions on major legislation.

Figure 5 provides a graphical summary of these results. Panel (a) shows a clear discontinuity in the SES gap between the winner and the runner-up at the electoral threshold, illustrating the sharp first stage generated by the design. Panel (b) shows the corresponding reduced-form relationship for legislative voting: just to the right of the cutoff, where the higher-SES candidate narrowly wins, the probability of a pro-tax vote is visibly lower than just to the left, where that candidate narrowly loses. The fitted lines on either side of the threshold reinforce the same conclusion as the regression estimates in Table 2. The visual evidence therefore aligns closely with the formal RD results, showing that threshold-crossing victories by candidates from more advantaged family backgrounds are followed by less support for taxation.

Taken together, these results suggest that the overrepresentation of children from advantaged households documented earlier in our representation analysis had substantive policy consequences. In one of the clearest distributional conflicts of the early twentieth century,

**Figure 5:** RD Evidence of the Effects of Representative SES Background




*Notes*: This figure reports regression discontinuity evidence from the close-election design. The running variable is the high-SES candidate's distance to victory in percentage points, centered at zero. Panel (a) plots the signed difference in surname-based predicted family wealth between the winner and the runner-up; positive values indicate that the higher-SES candidate won the election. Panel (b) plots an indicator equal to one if the elected representative casts a pro-tax vote on a major House tax bill. Dots denote binned means. The solid and dashed lines report local linear fits estimated separately on each side of the cutoff using bandwidths of 5 and 4 percentage points, respectively. The sample is restricted to close House elections in which the higher-SES candidate has predicted family wealth above the 60th percentile and the lower-SES candidate is below the 40th percentile. Panel (a) shows that crossing the electoral threshold generates a discrete shift in the socioeconomic background of the representative who takes office; panel (b) shows the corresponding reduced-form relationship for subsequent pro-tax voting.

districts that narrowly elected representatives from more advantaged family backgrounds were subsequently represented by legislators who were less supportive of higher taxation. More broadly, these findings imply that unequal access to office mattered not only for who governed, but also for whose interests public policy served. They also reinforce the view, emphasized in Section 2, that legislators' own backgrounds can shape behavior once in office rather than merely reflecting constituency demands (Carnes, 2012). Relatedly, and in the spirit of Feigenbaum, Palmer and Schneer (2025), our results show in another policy-relevant domain that family background can matter for congressional voting. The fact that we find this pattern in taxation—rather than immigration—suggests that the policy consequences

of unequal political representation may extend beyond any single issue area.

### *6.1 Additional Robustness Exercises and Validity of Surname SES Measure*

This subsection reports results from exercises that provide further evidence on the robustness of the RD findings and the validity of our surname-based approach to measuring family SES. First, we assess whether the RD results are sensitive to the way the SES contrast is defined and to the functional form used for the running variable. As noted above, our main design restricts attention to close House elections in which the higher-SES candidate lies above the 60th percentile of the surname-based predicted family wealth distribution and the lower-SES candidate lies below the 40th percentile. Our alternative is a tercile-based sample that retains only elections in which the higher-SES candidate falls in the top tercile and the lower-SES candidate falls in the bottom tercile. This restriction narrows the set of eligible close elections and therefore reduces the tercile-based bill-level sample size.

Appendix Table A12 shows that balance remains strong in this alternative sample. We continue to find no detectable discontinuities in observed winner characteristics or election- and district-level covariates in both bandwidth choices. Appendix Table A13 then shows that the main RD results are similar, with an even larger first stage, as expected under this sharper contrast definition. In the tercile sample, a high-SES victory increases the winner-loser SES gap by 27.9 percentile points in the 5 percentage-point window and by 29.9 percentile points in the 4 percentage-point window. The corresponding reduced-form estimates remain large and negative: districts that narrowly elect the higher-SES candidate are 28.3 to 39.9 percentage points less likely to cast a pro-tax vote in specifications without controls, and 31.0 to 43.1 percentage points less likely when winner characteristics are included as controls. These results indicate that the substantive conclusions of the RD analysis are not specific to the preferred family SES contrast in our main sample.

We next consider the sensitivity of the results to the functional form imposed on the

running variable. Appendix Table A14 returns to the main sample of close elections based on the 60th and 40th percentile SES thresholds and presents results from linear and quadratic polynomials within the 5 percentage-point bandwidth. The first stage remains strong under the more flexible specification, with a threshold-crossing effect of 26.3 percentile points on the SES gap in the quadratic model. The reduced-form results are likewise similar to the baseline estimates. In the quadratic specification, crossing the electoral threshold lowers the probability of a pro-tax vote by 39.6 percentage points without controls and by 44.1 percentage points with controls. Taken together, these exercises show that the RD findings are robust both to sharpening the SES contrast used to define treatment and to allowing a more flexible relationship between the running variable and legislative voting.

Second, because the close-election analysis relies on a surname-based proxy to measure family SES for both winning and losing candidates, we assess the validity of this proxy in linked samples where elected officials can be matched to their fathers and predicted family wealth can be observed directly. In the main linked elected-official sample, we merge surname-based predicted family wealth for 90.7 percent of linked elected officials, and Panel (a) of Appendix Figure A4 shows a clear positive relationship between the surname-based measure and the linked-father SES measure used elsewhere in our main analysis. The relationship is close to monotonic across the distribution, and a one-percentile increase in surname-based SES is associated with roughly a 0.72-percentile increase in linked-father SES. Reassuringly, a similar upward-sloping pattern also appears in the linked Congress sample that is closest to the close-election setting. Panel (b) of Appendix Figure A4 shows that members of Congress with higher surname-based SES also tend to have higher linked-father SES, although the relationship is somewhat attenuated relative to the broader elected-official sample. Taken together, these exercises indicate that surname-based SES is an informative proxy for politicians' family socioeconomic origins in the samples relevant for our analysis.

## 7 Conclusion

This paper returns to a longstanding concern in democratic politics: that political power may become concentrated among those advantaged by wealth and birth (Jefferson, 2010; Mosca, 1939). Recent academic work suggests that this concern remains politically salient today: voters care about candidates' class roots, often treating family origins as meaningful for representation (Vivyan et al., 2020). Yet, whether the concerns over representation have been borne out in the composition and behavior of elected officials remains less clear. Prior work documents the economic and occupational advantages of politicians as adults (Carnes and Lupu, 2023) and the self-perpetuation of political dynasties (Dal Bó, Dal Bó and Snyder, 2009), but systematic evidence on how family wealth translates into political office-holding remains limited. We address this gap by studying who enters elected office, whether elected official status survives a major wealth shock, and whether family origins shape legislative behavior once politicians govern.

Our first set of results shows that elected office was sharply skewed toward children from affluent households. Linking adult officeholders to their childhood households, we find that children from the top quintile of the family-background distribution accounted for roughly three times the share of officeholders as children from the bottom quintile. This overrepresentation appears across the political hierarchy, is especially pronounced in federal office, persists across the long period covered by our data, and is similar across the two major parties in Congress. Unequal political access was therefore not confined to political dynasties or a small set of famous families; advantaged family origins were a broad and persistent feature of the American political class. In this respect, U.S. elected office resembled other elite domains in which childhood socioeconomic advantage shapes access and advancement, including innovation, science, academia, and economics (e.g., Bell et al., 2019; Stansbury and Schultz, 2023; Airoldi and Moser, 2024), and it differed markedly from the more-equal

representation seen among politicians in Sweden (Dal Bó et al., 2017).

Our second set of results shows that the political advantage of affluent families did not depend narrowly on material wealth. Emancipation provides a sharp test because enslaved people constituted a central asset of the Southern elite, and their emancipation generated a large, concentrated wealth shock among slaveholding families. In our linked sample, we confirm that slaveholding fathers experienced substantial postwar wealth losses, but we find no corresponding decline in their sons' later political entry. To the contrary, sons of slaveowners were substantially more likely to enter elected office than sons of similarly wealthy non-slaveholding fathers, even after conditioning on fathers' prewar wealth, occupation, and elected-office status, and even in census years when slaveholding families' economic standing remained depressed. This result echoes recent evidence that elite families can persist despite revolutionary attempts to compress wealth, education, and formal status hierarchies (Alesina et al., 2020). Our contribution is to show that such persistence extended to political officeholding: the destruction of slave wealth did not sever elite access to political life, suggesting that family status, social networks, and local authority could sustain political advantage even after a major loss of material resources.

Our final analysis shows that family origins mattered not only for who entered office, but also for what politicians did once there. Using close U.S. House elections, we compare districts in which candidates from higher-SES family backgrounds narrowly won rather than narrowly lost. These comparisons generate a sharp shift in the family background of the representative who takes office and are balanced on key observed candidate and district characteristics, including pre-congress occupational standing. Districts that narrowly elected higher-family-SES candidates were subsequently represented by legislators who were substantially less likely to support pro-tax positions in major legislation. The result complements work linking politicians' class backgrounds to economic policy, but isolates a distinct channel: family origins mattered among candidates who had already entered the congressional pipeline and

who looked similar on observed occupational standing. Unequal political access therefore had consequences not only for descriptive representation, but also for legislative behavior in a central distributional conflict of the early twentieth century.

Taken together, the evidence in this paper suggests that concerns about inherited political advantage and about its consequences are empirically well founded in the United States. Across roughly eight decades—from the Civil War era through Reconstruction, industrialization, Progressive reform, the rise of federal income taxation, the New Deal, and the eve of World War II—family background was systematically related to who entered elected office. This pattern did not disappear across major institutional, economic, and political transformations: children from advantaged families remained overrepresented, and elite political access survived the destruction of slave wealth. Nor were these inequalities only superficial: as the close-election evidence shows, family origins also shaped how representatives voted when Congress confronted major redistributive conflicts over taxation. The result is a form of political persistence that is broader than dynasties of politicians, deeper than politicians' adult occupational status, and consequential for policy. Democratic competition did not eliminate inequalities rooted in family background; instead, those inequalities shaped both who governed and how representatives exercised power once in office.

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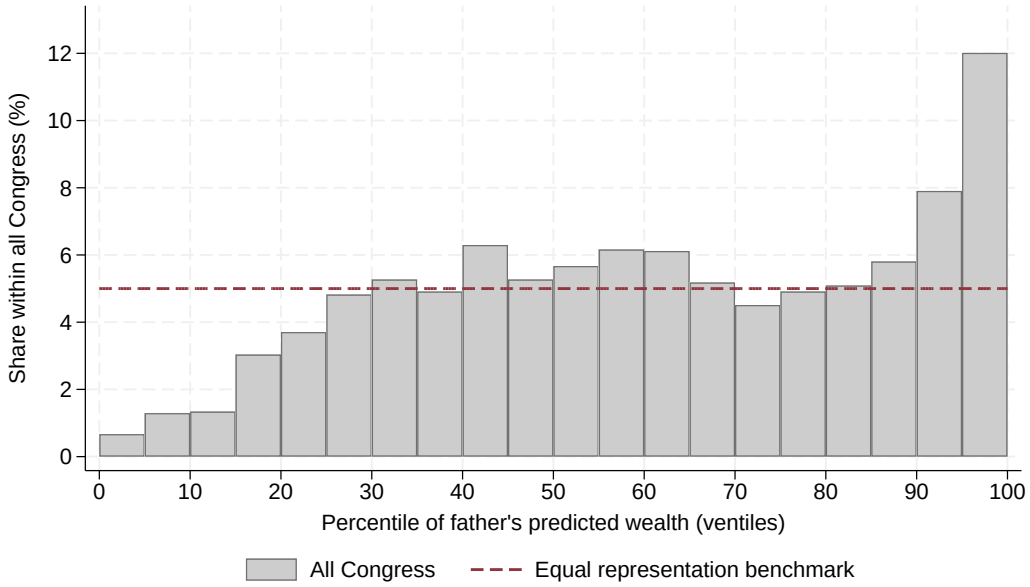
# Online Appendix

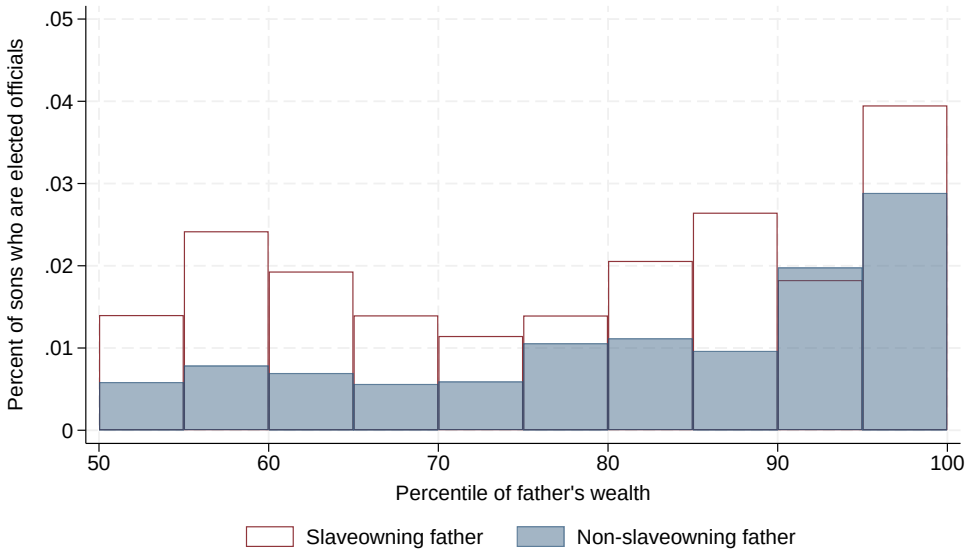
## A Appendix Figures and Tables

**Figure A1:** Representation Across Father Predicted Wealth Ventiles in Congress




*Notes:* This figure shows the distribution of linked Congress members across ventiles of father's predicted wealth, pooling Democratic and Republican members. The dashed horizontal line marks the equal-representation benchmark (5 percent in each ventile).

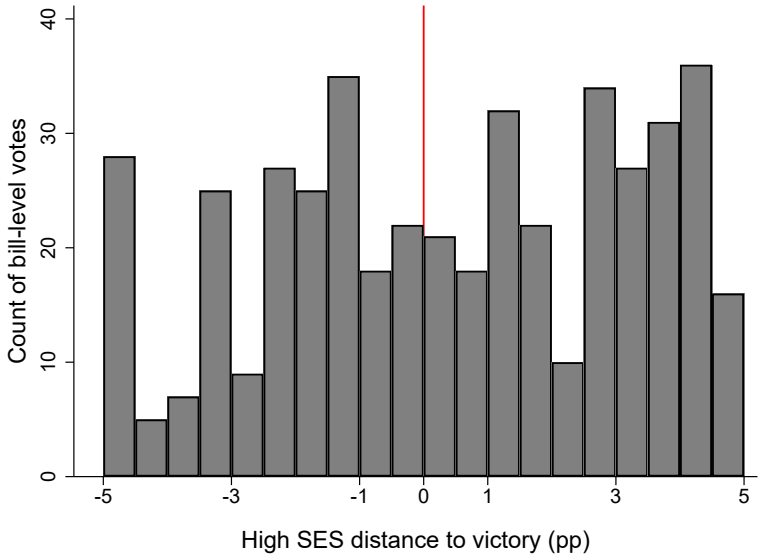
**Figure A2:** Elected Office by Father Wealth Bin and Slaveholder Status (Unadjusted)



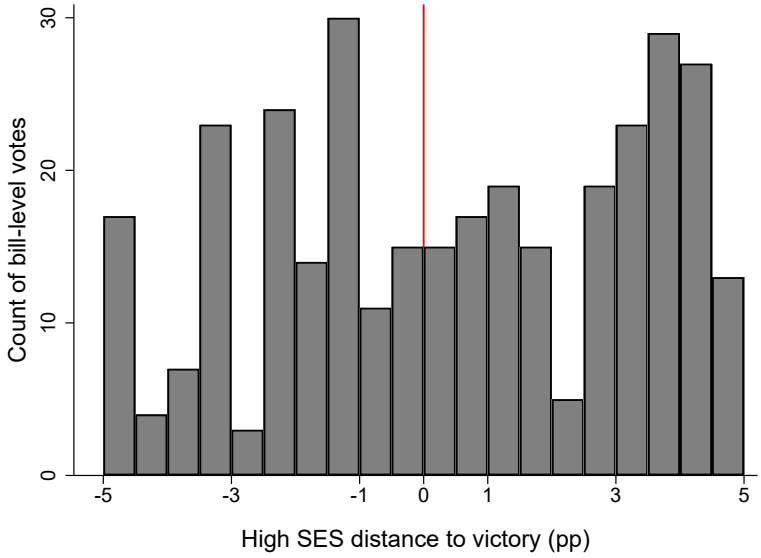

*Notes:* The figure plots the raw probability that a son is later observed as an elected official, separately by whether his father was a slaveowner. The sample is restricted to sons whose fathers fall between the 50th and 100th percentiles of the baseline wealth distribution, grouped into 5-percent bins. The sample pools sons observed in 1850 and 1860 households and follows them to later censuses through 1930.

**Figure A3:** Distribution of the Running Variable in Close Election Samples

(a) Preferred Sample (High SES = Above P60; Low SES = Below P40)



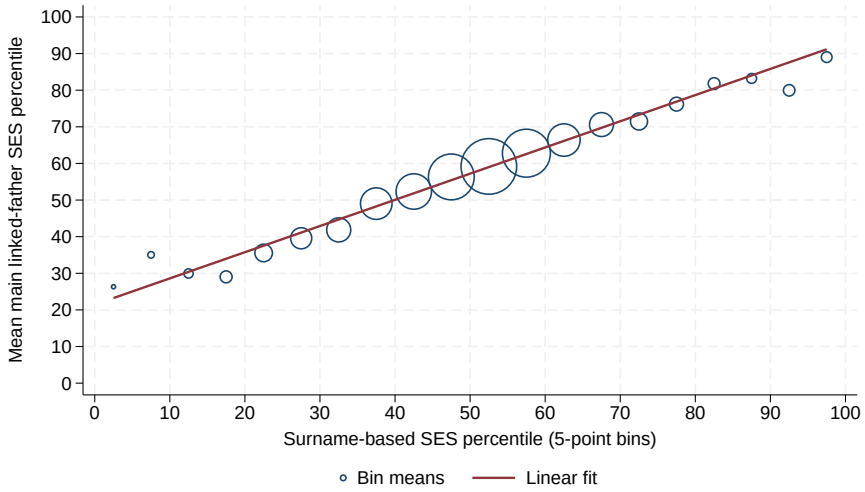

(b) Alternative Sample (High SES = Above P66; Low SES = Below P33)



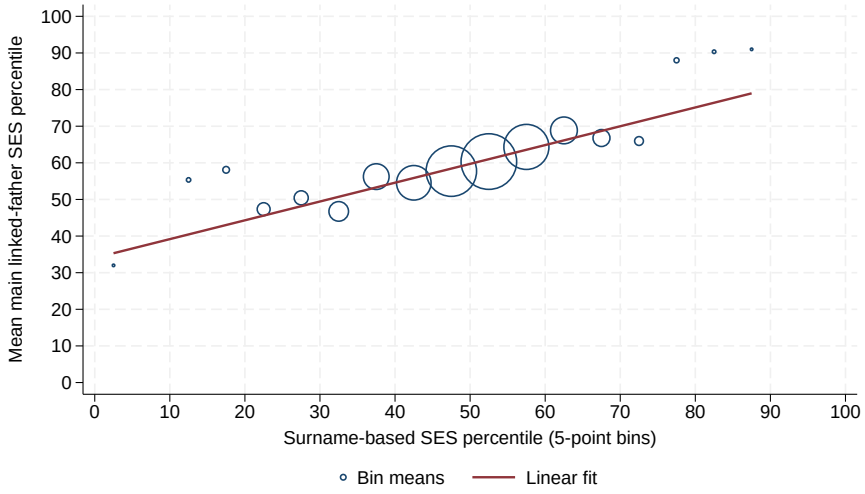

*Notes:* This figure plots histograms of the running variable, defined as the high-SES candidate's vote margin over the low-SES candidate (in percentage points), in the bill-level RD samples. Panel (a) uses the preferred sample which splits candidates based on whether they are above the 60th percentile or below the 40th percentile; Panel (b) uses the alternative sample that is split based on the 66th and 33rd percentile cutoffs. Positive values indicate high-SES victories and negative values indicate low-SES victories. The red vertical line marks the cutoff at zero. Bin width is 0.5 percentage points and the displayed range is $[-5, 5]$. McCrary density tests fail to reject continuity of the running variable at the cutoff in both samples, with robust $p$-values of 0.989 in Panel (a) and 0.579 in Panel (b).

**Figure A4:** Relationship Between Surname-Based SES and Linked-Father SES in Linked Political Samples

(a) Linked Elected-Official Sample




(b) Linked Congress Sample




*Notes:* This figure plots binned relationships between the surname-based SES measure and the linked-father SES measure in two linked samples of elected officials. In each panel, the $x$-axis reports surname-based SES percentiles grouped into 5-percent bins, and the $y$-axis reports mean linked-father SES percentile within each bin. Each circle denotes a bin mean, with circle size proportional to the number of observations in the bin. The solid line reports the linear fit. Panel (a) uses the main linked elected-official sample; Panel (b) uses the linked member of Congress sample.

**Table A1:** Matching Summary Statistics for Elected Officials

| | All elected officials (1) | Linked-back (2) | Not linked (3) |
|---|---|---|---|
| *Panel A: Linkage rates* | | | |
| N elected official records (all years, age-feasible) | 8,810 | 5,369 | 3,441 |
| N elected official records, 1860–1900 (age-feasible) | 2,140 | 1,045 | 1,095 |
| N elected official records, 1910–1940 (age-feasible) | 6,670 | 4,324 | 2,346 |
| Link-back rate, all years (%) | 60.9 | – | – |
| Link-back rate, 1860–1900 (%) | 48.8 | – | – |
| Link-back rate, 1910–1940 (%) | 64.8 | – | – |
| *Panel B: Pooled characteristics* | | | |
| Age (mean) | 49.4 | 50.2 | 48.2 |
| South (%) | 33.1 | 32.9 | 33.5 |
| Literate (%) | 99.7 | 99.8 | 99.6 |
| Urban (%) | 72.8 | 72.0 | 74.1 |
| *Panel C: Characteristics, 1860–1900* | | | |
| Age (mean) | 43.2 | 43.6 | 42.9 |
| South (%) | 36.2 | 36.1 | 36.3 |
| Literate (%) | 99.0 | 99.2 | 98.7 |
| Urban (%) | 65.3 | 63.5 | 67.0 |
| *Panel D: Characteristics, 1910–1940* | | | |
| Age (mean) | 51.4 | 51.8 | 50.7 |
| South (%) | 32.1 | 32.1 | 32.1 |
| Literate (%) | 100.0 | 100.0 | 100.0 |
| Urban (%) | 75.2 | 74.0 | 77.4 |

*Notes:* This table reports counts and summary statistics for elected officials identified in decennial U.S. Census records from 1860 to 1940 and linked back to childhood records using Census Tree links (Buckles et al., 2023). All entries in Panels A–D are computed on the age-feasible politician sample: individuals whose observed adult age is consistent with appearing as a child in at least one admissible earlier census year. Linked-back indicates a successful match to an earlier childhood record in which the individual is observed as a son. Literacy is coded from the census literacy variable when available; in 1940, literacy is proxied by an indicator for any positive schooling in that census wave.

**Table A2:** Matching Summary Statistics for Congress Party Analysis Sample

| | All age-eligible Congress members (1) | Census + Census Tree linked (2) |
|---|---|---|
| N Congress members | 4,421 | 2,286 |
| Share Census+Census Tree linked (%) | 51.7 | 100 |
| Mean first year in Congress | 1905.4 | 1906.7 |
| Mean age at first Congress entry | 44.8 | 45.2 |
| Mean total years served in Congress | 7.8 | 8.3 |
| Mean birth year | 1860.5 | 1861.5 |
| Share Democrat (%) | 50.0 | 48.7 |
| Share Republican (%) | 48.2 | 49.6 |
| Share foreign born (%) | 5.6 | 3.6 |
| Mean pre-congress occupation score | 51.9 | 51.6 |
| Share elected from Northeast (%) | 28.6 | 30.2 |
| Share elected from Midwest (%) | 34.0 | 34.7 |
| Share elected from South (%) | 28.8 | 26.8 |
| Share elected from West (%) | 8.6 | 8.3 |

*Notes:* This table reports matching summary statistics for the construction of the sample used in our analysis of representation by party within the U.S. Congress. Column (1) reports all relevant Congress members whose birth year implies potential childhood-link eligibility in the available Census years used for family-tree linkage. Column (2) reports the final linked analysis sample: members whose childhood family background can be recovered after matching congressional biography records to adult Census records and then linking those records through Census Tree to childhood households. Party categories are Democrat, Republican, and other. All statistics are based on the Congressional Biography ICPSR series (McKibbin, 1992).

**Table A3:** SES-Related Statistics by Matching Status

| | Obs. (N) (1) | Linked indicator (2) | Linked-back (3) | Not linked (4) |
|---|---|---|---|---|
| *Panel A: All Elected Officials* | | | | |
| Surname SES percentile | 6,736 | 0.66 | 51.2 | 51.4 |
| *Panel B: 1940 Elected Officials* | | | | |
| Surname SES percentile (1940) | 2,685 | 0.70 | 50.8 | 50.7 |
| Years schooling (1940) | 3,323 | 0.66 | 12.0 | 11.6 |
| Income/wage (1940, thousands) | 3,313 | 0.66 | 2.2 | 2.1 |
| ln(income/wage, 1940) | 3,039 | 0.67 | 7.4 | 7.4 |
| *Panel C: Congress Members* | | | | |
| Surname SES percentile | 2,381 | 0.78 | 49.9 | 49.3 |
| Pre-congress occupation score | 4,421 | 0.52 | 51.6 | 52.3 |

*Notes:* This table provides additional summary statistics for individuals who can and cannot be linked back to childhood records using Census Tree links (Buckles et al., 2023). Each row lists a given characteristic (e.g., a surname-based measure of SES) for the subset of the relevant sample for which the characteristic can be observed. These numbers are not directly comparable to the matching-summary tables because measures are not always consistently available for all persons. Column (1) provides the sample counts for each measure. Column (2) reports the mean link rate for the sample. Columns (3) and (4) report the mean among linked and not-linked observations.

**Table A4:** Matching Summary Statistics for Sons of Slaveholder Analysis Cohorts

| | 1850 baseline sons | | | 1860 baseline sons | | |
|---|---|---|---|---|---|---|
| Characteristic | All (1) | Linked (2) | Not linked (3) | All (4) | Linked (5) | Not linked (6) |
| N sons | 1,387,363 | 856,949 | 530,414 | 1,746,313 | 1,238,322 | 507,991 |
| Linked to any follow-up (%) | 61.8 | 100 | 0 | 70.9 | 100 | 0 |
| Son age at baseline | 7.8 | 7.3 | 8.5 | 7.8 | 7.8 | 7.8 |
| South (%) | 100 | 100 | 100 | 100 | 100 | 100 |
| Father slaveowner (%) | 25.0 | 25.5 | 24.1 | 19.7 | 20.5 | 17.9 |
| Father age at baseline | 40.3 | 39.8 | 41.0 | 40.4 | 40.4 | 40.4 |
| Father wealth percentile | 60.6 | 61.1 | 59.7 | 57.1 | 58.5 | 53.6 |
| Father farmer (%) | 70.7 | 71.8 | 69.1 | 58.9 | 61.2 | 53.4 |
| Father farm laborer (%) | 0.07 | 0.06 | 0.08 | 2.5 | 2.4 | 2.8 |

*Notes:* This table reports matching summary statistics for the baseline cohorts used in the slaveholder sons analysis. Baseline cohorts are white male sons age 18 or younger observed with identified fathers (age 18–64) in the 1850 or 1860 census. Linked indicates linkage to at least one follow-up census observation between 1870 and 1930. Father observed wealth percentile is measured directly in the baseline Census (1850 or 1860).

**Table A5:** Matching Summary Statistics for Slaveholder Father Wealth-Shock Cohorts

| | 1850 baseline fathers | | | 1860 baseline fathers | | |
|---|---|---|---|---|---|---|
| Characteristic | All (1) | Linked (2) | Not linked (3) | All (4) | Linked (5) | Not linked (6) |
| N fathers | 498,659 | 192,123 | 306,536 | 664,905 | 335,892 | 329,013 |
| Linked to 1870 (%) | 38.5 | 100 | 0 | 50.5 | 100 | 0 |
| Father slaveowner (%) | 25.6 | 26.9 | 24.8 | 19.8 | 22.2 | 17.4 |
| Father age at baseline | 39.4 | 37.3 | 40.7 | 39.6 | 39.4 | 39.8 |
| Father wealth percentile | 60.4 | 61.4 | 59.7 | 56.7 | 60.2 | 53.2 |
| Father farmer (%) | 69.7 | 71.5 | 68.5 | 58.0 | 60.6 | 55.3 |

*Notes:* This table reports matching summary statistics for baseline fathers used in the wealth-shock analysis. Baseline cohorts are fathers observed in the 1850 or 1860 census in the South and identified through the slaveholder-sons. Linked is defined as inclusion in the final 1870 father analysis sample (linked to an 1870 record, age-consistent, and with non-missing 1870 wealth outcomes). Father observed wealth percentile is measured directly in the baseline Census (1850 or 1860).

**Table A6:** Close Election Sample Summary Statistics

| | | | | RD bill-level sample | |
|---|---|---|---|---|---|
| | All close elections | High/low 66/33 | High/low 60/40 | High/low 66/33 | High/low 60/40 |
| | (1) | (2) | (3) | (4) | (5) |
| Elections (N) | 3,779 | 338 | 459 | 120 | 172 |
| Candidates (N) | 7,558 | 676 | 918 | 240 | 344 |
| Mean SES score | 49.8 | 50.5 | 50.2 | 50.4 | 50.2 |
| Mean Birth Year | 1841.1 | 1870.6 | 1872.2 | 1875.8 | 1875.6 |
| Median Birth Year | 1841.0 | 1870.0 | 1871.0 | 1875.0 | 1875.0 |
| Bill-level observations (N) | . | . | . | 330 | 448 |
| Linked to VoteView? | No | No | No | Yes | Yes |

*Notes:* This table provides summary statistics on the close-election data. Each column reports summary statistics for a different sample. Column (1) reports the entire close-election sample. Columns (2) and (4) use the high/low 66/33 SES contrast, where the high-SES candidate is at or above the 66th percentile of the surname-based SES distribution and the low-SES candidate is at or below the 33rd percentile. Columns (3) and (5) use the high/low 60/40 SES contrast, where the high-SES candidate is at or above the 60th percentile and the low-SES candidate is at or below the 40th percentile. Columns (4) and (5) are further restricted to observations with VoteView tax votes in the RD window (where the absolute value of the difference between the high and low SES candidate vote share in percentage points is less than or equal to 5). Bill-level observations (N) reports the number of representative-by-bill observations used in the RD outcome regressions.

**Table A7:** Impacts on Slaveholding Fathers' 1870 Wealth Outcomes

| | Outcome: ln(Wealth in 1870) | | Outcome: ln(1 + Wealth in 1870) | | Outcome: asinh(Wealth in 1870) | | Outcome: Wealth in 1870 (PPML) | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Father slaveowner | -0.07*** | -0.07*** | -0.13*** | -0.15*** | -0.13*** | -0.16*** | -0.15*** | -0.12*** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 462,780 | 462,758 | 528,015 | 527,993 | 528,015 | 527,993 | 528,015 | 527,989 |
| Mean dep. var. | 10.36 | 10.36 | 9.08 | 9.08 | 9.69 | 9.69 | 79112.8 | 79112.3 |
| Baseline wealth FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline census year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Father age-at-1870-bin FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline occupation FE | No | Yes | No | Yes | No | Yes | No | Yes |
| Estimator: PPML | No | No | No | No | No | No | Yes | Yes |

*Notes:* This table estimates 1870 wealth outcomes among baseline fathers from slaveholding and non-slaveholding households in the South. The sample consists of fathers observed in the 1850 or 1860 census (identified through the slaveholder-sons analysis sample) and linked to a 1870 census record with age-consistent linkage. The key regressor is an indicator that the father was a slaveowner in the baseline census. Columns (1)–(2) use ln(wealth) as the dependent variable and are estimated on fathers with positive 1870 wealth; columns (3)–(4) use ln(1 + wealth); columns (5)–(6) use asinh(wealth); columns (7)–(8) use Poisson pseudo-maximum likelihood (PPML) models on wealth levels (including fathers with zero wealth), and the reported results are transformed estimates (i.e., $\exp(\beta) - 1$) for comparability with log-specifications. Columns within each outcome differ by included fixed effects (as indicated in the table). Robust standard errors are reported in parentheses. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A8:** Slaveholding and Son Entry into Elected Office (Robustness)

| | Benchmark (1) | Inverse-weighted by son obs. (2) | First son observation (3) | + Baseline state FE (4) | + Father age-bin FE (5) | 1850 baseline cohort (6) | 1860 baseline cohort (7) |
|---|---|---|---|---|---|---|---|
| Father slaveowner | 0.007*** | 0.008*** | 0.006*** | 0.006*** | 0.007*** | 0.009*** | 0.005** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) |
| Observations | 5,354,196 | 5,354,196 | 2,095,265 | 5,354,196 | 5,354,196 | 2,080,406 | 3,273,785 |
| Mean dep. var. | 0.011 | 0.011 | 0.006 | 0.011 | 0.011 | 0.014 | 0.010 |
| Preferred FE set | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline state FE | No | No | No | Yes | No | No | No |
| Father age-bin FE | No | No | No | No | Yes | No | No |
| 1850 sample only | No | No | No | No | No | Yes | No |
| 1860 sample only | No | No | No | No | No | No | Yes |

*Notes:* The dependent variable is an indicator for being an elected official in the follow-up Census year. The analysis sample is restricted to post-emancipation adult follow-up years (1870–1930). Column (1) reports the preferred specification from Column (2) of Table 1. Column (2) weights each son-follow-up observation by the inverse of the son's total number of follow-up observations in the estimation sample. Column (3) keeps only the earliest observed follow-up observation per son. Column (4) adds baseline state fixed effects. Column (5) adds baseline father age-bin fixed effects. Columns (6) and (7) report results by the baseline census cohort (i.e., using sons linked in the 1850 or 1860 censuses, respectively). Standard errors are clustered at the father-level. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A9:** RD Balance Tests: Personal and Election/District-Level Characteristics

| | Mean (Left of Cutoff) (1) | RD Est. (2) | RD Est. (3) |
|---|---|---|---|
| *Panel A: Winner Personal Characteristics* | | | |
| Age at first Congress | 44.69 | -0.05 | 0.12 |
| | | (3.36) | (3.87) |
| No college | 0.25 | -0.14 | 0.02 |
| | | (0.13) | (0.13) |
| Any Ivy | 0.15 | 0.05 | 0.06 |
| | | (0.11) | (0.12) |
| Pre-congress occ. score | 54.45 | 2.16 | 3.71 |
| | | (4.32) | (4.86) |
| Winner birth South | 0.16 | 0.11 | 0.14 |
| | | (0.13) | (0.15) |
| *Panel B: Election/District Characteristics* | | | |
| Mean SES (two candidates) | 49.86 | 0.51 | 2.34 |
| | | (2.08) | (2.28) |
| # of bills | 3.61 | 0.02 | 0.28 |
| | | (0.91) | (0.90) |
| ln(Total population) | 12.35 | 0.20 | 0.13 |
| | | (0.22) | (0.22) |
| ln(Black population) | 8.18 | 0.27 | 0.91 |
| | | (0.62) | (0.66) |
| ln(Foreign-born population) | 9.52 | 0.49 | 0.53 |
| | | (0.56) | (0.61) |
| Share Black | 0.05 | 0.01 | 0.03 |
| | | (0.03) | (0.03) |
| Share foreign-born | 0.12 | 0.04 | 0.06 |
| | | (0.04) | (0.05) |
| Southern district | 0.22 | 0.00 | 0.01 |
| | | (0.15) | (0.17) |
| Functional form | | Linear | Linear |
| Bandwidth | | Within 5 pp | Within 4 pp |
| Joint test p-value (Panel A) | | 0.80 | 0.73 |
| Joint test p-value (Panel B) | | 0.91 | 0.39 |
| Joint test p-value (All) | | 0.93 | 0.57 |

*Notes:* This table reports RD balance tests for predetermined winner-level and election/district-level characteristics in the bill-level sample. Column (1) reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Columns (2) and (3) report the estimated threshold-crossing effect of a high-SES candidate victory from linear RD specifications. Column (2) uses races within 5 percentage points of the threshold, and Column (3) uses the restricted bandwidth within 4 percentage points. Standard errors clustered at the representative level are in parentheses. Joint-test rows report $p$-values for the null that all threshold-crossing effects are jointly zero within Panel A, Panel B, and both panels combined.

**Table A10:** RD Estimates: Winner is Democrat

| | RD Est. | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| High SES win | -0.237 | -0.334* | -0.256 | -0.378** |
| | (0.179) | (0.200) | (0.169) | (0.180) |
| Mean dep. var. | 0.577 | 0.542 | 0.577 | 0.542 |
| N | 448 | 363 | 448 | 363 |
| Model | Linear | Linear | Linear | Linear |
| Bandwidth | Within 5 pp | Within 4 pp | Within 5 pp | Within 4 pp |
| Controls | No | No | Yes | Yes |

*Notes:* This table reports linear RD estimates with an indicator for whether the election winner is a Democrat as the dependent variable. The sample is restricted to close House elections in which the higher-SES candidate has a surname-based SES score above the 60th percentile and the lower-SES candidate has a score below the 40th percentile. Reported coefficients are threshold-crossing effects of a high-SES candidate victory. Columns (1) and (3) use races within 5 percentage points of the threshold, while columns (2) and (4) use the restricted bandwidth within 4 percentage points. Columns (1)–(2) are estimated without additional covariates; columns (3)–(4) add winner-level controls: age at first Congress, no-college indicator, Ivy indicator, pre-congress occupation-based income score, and a born in the South indicator. The Mean dep. var. row reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Standard errors are clustered at the representative level.

**Table A11:** RD Estimates: Pro-Tax Voting Controlling for Democratic Party Affiliation

| | RD Est. | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| High SES win | -0.178** | -0.230*** | -0.171** | -0.228** |
| | (0.076) | (0.083) | (0.080) | (0.092) |
| Mean dep. var. | 0.731 | 0.720 | 0.731 | 0.720 |
| N | 448 | 363 | 448 | 363 |
| Model | Linear | Linear | Linear | Linear |
| Bandwidth | Within 5 pp | Within 4 pp | Within 5 pp | Within 4 pp |
| Winner Democrat control | Yes | Yes | Yes | Yes |
| Main controls | No | No | Yes | Yes |

*Notes:* This table reports linear RD estimates with bill-level pro-tax voting as the dependent variable. The sample is restricted to close House elections in which the higher-SES candidate has a surname-based SES score above the 60th percentile and the lower-SES candidate has a score below the 40th percentile. Reported coefficients are threshold-crossing effects of a high-SES candidate victory. All columns include a control for whether the election winner is a Democrat. Columns (1) and (3) use races within 5 percentage points of the threshold, while columns (2) and (4) use the restricted bandwidth within 4 percentage points. Columns (1)–(2) include only the winner Democrat control; columns (3)–(4) additionally include winner-level controls: age at first Congress, no-college indicator, Ivy indicator, pre-congress occupation-based income score, and a born in the South indicator. The Mean dep. var. row reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Standard errors are clustered at the representative level.

**Table A12:** Alternative Sample: RD Balance Tests (High = Above P66; Low = Below P33)

| | Mean (Left of Cutoff) (1) | RD Est. (2) | RD Est. (3) |
|---|---|---|---|
| *Panel A: Winner Personal Characteristics* | | | |
| Age at first Congress | 44.7 | 4.5 | 2.9 |
| | | (4.1) | (4.8) |
| No college | 0.28 | -0.15 | 0.01 |
| | | (0.15) | (0.16) |
| Any Ivy | 0.12 | 0.00 | 0.03 |
| | | (0.14) | (0.16) |
| Pre-congress occ. score | 54.3 | 3.9 | 6.2 |
| | | (5.1) | (5.7) |
| Winner birth South | 0.21 | 0.15 | 0.14 |
| | | (0.15) | (0.17) |
| *Panel B: Election/District-Level Characteristics* | | | |
| Mean SES (two candidates) | 50.1 | 0.15 | 2.1 |
| | | (2.8) | (3.0) |
| # of bills | 3.9 | -1.1 | -0.79 |
| | | (0.95) | (0.90) |
| ln(Total population) | 12.4 | 0.24 | 0.09 |
| | | (0.27) | (0.25) |
| ln(Black population) | 8.3 | 0.81 | 1.2 |
| | | (0.75) | (0.81) |
| ln(Foreign-born population) | 9.4 | 0.16 | 0.36 |
| | | (0.72) | (0.78) |
| Share Black | 0.05 | 0.03 | 0.04 |
| | | (0.03) | (0.04) |
| Share foreign-born | 0.11 | 0.02 | 0.06 |
| | | (0.05) | (0.06) |
| Southern district | 0.24 | 0.05 | -0.02 |
| | | (0.18) | (0.20) |
| Functional form | | Linear | Linear |
| Bandwidth | | Within 5 pp | Within 4 pp |
| Joint test p-value (Panel A) | | 0.72 | 0.81 |
| Joint test p-value (Panel B) | | 0.83 | 0.49 |
| Joint test p-value (All) | | 0.56 | 0.51 |

*Notes:* This table reports RD balance tests for predetermined winner-level and election/district-level characteristics in the bill-level sample. Column (1) reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Columns (2) and (3) report the estimated threshold-crossing effect of a high-SES candidate victory from linear RD specifications. Column (2) uses races within 5 percentage points of the threshold, and Column (3) uses the restricted bandwidth within 4 percentage points. Standard errors clustered at the representative level are in parentheses. Joint-test rows report $p$-values for the null that all threshold-crossing effects are jointly zero within Panel A, Panel B, and both panels combined.

**Table A13:** Alternative Sample: RD Estimates: SES Gap and Pro-Tax Voting (High = Above P66; Low = Below P33)

| | SES Gap | | Pro-Tax Vote (Bill Level) | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High SES win | 27.95*** | 29.88*** | -0.283** | -0.399*** | -0.310** | -0.431*** |
| | (4.63) | (4.93) | (0.120) | (0.130) | (0.120) | (0.125) |
| Mean dep. var. | -14.51 | -15.23 | 0.743 | 0.724 | 0.743 | 0.724 |
| N | 330 | 269 | 330 | 269 | 330 | 269 |
| Model | Linear | Linear | Linear | Linear | Linear | Linear |
| Bandwidth | Within 5 pp | Within 4 pp | Within 5 pp | Within 4 pp | Within 5 pp | Within 4 pp |
| Controls | No | No | No | No | Yes | Yes |

*Notes:* This table reports linear RD estimates for SES winner-loser gap (first stage) and pro-tax voting outcomes in the bill-level sample. The sample is restricted to close House elections in which candidates come from opposite SES terciles: the higher-SES candidate is in the top tercile of the surname-based SES distribution and the lower-SES candidate is in the bottom tercile. Reported coefficients are threshold-crossing effects of a high-SES candidate victory. Columns (1)–(2) use SES gap as the dependent variable; columns (3)–(6) use pro-tax voting. Columns (1), (3), and (5) are estimated within 5 percentage points of the threshold, while columns (2), (4), and (6) use the restricted bandwidth within 4 percentage points. Columns (1)–(4) are estimated without additional covariates; columns (5)–(6) add winner-level controls: age at first Congress, no-college indicator, Ivy indicator, pre-congress occupation-based income score, and a born in the South indicator. The Mean dep. var. row reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Standard errors are clustered at the representative level.

**Table A14:** Quadratic Model RD Estimates: SES Gap and Pro-Tax Voting

| | SES Gap | | Pro-Tax Vote (Bill Level) | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High SES win | 22.47*** | 26.30*** | -0.278** | -0.396** | -0.282*** | -0.441** |
| | (3.91) | (4.57) | (0.108) | (0.182) | (0.107) | (0.170) |
| Mean dep. var. | -11.90 | -11.90 | 0.731 | 0.731 | 0.731 | 0.731 |
| N | 448 | 448 | 448 | 448 | 448 | 448 |
| Model | Linear | Quadratic | Linear | Quadratic | Linear | Quadratic |
| Bandwidth | Within 5 pp | Within 5 pp | Within 5 pp | Within 5 pp | Within 5 pp | Within 5 pp |
| Controls | No | No | No | No | Yes | Yes |

*Notes:* This table reports RD estimates for SES winner-loser gap (first stage) and pro-tax voting outcomes in the bill-level sample. The sample is restricted to close House elections in which the higher-SES candidate has a surname-based SES score above the 60th percentile and the lower-SES candidate has a score below the 40th percentile. Reported coefficients are threshold-crossing effects of a high-SES candidate victory. Columns (1)–(2) use SES gap as the dependent variable; columns (3)–(6) use pro-tax voting. Columns (1)–(4) are estimated without additional covariates, while columns (5)–(6) add winner-level controls: age at first Congress, no-college indicator, Ivy indicator, pre-congress occupation-based income score, and a born in the South indicator. The Mean dep. var. row reports means for observations to the left of the threshold where the high-SES candidate loses (margin $< 0$). Standard errors are clustered at the representative level.

## B Data Construction Details

**Predicted Wealth Measure:** Our procedure adapts the parental-SES imputation strategy in Abramitzky et al. (2024) and Abramitzky et al. (2021). In our setting, we implement this approach using fathers' observed wealth in 1860. Specifically, our baseline measure is the father's predicted wealth percentile, which we construct in two steps. First, we begin with the 1860 full-count Census and regress fathers' observed wealth percentile on detailed occupation, state, age, and broad occupation-by-region interactions. Formally, we estimate:

$$W_{i,1860}^{obs} = \alpha + \sum_{o \in \mathcal{O}} \beta_o \mathbf{1}\{Occ1950_i = o\} + \sum_{s \in \mathcal{S}} \delta_s \mathbf{1}\{State_i = s\} + \gamma_1 Age_i + \gamma_2 Age_i^2 + \sum_{k \in \mathcal{K}} \sum_{r \in \mathcal{R}} \theta_{kr} \mathbf{1}\{OccGrp_i = k\} \mathbf{1}\{Region_i = r\} + \varepsilon_i, \tag{B1}$$

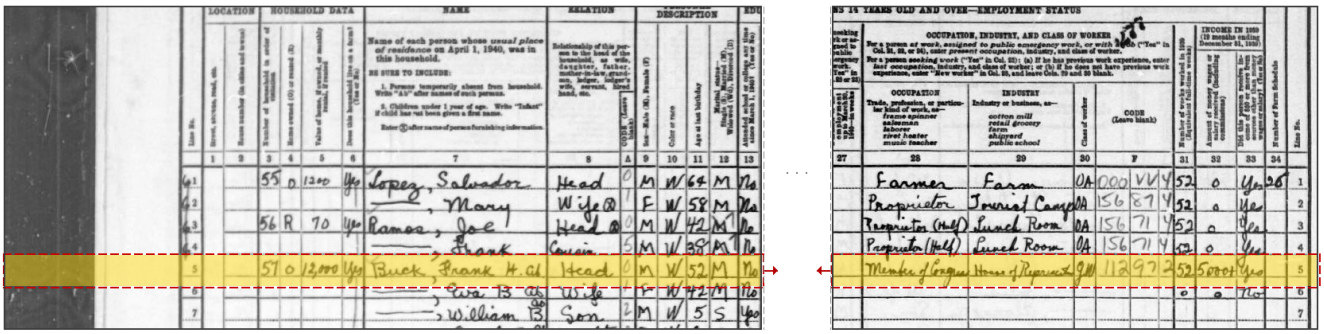
where $W_{i,1860}^{obs}$ is father $i$'s observed wealth percentile in 1860; $Occ1950_i$ is detailed OCC1950 occupation; $State_i$ is state of residence; $Age_i$ is age; $OccGrp_i$ is a broad occupation category; and $Region_i$ is a broad geographic region. The estimation sample consists of white male household heads age 18 or older. In constructing observed wealth, unknown property values are set to missing before computing wealth ranks. Observations with non-occupational response codes (e.g., students, retired, etc.) are excluded from estimation and prediction.

Second, we apply the estimated coefficients from equation (B1) to analogous father records in each census year from 1850 to 1940 to generate a predicted wealth score. Within each census year, we convert this score into percentile ranks and use the nationwide predicted wealth percentile as our baseline measure. If any prediction inputs are missing, these predicted wealth measures are coded as missing.

**Linked Sample of Officials for Representation Analysis of Elected Officials:** Our representation analysis is built from linked person-level census records that connect adult officeholders to childhood households. In each adult census wave, we first harmonize occupation coding using the occupation-string crosswalk from Ager, Boustan and Eriksson (2021). We then define a public-official pool as observations in public-administration occupations and industries, excluding armed-forces titles using a dedicated armed-forces string file. Within that pool, we classify elected officials using both exact and fuzzy title matching. The exact procedure searches occupation strings for office titles and common variants (e.g., governor/govenor, mayor/mayer, senator, congressman, representative/represenative, legislator, member of assembly, alderman, councilman). The fuzzy procedure computes Jaro-Winkler distance between each occupation string and a fixed elected-office dictionary (Governor, Mayor, Senator, U.S. Senator, United States Senator, Congressman, Member of Congress, Representative, State Senator, State Representative, Legislator, Lawmaker, Member of Assembly, Alderman, Councilman), and flags matches below the chosen threshold. We flag a record as an elected official if either exact or fuzzy matching is positive. Figure B1 illustrates this procedure using the 1940 Census entry for Frank H. Buck: panel (a) shows the individual record, while panel (b) shows the occupation and industry fields that identify him as a

member of Congress in the House of Representatives.

**Figure B1:** Illustration of Elected-Official Identification from Adult Census Records



**(a)** **(b)**

*Notes:* The figure reproduces the 1940 Census record for Frank H. Buck as an illustration of our elected official classification procedure. Panel (a) shows the identifying row in the population schedule. Panel (b) shows the occupation and industry fields from the same row, where Buck is recorded as a member of Congress in the House of Representatives. This is the type of adult Census entry that our exact and fuzzy title-matching procedures are designed to detect before we link adult officials backward to childhood households. The dashed boxes highlight the relevant row, and the arrows indicate that panels (a) and (b) correspond to the same Census record. *Source:* 1940 U.S. Census population schedule, reproduced from "Solano, California, United States records," images, FamilySearch, image 568 of 571, citing United States National Archives and Records Administration, Image Group Number 005456428.

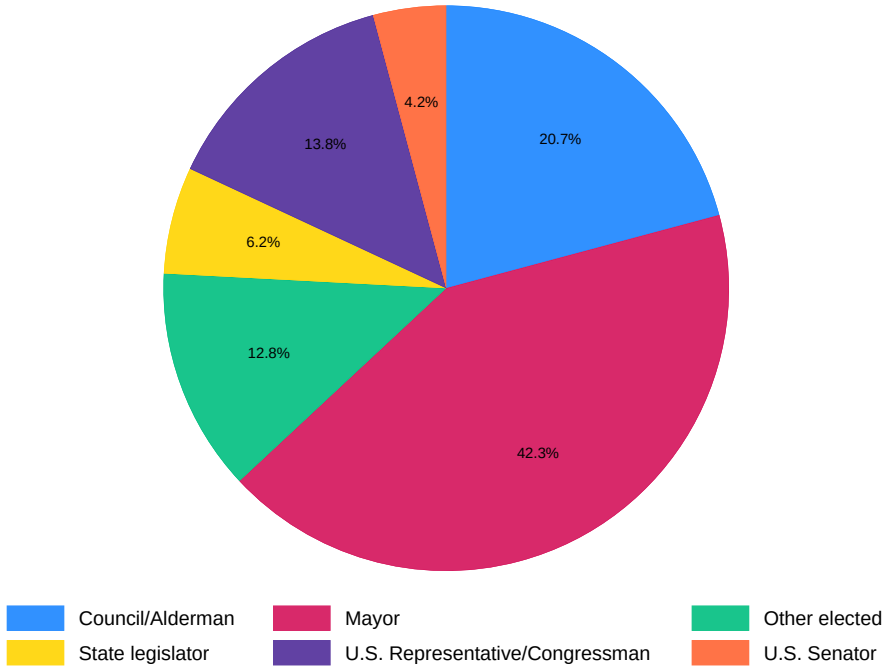
We link identified adult officials backward to childhood records using Census Tree links (Buckles et al., 2023) for feasible adulthood and childhood year pairs, and impose consistency checks on age progression across waves (within a narrow tolerance around the intercensal gap). In the childhood census, we retain observations coded as sons, male, white, and age 18 or younger. We then recover father characteristics from co-resident fathers in the same baseline household and merge father SES measures, including predicted wealth percentile and (when available in 1850/1860) observed wealth percentile measures.

Appendix Table A1 summarizes the realized linked sample after imposing the age-feasibility and consistency restrictions described above. Starting from 8,810 elected-official records in adult census waves that are age-feasible for backward linkage, 5,369 link successfully to childhood households, yielding an overall link-back rate of 60.9 percent. This overall link-back rate compares well with that of Abramitzky et al. (2024), who link 40.4 percent of their main academic faculty-roster sample to a childhood census overall. Linkage is lower in 1860—1900 than in 1910—1940 because officials observed in the earliest adult census waves have fewer admissible earlier childhood census years by construction. Reassuringly, linked and unlinked officials look broadly similar on basic observables in both the pooled sample and within broad periods. In the pooled sample, for example, differences in Southern residence, literacy, and urban status are modest, and the same is true within the 1860–1900 and 1910–1940 subsamples.

Beyond these basic comparisons of means, Appendix Table A3 reports additional diagnostics of whether backward linkage in the representation sample is systematically related to observable socioeconomic status. Specifically, we focus on a surname-based SES proxy that is available for both linked and unlinked officials. In the full pool of potentially linkable officeholders with available surname information, the average surname-based family SES measure is very similar: 51.2 for those who are linked back to their childhood home and 51.4 for others. As an additional test, the 1940 Census provides measures of schooling and income for adult officeholders themselves; in that sample, contemporaneous measures of SES are also very similar for linked and unlinked individuals. Taken together, these results suggest that differential linkage by observable socioeconomic status is limited in the elected-official sample.

Finally, to further characterize the composition of the elected-official sample, Appendix Figure B2 reports the distribution of offices observed. The sample is composed primarily of local officeholders, with mayors accounting for the largest share (42 percent), followed by council members and aldermen. State-level positions, such as state legislators, make up a smaller share, while federal officeholders—including U.S. Representatives (14 percent) and U.S. Senators (4 percent) constitute a modest but non-negligible portion of the sample.

**Figure B2:** Distribution of Elected Offices in the Linked Representation Sample




*Notes:* The figure reports the composition of the linked elected-official sample by office type. Shares are based on the 5,369 elected-official records in the main representation sample that link successfully to childhood households.

**Linked Sample of Congress Members:** Our party-level analysis within Congress is built from a linked sample of members for whom party affiliation is observed in the ICPSR Congressional Biography files (McKibbin, 1992). We begin from the ICPSR biography file and merge in supplemental scraped data that provide spouse information (used as an auxiliary matching-quality signal).[20] We then define a broad analysis universe as members first elected between 1850 and 1940 with nonmissing implied birth year. Next, we link these records to full-count census adults using `reclink2` on first name, last name, initials, middle initial, birth year, birthplace (state), and sex, requiring agreement on birthplace and sex. Candidate links are further refined using occupation consistency (including congressional occupation codes), spouse-first-name agreement when available, and high-score thresholds; remaining ambiguous cases are resolved with deterministic tie-breaking rules (including birth-year consistency, last-name string similarity, and state consistency). We then append hand-reviewed manual links and give manual links precedence when a manual and automated link refer to the same census person-year. The overall adult census-link rate from this stage is 72.7 percent.

We next merge adult-linked members to Census Tree to recover childhood households and father characteristics. For each adult census year, we use admissible childhood-adult pairings available in the data build (e.g., 1870 adults can link back to 1850; 1880 adults to 1850/1860; later adult years to a wider set of prior censuses). Because age-feasibility for childhood linkage is not imposed at the initial adult census-matching stage, some members are successfully linked to an adult census record but cannot be linked further to a childhood household. We therefore classify Congress members as age-eligible if their observed birth year is consistent with being observed as an adult in the matched census and as a child in at least one earlier census, allowing a $\pm 2$-year tolerance around the childhood age window. The age-eligible sample size is $N = 4{,}421$, and $N = 2{,}286$ observations are linked through both the Census and Census Tree stages, implying an overall Census+Census Tree linkage rate of 51.7% (Appendix Table A2). As with our representation analysis sample of elected officials, linked and non-linked members of Congress look broadly similar on key observables in this age-eligible sample: differences in party composition, pre-Congress occupation mix, and regional representation are modest, and linked members have only slightly earlier average first-Congress years and similar ages at first entry.

**Linked Sample of Sons of Slaveholders:** Our analysis of the sons of slaveholders is built from baseline cohorts in the 1850 and 1860 full-count censuses and follows sons forward to adult censuses through 1930. In each baseline wave, we retain white male children coded as sons in the household and age 18 or younger, recover co-resident fathers, and merge father characteristics from the same baseline household. Father slaveholding status is measured using the slave schedules in 1850/1860 from Hacker et al. (2025) and coded as an indicator for owning at least one enslaved person. We also measure fathers' observed wealth percentile

[20] The supplemental spouse file comes from a separate Congress–Wikipedia scraping exercise that compiles biographical and family fields for historical members of Congress using Wikipedia article text, infobox parsing, and targeted web-search augmentation. Spouse information is recovered for 64.6% of legislators. The merged file is linked to ICPSR records by legislator identifiers and is used here only to provide an auxiliary spouse-name match-quality check, not as a primary matching key.

(from 1850/1860 wealth) and harmonized occupation codes. A father elected-office indicator is constructed using the exact and fuzzy title matching process described above.

We then link baseline sons forward using Census Tree links (Buckles et al., 2023) applying age-consistency checks across waves. The analysis file stacks all valid son follow-up links, so the unit of observation is a linked son observed in a follow-up census year. Political entry outcomes are coded using the same elected-official procedure as in the representation analysis: occupation harmonization, public-administration filtering (excluding armed forces), and exact/fuzzy title matching for elected offices.

Appendix Table A4 summarizes matching in the South baseline cohorts used for the main Section 5 specifications. The baseline includes 1,387,363 sons in 1850 and 1,746,313 sons in 1860; linkage to at least one follow-up census is 61.8% and 70.9%, respectively. Linked and non-linked groups are broadly comparable on key baseline father characteristics, with some expected differences in farming and wealth-related measures. In the main estimation sample used for the entry regressions, the baseline specification contains about 5.35 million son-follow-up observations, with a mean elected-entry rate of about 1.1%.

**Linked Sample of Slaveholding Fathers:** Our father-level wealth-shock analysis is constructed from the same baseline family universe used in the sons of slaveholder analysis, but with fathers as the unit of analysis. We begin from white male sons observed in 1850 or 1860 as co-resident children (sons) and recover their co-resident fathers in the same baseline household. Baseline father characteristics include slaveholding status, directly observed wealth percentile in the baseline census (1850/1860) and harmonized occupation codes. To align the father first-stage directly with the intergenerational design, baseline fathers are restricted to those in the Section 5 sample (i.e., fathers of sons in the linked slaveholder sample).

We then link baseline fathers to 1870 census records using Census Tree links (Buckles et al., 2023), imposing a conservative one-to-one link requirement within each baseline cohort and an age-consistency screen. Specifically, for linked fathers we require observed 1870 age to be within a narrow tolerance of predicted age based on baseline age and intercensal timing. Father 1870 wealth is constructed from real-property and personal-property components in 1870 and used to define alternative outcomes: ln(wealth) (positive-wealth sample), ln(1 + wealth), and asinh(wealth).

Appendix Table A5 reports matching summary statistics for the South cohorts used in the main father first-stage specifications. The South baseline includes 498,659 fathers in 1850 and 664,905 fathers in 1860; 192,123 and 335,892, respectively, link to the final 1870 analysis sample (38.5% and 50.5%). The resulting South father analysis sample contains roughly 528,000 linked baseline fathers (with minor variation across specifications due to additional covariate requirements).

**Close Elections Samples:** Our close-elections analysis is built from the Our Campaigns universe of historical U.S. House candidates (1798–1950; Our Campaigns, 2025). We construct election-year-district candidate files, identify the top two vote-receiving candidates in

each race, and retain contests decided by at most five percentage points. The resulting close-race file contains candidate-pair records (winner and runner-up) with standardized election identifiers, vote shares, and winner status. This provides the baseline race structure for the regression discontinuity design.

Because the SES measure used in our analysis is indexed by surname and birth cohort, candidate birth year is a required input. Birth year is already available for many records in the close-race universe, but missingness is concentrated among narrowly defeated candidates. Targeted birth-year research is therefore applied only to candidates lacking reliable baseline birth-year information. In total, we searched for information on 1,549 candidates out of 7,558 close-election candidate records.

Birth-year collection combines three independent source streams: two manual researcher streams plus GPT-assisted source collection. These sources are then reconciled using structured checks against the candidate's name, office, geography, party, and election context. Manual reconciliation drew on similar combinations of source types, including reference biographies, memorial and genealogical records, and other web sources with election-context evidence. This process produces high birth-year coverage in the final close-election analysis sample: 97.3 percent of candidate-election observations.

Appendix Figure B3 provides a concrete example of the source documents identified during this process. The figure reproduces two pages from a biography of George Berry, the narrowly defeated Democratic candidate in Indiana's Fourth District in 1864. Human research assistants first located Berry's Find a Grave memorial, which pointed to a county-history biography in *Biographical and Genealogical History of Wayne, Fayette, Union and Franklin Counties, Indiana* (pp. 144–146). Panel (a) reproduces a page near the opening of that biography, which reports Berry's birth date as February 17, 1811. Panel (b) reproduces a later page from the same biography and states that "in 1864 he was the nominee of the Democratic party for congress." Because the same source links the name, party, office, geography, and election year to the biographical subject, the case provides both a birth-year assignment and an election-context validation for the candidate record.

Because the SES measure used in our analysis is indexed by surname and birth cohort, candidate birth year is a required input. Birth year is already available for many records in the close-race universe, but missingness is concentrated among narrowly defeated candidates. Targeted birth-year research is therefore applied only to candidates lacking reliable baseline birth-year information. For this matching exercise, we seek information on 1,549 candidates out of 7,558 close-election candidates. Birth year collection combines three independent source streams (two manual researcher streams plus GPT-assisted source collection), followed by structured reconciliation of disagreements using name, office, geography, and election-context checks. Manual reconciliation drew on multiple source types, including reference biographies (e.g., Wikipedia), memorial/genealogical records (e.g., FindAGrave), and other web sources with election-context evidence. This produces high birth-year coverage in the final close-election analysis sample (97.3 percent of candidate-election observations).

With birth year in hand, we assign candidates to surname-by-cohort SES values derived from full-count census data, mapping each candidate to the nearest supported cohort and

harmonizing surnames before merging. This places winners and runners-up on a common SES scale within each close race. We then define within-race SES contrast samples using pre-specified rules: (i) a preferred top/bottom 40 design, where the higher-SES candidate is above the 60th percentile and the lower-SES candidate is below the 40th percentile; and (ii) an alternative tercile design, where the higher-SES candidate is above the 66th percentile and the lower-SES candidate is below the 33rd percentile. Races not satisfying a given contrast rule are excluded from that estimation sample.

To construct legislative outcomes, we carry winners from these close races forward and merge them to House roll-call data from VoteView (Lewis et al., 2025), restricting to a selected set of major tax roll calls that occur after the ratification of the 16th Amendment, overlap with the period in which our elected-official sample can be observed in office, and can be assigned a clear pro-tax voting direction.[21] The resulting sample includes ten House roll calls: Revenue Act of 1916 (HR 16763) (Pro tax: Yea; Yea: 238 / Nay: 142); War Revenue Act of 1917 (HR 4280) (Pro tax: Yea; Yea: 329 / Nay: 76); Revenue Act of 1918 (HR 12863) (Pro tax: Yea; Yea: 312 / Nay: 11); Revenue Act of 1921 (HR 8245) (Pro tax: Nay; Yea: 232 / Nay: 109); Revenue Act of 1926 (HR 1) (Pro tax: Nay; Yea: 355 / Nay: 28); Revenue Act of 1932 (HR 10236) (Pro tax: Yea; Yea: 327 / Nay: 64); Revenue Act of 1934 (HR 7835) (Pro tax: Yea; Yea: 253 / Nay: 106); Revenue Act of 1935 (HR 8974) (Pro tax: Yea; Yea: 247 / Nay: 73); Revenue Act of 1936 (HR 12395) (Pro tax: Yea; Yea: 223 / Nay: 99); and Revenue Act of 1940 (HR 10039) (Pro tax: Yea; Yea: 396 / Nay: 6). These bills span several major tax-policy debates of the early twentieth century, encompassing both substantial tax increases and reductions: the Revenue Act of 1926, for example, was the centerpiece of the Mellon rate cuts and reduced top marginal income tax rates from 46 to 25 percent, while the Revenue Act of 1935, dubbed the "Soak the Rich" tax, sharply raised top marginal rates and increased estate and gift taxes as part of Roosevelt's Second New Deal. We exclude revenue bills that do not provide a clean measure of legislators' preferences over taxation. The Revenue Act of 1937 passed 173–0 and was enacted as a technical anti-avoidance measure, leaving no meaningful vote variation from which to infer legislators' underlying policy preferences. We exclude the Revenue Act of 1924 because it cannot be coded cleanly along a single pro-tax dimension, as it reduced income-tax rates while raising estate-tax rates and introducing the gift tax.

This yields a winner-by-bill panel in which the outcome indicates whether the elected legislator cast a vote in support of a higher tax position. The running variable is the higher-SES candidate's signed vote margin in the underlying House race, with treatment defined as the higher-SES candidate narrowly winning rather than narrowly losing; exact ties are excluded. Thus, identification comes from comparisons of races on either side of the zero-margin cutoff that are similar in electoral closeness but differ discretely in the SES background of the officeholder whose subsequent voting is observed.

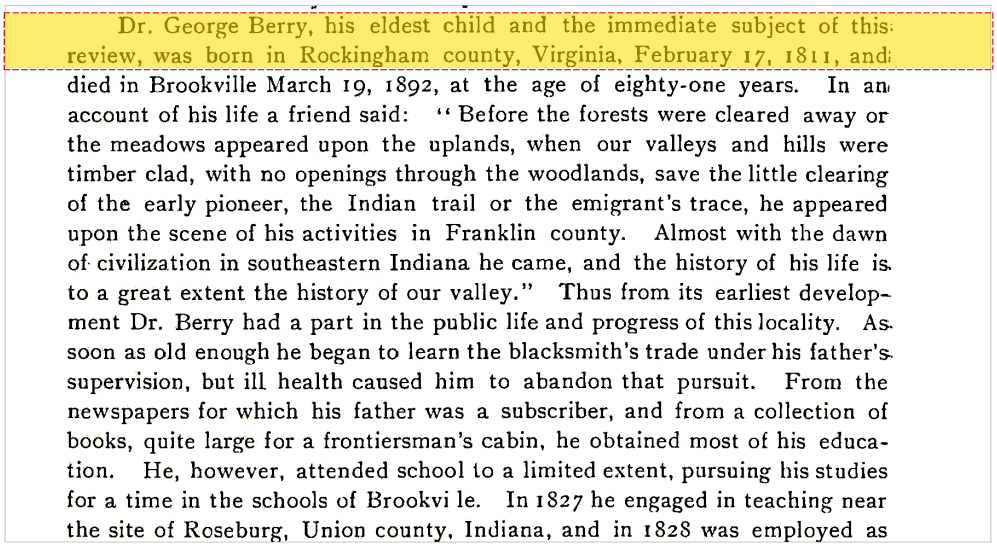
Appendix Table A6 reports summary statistics for the close-election samples used in

[21] Following ratification of the Sixteenth Amendment, the major tax legislation before 1916 was the Revenue Act of 1913. We exclude that bill from our analysis as it cannot be coded cleanly along a single pro-tax dimension because it paired a substantial tariff reduction with a newly created income tax that reached only a small fraction of households.

the RD analysis. The full close-election sample contains 3,779 elections and 7,558 top-two candidate observations. Applying SES-contrast restrictions substantially reduces the number of potential elections available by focusing on whether the top two candidates fall on opposite sides of the 66/33 or 60/40 surname-SES thresholds. Restricting further to races that can be linked to subsequent VoteView tax votes within the RD window yields bill-level estimation samples of 330 observations in the 66/33 design and 448 observations in the preferred 60/40 design. Candidates in the full close-election sample have a mean birth year of 1841, while the linked RD bill-level samples are centered on later cohorts, with mean birth years of roughly 1876, reflecting the overlap between the close-election data and the period of major House tax legislation used in our analysis.

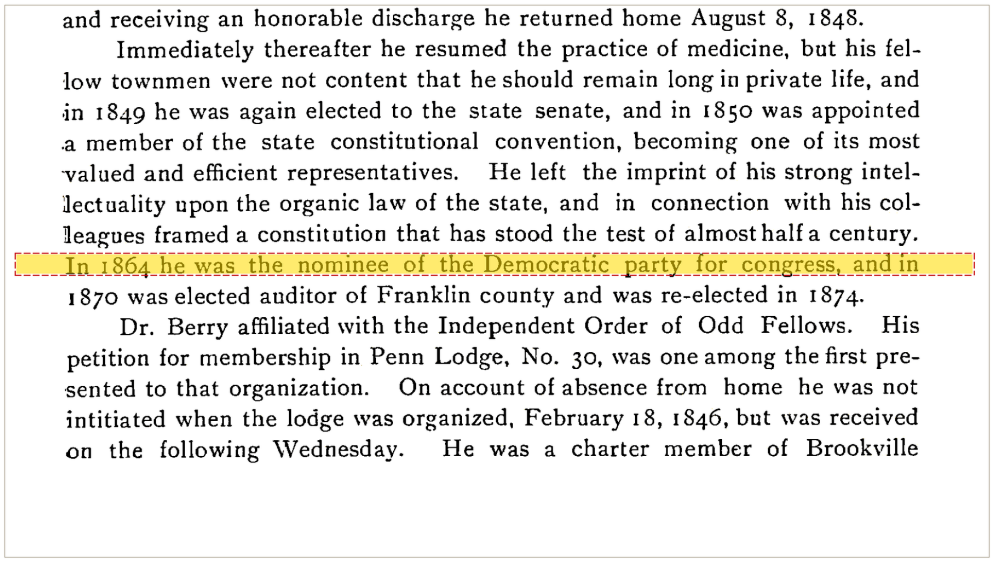
**Figure B3:** Illustration of Close Election Candidate Birth-Year Identification

**(a)**



Dr. George Berry, his eldest child and the immediate subject of this review, was born in Rockingham county, Virginia, February 17, 1811, and died in Brookville March 19, 1892, at the age of eighty-one years. In an account of his life a friend said: "Before the forests were cleared away or the meadows appeared upon the uplands, when our valleys and hills were timber clad, with no openings through the woodlands, save the little clearing of the early pioneer, the Indian trail or the emigrant's trace, he appeared upon the scene of his activities in Franklin county. Almost with the dawn of civilization in southeastern Indiana he came, and the history of his life is to a great extent the history of our valley." Thus from its earliest development Dr. Berry had a part in the public life and progress of this locality. As soon as old enough he began to learn the blacksmith's trade under his father's supervision, but ill health caused him to abandon that pursuit. From the newspapers for which his father was a subscriber, and from a collection of books, quite large for a frontiersman's cabin, he obtained most of his education. He, however, attended school to a limited extent, pursuing his studies for a time in the schools of Brookville. In 1827 he engaged in teaching near the site of Roseburg, Union county, Indiana, and in 1828 was employed as

**(b)**



and receiving an honorable discharge he returned home August 8, 1848.

Immediately thereafter he resumed the practice of medicine, but his fellow townmen were not content that he should remain long in private life, and in 1849 he was again elected to the state senate, and in 1850 was appointed a member of the state constitutional convention, becoming one of its most valued and efficient representatives. He left the imprint of his strong intellectuality upon the organic law of the state, and in connection with his colleagues framed a constitution that has stood the test of almost half a century. In 1864 he was the nominee of the Democratic party for congress, and in 1870 was elected auditor of Franklin county and was re-elected in 1874.

Dr. Berry affiliated with the Independent Order of Odd Fellows. His petition for membership in Penn Lodge, No. 30, was one among the first presented to that organization. On account of absence from home he was not intitiated when the lodge was organized, February 18, 1846, but was received on the following Wednesday. He was a charter member of Brookville

*Notes:* The figure reproduces two pages from a county-history biography of Dr. George Berry of Brookville, Franklin County, Indiana. Panel (a) shows Berry's birth record: born in Rockingham County, Virginia, February 17, 1811. Panel (b) shows the confirming congressional evidence: the highlighted passage states that "[i]n 1864 he was the nominee of the Democratic party for congress," matching the George Berry who appears in our close-election data as the Democratic candidate in Indiana's Fourth District in 1864 with 49.8 percent of the vote. This case illustrates the procedure used to obtain candidates' birth-year information: we accepted the birth-year assignment because the source tied the same name to the relevant geography, party, office, and election year. *Source: Biographical and Genealogical History of Wayne, Fayette, Union and Franklin Counties, Indiana*, vol. 1. Chicago: Lewis Publishing Company, 1899. Digitized copy from the Allen County Public Library Genealogy Center collection.