For Richer or Poorer? Marriages of the Extremely Wealthy in American History

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Abstract: To what extent did the rich marry the rich? We measure marital sorting using fathers' wealth observed before their children's marriages in the 1850–1870 U.S. Censuses. The relationship between fathers' and fathers-in-law's wealth ranks is sharply non-linear: children of top 1 percent fathers were 19 times more likely to marry into another top 1 percent family than baseline rates predict. Tracing wealth correlations through extended kin connected by up to four marriages, we find that sorting decays far more slowly than a linear model predicts, implying that elite families married within tightly connected networks. The Civil War's destruction of Southern wealth did not disrupt these patterns, suggesting that elite networks were sustained by social ties rather than wealth itself.

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1. Introduction

The tendency for individuals to marry those with similar characteristics, or assortative mating, has important consequences for inequality and social mobility. Modern research demonstrates that if marriages were random rather than assortative, the Gini coefficient would be significantly lower (Eika, Mogstad and Zafar, 2019; Greenwood et al., 2014). When individuals from wealthy backgrounds marry each other, they merge assets, networks, and future inheritances, affecting both cross-sectional inequality and subsequent generations. Children born into these unions benefit from compounded advantages in education, connections, and resources, cementing relative status across generations.

This paper examines marital sorting at the top of the wealth distribution, or the extent to which the rich marry the rich. Elites throughout world history have treated marriages as a strategic tool to forge alliances, consolidate wealth, and maintain control over society's resources. While this is well documented for ruling dynasties in Europe (Ottinger and Voigtländer, 2025), we know far less about how elite marriage markets operated in the United States during the nineteenth century. In development economics, research has shown that elite networks and clan ties can significantly influence economic outcomes (Amsden, DiCaprio and Robinson, 2012; Chen et al., 2024). In the United States context, wealthy Southern slaveholders used strategic marriages and extended kinship networks to mitigate the wealth shock from the Civil War (Ager, Boustan and Eriksson, 2021).

We measure *non-linearities* in assortative mating at the top of the wealth distribution – the extent to which elite circles were closed to outsiders. These non-linearities are difficult to capture with typical measures. Educational attainment fails to identify the extremely wealthy, labor earnings between spouses are confounded by selective labor force participation, and occupational categories, common in economic history, cannot distinguish exceptionally wealthy merchants or farmers from middling ones. Instead, we measure assortment based on *parental* wealth during childhood, which is observable for the entire census population between 1850 and 1870, is determined before marriage, and separates the well off from the exceptionally rich. Specifically, we use Census Tree record linkages to track married couples from the 1880–1920 censuses back to their childhood homes in the 1850–1870 censuses, where we observe each spouse's household wealth. Wealth measures are then converted into a wealth rank.

We find strong non-linearities in marital sorting at the top of the wealth distribution. Below the 90th percentile, the relationship between father's and father-in-law's wealth ranks is modest, with a rank–rank slope of 0.21. Above the 90th percentile, however, the relationship becomes much steeper. A linear model predicts that a child from a top 1 percent family would marry into a family

at the 68th percentile, but in practice the average father-in-law rank is nearly 10 percentiles higher, at 78. Equivalently, children of top 1 percent fathers were 19 times more likely to marry into another top 1 percent family than would be expected under baseline rates. These patterns vary by region. Non-linearities at the top are strongest in the South, where children from top 1 percent families were 25 times more likely to marry into another top 1 percent family, compared with 16 times outside the South.

Estimates of marital sorting based only on fathers and fathers-in-law are likely to understate the degree of sorting within elites. Observable wealth is measured with error and does not fully capture elite status, both because wealth is misreported and because latent characteristics that families match on, like social and business connections, are not directly observed. Studies that measure assortative mating through a single link between spouses or their parents cannot separate sorting on observed characteristics from sorting on these latent dimensions. To capture sorting on latent characteristics, we follow recent methodological approaches that exploit links across extended kin networks (Collado, Ortuño-Ortín and Stuhler, 2023). The key advantage is that correlations across multiple kin links should decay rapidly if matching is based only on noisily observed wealth, but persist if families sort on durable latent characteristics. We expand the data by constructing "horizontal" links through siblings and spouses. For example, starting from a given father, we trace through his child's marriage to a spouse, then to that spouse's sibling, and then to the sibling's spouse's father, yielding a pair of fathers connected by two marriages (Collado, Ortuño-Ortín and Stuhler, 2023). By iteratively moving through siblings and their marriages, we construct extended kin networks spanning up to four marriages away.

Applying this approach, we ask how quickly wealth similarity decays as kinship distance increases. The decay is strikingly slow. Conditional on a father being in the top 1 percent, the probability that a relative connected through four marriages also has a top 1 percent father is 10 percent – ten times the baseline rate. Under a linear matching model, correlations should fall off rapidly with each additional marriage link, as measurement error compounds at every step. Instead, elite correlations retain over 80 percent of their value at each successive link, suggesting that elite families married within interconnected networks rather than independently selecting high-wealth partners.

Such strong marital sorting at the top of the distribution could be driven by wealth itself, or it could reflect deeper social and cultural connections that persist independently of economic resources. To distinguish between these explanations, we subject the elite marriage network to a series of tests. First, the Civil War provides a natural experiment: it destroyed slaveholding wealth through emancipation and killed hundreds of thousands of young men, disproportionately in the South. We

estimate event-study designs comparing sorting patterns for birth cohorts who entered marriage markets before versus after the war, using both a broad South versus non-South comparison and a sharper test exploiting counties affected by Sherman's March to the Sea (Feigenbaum, Lee and Mezzanotti, 2022). Second, we examine whether the loss of a parent before a child entered the marriage market disrupted elite matching. If a father's or mother's social connections were essential for facilitating elite marriages, then early parental death should weaken sorting at the top.

Across all of these tests, we find no evidence that elite marriage patterns were disrupted. Neither the massive wealth destruction of the Civil War, nor the acute capital destruction of Sherman's March, nor the loss of a parent significantly affected the probability that children of top 1 percent fathers married into other top 1 percent families. These null results hold not only for direct marriages but also across extended kin networks at two, three, and four marriages away. The consistency of these findings points to a clear conclusion: elite marriage networks were sustained not by wealth itself or by any single parent's connections, but by families' membership in a social class that channeled marriages toward other elite families across generations.

This paper contributes to several literatures. First, we contribute to research on assortative mating and inequality (Eika, Mogstad and Zafar, 2019; Greenwood et al., 2014). Existing studies typically measure sorting using education or labor earnings, which are too coarse to identify the extremely wealthy. We show that the relationship between parental wealth and spousal wealth is sharply nonlinear at the top, a pattern invisible to standard rank–rank correlations or educational sorting measures. Second, we contribute to the growing literature on multigenerational mobility and extended kin networks (Collado, Ortuño-Ortín and Stuhler, 2023; Adermon, Lindahl and Palme, 2021; Ward, Buckles and Price, 2025). Adermon, Lindahl and Palme (2021) argue that sorting on latent characteristics is central to explaining why dynastic persistence exceeds what two-generation estimates predict. We provide direct evidence of this sorting by exploiting extended kin networks horizontally, linking individuals through chains of siblings and their spouses. The slow decay of wealth correlations across up to four marriage links reveals that elite families sorted on durable, unobserved characteristics beyond what a single census wealth observation can capture.

Third, we contribute to economic history research on wealth inequality and elite persistence in nineteenth-century America (Atack and Bateman, 1981; Steckel, 1990; Dupont and Rosenbloom, 2022; Ager, Boustan and Eriksson, 2021). Our Civil War analysis provides direct evidence that elite marriage networks were sustained through social ties and shared culture rather than wealth itself, complementing development economics research showing that elite networks shape economic outcomes more broadly (Amsden, DiCaprio and Robinson, 2012; Chen et al., 2024).

2. Related Literature and Historical Background

Modern studies of assortative mating emphasize sorting on spousal earnings or education, reflecting a setting in which both partners contribute income to the household (Eika, Mogstad and Zafar, 2019; Greenwood et al., 2014). Nineteenth-century marriage markets operated under a fundamentally different logic. Married women's labor force participation was extremely low, particularly in wealthy families, so the gains from marriage had little to do with a wife's ability to contribute income. The Beckerian gains from trade arose instead through family wealth and social capital: marriage consolidated fortunes, forged alliances between elite households, and provided access to social and business networks that shaped economic opportunity for generations.

The legal framework amplified the economic stakes of these decisions. Under the common-law doctrine of coverture, a wife's legal identity merged with her husband's upon marriage, and her property became his (Chused, 1983). A poorly chosen match could therefore expose a family's wealth to a husband's creditors or mismanagement. Married women's property acts, introduced in the mid-nineteenth century, allowed wives to retain separate ownership of property and were intended less as instruments of gender equality than as shields against such risks (Chused, 1983; Koudijs and Salisbury, 2020). Whether these reforms actually changed marriage market behavior remains an open question, as direct evidence on their effects is largely absent from the literature. For elite families, the marginal benefit may have been small: wealthy fathers routinely placed a daughter's marriage portion in a trust managed by independent trustees, insulating it from both profligate husbands and outside claims well before the legal changes (Lebsock, 1977). Elite families, in other words, had already developed private mechanisms to manage the risks of marriage, precisely because the stakes were so high.

Given these stakes, families tightly controlled the process through which marriages were formed. Courtship was organized through formal social institutions: high-society balls, dinner parties, and a defined social "season" in which eligible young people were introduced under parental supervision, all restricted to families of recognized social standing. Fathers managed the business relationships and community standing that determined which families were considered acceptable, while mothers maintained the social ties through which eligible partners were identified. Marriage was a family decision as much as an individual one, and the matching process was embedded in social networks that were difficult for outsiders to penetrate.

These networks were reinforced by explicit norms of class endogamy. "Marrying beneath one's station" carried social stigma, and families actively policed the boundaries of acceptable matches. In an era without standardized credit reporting or public records of wealth, a family's reputation

within its social network served as the primary source of information about a prospective match's true economic standing and character. The information that mattered for marriage was carried by the network itself, not by any single observable measure of wealth.

A growing literature in economic history examines assortative mating in historical settings. Eriksson et al. (2023) study marriage and intergenerational mobility for women using marriage certificates from 1850 to 1920, while Clark and Cummins (2022) document assortative mating patterns in England over two centuries. Curtis (2022) shows that marriage matching has been a persistent determinant of inheritance patterns in Quebec from 1800 to 1970, and Ottinger and Voigtländer (2025) provides a broader overview of how European dynasties used marriage strategically to forge political and economic alliances. Our contribution differs by measuring sorting on parental wealth rather than occupation or social status. This distinction is critical: the sharp nonlinearities at the top of the distribution that we document are invisible to occupational measures, which cannot distinguish an exceptionally wealthy farmer from a middling one.

3. Data

3.1 Census Data and Record Linkage

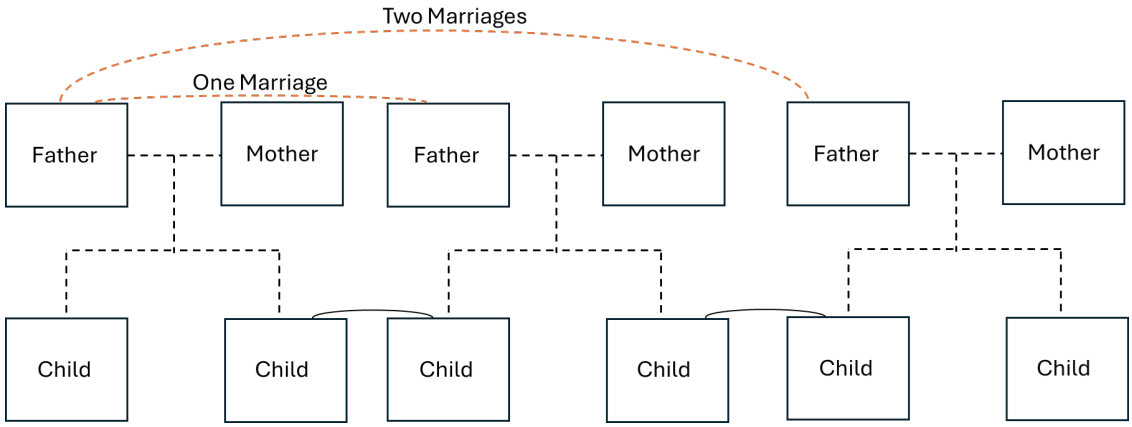
Our analysis requires a dataset with two features: parental wealth observed for both spouses in a marriage, and links across extended kin to trace wealth correlations through multiple marriages within a family network. We construct this dataset using the complete-count census files from IPUMS-USA for 1850 through 1910 (Ruggles et al., 2024). These data provide the universe of enumerated individuals, allowing us to construct large samples of linked family networks that would be impossible with samples.

The baseline dataset links fathers to their children's fathers-in-law through three steps. First, we identify parent-child pairs in the 1850, 1860, or 1870 censuses using IPUMS household relationship codes, restricting to children aged 14 or younger who co-reside with a parent. Second, we link these children forward to a census in which they appear as married adults (1870, 1880, 1900, or 1910), using the Census Tree linking files, which match individuals across enumerations based on name, age, birthplace, and other characteristics (Buckles et al., 2025). From the adult household, we identify each person's spouse using relationship codes. Third, we link each spouse back to a childhood census to identify the spouse's father.

This procedure yields pairs of fathers connected by a single marriage–the union of their respective children. We refer to this as the *one-marriage sample*. The final sample contains over 1.5 million father/father-in-law pairs with valid wealth observations. Figure 1 illustrates the linking structure.

Central to our contribution is the construction of extended kin networks that go well beyond the immediate father/father-in-law pair. Most studies of assortative mating are limited to a single link between spouses or their parents; we instead construct datasets linking fathers through two, three, and four marriages. The additional links come from siblings' spouses: once we identify a married individual in adulthood, we can also identify their siblings, since we have already linked them back to their childhood household. If a sibling is also married, we can link that sibling's spouse back to their own childhood household, identifying yet another father. This allows us to estimate the correlation between a father's wealth and the wealth of a spouse's sibling's father-in-law. Because there are two marriage links (and one sibling link) connecting these fathers, we call this the *two-marriage sample*.

Figure 1: Process of linking families and extended kin



Notes: Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

We extend this logic to the *three-marriage* and *four-marriage samples*, tracing increasingly distant connections through the marriage network. Each additional marriage link requires identifying a sibling of the last spouse in the chain and finding that sibling's spouse and their father. Because wealth is observed only in 1850, 1860, and 1870, all fathers in these chains – regardless of how many marriages separate them – must be linked back to one of these census years to obtain valid wealth measures. These extended networks allow us to examine whether wealth correlations persist across broader kinship ties, not merely through a single child's marriage choice.

We impose several restrictions to ensure valid wealth comparisons. First, children must be observed at age 14 or younger in their childhood household to avoid selective migration out of the parental home at older ages, which could bias the sample toward children from particular family backgrounds. For instance, children of farmers tended to stay longer to help with the family farm, so including older children biases the sample toward farming families. Second, fathers must be between ages 20 and 60 at the time of wealth observation so that wealth more reliably reflects economic standing.

Although we refer to "father's wealth" throughout, we aggregate wealth to the household level by summing the reported wealth of household heads and spouses. Enumerators typically recorded wealth only for the head, but some households show separate entries for spouses; summing ensures we capture total family resources regardless of reporting conventions.

Linked historical samples may differ systematically from the underlying population if certain groups are more or less likely to be successfully matched across censuses (Bailey et al., 2020). Individuals with common names, the foreign-born, the geographically mobile, and urban residents are typically harder to link, potentially biasing estimates if these characteristics correlate with marriage patterns or wealth. Further, there is concern that as we add additional links to find extended kin, samples become less representative the further one goes out.

Appendix Table A.1 compares our linked one-marriage sample to the full population of white male household heads aged 20–60 with children. This comparison is imperfect. Inclusion in the one-marriage sample requires that children eventually marry, so any childhood characteristics that predict marriage will mechanically affect sample composition. In that case, differences between the linked sample and the population reflect selection into marriage rather than selection induced by record linkage. We cannot separately identify these two sources of selection. For this reason, in the main analysis we weight the linked sample to match the distribution of observables for all fathers with children.

The unweighted linked sample differs from the population in predictable ways. Linked fathers are less likely to be foreign-born (16% vs. 25%), less likely to reside in urban areas (12% vs. 24%), and more likely to be farmers (59% vs. 44%). These patterns are consistent with greater linking difficulty for individuals who moved frequently or had common names in immigrant communities. Linked fathers are also wealthier than the full population: the mean wealth percentile rank in the unweighted linked sample is 0.66, compared to 0.55 in the population.

There is some evidence that selection into the linked sample becomes stronger on observables as we move to more distant kin. Appendix Tables A.2–A.4 report representativeness statistics for the two-, three-, and four-marriage samples. These samples are increasingly likely to consist of farmers and are modestly wealthier. For example, the four-marriage sample has a mean wealth percentile rank of 0.68, compared to 0.66 in the one-marriage sample. The rising share of farming families may also reflect differences in family size. Farmers tended to have larger families, which mechanically increases the probability of observing extended marriage chains. Consistent with this interpretation, the average number of children in the household is 4.1 in the one-marriage sample, compared to 4.4 in the four-marriage sample.

To address this selection, we estimate a probit model predicting successful linkage as a function of age, wealth percentile, literacy, nativity, occupation, urban status, and region. We then construct inverse probability weights as $(1 - \hat{p})/\hat{p}$, where $\hat{p}$ is the predicted probability of being linked. This weighting scheme upweights observations that resemble the hard-to-link population and downweights those that are overrepresented. We winsorize extreme weights at the 1st and 99th percentiles to limit the influence of outliers.

The weighted columns in Table A.1 show that reweighting substantially improves balance. After weighting, the mean wealth percentile falls from 0.66 to 0.56, closely matching the population mean of 0.55. The linked sample also matches the full population on foreign-born status (24% vs. 25%), urban residence (22% vs. 24%), and occupational distribution. Similar patterns hold for the extended kin samples: weighting reduces the wealth percentile gap from 0.12–0.13 to approximately 0.01–0.02 at all network distances. All regression and summary statistics use these weights unless otherwise noted.

3.2 Measuring Wealth

We measure fathers' wealth from the complete-count IPUMS files for 1850, 1860, and 1870–the only censuses that reported household wealth (Ruggles et al., 2024). In 1850, enumerators recorded only real property (land and buildings) at full market value without deducting mortgages. The 1860 census added personal property–"bonds, stocks, mortgages, notes, live stock, plate, jewels, or furniture," and, in slave states, enslaved persons. The 1870 schedule retained both columns but required that personal property below $100 be left blank, introducing left-censoring at the bottom of the distribution. Because we are primarily interested in non-linearities at the top of the wealth distribution, this lower bound is unlikely to strongly impact results.

We sum real and personal property (where available) to create total household wealth, then convert each father's wealth to a percentile rank within his ten-year age group and census year. This within-age-group ranking accounts for the life-cycle accumulation of wealth: a 30-year-old with $1,000 occupies a different position in his cohort's distribution than a 50-year-old with the same amount. For fathers observed in multiple census years, we take the maximum percentile rank across years, using the observation that places the father highest in his age-specific distribution. We define "top 1 percent" as fathers at or above the 99th percentile of this within-age-group distribution.

For Southern households in 1850 and 1860, we also observe slave holding from linked slave schedules now available through IPUMS. This allows us to examine whether slave holding status–independent of total wealth–shaped marriage market outcomes, and whether the correlation between slave holding and in-law wealth differs from the correlation based on other forms of property.

A key measurement concern is that wealth values are self-reported and therefore measured with error. In addition, some very wealthy individuals underreported or did not report their wealth holdings (Lindert and Williamson, 2016). At the same time, prior work shows that reported census wealth aligns closely with wealth recorded in tax data (Bleakley and Ferrie, 2016; Steckel, 1994, Appendix F). This form of measurement error mainly affects classification at the very top of the wealth distribution. Misclassification of extremely wealthy individuals will tend to attenuate estimated associations, including the odds ratio of marrying a top 1 percent individual conditional on being in the top 1 percent oneself. The strong nonlinearities in sorting that we document are therefore unlikely to be driven by measurement error; if anything, measurement error biases our estimates toward zero.

4. The Rich Marry the Rich

4.1 Father and Father-in-Law

Figure 2 presents the relationship between father's wealth percentile and father-in-law's wealth percentile for married couples in our linked sample.

Marital sorting on parental wealth is sharply non-linear. Panel A overlays a linear fit estimated on observations below the 90th percentile. This linear relationship captures the lower part of the distribution well, but above the 90th percentile, the actual relationship diverges sharply from the prediction. At the 100th percentile, the actual father-in-law's wealth percentile is 77.3, compared to a predicted value of 69.5. Children of the wealthiest fathers married into families substantially richer than a linear model would predict.

Panel B plots the probability that a father-in-law is also in the top 1 percent. For most of the distribution, this probability hovers around 1–2 percent, as expected. Above the 90th percentile, however, the probability jumps sharply. For children of top 1 percent fathers, the probability that their father-in-law is also in the top 1 percent is 18.7 percent – roughly 19 times the baseline rate.

Our primary results pool all children together, whether their fathers are observed in 1850, 1860, or 1870. However, there are differences across these years in what type of wealth was recorded: the 1850 census captured only real property wealth, while the 1860 and 1870 censuses also include personal property. Further, there was significant wealth compression between 1860 and 1870, raising the possibility that patterns of assortative mating differ across census years.

Figure 3 examines this by showing the top 1 percent probability and wealth percentile correlations separately for fathers observed in each census year. Strong nonlinearities are present regardless of which year is used, but assortative mating at the top appears weaker for children whose fathers are observed in 1870. The probability that a top percentile father's child married into another top percentile family is lower in 1870 than in earlier years. This pattern may reflect disruption to elite marriage markets following the Civil War, a point we return to when examining birth cohorts in Section 6.

The differences across census years also allow us to examine sorting by wealth type. The 1860 and 1870 censuses recorded both real property (land and buildings) and personal property (cash, stocks, livestock, and other movable assets), while the 1850 census captured only real property. If nonlinear sorting at the top reflects an agricultural elite closing ranks through landownership, one would expect tighter sorting on real property than on personal property. Figure 4 shows the opposite. Restricting to fathers observed in 1860 or 1870, sorting on personal property is substantially stronger at the top of the distribution: a father in the top percentile of personal property wealth has a 21 percent probability of his child marrying into another top percentile personal property family, compared to 15 percent for real property. This pattern suggests that the mechanisms driving elite assortative mating extend beyond visible landholdings to encompass broader forms of economic capital.

Similarly, elite sorting varies by geography within the wealth distribution. Figure 5 compares urban and rural areas. The overall rank–rank slope is similar in both settings (0.206 rural, 0.192 urban), but the nonlinearity at the top is substantially stronger in cities. A top 1 percent urban father has a 21 percent probability of his child marrying into another top 1 percent family, compared to 17 percent in rural areas. This gap is consistent with a more closed elite class within cities, where

wealthy families lived in closer proximity and could more easily restrict their social circles to others of similar standing.

Figure 6 examines regional heterogeneity by comparing the South to the non-South. The antebellum South was characterized by a small but powerful planter elite whose wealth was concentrated in land and enslaved persons. Historical accounts suggest that these elite families cultivated extensive kinship networks through strategic marriages, maintaining social cohesion and consolidating economic power within a relatively small circle. Panel A shows that the wealth percentile correlation is similar across regions, indicating that the overall pattern of assortative mating was comparable. However, Panel B reveals that elite sorting at the top 1 percent is substantially stronger in the South. Southern families with top 1 percent fathers were more likely to marry into other top 1 percent families than their Northern counterparts, consistent with tighter elite marriage networks in the antebellum South. This regional difference foreshadows our later analysis of the Civil War, which tests whether the destruction of Southern wealth disrupted these entrenched patterns.

Our approach differs from studies that use occupation-based measures of economic status (Eriksson et al., 2023). Because we observe actual wealth, we can differentiate between rich and poor families within the same occupation. This distinction is critical: the non-linear relationship at the top of the distribution cannot be identified using standard occupational proxies.

To demonstrate this, we construct two alternative measures: (1) an occupational wealth score, defined as the mean wealth of individuals in each occupation using 1860–1870 census data, and (2) a predicted wealth score that incorporates demographic characteristics and occupation, similar to the approach developed by Saavedra and Twinam (2020).[1] Figure 7 shows the results using these measures.[2] Neither produces the sharp non-linearity we observe with actual wealth. The binscatter relationships are approximately linear throughout the distribution, and the probability of matching with a "top 1 percent" family (defined by the alternative score) shows only modest increases at the top.

[1]The predicted wealth score is constructed following an approach similar in spirit to Saavedra and Twinam (2020). Using the pooled 1860–1870 full-count census data, we regress total wealth (real plus personal property) on a rich set of covariates for men aged 25–60: detailed occupation indicators (1950 basis), occupation-by-race interactions, state of residence, race, race-by-region interactions, urban status, occupation-by-region interactions, and country of birth and its interaction with broad occupation categories. The predicted values from this regression serve as the wealth score. To address imprecision in small cells, we smooth the predictions: when an occupation-state-race-urban-birthplace cell contains fewer than 30 observations, we replace the predicted value with the corresponding region-level mean; if the region-level cell is also small, we use the national-level mean.

[2]Appendix Figures B.6 and B.7 show the corresponding results for four marriages away.

The reason is that occupational categories obscure substantial within-occupation variation in wealth. This is particularly true for farmers, who comprised the majority of household heads in this period. A farmer with 500 acres and substantial livestock occupied a fundamentally different economic position than a farmer with 20 acres, yet both receive the same occupational score. The wealth data available in the 1850–1870 censuses are therefore essential for identifying the non-linear sorting patterns that characterize elite marriage markets.

4.2 Marriage Rates by Parental Wealth

The strong nonlinearities documented above raise the question of whether they reflect selection into marriage. If children from wealthy families were less likely to marry, and those who did marry were an unusual subset, the patterns could partly reflect differential selection rather than differential sorting.

Figure 8 plots the probability of being married for individuals observed between ages 35 and 50 as a function of father's wealth percentile, separately for men and women. Marriage rates exceed 90 percent for both sexes across most of the wealth distribution. Beyond roughly the 90th percentile of father's wealth, however, marriage rates decline, with the drop concentrated for women. Daughters of the wealthiest fathers are 5–10 percentage points less likely to be married in midlife, with marriage rates falling from about 90 percent to roughly 80–85 percent.

This pattern is consistent with an acceptance threshold in elite marriage markets. In search models, individuals with attractive outside options set a higher reservation quality for prospective partners (Burdett and Coles, 1997). Children from wealthy families faced weaker incentives to marry for economic security, since parental wealth could substitute for a spouse's resources, effectively raising the threshold for an acceptable match. This mechanism generates a joint prediction of lower marriage rates and stronger sorting conditional on marriage. The data match this prediction: although children from wealthy families were less likely to marry, those who did marry were much more likely to match with partners from similarly wealthy backgrounds. The decline in marriage rates at the top thus reflects selective participation rather than weaker sorting: elite marriage markets combined strong assortative matching with a lower overall propensity to marry.

The gendered pattern in marriage rates is itself suggestive of hypergamy, the tendency for women to marry up in wealth relative to their fathers (Becker, 1991). If daughters of wealthy men preferred husbands from equally or more wealthy families, the pool of acceptable matches would shrink at the top of the distribution, reducing marriage rates for elite women more than for elite men. However, conditional on marrying, we find no gender difference in sorting. Sons and daughters from top 1

percent families are equally likely to match with another top 1 percent family (18.6 and 18.8 percent, respectively), and the wealth percentile correlations are nearly identical across the full distribution. Hypergamy may therefore operate only on the extensive margin, by reducing the probability of marriage for daughters of the wealthy, rather than on the intensive margin of who marries whom, consistent with Clark and Cummins (2025) who find no evidence of hypergamy in England from 1837 to 2021. The nonlinearities at the top are driven by elite families matching with other elite families regardless of the child's gender. Birth order also has no detectable effect on elite matching: first-born, second-born, and later-born children all exhibit the same sharp nonlinearity at the top, and this null result holds separately for sons and daughters (Appendix Figures B.9 and B.10).

Part of the assortative mating we observe could operate through children's own wealth: if children of rich fathers were themselves wealthy by the time they married, they may have attracted wealthy spouses directly. Appendix Figure B.2 examines direct intergenerational transmission, linking fathers observed in 1850 to their children's wealth in 1870. Children of top 1 percent fathers are over 10 times more likely to reach the top 1 percent themselves by ages 28–35, suggesting that inter vivos transfers helped transmit parental advantages well before inheritance.

5. Elite Marriage Networks

The previous section documented strong assortative mating one marriage away: children of wealthy fathers married children of wealthy fathers-in-law. But wealthy families in nineteenth-century America were embedded in broader kinship networks that extended well beyond a single marriage. If elite families systematically married within interconnected circles, wealth correlations should persist across multiple marriage links–not just between a husband's father and wife's father, but between more distant relatives connected through chains of marriages. The rate at which these correlations decay as we move through the network is informative about whether elite families formed closed social circles or simply matched independently on wealth.

5.1 Sorting Across Multiple Marriages

We construct measures of relatedness across two, three, and four marriages by tracing connections through siblings and their spouses, as described in Section 3. Two marriages away captures the relationship between an individual's father and the father of their sibling's spouse. Each additional step extends the chain through another sibling link and marriage.[3] Four marriages away represents

[3]Because our extended kin networks link through siblings at each step, one might expect sibling wealth correlations to attenuate the signal at each link. However, since our outcome variables measure *father's* wealth and all siblings share the same father, the sibling correlation in the relevant variable is mechanically one. That said, siblings' own adult

the most distant in-law connection we can reliably trace. Under random matching, these correlations should decay rapidly toward zero as network distance increases, with each additional marriage link attenuating the relationship.

The resulting samples are large: over 600,000 observations at two marriages, 1.7 million at three marriages, and 1.9 million at four marriages.

5.2 Decay Across Marriage Links

Before examining decay patterns, we note that our extended kin samples necessarily exclude "self-matches"–cases where tracing a path through the marriage network returns to the same family. If wealthy families formed interconnected networks where the same families appeared repeatedly across different marriage chains, we would expect elevated rates of such circular connections at the top of the distribution. Figure 11 confirms this prediction: self-matching rates are substantially higher for top 1 percent families, particularly at four marriages away. This pattern suggests that elite families were more likely to be connected through multiple marriage pathways, exactly what we would expect if elite marriages occurred within a relatively closed set of families who intermarried repeatedly across generations. Importantly, because we exclude self-matches from our main analysis, the correlations we report likely *understate* the true degree of tightness within elite marriage networks.

Figure 9 presents the probability of extended family being in the top 1 percent across all four marriage distances. At one marriage, a top 1 percent father has an 18 percent chance of being connected to another top 1 percent family. At two marriages, this probability is 13 percent. At three marriages, 10 percent. At four marriages away, the probability remains approximately 8–10 percent, still far above the 1 percent baseline that would prevail under random matching. The slow decay of these correlations suggests that elite families married within interconnected networks rather than independently selecting high-wealth partners.

Figure 10 presents the binscatter wealth percentile correlations across all four marriage distances, while Table 1 reports the corresponding correlations. The positive relationship between father's wealth and extended family wealth persists at all distances, with the slope declining gradually as network distance increases. The wealth percentile correlation falls from 0.207 at one marriage to 0.132 at two marriages, 0.100 at three marriages, and 0.084 at four marriages. These correlations

wealth does exhibit non-linearity at the top of the distribution: for individuals in the top 1 percent, about 13 percent have a sibling also in the top 1 percent, 13 times the baseline rate (Appendix Figure B.1).

remain highly statistically significant even at four marriages away, with sample sizes exceeding 1.6 million observations.

Table 1: Wealth Correlations Across Extended Kin

	Wealth Percentile			Top 1 Percent		
Marriages	Correlation	Std. Error	Obs.	Correlation	Std. Error	Obs.
1	0.207	0.001	2,200,684	0.114	0.001	2,200,684
2	0.132	0.001	1,664,396	0.083	0.001	1,664,396
3	0.100	0.001	1,600,568	0.070	0.001	1,600,568
4	0.084	0.001	1,613,485	0.061	0.001	1,613,485

Notes: Rank-rank slopes from regressions of the connected relative's father's wealth percentile on own father's wealth percentile. "Marriages" indicates the number of marriage links separating the two individuals whose fathers' wealth is being compared. One marriage refers to the correlation between father's and father-in-law's wealth. Two marriages refers to the correlation between father's wealth and the father's wealth of a sibling's spouse. Weighted using inverse probability weights. Standard errors in parentheses. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

A natural concern is that these elite marriage networks simply reflect geographic concentration of wealth. If wealthy families clustered in the same communities – living on adjacent plantations, attending the same churches, or participating in the same social institutions – their children might marry each other simply because they were neighbors, making the wealth correlation a mechanical consequence of residential sorting.

To address this concern, we re-estimate correlations after demeaning wealth within geographic cells defined by latitude and longitude. We obtain geographic coordinates for each individual using the Census Place Project (Berkes, Karger and Nencka, 2023), which geolocates historical census enumeration places. This approach removes any sorting that operates through local marriage markets, isolating the component of assortative mating that occurs across geographic boundaries. Appendix Figure B.8 plots both the original relationship and the geographically-controlled relationship on the same axes, allowing direct comparison. While geographic controls reduce the overall slope–indicating that a portion of sorting does reflect local marriage markets–the non-linear patterns at the top of the distribution remain strong. Children of top 1 percent fathers continue to marry into other top 1 percent families at elevated rates even after removing within-location variation. This pattern holds across all four marriage distances: elite sorting is not simply a consequence of geographic concentration. Wealthy families at the very top of the distribution found each other across county and state lines, suggesting active search within networks that transcended local communities.

5.3 *Interpreting the Decay Rate*

The rate at which wealth correlations decay across extended kin provides a test of the matching process. The central observation of this paper is that matching is nonlinear at the top of the distribution: elite families match with other elite families at rates far exceeding what linear extrapolation would predict. This nonlinearity has direct implications for how correlations should decay across the marriage network, and specifically predicts that decay should be *slower* at the top of the distribution than elsewhere. We find exactly this pattern in the data.

What Decay Rates Reveal About Latent Matching

The rate at which correlations decay across marriage distances is informative about the nature of the underlying matching process. Under linear matching, the correlation at distance k satisfies $\text{Corr}_k \propto \rho^k$, where ρ is a composite parameter that captures the strength of sorting and sibling wealth transmission.[4] The ratio $\text{Corr}_{k+1}/\text{Corr}_k$ should therefore be approximately constant. If families matched solely on observed wealth, correlations should decay at this geometric rate, with measurement error compounding at each additional link. Slower decay than this benchmark would indicate that families sort on latent characteristics that persist across extended kin more durably than a single snapshot of census wealth would suggest. Table 2 tests this prediction by reporting average decay ratios across the distribution.

Table 2: Decay Rates Across the Wealth Distribution

	Marriages Away				
	1	2	3	4	Avg. Decay
Panel A: Binary Indicator Correlations					
Top 1%	0.114	0.083	0.070	0.061	0.81
Top 2%	0.134	0.095	0.078	0.068	0.80
Top 10%	0.174	0.122	0.099	0.085	0.79
Top 50%	0.149	0.092	0.070	0.060	0.75
Panel B: Within-Stratum Rank-Rank Slopes					
Bottom 90%	0.176	0.107	0.078	0.064	0.72
90–100%	0.785	0.591	0.443	0.380	0.79

Notes: Panel A reports correlations of binary indicators for whether the father and in-law were in the top 1, 2, 10, or 50%. Panel B reports within-stratum rank-rank slopes for the bottom 90% and top decile. "Avg. Decay" is the mean ratio of successive correlations across marriage distances. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

[4] In the full model, the decay rate depends on both the sorting parameter and the sibling correlation. We use ρ as a reduced-form summary of their combined effect.

Both panels reveal that correlations decay more slowly at the top of the distribution. Panel A reports correlations based on binary indicators of elite status. For the top 50 percent indicator, the average decay ratio is 0.75, while for the top 1 percent indicator, decay is markedly slower at 0.81. The pattern is monotonic: narrower elite thresholds exhibit slower decay (0.79 for the top 10 percent, 0.80 for the top 2 percent). Panel B tells a consistent story using within-stratum rank-rank slopes. Wealth rank correlations within the bottom 90 percent decay at a rate of 0.72 per marriage link, whereas within the top decile the decay rate is 0.79. Across both measures, correlations at the top of the distribution lose roughly 20 percent of their value at each successive link, compared with 25–28 percent elsewhere.

This difference is inconsistent with a model in which elites match on the same variables as everyone else, just more intensely. Instead, the slow decay implies that elite families sort on latent characteristics – social connections, human capital, and family reputation – that are transmitted more reliably across extended kin than census wealth alone can capture. Each additional link in the network introduces measurement error in observed wealth, which should erode correlations rapidly. Yet the top 1 percent correlation at four marriages away (0.061) remains over half the one-marriage-away correlation of 0.114, indicating that the unobserved components on which elites match are both durable and heritable. There is also evidence that slower decay extends beyond the very richest families: the top decile exhibits a decay rate of 0.79, suggesting that this pattern intensifies at the extreme upper tail but is present more broadly within the upper distribution.

Consequences of Elite Sorting for Dynastic Wealth

Why did elite assortative mating matter? Evidence from Kalsi and Ward (2025) suggests that marrying within the elite was a strategy for maintaining dynastic wealth. They find that 92 percent of grandchildren whose grandfather was in the top 1 percent of wealth fell out of the top 1 percent by the third generation. However, when *both* paternal and maternal grandfathers were in the top 1 percent, only 84 percent fell out, meaning that grandchildren with two elite grandparents were twice as likely to remain elite (16 percent vs. 8 percent). This pattern is consistent with a model where elite families sought marriages within their stratum not merely for consumption or prestige, but as a wealth preservation strategy. By marrying into another top 1 percent family, parents could effectively double their grandchildren's odds of maintaining elite status. The nonlinearity in sibling correlations documented in Appendix Figure B.1 helps explain this mechanism: when both parents come from elite backgrounds, their children's aunts, uncles, and extended kin on both sides of the family are more likely to also be elite, providing a broader network of advantageous connections.

6. Did Wealth Destruction Disrupt Elite Marriage Networks?

The Civil War provides a natural experiment for testing whether massive wealth destruction can disrupt elite marriage networks. The war and subsequent emancipation destroyed an estimated 60 percent of Southern wealth, with slaveholding families experiencing near-total losses of their human property (Ransom and Sutch, 1988). Ager, Boustan and Eriksson (2021) show that despite this wealth destruction, the sons of former slaveholders recovered their economic status within a generation, and they attribute this persistence to social networks and marriage connections that survived the war. Our analysis extends their work by directly measuring whether the *pattern* of elite assortative mating–the tendency for wealthy families to marry other wealthy families–was itself disrupted by the war. If elite marriage patterns are maintained primarily through economic incentives, this wealth shock should have weakened assortative mating in the South. Alternatively, if elite networks are sustained through social ties, shared culture, or kinship connections that persist independently of wealth, the shock may have had little effect on marriage patterns.

We take two approaches to test whether wealth destruction disrupted elite marriage networks. First, we compare the South to the non-South in a simple difference-in-differences framework, exploiting variation in birth cohorts to compare those who entered marriage markets before versus after the war. Second, we exploit geographic variation in exposure to Sherman's March to the Sea, following the identification strategy of Feigenbaum, Lee and Mezzanotti (2022). This approach compares counties directly in Sherman's path–which experienced destruction of physical capital in addition to emancipation–to nearby counties that experienced emancipation but not physical destruction.

6.1 Empirical Strategy

For both approaches, we estimate a triple-interaction model:

$$\text{FIL Wealth}_{i,c,t} = \sum_t \beta_t \cdot \text{Father Wealth}_i \times \text{Treatment}_c \times \text{BirthYear}_t + \gamma X_{i,c,t} + \epsilon_{i,c,t} \qquad (1)$$

where FIL Wealth is the father-in-law's wealth (either percentile rank or top 1 percent indicator), Father Wealth is the father's wealth, Treatment is either an indicator for the South or for Sherman March counties, and BirthYear are dummies for each birth cohort from 1836 to 1859. The coefficients β_t capture how the *differential* father–father-in-law relationship in treatment versus control areas varies across birth cohorts.

Cohorts born in the late 1830s would have entered marriage markets in the 1850s, before the conflict began in 1861. Cohorts born in the late 1850s would have entered marriage markets in the 1870s and 1880s, after the war's destruction. If wealth destruction weakened elite marriage networks, we would expect β_t to decline for post-war cohorts, indicating that the treatment-control gap in assortative mating narrowed after the war.

6.2 South versus Non-South

Figure 12 presents the event study results for the South versus non-South comparison. Each panel shows the differential treatment effect of being in the South on the father–father-in-law wealth relationship, estimated separately for each birth cohort. The coefficient represents how much stronger (positive) or weaker (negative) the wealth assortative mating relationship is in the South relative to the non-South for that cohort.[5]

The results show no systematic decline in the South's differential assortative mating for post-war cohorts. Across all four marriage distances, the coefficients fluctuate around zero with no clear trend. For the top 1 percent specification (the most relevant for elite networks), the coefficients are noisy but show no evidence of convergence between South and non-South after the war. If anything, some panels show slightly positive coefficients for later cohorts, suggesting that Southern elite marriage patterns remained at least as strong as non-Southern patterns even after the massive wealth destruction of the Civil War.

6.3 Sherman's March

The South versus non-South comparison may be confounded by other regional differences that changed over time. To address this, we exploit within-South variation using Sherman's March to the Sea. General William T. Sherman's army marched from Atlanta to Savannah in late 1864, deliberately destroying railroads, factories, and agricultural infrastructure in a 60-mile-wide swath across Georgia. Counties in Sherman's path experienced destruction of physical capital on top of the emancipation of enslaved workers that affected all Southern counties.

Following Feigenbaum, Lee and Mezzanotti (2022), we define treatment as counties with any area within 5 miles of Sherman's march route, identified using GIS data. The control sample consists of counties in Georgia, South Carolina, and North Carolina within 100 miles of the march route but not directly in its path. We exclude vertex cities (Atlanta, Savannah, Columbia, and

[5]Appendix Figure B.4 shows binscatter plots of father's wealth versus father-in-law's wealth separately for the South and non-South. The raw relationship is similar across regions, with both showing strong positive assortative mating.

Goldsboro) where the march began, ended, or turned. This yields 57 treatment counties and 165 control counties.

Figure 13 presents the event study results for the Sherman March comparison. The specification is identical to the South versus non-South analysis, but now the treatment indicator identifies counties in Sherman's path rather than all Southern counties. Control variables include 1860 county characteristics (population, cotton agriculture share, average slaves per farm, and manufacturing employment share) interacted with birth year fixed effects, following Feigenbaum, Lee and Mezzanotti (2022).[6]

The results again show no evidence that wealth destruction disrupted elite marriage networks. For the top 1 percent specification, the coefficients fluctuate around zero across all birth cohorts and all marriage distances. There is no systematic pattern of declining coefficients for post-war cohorts. Even at one marriage away–where we have the most statistical power–the confidence intervals comfortably include zero throughout the sample period.

6.4 Interpretation

The null results from both approaches suggest that elite marriage networks were remarkably resilient to wealth destruction. Despite the South losing the majority of its wealth and Sherman's March counties experiencing additional physical destruction, elite families continued to marry other elite families at similar rates before and after the war.

Several mechanisms could explain this resilience. First, elite networks may have been maintained through social ties and shared culture that persisted independently of wealth. Families that were "elite" before the war remained socially connected even after losing their economic resources, and these connections continued to shape marriage patterns. Second, relative wealth rankings may have been more stable than absolute wealth levels. If wealthy families lost similar proportions of their wealth, the relative ranking across families would be preserved, and families would continue to match on relative position. Third, pre-war wealth may have signaled family quality or human capital that remained valuable in marriage markets even after the wealth itself was destroyed.

These findings provide direct evidence for the mechanism proposed by Ager, Boustan and Eriksson (2021), who argue that former slaveholders maintained their economic status through social networks and marriage connections that survived the war. While they document that sons

[6]Appendix Figure B.5 shows binscatter plots of father's wealth versus father-in-law's wealth separately for Sherman March counties and control counties. The raw relationship is similar across both groups, with both showing strong positive assortative mating.

of slaveholders recovered economically and attribute this to network effects, our analysis directly measures the marriage networks themselves. We show that the broader pattern of elite assortative mating–the tendency for top 1 percent families to marry other top 1 percent families–was unchanged by the war, and that this pattern extended across multiple marriages within kinship networks. The persistence of these marriage patterns, even four marriages away, suggests that elite networks were so deeply rooted in Southern society that massive economic shocks could not disrupt them.

The Civil War analysis tests whether shocks to wealth disrupted elite marriage networks. A complementary question is whether the loss of a parent disrupted the social connections that facilitated elite matches. Parents plausibly played a direct role in arranging or facilitating their children's marriages: fathers managed business relationships and community standing, while mothers maintained the social ties and household networks through which eligible partners were identified. If elite sorting depended on these parental connections, then early parental death should weaken the relationship between father's wealth and the wealth of the child's eventual father-in-law.

We identify early parental death by linking fathers observed in the 1870 census to the 1880 census. If the father's marital status in 1880 is widowed, we classify the mother as having died before the child entered the marriage market. Similarly, we identify the father's spouse in 1870, link her to the 1880 census, and classify the father as having died if the mother is widowed by 1880. We then compare the probability of marrying into a top 1 percent family for children of top 1 percent fathers, separately by whether a parent died early.

Neither mother's death nor father's death significantly predicts the probability of a top 1 percent match (Appendix Figure B.11 and Table B.7). Children of top 1 percent fathers whose mother died by 1880 have a 17.6 percent probability of marrying into another top 1 percent family, compared to 15.7 percent for children with both parents alive; children whose father died have a probability of 21.7 percent. None of these differences are statistically significant, and the null results hold at two, three, and four marriages away. This is consistent with elite status being rooted in the broader family and social class rather than maintained through any single individual's connections. Even after losing a parent, children from elite families continued to marry into other elite families at similar rates, reinforcing the conclusion from the Civil War analysis that these networks were sustained through durable social structures that transcended the role of any one family member.

7. Did Property Rights Reform Weaken Elite Marriage Networks?

The Civil War results demonstrate that wealth destruction alone was insufficient to disrupt elite marriage networks. Neither the massive wealth losses of the South nor the physical destruction of

Sherman's March produced detectable changes in the pattern of assortative mating at the top of the distribution. A complementary question is whether changes to the *legal framework* governing marital property had any effect. Married women's property acts (MWPAs), adopted by states over the second half of the nineteenth century, altered the economic incentives surrounding marriage by allowing wives to retain separate ownership of property brought into or acquired during marriage (Chused, 1983). Under coverture, a wife's property became her husband's upon marriage, giving wealthy fathers a strong incentive to vet potential sons-in-law and to match within networks of known, trusted families. By reducing the downside risk of a poorly chosen match, MWPAs may have weakened these incentives and loosened the grip of elite families on the marriage market.

Whether MWPAs would have meaningfully affected elite sorting is an open question. As discussed in Section 2, wealthy families routinely used trusts and other legal instruments to protect a daughter's marriage portion well before the statutory reforms (Lebsock, 1977). For these families, the marginal change in legal protection was arguably small. On the other hand, MWPAs may have expanded the set of acceptable matches by reducing the perceived risk of marrying outside one's immediate social circle, even for families that already used trusts. We use the staggered adoption of MWPAs across states to test whether these reforms were associated with changes in elite assortative mating.

7.1 Empirical Strategy

We exploit the staggered timing of MWPA adoption across U.S. states to estimate an event study. States adopted MWPAs between 1848 (New York) and 1943 (Florida), with most adoptions occurring between 1861 and 1895 (Appendix Table B.8). We define treatment exposure based on birth cohort: for a state that adopted its MWPA in year Y, the first affected cohort consists of individuals born in year $Y - 20$, on the assumption that they entered the marriage market at approximately age 20. Cohorts born before this cutoff would have married under the old legal regime; cohorts born after would have married under the new one. We drop always-treated states (those with MWPA adoption before 1856, so that all cohorts in our 1836–1870 sample window were already exposed) because they provide no pre-treatment variation.

We estimate the following two-way fixed effects specification:

$$\begin{aligned} \text{Top1}_{i,s,c}^{\text{FIL}} = &\sum_{\tau \neq -1} \beta_\tau \cdot \text{Top1}_i^{\text{Father}} \times \mathbf{1}[\text{EventTime}_{s,c} = \tau] + \delta \cdot \text{Top1}_i^{\text{Father}} \\ &+ \sum_{\tau} \phi_\tau \cdot \mathbf{1}[\text{EventTime}_{s,c} = \tau] + \alpha_s + \gamma_c + \epsilon_{i,s,c} \end{aligned} \tag{2}$$

where $\text{Top1}_{i,s,c}^{\text{FIL}}$ is an indicator for the father-in-law being in the top 1 percent, $\text{Top1}_{i}^{\text{Father}}$ is an indicator for the father being in the top 1 percent, and $\text{EventTime}_{s,c}$ is the event time for state s and birth cohort c (measured as the approximate marriage year minus the state's MWPA adoption year, where marriage year is birth year plus 20). The specification includes main effects for father's top 1 percent status (δ), event-time dummies (ϕ_τ), state fixed effects (α_s), and birth cohort fixed effects (γ_c). Event time is measured in 1-year bins, trimmed to the window $[-6, +10]$ with tails collapsed into the endpoint bins. The coefficients of interest are β_τ, which capture how the relationship between father's top 1 percent status and father-in-law's top 1 percent status changes at each event time relative to the omitted period ($\tau = -1$, one year before MWPA adoption). We also estimate a continuous specification replacing the top 1 percent indicators with wealth percentile ranks, restricted to fathers in the top decile. We cluster standard errors at the state level to account for within-state correlation.

The identifying assumption is parallel trends: in the absence of MWPA adoption, the association between father's and father-in-law's wealth would have evolved similarly across states with different adoption dates. The inclusion of state and birth cohort fixed effects absorbs level differences across states and secular trends.

Because MWPAs were adopted at different times across states, this setting involves staggered treatment adoption. The standard TWFE estimator can produce biased estimates in such settings if treatment effects are heterogeneous across cohorts or over time (Callaway and Sant'Anna, 2021). To address this, we supplement our TWFE estimates with the Callaway and Sant'Anna (2021) estimator in a triple-difference framework: we estimate group-time average treatment effects separately for top 1 percent fathers and bottom 99 percent fathers using not-yet-treated states as controls, then compute the difference in their ATTs. This triple-difference isolates the effect of MWPAs on the *differential* matching behavior of elite versus non-elite families.

It is important to note that this estimator identifies the average treatment effect on the treated (ATT), not the average treatment effect. In our setting, the treated states adopted MWPAs between 1861 and 1895, while the never-treated controls adopted after 1895 (Idaho, Texas, Louisiana, Tennessee, and Florida). These control states are predominantly Southern, raising the possibility that unobserved regional differences could confound the comparison. We return to this caveat below.

7.2 Results

Figure 14 presents the TWFE event study results for the top 1 percent specification. Each panel shows the interaction coefficients $\hat{\beta}_\tau$ from Equation 2, estimated separately for one through four

marriage distances. At one marriage away (N = 1,456,847), the pre-treatment coefficients are positive, with several significantly above zero (for example, event time -6: 0.060, $p = 0.001$). The post-treatment coefficients drift downward relative to the pre-period: they are near zero in the years immediately following MWPA adoption and turn modestly negative at longer horizons (event time $+9$: -0.026, $p = 0.079$; event time $+10$: -0.020, $p = 0.270$). The overall visual pattern suggests a decline in elite matching from pre to post, but the elevated pre-treatment coefficients complicate interpretation, as they may indicate pre-existing trends that violate the parallel trends assumption. At two through four marriages away, the coefficients are noisier and no clear pattern emerges.

The main effect of father's top 1 percent status in the one-marriage specification is 0.122, so the late post-treatment coefficients (approximately -0.02 to -0.03) would represent a 16–25 percent reduction in the elite matching premium if taken at face value. However, given the pre-trend concerns, these magnitudes should be interpreted cautiously.

Figure 15 presents results from the continuous rank-rank specification, which replaces the binary top 1 percent indicators with wealth percentile ranks for fathers in the top decile. At one marriage away, the rank-rank slope shows a clearer decline in the post-treatment period than the top 1 percent specification, with post-treatment coefficients trending negative. At extended marriage distances, the results are imprecise.

The Callaway and Sant'Anna (2021) triple-difference yields mixed results. The post-treatment average ATT for top 1 percent fathers at one marriage away is positive (0.043, $p = 0.134$), while at three marriages away it is negative (-0.059, $p = 0.043$). At extended marriage distances, pre-trends are more pronounced, limiting the informativeness of the estimates.

7.3 Interpretation

The MWPA results are suggestive but not definitive. The TWFE event study at one marriage away shows a visual pattern of declining elite matching from pre to post, but positive pre-treatment coefficients raise the possibility of pre-existing trends. The Callaway–Sant'Anna triple-difference does not resolve this ambiguity, showing positive effects at one marriage and negative effects at three marriages.

If the decline is real, one interpretation is that the legal protections afforded by MWPAs reduced the stakes of marriage for wealthy families. Under coverture, a daughter's wealth was fully exposed to her husband's management and creditors, creating strong incentives for fathers to restrict matches to a trusted circle of equally wealthy and well-known families. Once MWPAs insulated the wife's

property from these risks, the cost of matching outside the elite circle declined, and wealthy families became somewhat less exclusive in their marriage patterns.

Several caveats are in order. First, the never-treated control group consists of only five states (Idaho, Texas, Louisiana, Tennessee, and Florida), all predominantly Southern. The small and geographically concentrated control group limits external validity and may introduce confounding if these states experienced different trends in marriage patterns for reasons unrelated to MWPAs. Second, MWPA adoption was not random, and states that adopted earlier may have differed from later adopters in ways that also affected marriage markets. Third, the pre-treatment coefficients in the TWFE specification are not uniformly close to zero, particularly at one marriage away, where several are significantly positive. This pattern is consistent with a pre-existing downward trend in elite matching that predates MWPA adoption, which would bias our estimates. Fourth, as discussed in Section 2, wealthy families routinely used trusts to protect a daughter's marriage portion before the statutory reforms, so the marginal legal change may have been small for the families most affected by elite sorting.

Given these limitations, we view the MWPA results as providing weak evidence that changes in the legal framework governing marital property may have been associated with changes in elite marriage patterns. The contrast with the clear null results from the Civil War is itself informative: wealth destruction left elite networks intact, but the direction and magnitude of MWPAs' effects remain uncertain. Resolving this question may require additional sources of variation in property rights institutions or finer measures of which families were most affected by the legal changes.

8. Conclusion

This paper documents strong and persistent wealth assortative mating at the top of the distribution in nineteenth-century America. Using linked census records that trace over 1.5 million fathers from childhood households to their children's marriages, we measure the relationship between father's wealth and father-in-law's wealth. The overall rank–rank correlation between father's and father-in-law's wealth is 0.207, a figure likely attenuated by measurement error in census wealth. The slow decay of correlations across extended kin suggests that measurement error is substantial: elite correlations retain 81 percent of their value at each additional marriage link, compared to 72 percent for the bottom 90 percent. If families matched only on noisily observed wealth, correlations would decay far more rapidly. More striking is the non-linearity at the top: children of top 1 percent fathers were 19 times more likely to marry into another top 1 percent family than baseline rates

would predict. This pattern suggests that elite marriage markets operated under different rules than those governing the rest of the population.

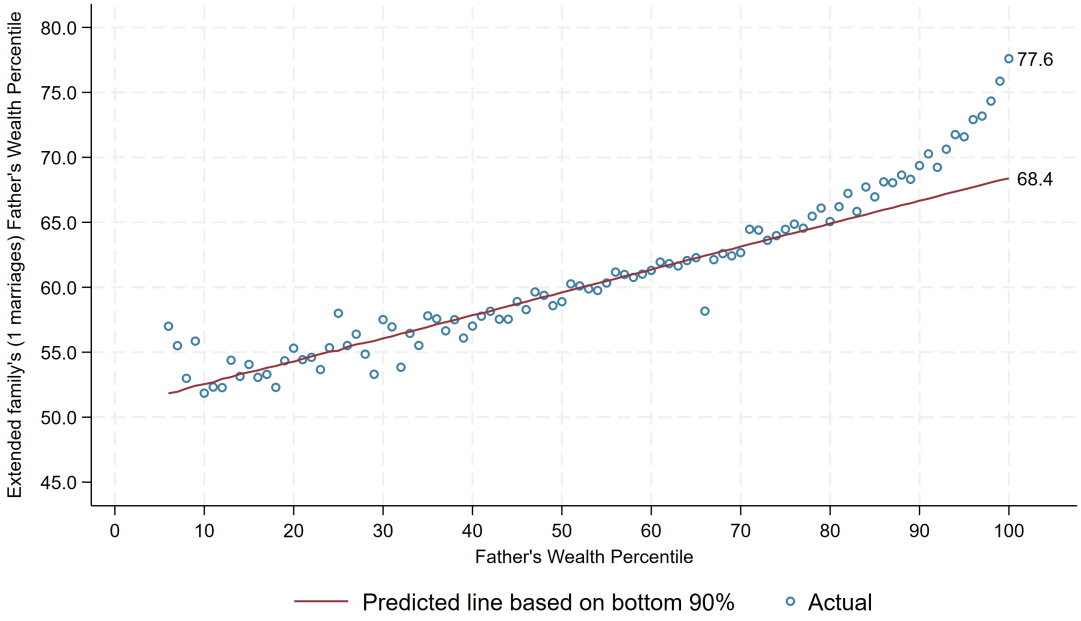
These elite marriage patterns extended well beyond immediate in-laws, creating interconnected family networks that spanned multiple marriage links. Even four marriages away–connecting individuals through chains of siblings and their spouses–the probability that two connected individuals both have top 1 percent fathers remains 8–10 percent, far above the 1 percent baseline that would prevail under random matching. The rate of decay across marriage links is far slower than a linear matching model would predict, consistent with elite families forming closed social circles rather than independently selecting wealthy partners. Geographic controls explain only a small portion of this sorting, indicating that elite networks transcended local marriage markets and connected families across county and state lines.

The Civil War provides particularly compelling evidence on the nature of these networks. Despite destroying an estimated 60 percent of Southern wealth through emancipation and physical destruction, the war had no discernible effect on elite marriage patterns. Cohorts entering marriage markets after the war exhibited the same degree of assortative mating as pre-war cohorts, both in the South and in counties directly affected by Sherman's March. This resilience suggests that elite networks were maintained through social ties, shared culture, and kinship connections that persisted independently of the wealth that originally defined elite status. Exploratory analysis of married women's property acts yields suggestive but inconclusive evidence that changes in the legal framework governing marital property may have weakened elite sorting, though pre-trends and sensitivity to specification prevent strong conclusions.

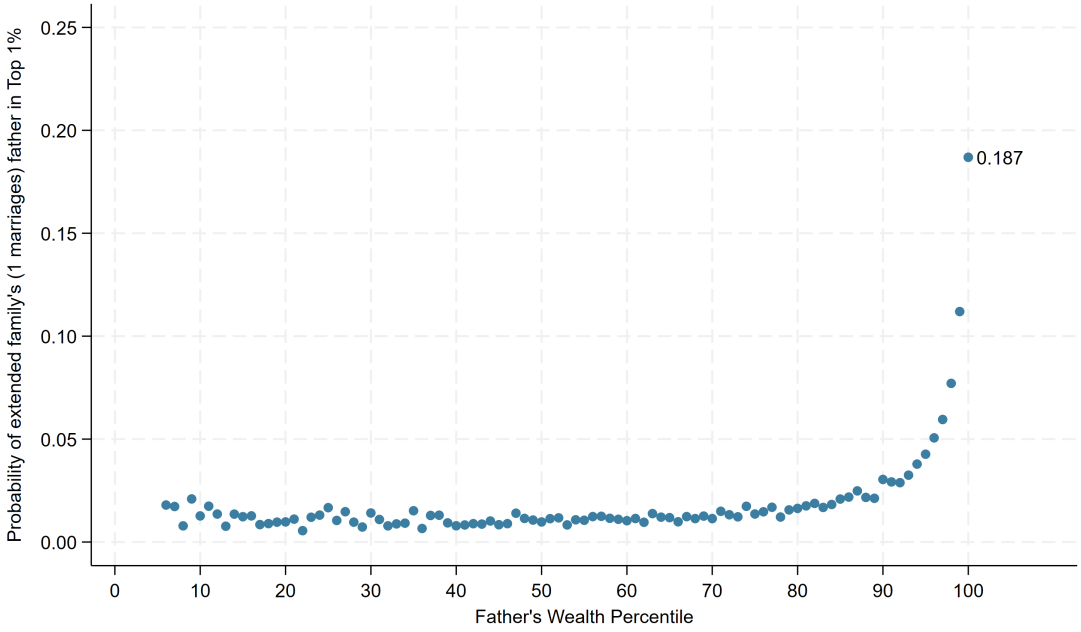
Our findings illuminate how economic elites maintained their position across generations in American history. Beyond direct wealth transfers through inheritance, marriage served as a mechanism for consolidating family fortunes and creating alliances within the wealthy. The persistence of elite marriage networks across multiple marriage links suggests that these connections were not merely individual choices reflecting similar preferences, but instead reflected coordinated strategies within extended family groups that cultivated relationships across generations. Understanding these marriage networks may help explain why wealth inequality, once established, proves so difficult to reverse: elite marriage networks survived catastrophic wealth shocks, and the institutions that sustained them proved remarkably durable.

Figure 2: Father's Wealth and Father-in-Law's Wealth

(a) Wealth Percentile Correlation



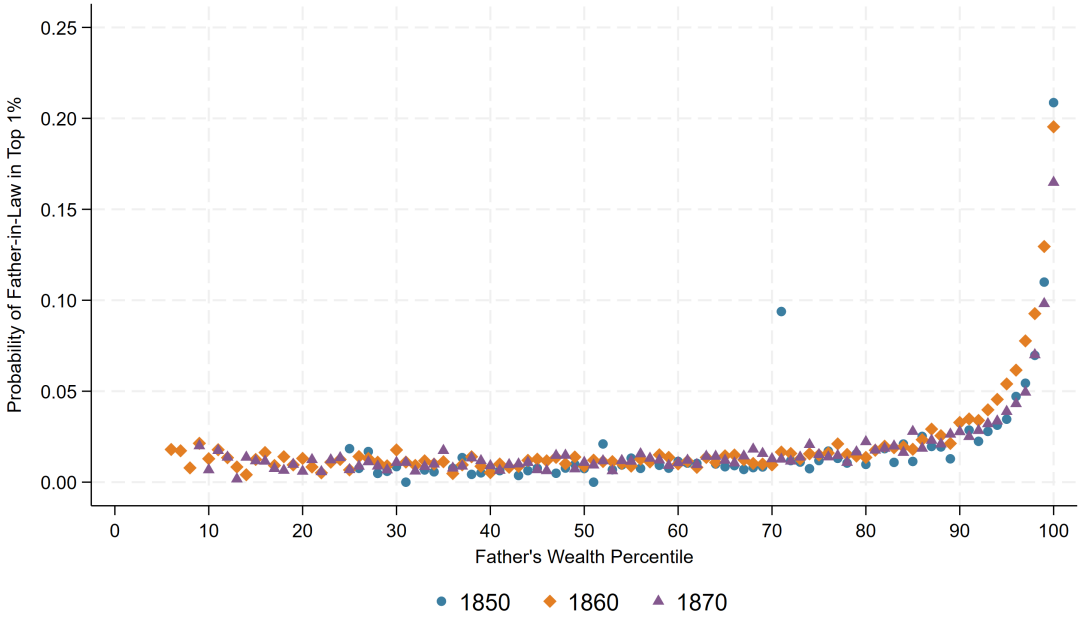
(b) Top 1 Percent Probability



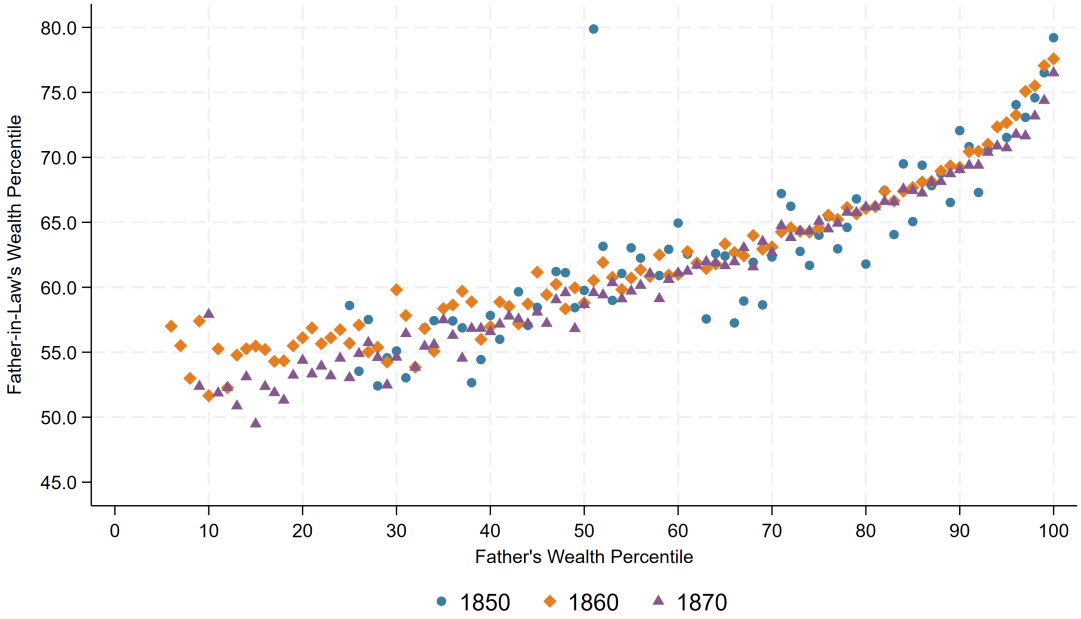
Notes: Panel A shows binned averages of father-in-law's wealth percentile for each father's wealth percentile. The red line shows the predicted relationship from the bottom 90 percent. Panel B shows the probability that the father-in-law is in the top 1 percent of the wealth distribution. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 3: Assortative Mating by Census Year

(a) Top 1 Percent Probability

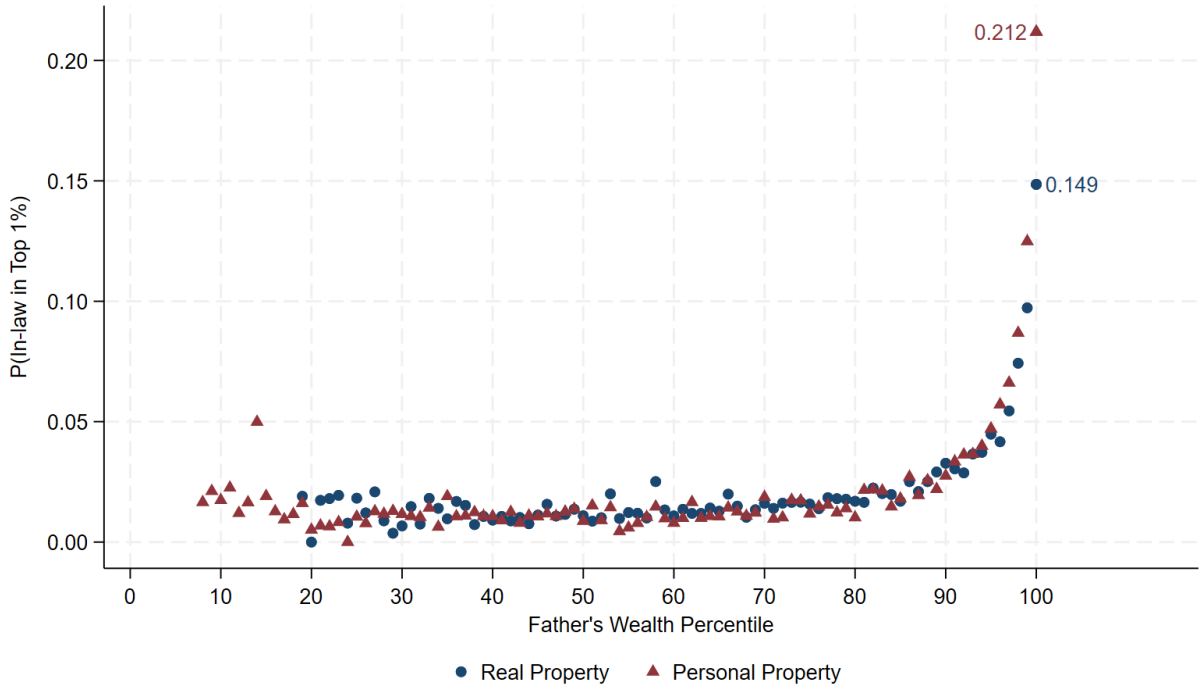


(b) Wealth Percentile Correlation



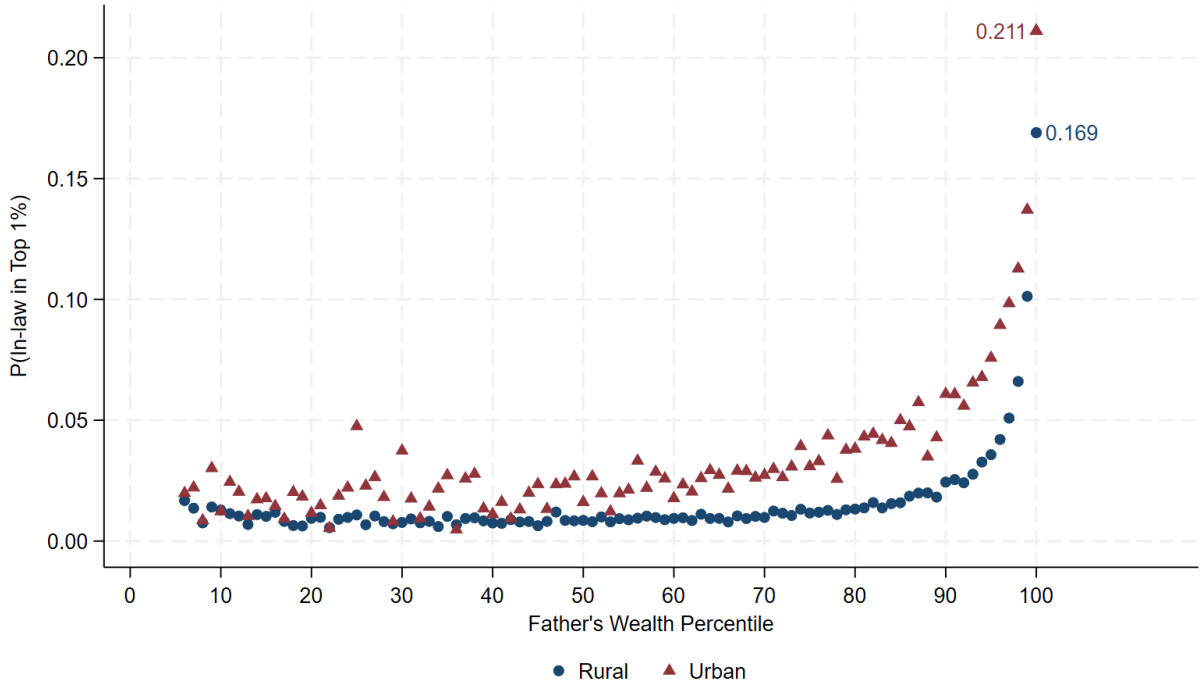
Notes: Panel A shows the probability of marrying into a top 1 percent family by father's census year. Panel B shows the binscatter of father-in-law's wealth percentile by father's census year. Census year refers to when the father's wealth is observed (1850, 1860, or 1870). Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 4: Assortative Mating by Wealth Type



Notes: Probability that the father-in-law is in the top 1 percent of the wealth distribution, shown separately for real property (land and buildings) and personal property (cash, stocks, livestock, and other movable assets). Sample restricted to fathers observed in the 1860 or 1870 census, when both wealth types were recorded. At the top of the distribution, sorting is stronger on personal property (21.2 percent) than on real property (14.9 percent). Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

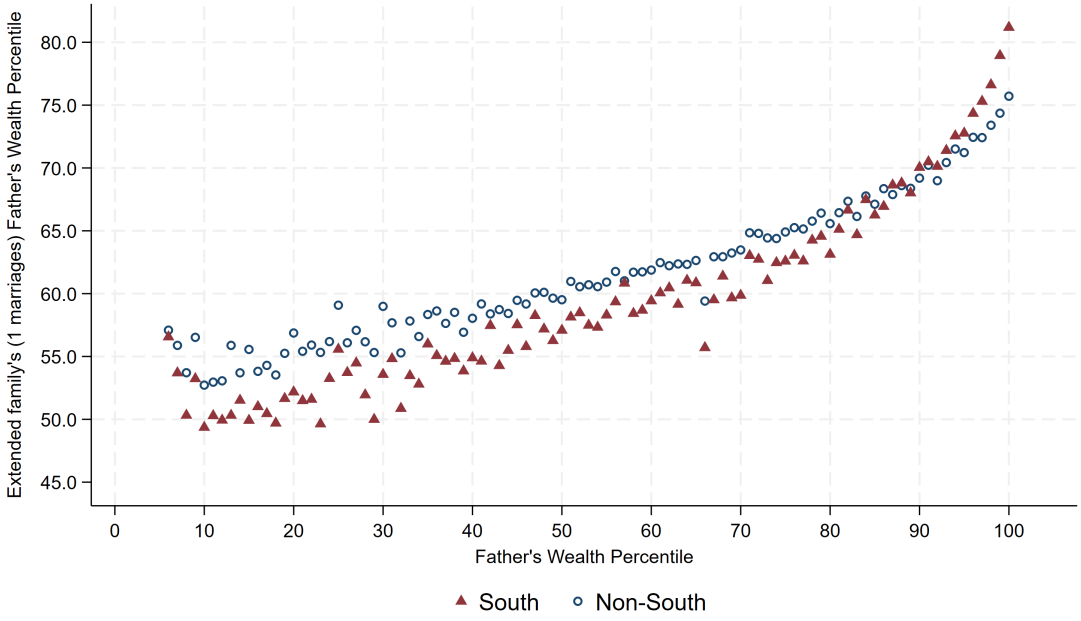
Figure 5: Assortative Mating by Urban/Rural Residence



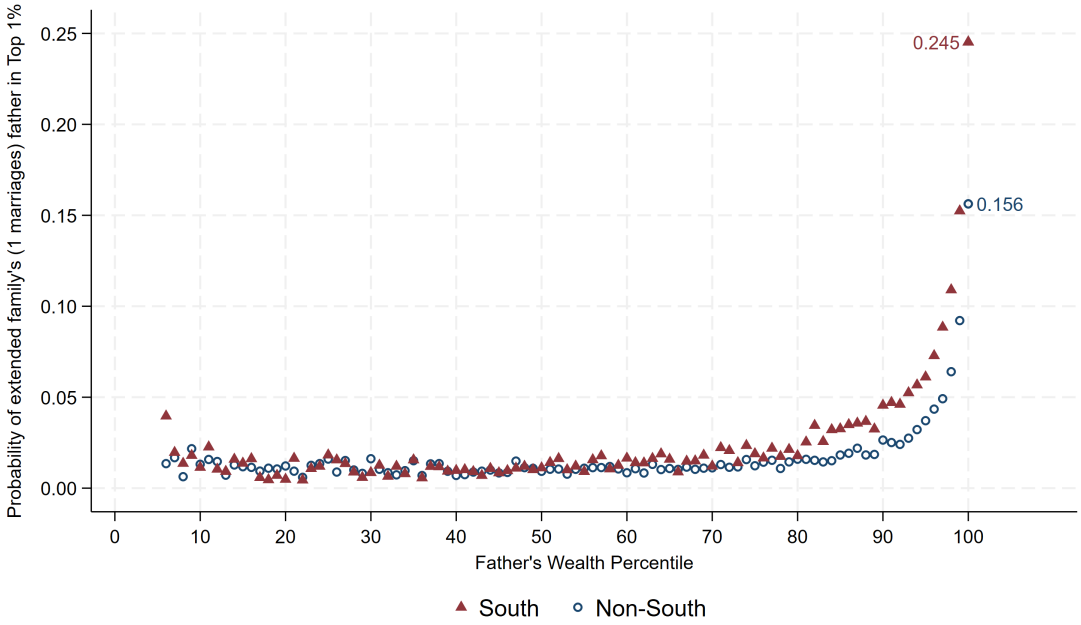
Notes: Probability that the father-in-law is in the top 1 percent of the wealth distribution, shown separately for urban and rural fathers. Urban status based on the IPUMS urban variable in the father's census year. At the top of the distribution, sorting is stronger in urban areas (21.1 percent) than in rural areas (16.9 percent). Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 6: Regional Heterogeneity: South vs. Non-South

(a) Wealth Percentile by Region



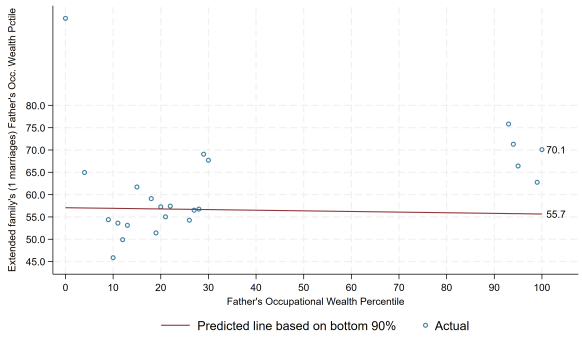
(b) Top 1 Percent by Region



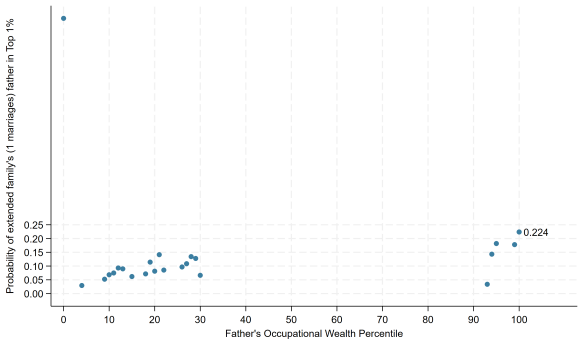
Notes: Panel A shows wealth percentile correlations separately for the South and non-South. Panel B shows the probability of marrying into a top 1 percent family by region. The South includes the former Confederate states. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 7: Assortative Mating Using Alternative Wealth Measures

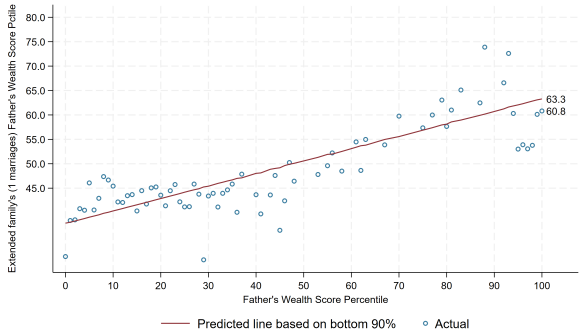
(a) Occ. Wealth Score: Wealth Percentile



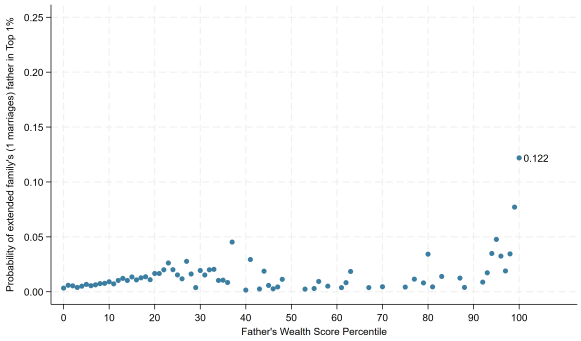
(b) Occ. Wealth Score: Top 1 Percent



(c) Predicted Wealth Score: Wealth Percentile

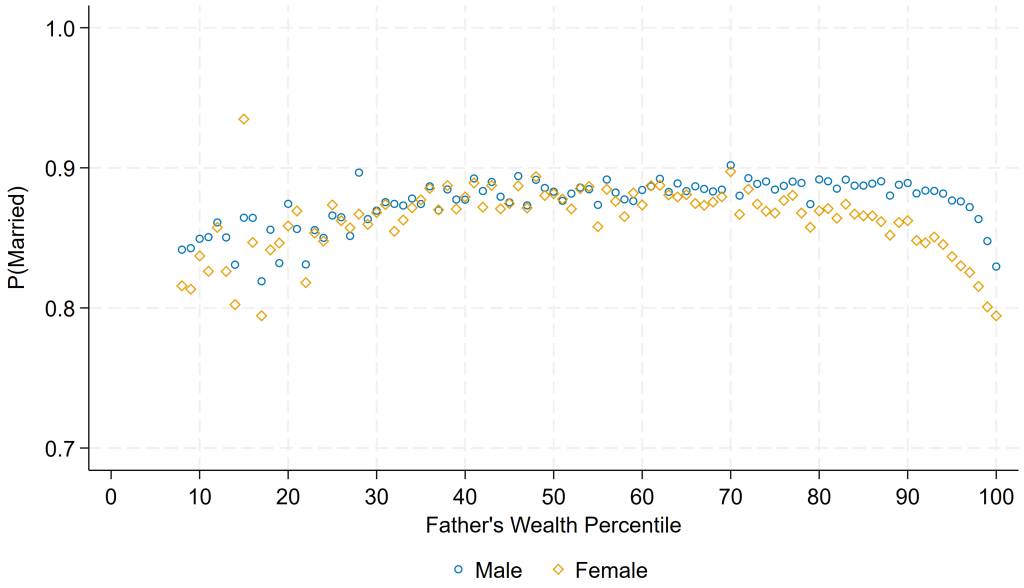


(d) Predicted Wealth Score: Top 1 Percent



Notes: Panels A and B use an occupational wealth score, defined as the mean wealth of individuals in each occupation calculated from 1860–1870 census data. Panels C and D use a predicted wealth score constructed following Saavedra and Twinam (2020), using demographic characteristics and occupation to predict wealth. Panels A and C show binned averages of father-in-law's score percentile against father's score percentile. Panels B and D show the probability that the father-in-law is in the top 1 percent of the score distribution. Neither measure produces the sharp non-linearity at the top observed with actual wealth. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 8: Marriage Probability by Father's Wealth



Notes: Each point represents the probability of being married (at approximately age 40) for individuals with fathers at a given wealth percentile, shown separately for men (blue circles) and women (orange diamonds). Sample restricted to individuals aged 35–50. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 9: Probability of Extended Family in Top 1 Percent by Marriage Distance



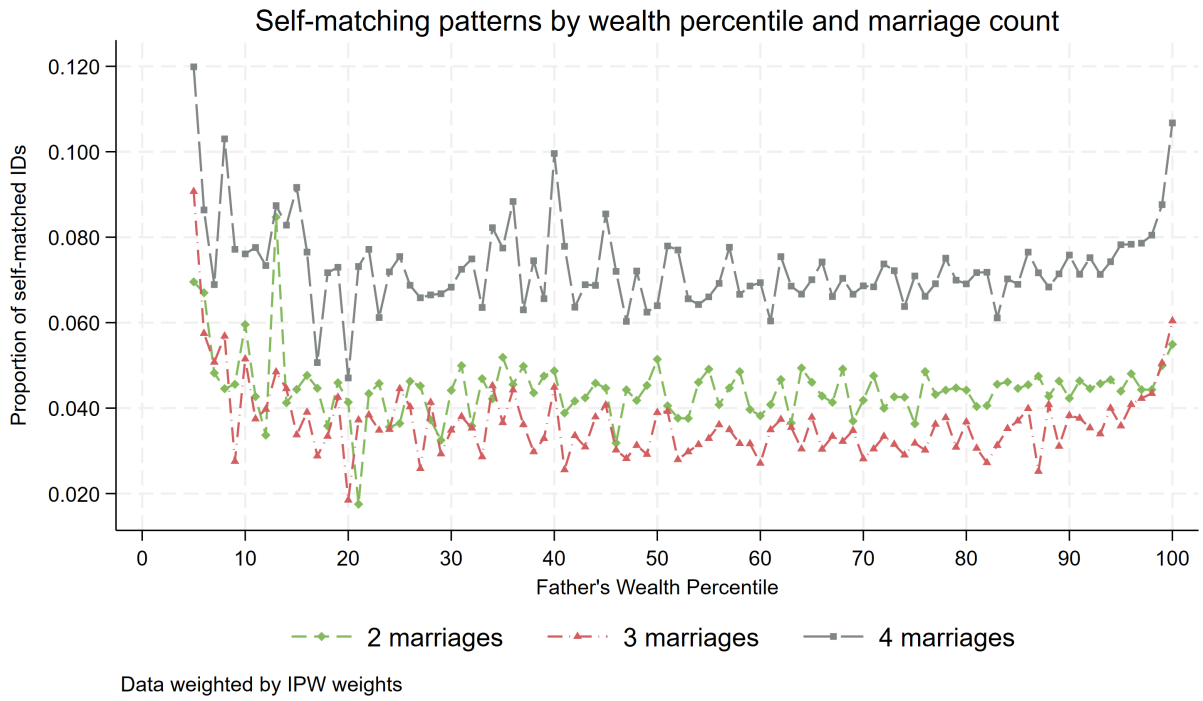
Notes: Each panel shows the probability that an extended family member's father is in the top 1 percent, plotted against the individual's own father's wealth percentile. Panels show relationships at one, two, three, and four marriages away. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 10: Father's Wealth and Extended Family Wealth: All Marriage Distances



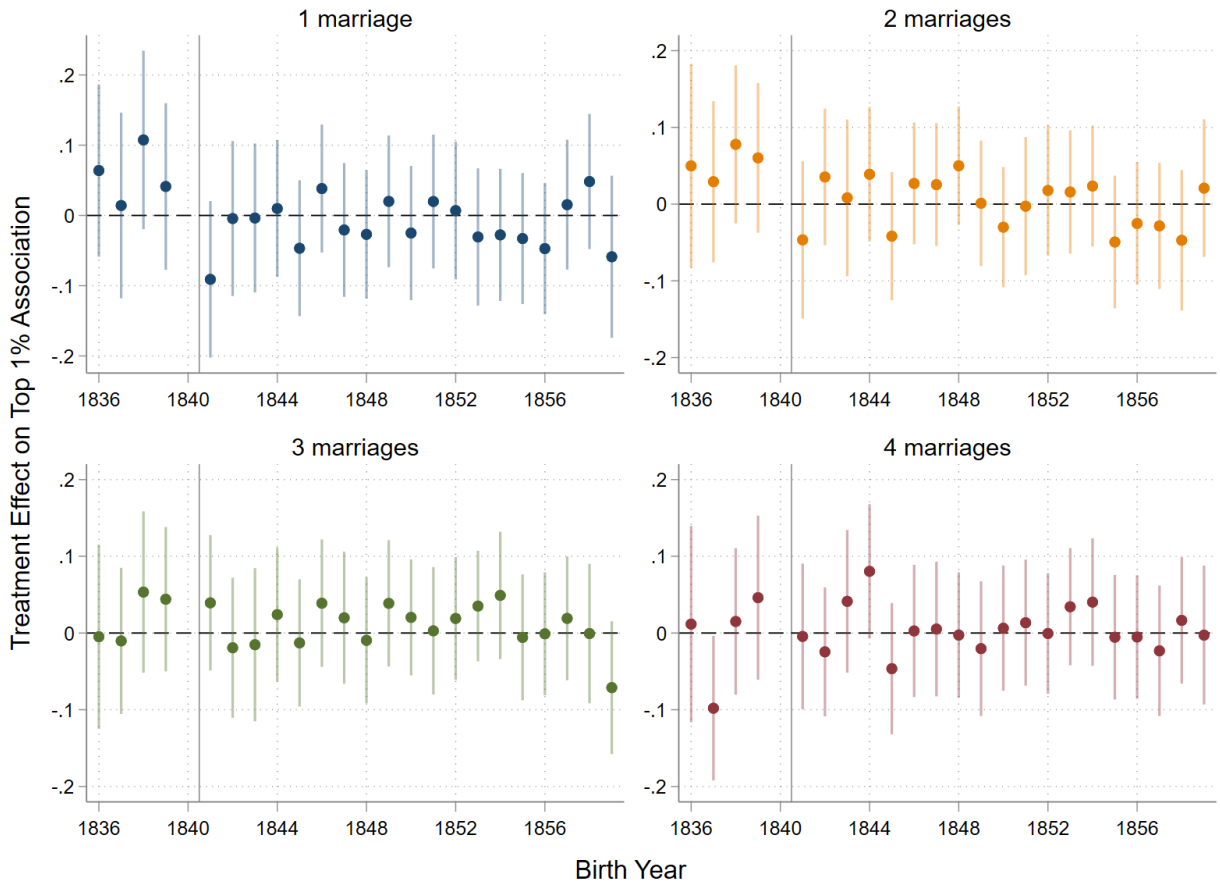
Notes: Each panel shows a binscatter of extended family father's wealth percentile against own father's wealth percentile. Panels progress from one marriage (spouse's father) to four marriages away. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 11: Self-Matching Patterns by Wealth Percentile and Marriage Distance



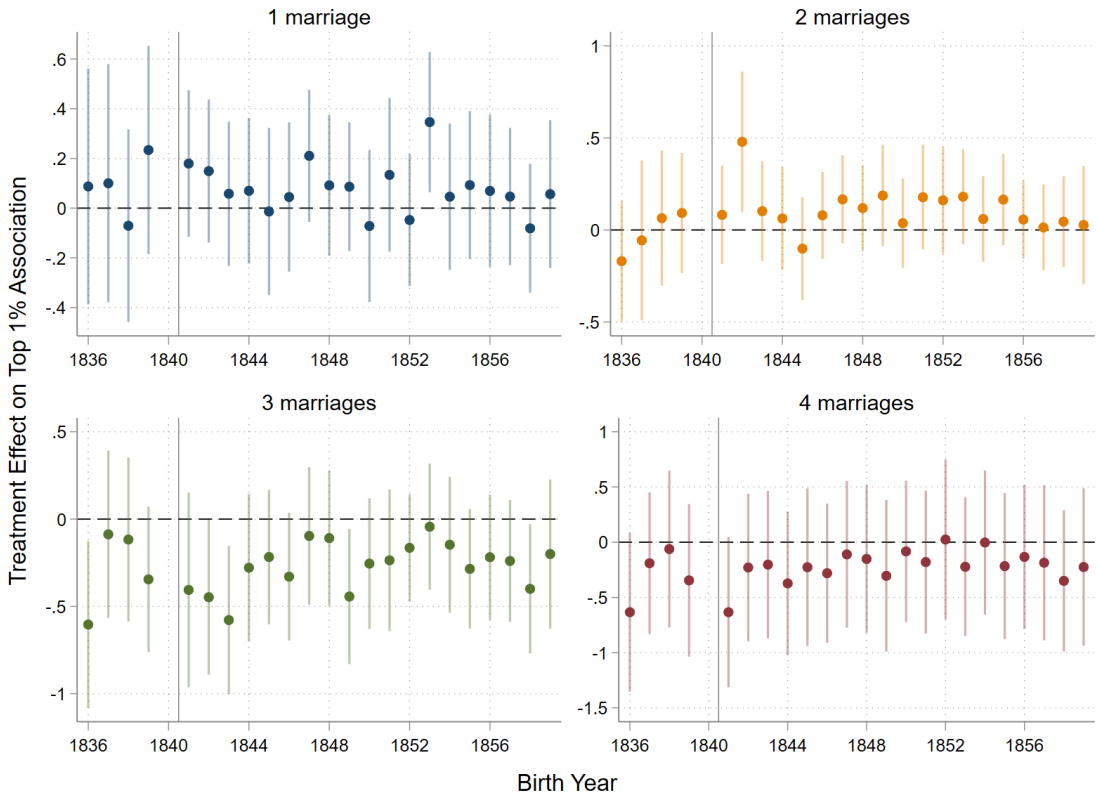
Notes: Self-matching occurs when the same father appears on both sides of an extended kin relationship, indicating circular marriage networks. Higher proportions suggest marriages occurred within closed social circles. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 12: Civil War Event Study: South vs. Non-South Treatment Effect on Elite Marriage



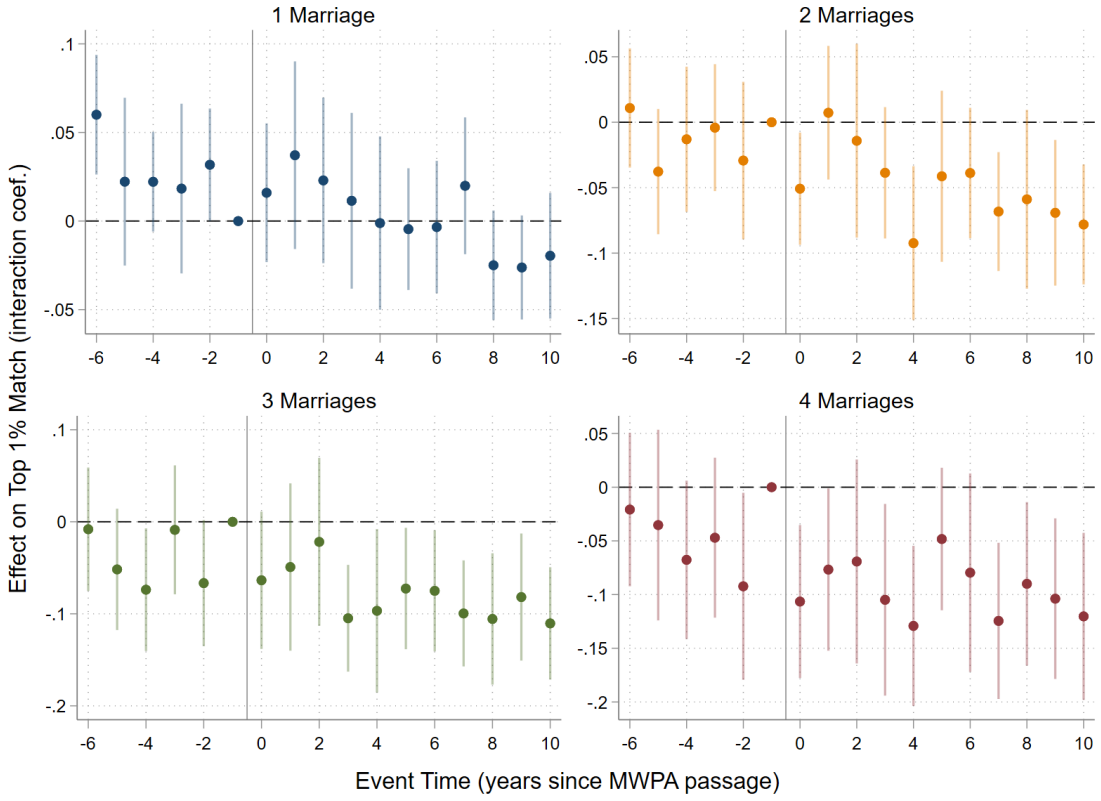
Notes: Each panel shows coefficients from regressions of father-in-law top 1 percent status on the triple interaction of father's top 1 percent status × South indicator × birth year. Coefficients represent the differential treatment effect of being in the South on elite assortative mating, relative to the non-South. Panels show results at one, two, three, and four marriages away. The vertical line marks 1841 (cohorts born before this year would have entered marriage markets before the Civil War). Error bars show 95 percent confidence intervals with robust standard errors. Sample restricted to fathers observed in 1850 or 1860 (pre-war wealth). Birth cohorts 1836–1859. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 13: Sherman's March Event Study: Treatment Effect on Elite Marriage



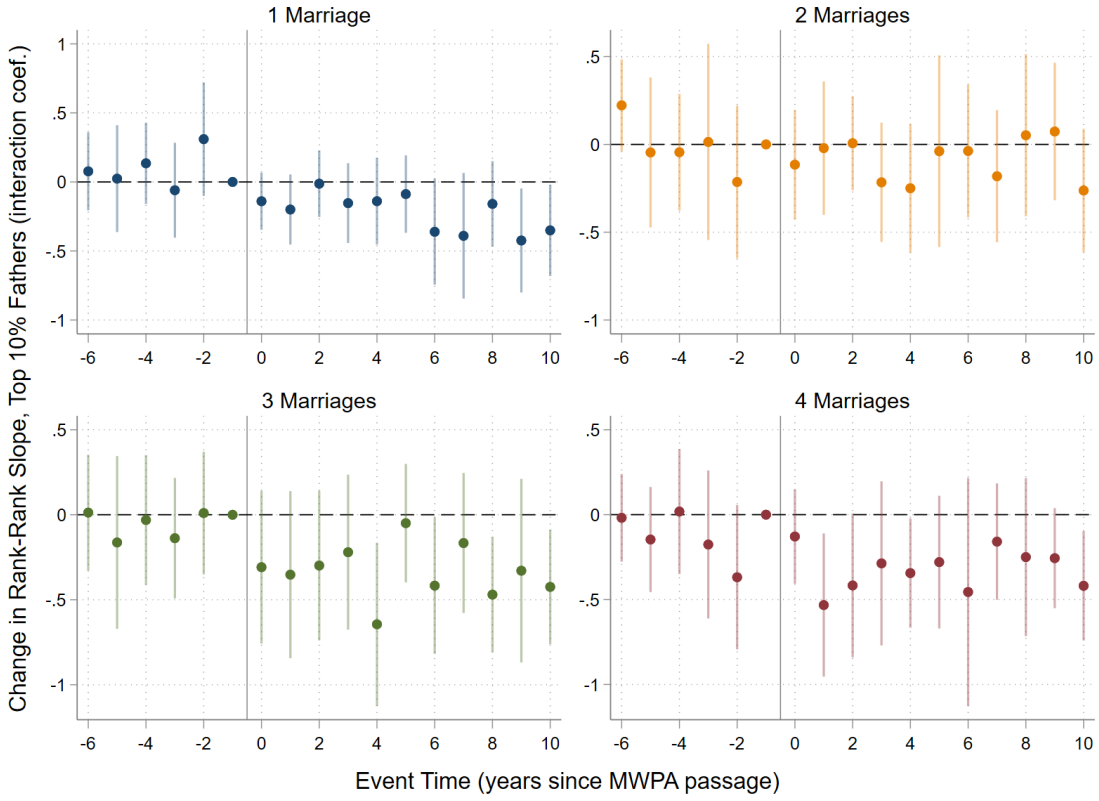
Notes: Each panel shows coefficients from regressions of father-in-law top 1 percent status on the triple interaction of father's top 1 percent status × Sherman March indicator × birth year. Coefficients represent the differential treatment effect of being in a Sherman March county on elite assortative mating, relative to control counties within 100 miles. Controls include 1860 county characteristics (population, cotton agriculture share, average slaves per farm, manufacturing employment share) interacted with birth year fixed effects. Panels show results at one, two, three, and four marriages away. The vertical line marks 1841. Error bars show 95 percent confidence intervals with robust standard errors clustered at the county level. Sample restricted to fathers observed in 1850 or 1860 in Georgia, South Carolina, or North Carolina counties within 100 miles of Sherman's march route. Vertex cities (Atlanta, Savannah, Columbia, Goldsboro) excluded. Birth cohorts 1836–1859. Treatment definition and sample construction follow Feigenbaum, Lee and Mezzanotti (2022). Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 14: MWPA Event Study: Effect on Elite Assortative Mating



Notes: Each panel shows interaction coefficients from regressions of father-in-law top 1 percent status on father's top 1 percent status $\times$ event time dummies, with state and birth cohort fixed effects. Event time is the approximate marriage year (birth year $+$ 20) minus the state's MWPA adoption year, in 1-year bins trimmed to $[-6, +10]$. The omitted category is event time $= -1$. The vertical line at -0.5 separates pre- and post-treatment periods. Error bars show 95 percent confidence intervals with standard errors clustered at the state level. Panels show results at one, two, three, and four marriages away. Always-treated states (MWPA $\leq$ 1855) are dropped. Sample includes father–father-in-law pairs with birth cohorts 1836–1870. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure 15: MWPA Event Study: Effect on Rank-Rank Association



Notes: Each panel shows interaction coefficients from regressions of father-in-law's wealth percentile rank on father's wealth percentile rank $\times$ event time dummies, with state and birth cohort fixed effects, restricted to fathers in the top decile. Event time is the approximate marriage year (birth year + 20) minus the state's MWPA adoption year, in 1-year bins trimmed to $[-6, +10]$. The omitted category is event time $= -1$. The vertical line at -0.5 separates pre- and post-treatment periods. Error bars show 95 percent confidence intervals with standard errors clustered at the state level. Panels show results at one, two, three, and four marriages away. Always-treated states (MWPA $\leq$ 1855) are dropped. Sample includes father–father-in-law pairs with birth cohorts 1836–1870. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

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Online Appendix for

For Richer or Poorer? Marriages of the Extremely Wealthy in American History

February 2026

A. Sample Representativeness

B. Additional Figures

Table A.1: Sample Representativeness: One-Marriage Sample

		Linked Sample		
	Full	Unweighted	Weighted	Link Coef.
Age	39.378	38.710	38.763	-0.615
	(9.935)	(9.551)	(9.844)	(0.011)
Number of children in home	3.284	4.055	3.346	0.062
	(2.033)	(2.154)	(2.032)	(0.002)
Top 1 percent	0.010	0.018	0.011	0.000
	(0.101)	(0.132)	(0.103)	(0.000)
Percentile rank wealth	0.546	0.662	0.556	0.010
	(0.249)	(0.226)	(0.244)	(0.000)
Literate	0.902	0.915	0.906	0.003
	(0.297)	(0.278)	(0.292)	(0.000)
Foreign-born	0.254	0.157	0.241	-0.013
	(0.436)	(0.364)	(0.428)	(0.001)
White Collar	0.110	0.094	0.112	0.002
	(0.313)	(0.292)	(0.316)	(0.000)
Farmer	0.444	0.590	0.453	0.009
	(0.497)	(0.492)	(0.498)	(0.001)
Craftsmen	0.159	0.125	0.159	-0.001
	(0.366)	(0.330)	(0.365)	(0.000)
Operative	0.087	0.063	0.086	-0.001
	(0.282)	(0.242)	(0.281)	(0.000)
Unskilled	0.149	0.090	0.141	-0.007
	(0.356)	(0.287)	(0.348)	(0.000)
Urban	0.235	0.124	0.224	-0.011
	(0.424)	(0.330)	(0.417)	(0.001)
Northeast	0.391	0.314	0.381	-0.010
	(0.488)	(0.464)	(0.486)	(0.001)
Midwest	0.352	0.409	0.358	0.006
	(0.478)	(0.492)	(0.479)	(0.001)
South	0.241	0.263	0.245	0.004
	(0.428)	(0.440)	(0.430)	(0.000)
West	0.016	0.014	0.016	-0.001
	(0.127)	(0.117)	(0.125)	(0.000)
Sample size	10485596	1563768	1563768	

Notes: Comparison of full population of white male household heads aged 20–60 with children to the linked one-marriage sample. "Full" shows population means; "Unweighted" and "Weighted" show linked sample means before and after IPW adjustment. "Link Coef." shows the coefficient from a probit model predicting successful linkage. Standard deviations in parentheses for means; standard errors in parentheses for probit coefficients. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Table A.2: Sample Representativeness: Two-Marriage Sample

		Linked Sample		
	Full	Unweighted	Weighted	Link Coef.
Age	39.378	39.194	38.771	-0.606
	(9.935)	(9.644)	(9.894)	(0.016)
Number of children in home	3.284	4.252	3.350	0.066
	(2.033)	(2.194)	(2.030)	(0.003)
Top 1 percent	0.010	0.016	0.011	0.001
	(0.101)	(0.124)	(0.103)	(0.000)
Percentile rank wealth	0.546	0.674	0.559	0.013
	(0.249)	(0.217)	(0.241)	(0.000)
Literate	0.902	0.914	0.907	0.005
	(0.297)	(0.280)	(0.290)	(0.000)
Foreign-born	0.254	0.129	0.230	-0.024
	(0.436)	(0.335)	(0.421)	(0.001)
White Collar	0.110	0.077	0.113	0.003
	(0.313)	(0.267)	(0.316)	(0.001)
Farmer	0.444	0.642	0.458	0.014
	(0.497)	(0.479)	(0.498)	(0.001)
Craftsmen	0.159	0.111	0.157	-0.002
	(0.366)	(0.314)	(0.364)	(0.001)
Operative	0.087	0.052	0.086	-0.001
	(0.282)	(0.222)	(0.280)	(0.001)
Unskilled	0.149	0.079	0.139	-0.010
	(0.356)	(0.269)	(0.345)	(0.001)
Urban	0.235	0.086	0.214	-0.020
	(0.424)	(0.280)	(0.410)	(0.001)
Northeast	0.391	0.278	0.375	-0.016
	(0.488)	(0.448)	(0.484)	(0.001)
Midwest	0.352	0.430	0.360	0.008
	(0.478)	(0.495)	(0.480)	(0.001)
South	0.241	0.279	0.249	0.008
	(0.428)	(0.449)	(0.433)	(0.001)
West	0.016	0.013	0.016	-0.001
	(0.127)	(0.113)	(0.125)	(0.000)
Sample size	10485596	884109	884109	

Notes: Comparison of full population of white male household heads aged 20–60 with children to the linked two-marriage sample. "Full" shows population means; "Unweighted" and "Weighted" show linked sample means before and after IPW adjustment. "Link Coef." shows the coefficient from a probit model predicting successful linkage. Standard deviations in parentheses for means; standard errors in parentheses for probit coefficients. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Table A.3: Sample Representativeness: Three-Marriage Sample

		Linked Sample		
	Full	Unweighted	Weighted	Link Coef.
Age	39.378	39.333	38.808	-0.569
	(9.935)	(9.665)	(9.913)	(0.020)
Number of children in home	3.284	4.356	3.357	0.073
	(2.033)	(2.214)	(2.029)	(0.004)
Top 1 percent	0.010	0.014	0.011	0.001
	(0.101)	(0.117)	(0.104)	(0.000)
Percentile rank wealth	0.546	0.677	0.560	0.014
	(0.249)	(0.213)	(0.239)	(0.001)
Literate	0.902	0.910	0.907	0.005
	(0.297)	(0.286)	(0.290)	(0.001)
Foreign-born	0.254	0.118	0.225	-0.029
	(0.436)	(0.322)	(0.418)	(0.001)
White Collar	0.110	0.068	0.112	0.002
	(0.313)	(0.252)	(0.315)	(0.001)
Farmer	0.444	0.668	0.461	0.017
	(0.497)	(0.471)	(0.498)	(0.001)
Craftsmen	0.159	0.104	0.157	-0.003
	(0.366)	(0.305)	(0.363)	(0.001)
Operative	0.087	0.047	0.085	-0.002
	(0.282)	(0.212)	(0.278)	(0.001)
Unskilled	0.149	0.074	0.138	-0.011
	(0.356)	(0.262)	(0.345)	(0.001)
Urban	0.235	0.069	0.209	-0.026
	(0.424)	(0.254)	(0.406)	(0.001)
Northeast	0.391	0.252	0.372	-0.018
	(0.488)	(0.434)	(0.483)	(0.001)
Midwest	0.352	0.442	0.361	0.009
	(0.478)	(0.497)	(0.480)	(0.001)
South	0.241	0.293	0.251	0.010
	(0.428)	(0.455)	(0.434)	(0.001)
West	0.016	0.013	0.016	-0.001
	(0.127)	(0.114)	(0.125)	(0.000)
Sample size	10485596	594263	594263	

Notes: Comparison of full population of white male household heads aged 20–60 with children to the linked three-marriage sample. "Full" shows population means; "Unweighted" and "Weighted" show linked sample means before and after IPW adjustment. "Link Coef." shows the coefficient from a probit model predicting successful linkage. Standard deviations in parentheses for means; standard errors in parentheses for probit coefficients. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Table A.4: Sample Representativeness: Four-Marriage Sample

		Linked Sample		
	Full	Unweighted	Weighted	Link Coef.
Age	39.378	39.438	38.870	-0.507
	(9.935)	(9.692)	(9.938)	(0.024)
Number of children in home	3.284	4.423	3.364	0.080
	(2.033)	(2.224)	(2.028)	(0.004)
Top 1 percent	0.010	0.013	0.011	0.001
	(0.101)	(0.112)	(0.105)	(0.000)
Percentile rank wealth	0.546	0.678	0.561	0.015
	(0.249)	(0.211)	(0.238)	(0.001)
Literate	0.902	0.907	0.907	0.005
	(0.297)	(0.291)	(0.290)	(0.001)
Foreign-born	0.254	0.113	0.223	-0.032
	(0.436)	(0.316)	(0.416)	(0.001)
White Collar	0.110	0.063	0.112	0.002
	(0.313)	(0.243)	(0.315)	(0.001)
Farmer	0.444	0.684	0.462	0.018
	(0.497)	(0.465)	(0.499)	(0.001)
Craftsmen	0.159	0.099	0.156	-0.004
	(0.366)	(0.298)	(0.363)	(0.001)
Operative	0.087	0.044	0.085	-0.002
	(0.282)	(0.205)	(0.279)	(0.001)
Unskilled	0.149	0.072	0.138	-0.011
	(0.356)	(0.258)	(0.345)	(0.001)
Urban	0.235	0.060	0.206	-0.029
	(0.424)	(0.238)	(0.404)	(0.001)
Northeast	0.391	0.234	0.371	-0.020
	(0.488)	(0.423)	(0.483)	(0.001)
Midwest	0.352	0.447	0.361	0.009
	(0.478)	(0.497)	(0.480)	(0.001)
South	0.241	0.306	0.253	0.012
	(0.428)	(0.461)	(0.435)	(0.001)
West	0.016	0.013	0.016	-0.001
	(0.127)	(0.115)	(0.125)	(0.000)
Sample size	10485596	450546	450546	

Notes: Comparison of full population of white male household heads aged 20–60 with children to the linked four-marriage sample. "Full" shows population means; "Unweighted" and "Weighted" show linked sample means before and after IPW adjustment. "Link Coef." shows the coefficient from a probit model predicting successful linkage. Standard deviations in parentheses for means; standard errors in parentheses for probit coefficients. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Table A.5: Sample Sizes by Marriage Distance

	Pairs	Unique Fathers	Unique Individuals
1 Marriage	2,209,497	1,555,406	1,623,120
2 Marriages	1,759,185	884,109	926,455
3 Marriages	1,665,410	594,263	696,329
4 Marriages	1,749,305	450,546	549,842

Notes: Pairs is the number of father/in-law pair observations in each dataset. Unique Fathers is the number of distinct ego fathers. Unique Individuals is the number of distinct person IDs appearing as either an ego father or an in-law father. An individual can appear in multiple pairs. At one and two marriages, the datasets are symmetric: every in-law father also appears as an ego father. At three and four marriages, some distant in-law fathers do not appear as ego fathers, so the number of unique individuals exceeds the number of unique fathers. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

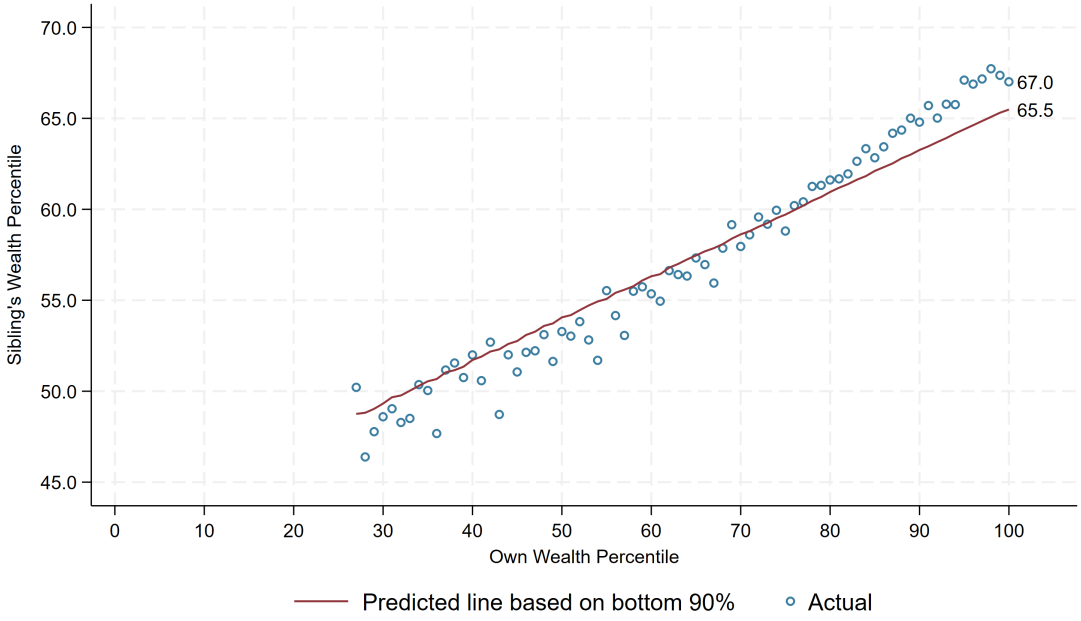
Table A.6: Wealth Correlations with Geographic Controls

	Wealth Percentile			Top 1 Percent		
Marriages	Correlation	Std. Error	Obs.	Correlation	Std. Error	Obs.
1	0.157	0.001	2,200,684	0.091	0.001	2,200,684
2	0.090	0.001	1,664,396	0.057	0.001	1,664,396
3	0.059	0.001	1,600,568	0.044	0.001	1,600,568
4	0.043	0.001	1,613,485	0.035	0.001	1,613,485

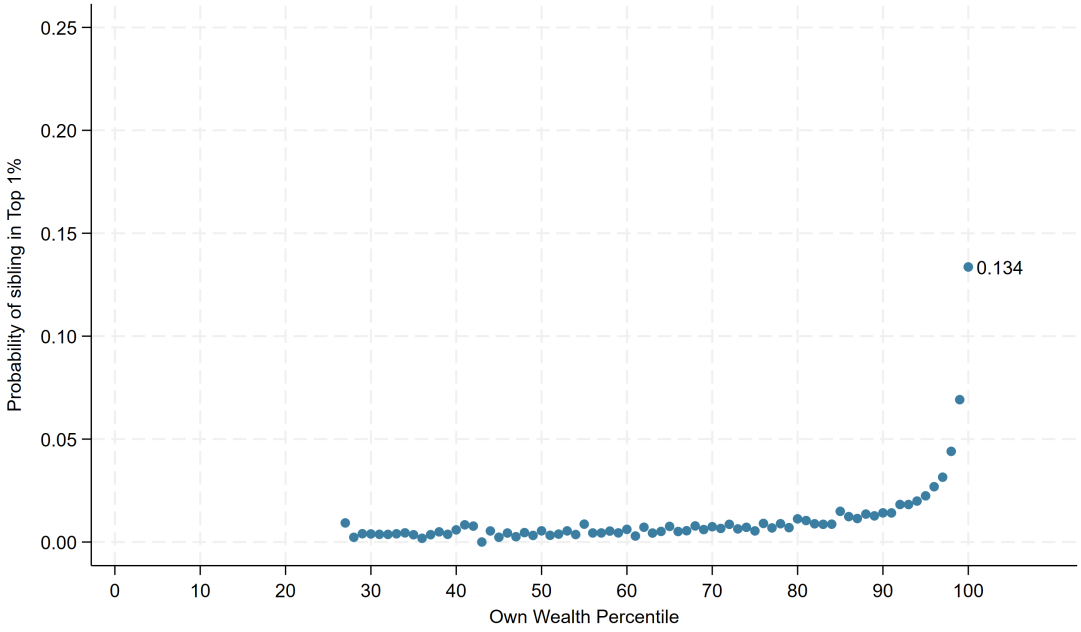
Notes: Rank-rank slopes estimated after demeaning wealth within latitude-longitude cells using coordinates from the Census Place Project (Berkes, Karger and Nencka, 2023). Geographic controls reduce but do not eliminate wealth correlations, indicating that elite sorting occurs across geographic boundaries as well as within local communities. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.1: Sibling Wealth Correlations

(a) Wealth Percentile Correlation



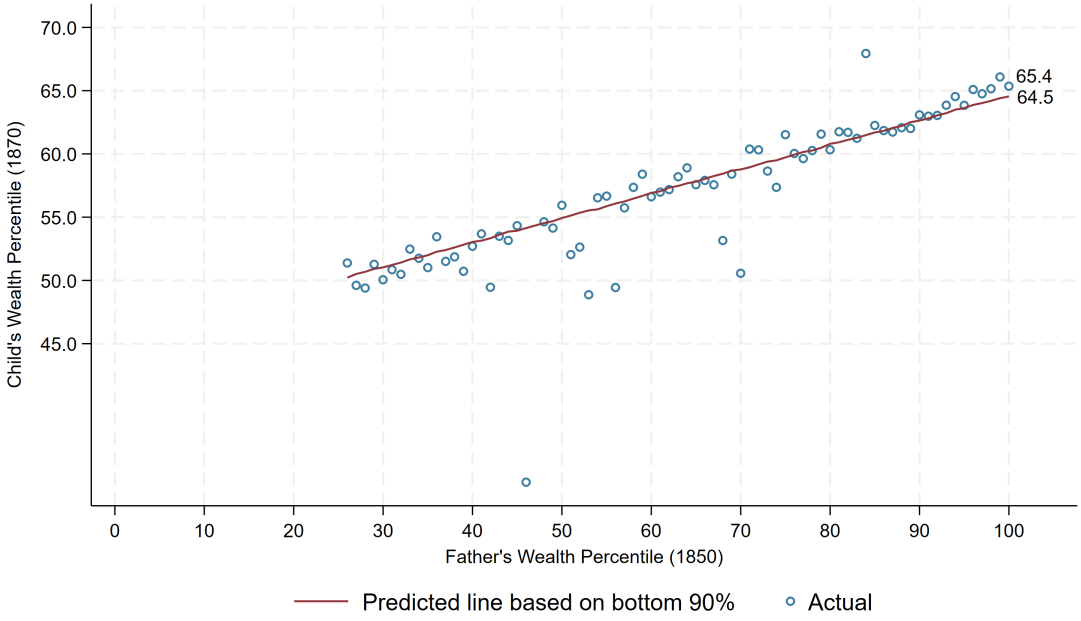
(b) Top 1 Percent Probability



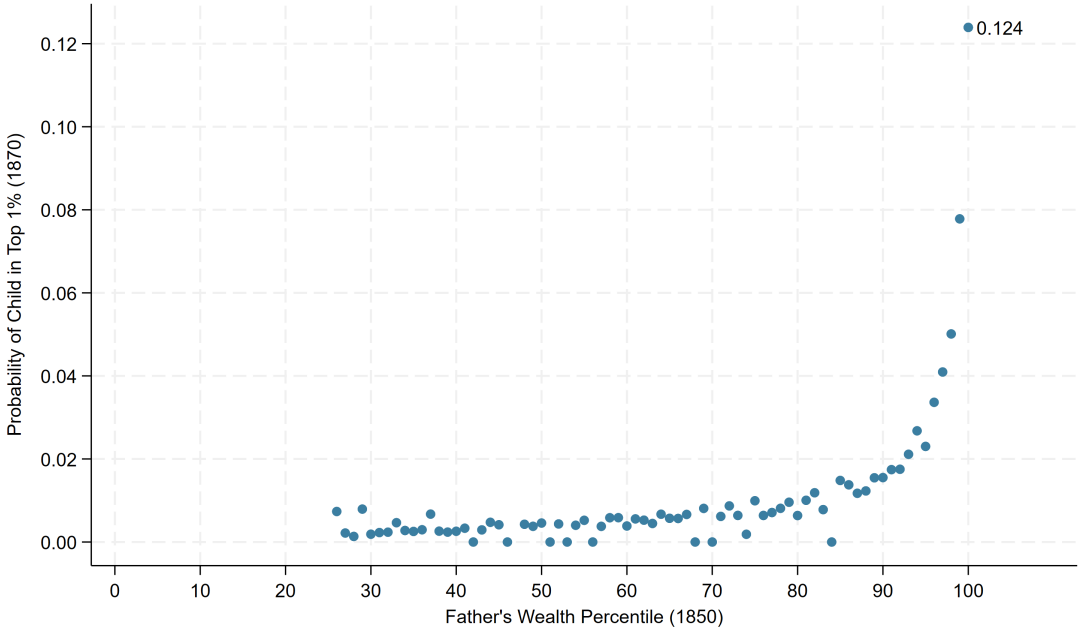
Notes: Panel A shows binned averages of sibling's wealth percentile for each own wealth percentile. The red line shows the predicted relationship from the bottom 90 percent. Panel B shows the probability that a sibling is in the top 1 percent of the wealth distribution. Sample restricted to siblings aged 25–40 observed in 1870. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.2: Parent-Child Wealth Transmission

(a) Wealth Percentile Correlation



(b) Top 1 Percent Probability



Notes: Panel A shows binned averages of child's wealth percentile (1870) for each father's wealth percentile (1850). The red line shows the predicted relationship from the bottom 90 percent. Panel B shows the probability that a child is in the top 1 percent of the wealth distribution. Sample restricted to children aged 28–35 in 1870 with fathers observed in 1850. N = 733,775. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.3: Heterogeneity in Parent-Child Transmission

(a) By Wealth Type



(b) By Region



Notes: Panel A shows the probability of reaching top 1 percent status in real property versus personal property. Panel B shows intergenerational transmission separately for the South and non-South. Sample restricted to children aged 28–35 in 1870 with fathers observed in 1850. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.4: Assortative Mating: South vs. Non-South (Binscatter)

(a) Pre-War Cohorts (Born 1836–1844)



(b) Post-War Cohorts (Born 1845–1859)

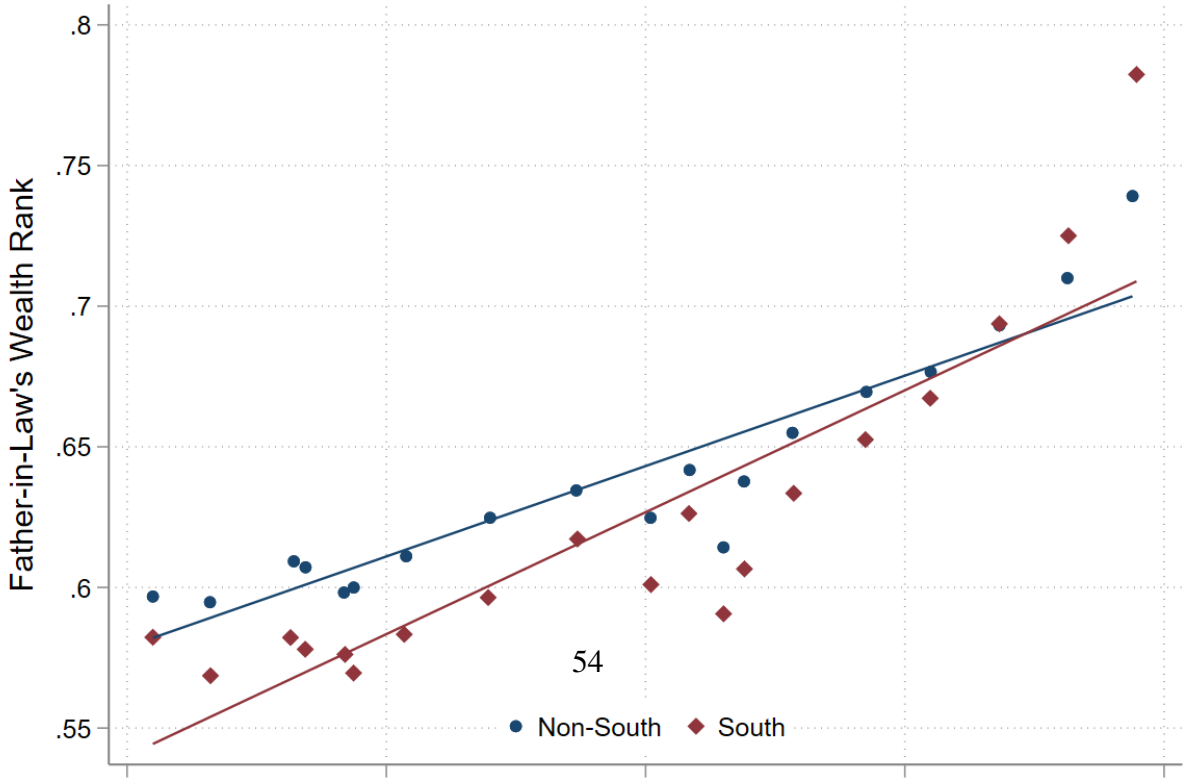
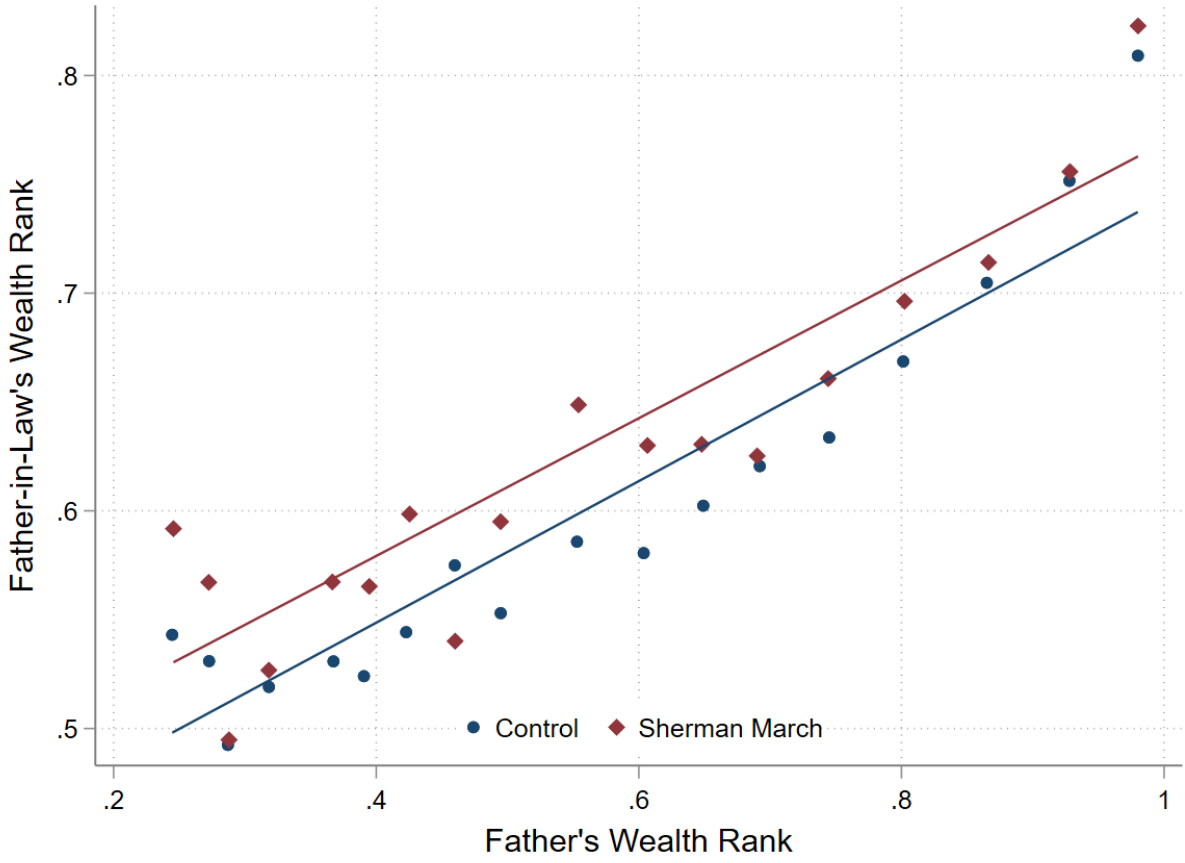


Figure B.5: Assortative Mating: Sherman's March Counties vs. Control (Binscatter)

(a) Pre-War Cohorts (Born 1836–1844)



(b) Post-War Cohorts (Born 1845–1859)

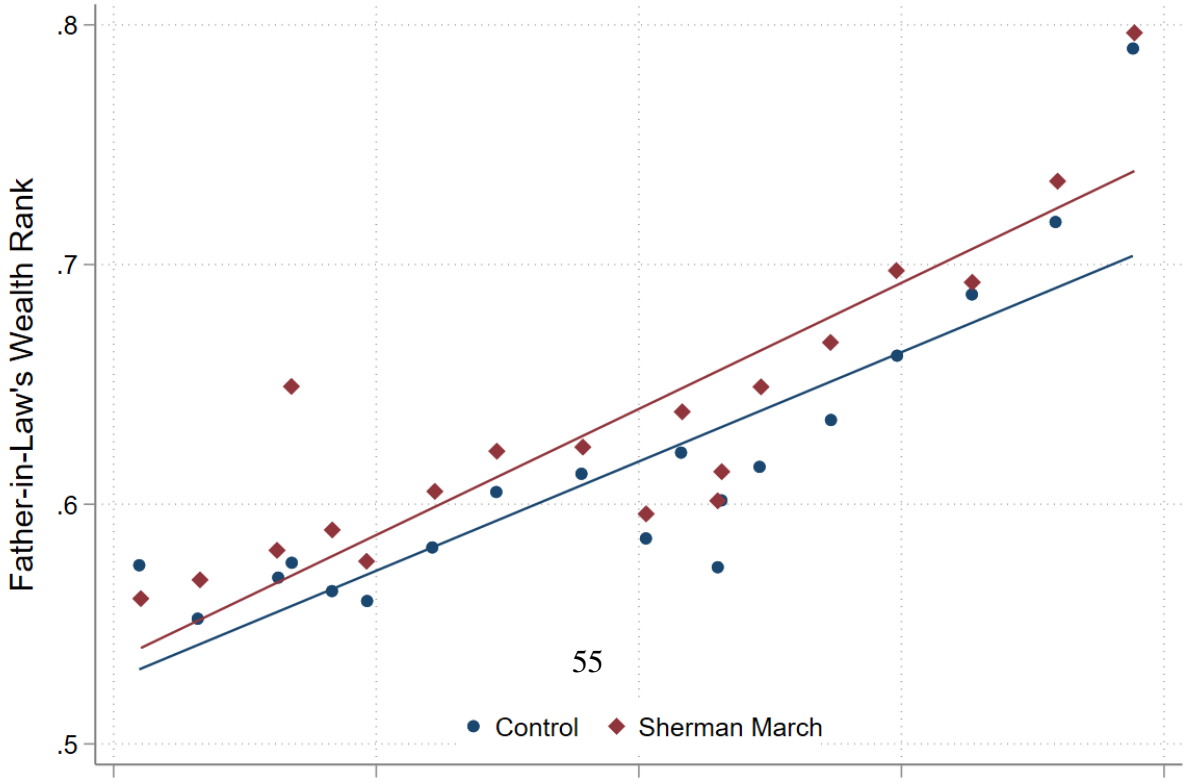
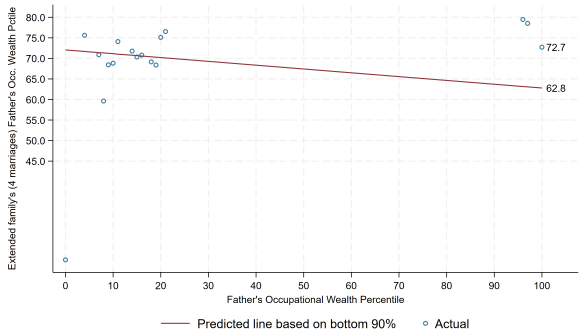
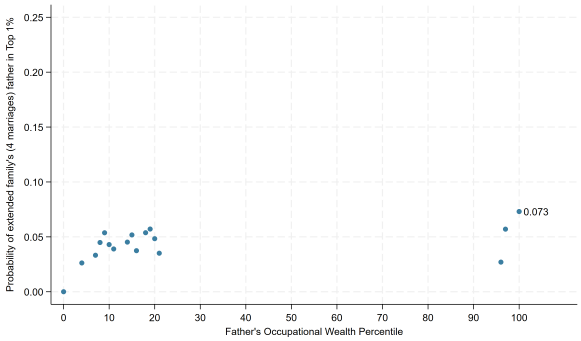


Figure B.6: Assortative Mating Using Occupational Wealth Scores: Four Marriages Away

(a) Wealth Percentile



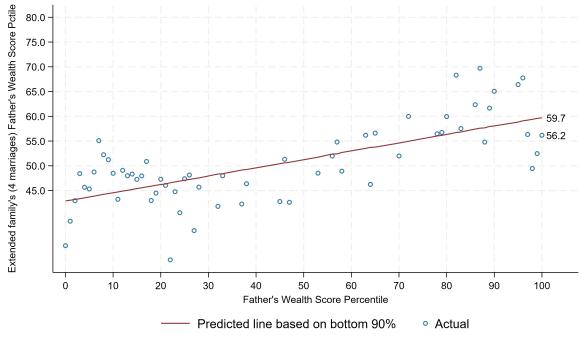
(b) Top 1 Percent



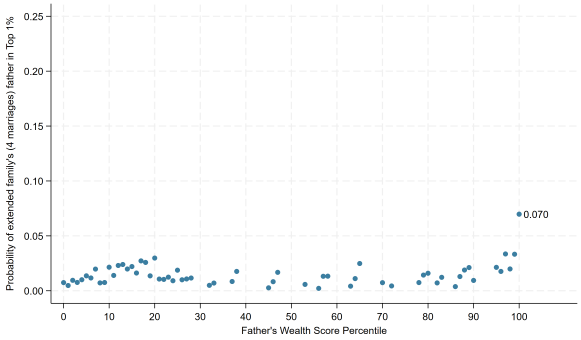
Notes: Panel A shows binned averages of the extended kin father's occupational wealth score percentile against own father's occupational wealth score percentile at four marriages away. Panel B shows the probability that the extended kin father is in the top 1 percent of the occupational wealth score distribution. Occupational wealth score is the mean wealth of individuals in each occupation calculated from 1860–1870 census data. The relationship is approximately linear throughout the distribution, with no sharp non-linearity at the top comparable to that observed with actual wealth. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.7: Assortative Mating Using Predicted Wealth Scores: Four Marriages Away

(a) Wealth Percentile



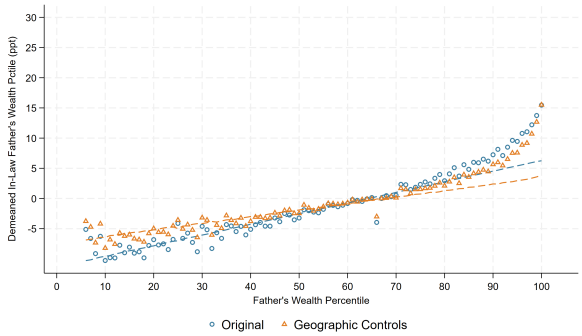
(b) Top 1 Percent



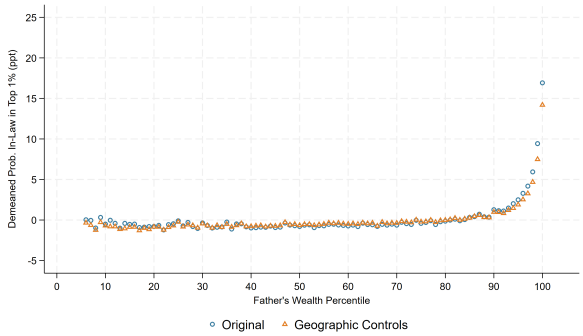
Notes: Panel A shows binned averages of the extended kin father's predicted wealth score percentile against own father's predicted wealth score percentile at four marriages away. Panel B shows the probability that the extended kin father is in the top 1 percent of the predicted wealth score distribution. Predicted wealth score is constructed following Saavedra and Twinam (2020), using demographic characteristics and occupation to predict wealth. As with occupational wealth scores, the predicted measure fails to capture the sharp non-linearity at the top observed with actual wealth. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.8: Geographic Controls: Original vs. Demeaned Relationships

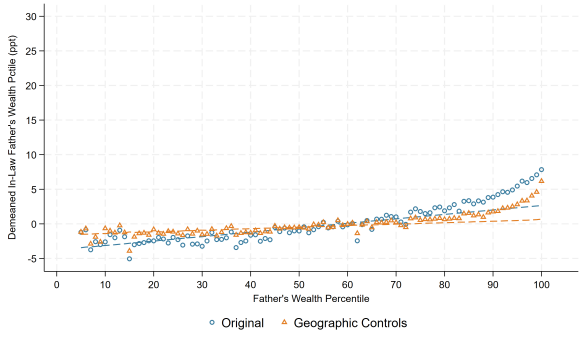
(a) One Marriage: Wealth Percentile



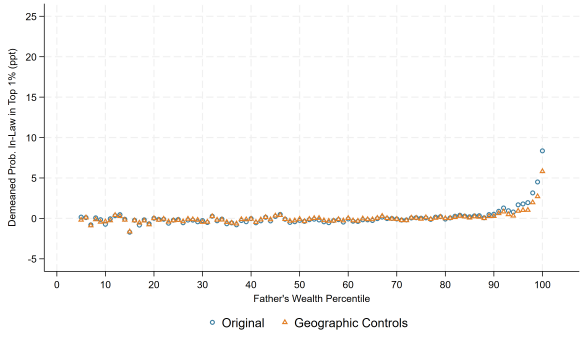
(b) One Marriage: Top 1 Percent



(c) Four Marriages: Wealth Percentile

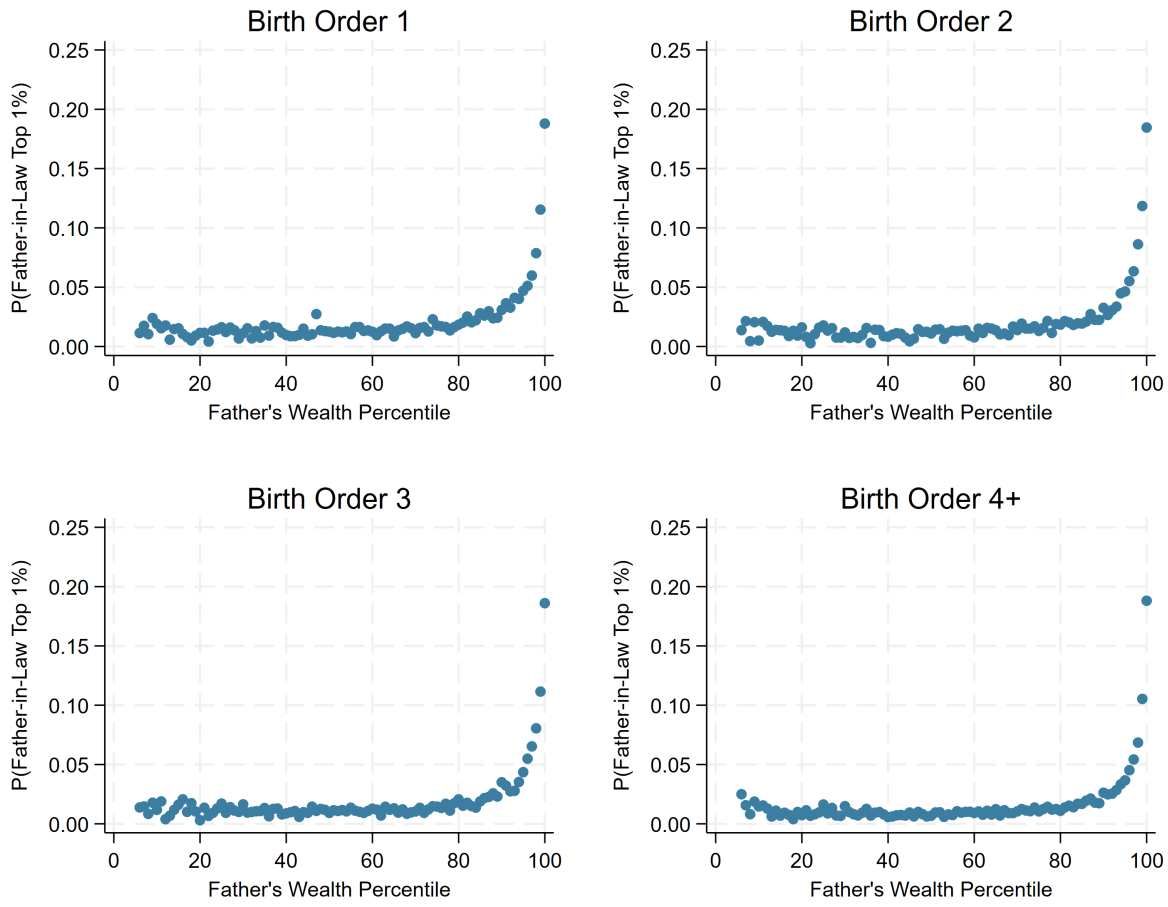


(d) Four Marriages: Top 1 Percent



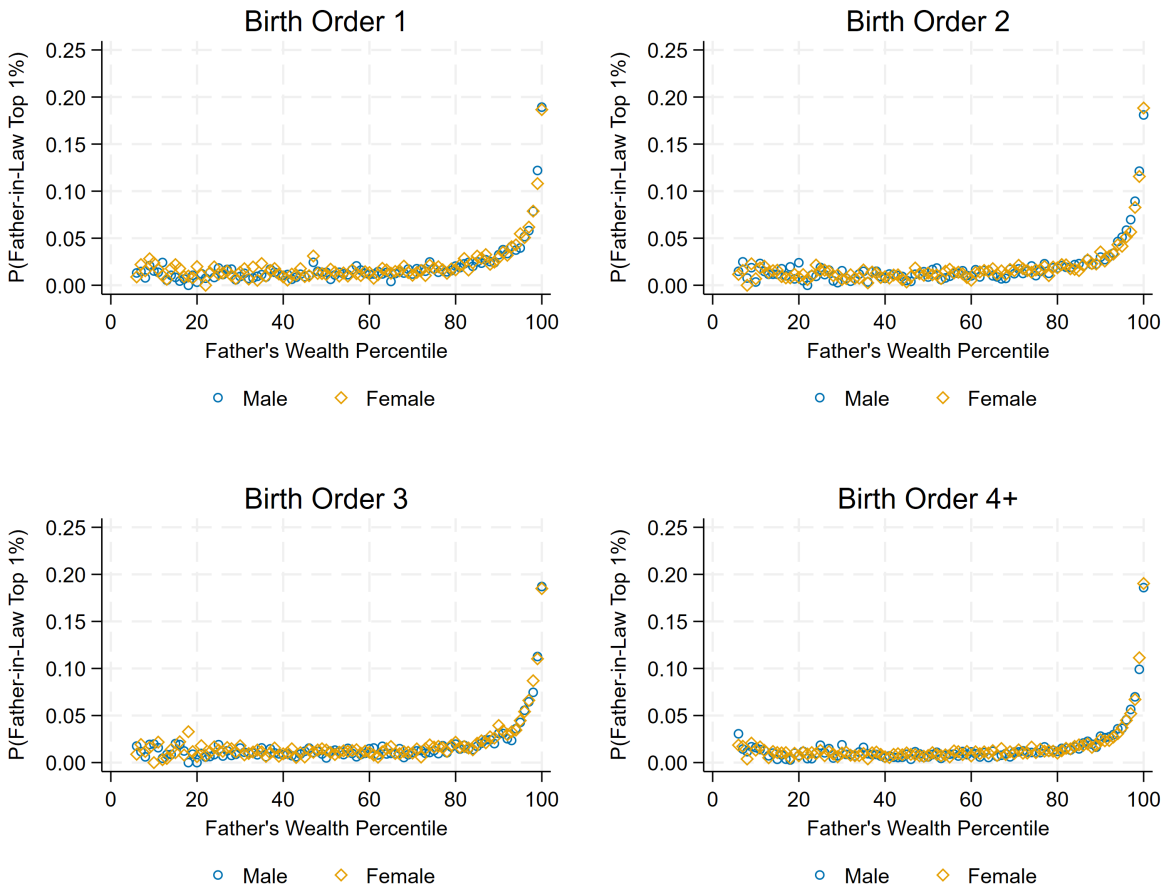
Notes: Blue circles show the original relationship (demeaned by national mean); orange triangles show the relationship after geographic controls (demeaned by local mean within latitude-longitude cells). Panels A and C show the relationship between father's wealth percentile and in-law father's wealth percentile. Panels B and D show the probability that the in-law father is in the top 1 percent. Elite marriage networks do not simply reflect geographic concentration of wealth in a few areas where individuals marry each other. The non-linear patterns at the top of the distribution persist after geographic controls, indicating that elite families found each other across geographic boundaries. Geographic coordinates from the Census Place Project (Berkes, Karger and Nencka, 2023). Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.9: Assortative Mating by Birth Order



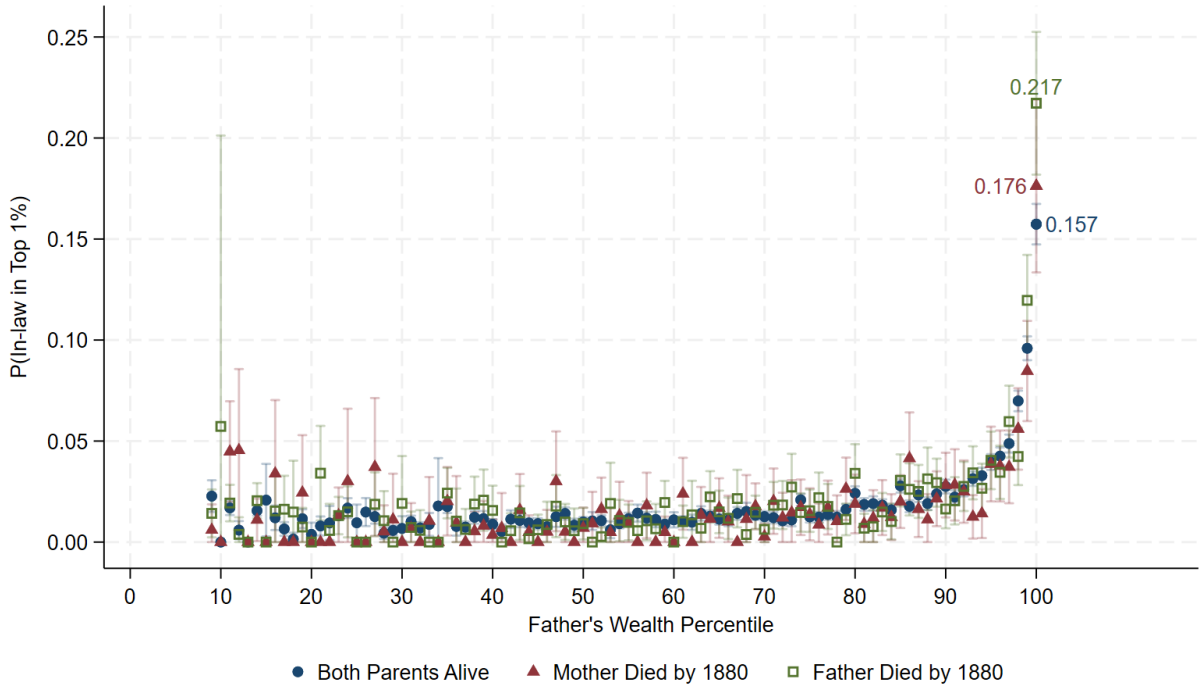
Notes: Each panel shows the probability that the father-in-law is in the top 1 percent of the wealth distribution, by father's wealth percentile, separately for children of each birth order (1, 2, 3, and 4+). Birth order is determined by the child's age rank within the father's household. The sharp nonlinearity at the top of the distribution is present for all birth orders. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.10: Assortative Mating by Birth Order and Gender



Notes: Each panel shows the probability that the father-in-law is in the top 1 percent of the wealth distribution, by father's wealth percentile, shown separately for males (blue circles) and females (orange diamonds) within each birth order group (1, 2, 3, and 4+). Neither birth order nor gender produces systematic differences in elite matching patterns. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Figure B.11: Assortative Mating by Early Parental Death



Notes: Probability that the father-in-law is in the top 1 percent of the wealth distribution, by father's wealth percentile, shown separately for children with both parents alive, mother died by 1880, and father died by 1880. Sample restricted to fathers observed in 1870 with children linked to the 1880 census. Parental death identified by the surviving spouse's widowed marital status in the 1880 census. Error bars show 95 percent confidence intervals. Weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Table B.7: Early Parental Death and Top 1 Percent Matching

	(1) 1 Marriage	(2) 2 Marriages	(3) 3 Marriages	(4) 4 Marriages
top1_2				
Mother Died	0.780	0.882	1.474	2.925
	(0.414)	(0.725)	(1.903)	(2.073)
Father Died	1.685	0.630	0.169*	0.674
	(0.614)	(0.318)	(0.175)	(0.389)
Observations	7784	5611	5438	5423
Log-likelihood	-1732.85	-1215.87	-1063.88	-971.91

Sample: Top 1% fathers only. Logit odds ratios. Robust SE in parentheses. IPW weighted.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Logit odds ratios for the probability that the connected relative's father is in the top 1 percent of wealth. Sample restricted to top 1 percent fathers observed in 1870. Mother Died and Father Died indicate whether the surviving spouse was widowed by the 1880 census. Robust standard errors in parentheses. All regressions weighted using inverse probability weights. Data from IPUMS USA full-count census files 1850–1910 (Ruggles et al., 2024) linked using Census Tree (Buckles et al., 2025).

Table B.8: Married Women's Property Act Adoption Dates and Classification

State	MWPA Year	State	MWPA Year
Panel A: Always Treated (Dropped from CS Analysis, 7 states) MWPA adopted before 1856; no pre-treatment cohorts available			
New York	1848	Maine	1855
Pennsylvania	1848	Massachusetts	1855
Wisconsin	1850	Michigan	1855
New Jersey	1852		
Panel B: Treated States (34 states) MWPA adopted 1856–1890; staggered treatment			
Kansas	1858	Delaware	1873
Maryland	1860	Georgia	1873
New Hampshire	1860	Iowa	1873
Colorado	1861	Nevada	1873
Illinois	1861	Missouri	1875
Ohio	1861	Connecticut	1877
North Carolina	1868	North Dakota	1877
South Carolina	1868	South Dakota	1877
West Virginia	1868	Virginia	1877
Minnesota	1869	Oregon	1878
Wyoming	1869	Indiana	1879
Arizona	1871	Mississippi	1880
Nebraska	1871	Vermont	1881
California	1872	Washington	1881
Rhode Island	1872	Oklahoma	1883
Utah	1872	New Mexico	1884
Arkansas	1873	Montana	1887
Panel C: Never Treated (Control Group, 6 states) MWPA adopted after 1890; not-yet-treated controls			
Kentucky	1894	Louisiana	1916
Idaho	1903	Tennessee	1919
Texas	1913	Florida	1943

Notes: Classification of states for the Callaway-Sant'Anna staggered difference-in-differences analysis of Married Women's Property Acts (MWPA). Treatment exposure is defined as $G = \text{MWPA Year} - 20$, representing the birth cohort first exposed at the approximate age of marriage. Panel A lists states where all birth cohorts in our sample (1836–1870) were already exposed to the MWPA; these are dropped from the CS estimation because no pre-treatment cohorts are available. Panel B lists treated states with staggered adoption timing that provides identifying variation. Panel C lists states that adopted MWPAs after 1895, so no birth cohort in our sample was exposed; these serve as not-yet-treated controls. Alabama is excluded entirely because it never adopted a MWPA.