ECONOMIC CONSEQUENCES OF KINSHIP: EVIDENCE FROM U.S. BANS ON COUSIN MARRIAGE*

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Close-kin marriage, by sustaining tightly knit family structures, may impede development. We find support for this hypothesis using U.S. state bans on cousin marriage. Our measure of cousin marriage comes from the excess frequency of same-surname marriages, a method borrowed from population genetics that we apply to millions of marriage records from the eighteenth to the twentieth century. Using census data, we first show that married cousins are more rural and have lower-paying occupations. We then turn to an event study analysis to understand how cousin marriage bans affected outcomes for treated birth cohorts. We find that these bans led individuals from families with high rates of cousin marriage to migrate off farms and into urban areas. They also gradually shift to higher-paying occupations. We observe increased dispersion, with individuals from these families living in a wider range of locations and adopting more diverse occupations. Our findings suggest that these changes were driven by the social and cultural effects of dispersed family ties rather than genetics. Notably, the bans also caused more people to live in institutional settings for the elderly, infirm, or destitute, suggesting weaker support from kin. *JEL Codes:* D00, N00.

I. Introduction

> "Despite their capacity to form capital, kinship societies remain poor. To explore the economics of kinship societies is thus to explore the economics of underdevelopment."
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> —Bates, Greif, and Singh (2004, 66)

The weakening of ties among extended family has, since Weber (1951), been associated with development. Recent work by Henrich (2020) and Schulz (2022) suggests that loosened kinship

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ties were key to the historical development of Europe.[1] Similarly Enke (2019) argues that weaker kinship ties became advantageous in the industrial era and are linked to urbanization and economic growth. Consistent with this, strong family ties have been linked to lower contemporary growth rates through their effect on geographic mobility and generalized trust (Alesina and Giuliano 2014). The causal relationship underlying this link is still unclear. Kinship ties may react flexibly to changes in incentives rather than being fundamental causes of economic outcomes (Bau 2021).[2]

This article uses an exogenous decline in marriage between first cousins to estimate the effect of weakening kinship ties on geographic and occupational mobility, rural-urban migration, and income. We do this using U.S. data from the mid-eighteenth to the mid-twentieth centuries, where state bans on first-cousin marriage allow us to identify the causal effect of cousin marriage.[3] While now rare in the United States, we estimate that about 7% of marriages were between first cousins in the late eighteenth century. Sociologists have argued that the decline of cousin marriage in the United States and the associated weakening of kinship ties was closely connected to urbanization and industrialization during this period (Goode 1963; Farber 1968). We confirm this hypothesis by combining marriage records with census data from 1850 to 1940. We find that these bans led to rural-to-urban migration and higher-paying occupations. Our results suggest these effects are not driven by the genetic consequences of inbreeding but by the social and cultural consequences of in-marriage. These findings rely on two key contributions: a method for calculating cousin marriage borrowed from human biology and an identification strategy that exploits variation from state bans on cousin marriage.

1. See also Bahrami-Rad et al. (2022); Schulz et al. (2019); Akbari, Bahrami-Rad, and Kimbrough (2019); Greif and Tabellini (2017); Fukuyama (2011); and Korotayev (2000). Notably, the decline of tribes in Europe and the rise of the nuclear family in the late medieval period long preceded the industrial revolution (Greif 2006).

2. Empirical analysis of this link is further complicated by the correlation of strong kinship ties with historical characteristics, such as disease burdens and type of agriculture, which may directly affect development. See Walker and Bailey (2014) and Denic and Nicholls (2007).

3. Fittingly, the prohibition of marriage between cousins is thought to have been central to the dissolution of clans in Europe and the loosening of kin ties (Goody 1983; Schulz et al. 2019).

Our measure of cousin marriage comes from the excess frequency of marriages where spouses share a surname. The rate of isonymous (same-surname) marriages has been widely used to estimate rates of cousin marriage in a population (Darwin 1875; Crow and Mange 1965). This article introduces the use of marital isonymy to economics and applies it to a far larger set of marriages than has been done in other fields, to our knowledge. Our measure adjusts for both false positives (unrelated spouses who share a surname) and false negatives (cousins who do not share a surname). We apply this method to a data set of 12 million U.S. marriage records from 1750 to 1940. These publicly available records, digitized and transcribed at scale for use by amateur genealogists, contain the (premarital) names of the spouses and the date and place of their marriage. We estimate that cousin marriage rates fell from around 7% in the late eighteenth century to 1.5% in the early twentieth century.

Before turning to our causal analysis, we compare same-surname couples to their otherwise similar peers. This provides a reasonable estimate of the difference between cousin and noncousin marriages so long as one of the following two conditions hold: same-surname marriages among noncousins are (i) relatively infrequent, and/or (ii) they are broadly representative of the overall population.[4] We link census couples to marriage records to identify those who shared a (premarital) surname.[5] We find that same-surname spouses are more likely to live on a farm, less likely to be urban, and less likely to live outside their state of birth. We also find that husbands have lower-paying occupations. Notably, occupational income is lower even within census enumeration districts, which are typically just a few hundred households. Do individuals with these characteristics simply select into cousin marriage, or is some part of the relationship causal?

Our event study analysis of state bans on cousin marriage explores whether cousin marriage has a causal effect on geographic and occupational mobility. Using all available rounds of census

4. Another more subtle requirement is that same-surname cousin marriages are representative of all cousin marriages.

5. To study how married cousins differ from otherwise similar couples, we include year of birth, state of birth, and county of residence fixed effects. Since noncousin same-surname marriage are more likely for common surnames ("Smiths"), we also control for surname frequency. Results are insensitive to adding surname fixed effects.

data from 1850 to 1940, we track the outcomes of birth cohorts six decades prior to and following these bans. To study the effect of these bans on those most exposed to them, we use surnames to identify families with high rates of cousin marriage. One twelfth of our sample has a surname with a rate of cousin marriage above 10% in the period preceding the first state ban. Our results compare the outcomes of men with one of these “high cousin marriage” surnames to other men born in the same state and decade.

We find that bans on cousin marriage led men with high-cousin-marriage surnames to move off farms, into more populous (urban) locations, and across states. They also shifted to higher-paying occupations. These effects are substantial in magnitude. Men with high-cousin-marriage surnames born five decades after a ban (roughly the grandchildren of those born in the same decade as the ban) are 8 percentage points less likely to live on a farm and 3 percentage points more likely to live in an urban location. Occupational income goes up by about 4%. Why would bans on cousin marriage lead to these changes?

A decline in cousin marriage could have affected residential and occupational outcomes through genetic or social-cultural channels. Our results suggest that improvements in genetic health outcomes from reduced in-marriage are unlikely to have been large enough to account for these effects. We combine census questions on physical disability with an indicator for living in a medical or mental institution and find no evidence of a reduction in these proxies for genetic disability.

Instead, we argue that these effects were consequences of new marriage patterns that led to weaker attachments to locations of origin. This could be some combination of a direct decline in the strength of kinship ties, the need to migrate to find a spouse, or changes in inheritance patterns. Results are consistent with weaker kinship ties being an important channel. Our conceptual framework describes how a decline in cousin marriage would have weakened kinship ties, and how this in turn could have resulted in geographic and occupational mobility. In an era of increasing opportunities off farms and especially in urban areas, this may have had substantial economic returns.

We find that treatment led to dispersion across space before it led to rural-urban migration. State-decade birth cohorts from a given surname became more dispersed across enumeration districts (and counties), and this effect precedes the move to urban locations by one to two (birth) decades. That is, cousin marriage bans first led to horizontal dispersion, then to migration toward more densely populated locations. Similarly, we see increased dispersion across occupations before we see a move to higher-paying occupations. We interpret this as the effect of looser kinship ties, allowing for more experimentation and individual preference. These, with a lag, could have led to the rural-urban migration and movement up the income ladder that we observe.

Consistent with a shift toward nuclear (or conjugal) households described in our conceptual framework, we also find evidence for changes in household composition. Household size goes down, in part due to having fewer children and fewer cohabiting relatives. We also find evidence of a dark side to this transition: an increase in the likelihood of living in institutional settings for the elderly, infirm, or destitute. Without strong kin ties, vulnerable people may have been more likely to be left to such institutions rather than being cared for by relatives.

In contrast, treatment effects do not seem to be driven by migration to marry. If this were the case, we would expect the bans to affect the migration outcomes of cohorts born shortly before and after the ban, who may now need to migrate to find a non-cousin spouse. Instead we only find substantial treatment effects on rural-urban and interstate migration for cohorts born a full generation after the passing of the bans. Furthermore, because the census captures each birth cohort every 10 years, we can see how effects change across the life span. We find that much of the treatment effects on migration appear after the age of 30, past the mean age of marriage.

Similarly, changing inheritance patterns do not seem to explain our results. Although cousins may marry as a way of keeping wealth within a family, we find that treatment effects do not greatly differ for families with very little real estate wealth. This makes it unlikely that effects are driven by an increased dispersal of land across generations.

Our causal interpretation of the results rests on a key identifying assumption: without cousin marriage bans, the relative outcomes of men with high- and low-cousin-marriage surnames would not have diverged after a ban on cousin marriage. A general concern is that high-cousin-marriage surnames, perhaps because they are somewhat more rural, may gradually catch up with their peers around the time bans were passed. Reassuringly, however, we find pretrends that are remarkably flat in the few decades before the passing of these bans.

We show that our results are robust to controlling for a range of potential time-varying confounders, including compulsory schooling, minimum age of marriage, railroad coverage, statehood, and frontier experience. We also show that our results are not driven by selective migration from states with a ban to states without one. Finally, we explore the robustness of results to a range of alternative measurements and empirical specifications. We show that various changes to our measure of cousin marriage or to how we categorize surnames as having high cousin marriage rates do not affect our results.

Our findings add to the literature on the effect of kinship on economic and political outcomes. Our causal micro evidence supports the finding of this literature that tight kinship hinders political and economic modernization. This work typically uses premodern measures of kinship tightness from Murdock (1949)'s Ethnographic Atlas and links them to contemporary outcomes (Akbari, Bahrami-Rad, and Kimbrough 2019; Schulz et al. 2019; Lowes 2020; Moscona, Nunn, and Robinson 2020; Bau 2021; Bahrami-Rad et al. 2022). Notably, Enke (2019) finds that after the onset of the Industrial Revolution, societies with higher kinship tightness exhibit slower economic development. Complementary work uses survey measures of the strength of family ties and links these to rich individual-level data on household composition, political participation, and economic outcomes (Alesina and Giuliano 2010, 2014; Ermisch and Gambetta 2010). Our nineteenth-and early twentieth-century U.S. setting offers a window into a society undergoing a substantial shift in marriage practices while providing individual-level, population-scale data.

The practice of cousin marriage in particular has been a focus of this literature. This has partly been driven by the influential idea that restrictions on cousin marriage loosened kinship bonds in Europe and led to European economic development (Goody 1983; Schulz et al. 2019; Henrich 2020; Bahrami-Rad et al. 2022). Schulz (2022) and Akbari, Bahrami-Rad, and Kimbrough (2019) show that cousin marriage leads to worse institutional outcomes and higher corruption. Research in contemporary societies has focused instead on the functional benefits that motivate cousins to marry (Do, Iyer, and Joshi 2013; Mobarak, Kuhn, and Peters 2013; Edlund 2018; Hotte and Marazyan 2020).[6] The reasons

6. These may explain the continued widespread practice of cousin marriage in many contemporary societies: Bittles (2001) estimates that about 10% of marriages

they emphasize (dowry payments, inheritance, and providing kin-based insurance) may have been relatively unimportant in the nineteenth-century United States, leading the practice to eventually die out. Another rationale for its disappearance in the United States was growing concern over its genetic consequences. However, recent surveys have concluded that the health consequences of cousin marriages are modest and do not justify legal restrictions (Bennett et al. 2002; Bittles 2012). Mobarak et al. (2019) offer the best causal micro evidence available on this, using unmarried opposite-sex cousins as an instrument for cousin marriage. Their findings suggest that observational estimates of the negative consequences of cousin marriage on child health are exaggerated and that the true effects are small.

Our use of surnames to measure kinship and marital ties builds on work such as Cruz, Labonne, and Querubín (2017), Fafchamps and Labonne (2017), and Angelucci et al. (2010). The term "isonymy" is used for a separate but complementary measurement aimed at identifying how much mixing there is within and between populations. While we use marital isonymy (the rate of same-surname marriages), Artiles (2022) uses the distribution of surnames across locations to estimate social closure. Buonanno and Vanin (2017), in work conceptually related to our own, find that low surname diversity in Italian localities (evidence of in-marriage or limited migration) predicts higher tax evasion but lower crime rates.

The rest of the article proceeds as follows. Section II includes our conceptual framework and the historical context; Section III presents our data and the method we use to estimate rates of cousin marriage. Section IV presents correlates of cousin marriage using OLS, and Section V presents our causal analysis using variation from state bans. Section VI concludes.

II. Background

II.A. Conceptual Framework

A decline in the importance of kinship has long been associated with modernity, industrialization, and economic growth (Weber 1951; Goode 1963). Henrich (2020) has further argued

worldwide are between first or second cousins. An alternative interpretation is the high degree of persistence in the custom of cousin marriage, as seen in Giuliano and Nunn (2021).

that in Europe, the breakdown of kin ties not only preceded but caused greater urbanization and the scientific and Industrial Revolutions. This may be because while intensive kinship networks enable the cooperation needed in agrarian societies, they come at the cost of reducing mobility. Central to Henrich (2020)'s argument is that this decline in the strength of kin ties was a result of a long-standing prohibition on cousin marriage by the Catholic Church. In this section, we link theories from anthropology, sociology, and psychology to explain how a similar ban on cousin marriage in the United States may have affected economic outcomes. We first discuss reasons a decline in cousin marriage would lead to a broader weakening of kinship norms and practices and lead to higher mobility, and then discuss how weaker kinship ties and the rise of the isolated nuclear family are linked to our geographic and occupational outcomes of interest.

Marriage between close kin is a central characteristic of what anthropologists refer to as intensive kinship. In addition to practicing cousin marriage, these societies are marked by the presence of extended rather than nuclear households, low migration rates and marriage distance (spouses are from the same or nearby communities), unilineal descent, and matri- or patrilocality (spouses residing with or near the bride's or the groom's parents) (Walker and Bailey 2014; Shenk et al. 2016; Henrich 2020). Intensive kinship norms are particularly strong in societies with competition over resources that can be monopolized, such as farm land (Walker and Bailey 2014; Johow, Willführ, and Voland 2019). Cousin marriage intensifies the bonds among kin, which enhances cooperation and mutual defense. It also limits competing claims on inheritance between kin, and facilitates consolidated kin-group ownership of wealth. What effect would an exogenous reduction in cousin marriage have on societies with some of these characteristics?

Henrich (2020) argues that the prohibition on cousin marriage by the Catholic Church in Europe led to the decline of intensive kinship practices, the dissolution of tribes and clans, and the transition to nuclear families and greater geographic mobility.[7] In related work, Schulz et al. (2019) suggest this happened because

7. The Church also imposed other restrictions on marriage and inheritance that contributed to these changes. But Henrich (2020) and Schulz et al. (2019) argue that these other policies were similar in Eastern churches, which did not see declines in intensive kinship practices to the same extent. The clearest

cousin marriages create tighter kin groups by eliminating nonkin affinal ties (in-laws), greater similarity in socialization, as two of the four parents of first cousins were raised in the same household, and redirection of the evolved tendency for kin altruism toward fewer but more related kin.

Although it is plausible that a decline in cousin marriage would weaken kin bonds, existing work on this topic generally focuses on societies removed from our setting. While not focused on cousin marriage, sociologists Parsons (1943) and Goode (1963) have highlighted the fit between the isolated nuclear family, with weak ties to kin, and the demands of an urban, industrial economy. Goode argued that "an industrial society is necessarily open-class, requiring both geographic and social mobility," and that the isolated nuclear family "frees the individual from ties to the specific geographical location where his parental family lives" (Goode 1963, 11–12). The isolated nuclear family is contrasted with the extended family, characterized by "geographical propinquity, occupational dependence and nepotism, a sense that extended family relations are most important" (Litwak 1960, 9).

We propose two related ways to think about how tight kinship bonds, reinforced through cousin marriage, might directly affect economic decisions in our context. The first is through a preference for contact with kin. Individuals from tightly knit families with strong ties to their relatives may develop a preference for living and working alongside kin, even at the cost of lower wages. Given the difficulty of coordinating a move with one's extended family, individuals raised in farm or rural families may choose to forgo potential jobs in nearby towns and cities to maintain close contact with their kin.

A second way to think about how kinship bonds might affect mobility is through relational mobility, a form of human capital. This refers to the ability to create new relationships and break existing ones that are not beneficial (Yuki and Schug 2012). Henrich (2020) emphasizes that cousin marriage leads to lower relational mobility. With the church-imposed decline of cousin marriage, individuals in medieval Europe became "increasingly free to move, both relationally and residentially. Released from family obligations and inherited interdependence, individuals began to choose their own associates—their friends, spouses, business partners,

differences in terms of marriage and family policies between these churches is in the restriction on cousin marriage.

and even patrons—and construct their own relational networks. This relational freedom spurred residential mobility, as individuals and nuclear families relocated to new lands and growing urban communities" (Henrich 2020, 191).

Relational mobility requires specific skills (Oishi et al. 2015). Those raised in high relational-mobility settings learn from a young age to be on the lookout for and open to new beneficial relationships. They must be able to discern the value and trustworthiness of a potential relation and invest effort in appearing desirable to others. In contrast, low relational mobility requires a focus on maintaining relationships with in-group members, because these are not easily replaced. Avoiding ostracism emphasizes conformity and obedience.

Relational and geographic mobility are closely linked (Oishi 2010). Low relational mobility might impede migration due to the additional cost of creating a new social network. Moving from a rural to an urban area may present an additional challenge for people with low relational mobility. Urban areas demand more frequent interactions with strangers, which may be a more challenging environment for individuals with low relational mobility (Wirth 1938; Fischer 1975).

Occupational mobility may likewise be hindered by low relational mobility or a preference for interaction with kin. Difficulty building relationships with new coworkers might encourage work alongside kin, limiting the number of attractive workplaces and occupations. Upward occupational mobility may be particularly costly for those with strong ties to kin. Goode (1963) emphasizes that substantial movement up in social class (for example, taking on a white-collar occupation) may require a break with one's kin. Obligations to provide support to kin could also discourage movement up the social ladder (Squires 2018).

To summarize, strong bonds with one's kin are thought to crowd out and discourage relationships with nonrelatives. This could reduce both geographic and occupational mobility. As the United States urbanized and industrialized in the nineteenth and early twentieth centuries, the returns to such mobility were surely increasing. Cousin marriages, by strengthening family relationships, may have entailed substantial economic cost. Before attempting to estimate these costs, we turn to a discussion of the historical context of cousin marriage in the nineteenth-century United States.

II.B. Historical Context

Americans' aversion to marriage between cousins is a relatively recent phenomenon. Today, the United States has remarkably low rates of cousin marriage: researchers estimate that well below 1% of U.S. marriages are between first or second cousins (Bittles and Black 2015). Such a low rate is unusual globally, as about 10% of marriages worldwide are between cousins (Bittles and Black 2010). It is also a relatively recent phenomenon for the United States. There is wide agreement that historical U.S. rates of cousin marriage were once substantially higher and started declining around the middle of the nineteenth century (Ottenheimer 1996; Bittles 2012; Kaplanis et al. 2018). This section discusses what we know about historical rates of cousin marriage in the United States, what led to their general decline, and why some states chose to ban the practice.

No national estimate of cousin marriage exists for the nineteenth-century United States, though some evidence exists in studies of small subpopulations. Reid (1988) traced lineages of a set of Scotch-Irish families, finding rates of first-cousin marriage of 20% in the first half of the nineteenth century and 3% in the second half. Arner (1908) used the frequency of same-surname marriages to estimate a rate of 3% in eighteenth-century New York and 1% in Ashtabula County, Ohio, in the nineteenth century.[8] Estimates for the twentieth century are also sparse. Dispensations from Catholic marriage records suggest a rate of 0.2% in 1960 among Americans married in a Catholic church (Freire–Maia 1968).[9] Despite this low overall rate of cousin marriage, some pockets of consanguinity survived at least partway through the twentieth century. A 1942 survey of a Kentucky community found that 6.5% of couples were first cousins (Brown 1951). High rates of cousin marriage have also been documented among specific contemporary groups, such as Roma and Mennonites (Moore 1987; Thomas et al. 1987).

This decline in cousin marriage was likely caused by a fall in supply (fewer cousins due to lower fertility) and demand (a growing aversion to close-cousin marriage). Marriage between close kin is now viewed, as in most of the Western world, as taboo

8. Arner attributed this low rate in part to the "comparative newness of the Ohio community, in which few families would be interrelated" (Arner 1908, 25).

9. In contrast, Pinto-Cisternas, Zei, and Moroni (1979), using similar data, found a rate of 4% in Spain from 1940 to 1943.

(Bittles 2012). What led to this change in attitudes? In the most sustained treatment of the topic, Ottenheimer (1996) argues that U.S. attitudes turned so decisively against cousin marriage in the nineteenth century largely because of a growing belief in its negative health consequences. Much of this, he argues, was due to sensationalist news articles and studies such as the Bemiss Report (Bemiss 1858), which exaggerated the health risks of cousin marriage. The United Kingdom and much of Europe, however, saw attitudes toward cousin marriage change around the same period. The more pronounced shift in the United States, according to Ottenheimer (1996), was due in part to the influence of theories of civilizational progress that saw family structures of Native American tribes as evidence that cousin marriage was a form of backwardness (Morgan 1877; Ottenheimer 1990). McKinnon (2019) further argues that the association of cousin marriage with European royalty made it a target of the movement to deepen the egalitarian and republican American ethos (see also Paul and Spencer 2016).

In the context of these increasingly negative attitudes, 32 U.S. states, beginning with Kansas in 1858, began to enact legislation forbidding first-cousin marriage.[10] Table I lists each U.S. state and the year in which it enacted a ban on first-cousin marriage, if ever.[11] There is wide variation in the timing of bans. Kansas was joined by eight states in the 1860s; two in the 1870s, 1880s, and 1890s; six in the 1900s; five in the 1910s; and six thereafter.

What explains this state-level variation, which we use for causal identification? One theory, by Farber (1968), suggests that the greater individualism and heterogeneous ethnic origins of settlers in the Midwest and West led them to more forcefully oppose first-cousin marriages as a means of increasing assimilation. In

10. Perhaps surprisingly, such bans are rare globally. The only other countries that prohibit first-cousin marriage are China, Taiwan, Vietnam, North and South Korea, and the Philippines (Bittles 2012).

11. Although our analysis does not differentiate between types of bans, there are some differences in their details across states. For example, Indiana allows first cousins to marry if they are both above age 65. Illinois allows them to marry if they are both above age 50 or either is sterile. See Paul and Spencer (2016) for more details on these bans and for references to the specific legal statutes by which they were enacted. See also Bratt (1984) for a discussion of these bans from a legal perspective. We do not know of systematic data on enforcement of these bans. However, Online Appendix Figure A.9 presents historical news articles that illustrate at least some enforcement.

TABLE I
YEAR OF ENACTMENT OF STATE LAWS BANNING FIRST-COUSIN MARRIAGE

State	Year	State	Year
Alabama	Never ban	Nebraska	1911
Arizona	1901	Nevada	1861
Arkansas	1875	New Hampshire	1869
California	Never ban	New Jersey	Never ban
Colorado	1864	New Mexico	Never ban
Connecticut	Never ban	New York	Never ban
Delaware	1921	North Carolina	Never ban
Florida	Never ban	North Dakota	1862
Georgia	Never ban	Ohio	1869
Idaho	1921	Oklahoma	1890
Illinois	1887	Oregon	1893
Indiana	1877	Pennsylvania	1902
Iowa	1909	Rhode Island	Never ban
Kansas	1858	South Carolina	Never ban
Kentucky	1946	South Dakota	1862
Louisiana	1900	Tennessee	Never ban
Maine	1985	Texas	2005
Maryland	Never ban	Utah	1907
Massachusetts	Never ban	Vermont	Never ban
Michigan	1903	Virginia	Never ban
Minnesota	1911	Washington	1866
Mississippi	1923	West Virginia	1917
Missouri	1889	Wisconsin	1914
Montana	1919	Wyoming	1869

Source. Paul and Spencer (2016). Alaska and Hawaii are omitted since they achieved statehood after 1940. Neither has banned first-cousin marriage, nor has Washington, DC.

contrast Ottenheimer (1996) argues that a parsimonious theory fits the data better: widespread national change in attitudes toward first-cousin marriage only took legal shape when new marriage laws were drafted as territories achieved statehood. Older states, therefore, were less likely to amend their long-standing marriage statutes. Finally, Yamin (2009) argues that activists and lawmakers pushed in some places to extend the reach of the state with an aim to reshape families. This movement, which reached its peak in the Progressive Era, likewise led states to introduce compulsory schooling, child labor laws, and compulsory sterilization.

We test these theories in Online Appendix B.7. We find that some state-level characteristics do predict whether a state bans cousin marriage, but none have much predictive power for the timing of these bans. Consistent with this result, some historians have emphasized the haphazard nature of the legislation against cousin marriage. Discussing these bans, Paul and Spencer (2008, 2628) highlight "the ease with which a handful of highly motivated activists—or even one individual—can be effective in the decentralized American system, especially when feelings do not run high on the other side of an issue. The recent Texas experience, where a state representative quietly tacked an amendment barring first-cousin marriage onto a child protection bill, is a case in point."

III. Data

This section begins with a description of our data set of marriage records. We then discuss how we use the rate of same-surname marriages to calculate rates of cousin marriage across surnames and over time. Finally we briefly discuss the census data we use for measuring outcomes. Online Appendix B.3 describes genealogical data that, while not used in our main analysis, validates our use of isonymy (same-surname marriages) to infer cousin marriage rates, as discussed below.

III.A. Marriage Records

The marriage records in our data set come from original documents that have been scanned, transcribed, and made publicly available by Family Search (familysearch.org). We retrieved this data for all U.S. states between 1750 and 1940. The transcribed marriage records typically include names of both spouses and the date and location of marriage. Online Appendix B.1 includes a scanned image of a sample marriage record, details about what other information these records contain, and our data-cleaning procedure.

How good is the coverage of our marriage records? The left panel of Online Appendix Figure A.10 shows the number of marriages in our data on an annual basis for the period we use in our analysis (up to 1858). The right panel shows this rate relative to the overall U.S. population. Assuming an annual rate of 10 new marriages per 1,000 people, our records include about 35% to 60% of marriages in this period.[12] This suggests that while our

12. Stevenson and Wolfers (2007) find that an annual rate of 10 new marriages per 1,000 people is a reliable benchmark for the United States.

data set is not comprehensive, it does include a substantial share of marriages in a given year.

Online Appendix Table A.1 provides summary statistics of our marriage records. The first column includes all marriage records, and the second and third columns include only records either before or after 1858, the year of the first state ban on cousin marriage. While the rate of marriages where the spouses share a surname is low, it is noticeably lower in the latter period of our sample. Furthermore, while we provide more nuance below, a rough benchmark is that the rate of cousin marriage is roughly four times the rate of isonymy, suggesting cousin marriage rates declined from approximately 5% pre-1858 to 2.5% from 1859 to 1940.

III.B. Measuring Cousin Marriage Rates

In the absence of direct measures of cousin marriage during our period of interest, we use a method taken from population genetics to estimate these rates from our data set of marriages.[13] The basic insight behind the method is straightforward. First-degree cousins, who share grandparents, often share a surname. A population where cousins frequently marry will therefore tend to have a higher share of same-surname (isonymous) marriages than one where they do not. This section describes the formal application of marital isonymy to our data set of marriages, including corrections to account for false positives and false negatives.[14]

The use of surnames at marriage to estimate rates of cousin marriage was first proposed by Darwin (1875). Crow and Mange (1965) formalized this approach and showed that the rate of inbreeding in a human population can in some cases be derived from marriage records. That seminal paper spurred a large literature applying their technique to various populations. (Lasker 1985 and Colantonio et al. 2003 review this literature. For examples of marital isonymy applied to U.S. populations, see Swedlund and Boyce 1983; Jorde 1989; Relethford 2017.) The link between isonymy and

13. The only relevant data set we know of with direct measures of consanguinity is Familinx (Kaplanis et al. 2018), which is derived from online genealogies. However, as we describe in Online Appendix B.3, it is anonymized and hence cannot be used for our main analysis. It is useful, however, in allowing us to perform a number of validation exercises.

14. False positives are isonymous marriages between unrelated individuals, and false negatives are nonisonymous marriages between first cousins.

inbreeding has more recently been bolstered by studies that combine surnames with DNA results (Sykes and Irven 2000; Gymrek et al. 2013; Calafell and Larmuseau 2017).

Some isonymous marriages are between unrelated people who happen to share a surname. Not all Smiths are cousins. To deal with this, we make use of Crow and Mange (1965)'s decomposition of total isonymy into its random and nonrandom components. Total or observed isonymy P is simply the fraction of marriages where spouses share a surname. (Throughout this article, we refer to the premarital, or maiden, surnames of marriage partners. Once married, almost all couples in our setting share a surname.) Random isonymy P^r is defined as the share of marriages we would expect to be isonymous in a population if individuals chose their partners at random. This rate is derived solely from the distribution of surnames in a pool of marriage partners. We treat each state-decade as a separate marriage pool. For a given surname f (for family) in a state s in decade d, the rate of random isonymy is simply the share of people in that marriage pool with that surname: $P^r_{fsd} = \frac{N_{fsd}}{N_{sd}}$, where N is a number of individuals.[15]

Total or observed isonymy can be decomposed into its random and nonrandom components such that nonrandom isonymy P^n is the excess share of isonymous marriages—deviation from the rate we would expect if individuals were marrying at random. We use nonrandom isonymy to calculate cousin marriage rates since, in expectation, it nets out marriages between unrelated partners who happen to share a surname.[16] Nonrandom isonymy, then, adjusts for common surnames having more same-surname

15. This assumes that the male and female shares of a given surname in a population are equal to each other. In practice this is almost never true, and the method we use in our analysis accounts for this. See Online Appendix B.8 for the more general formula.

16. This is a deviation from the typical use of nonrandom isonymy. In isolated populations where each surname can be traced back to a single ancestor, total isonymy is the true measure of inbreeding, and random isonymy simply captures the component of inbreeding that would result from marriage at random, with individuals neither favoring nor avoiding their relatives. In our setting, it is reasonable to assume that if marriages were done at random, the vast majority of same-surname marriages would not be between first cousins. This is in part because we are not interested in kin relations more distant than first cousins and because most common surnames in the United States do not have a single shared ancestor (at least not one recent enough to be relevant for calculating the rate of first-cousin marriage). Our procedure is therefore closer in spirit to Darwin (1875) than to most of the modern literature on marital isonymy.

marriages by chance and not due to a preference for cousin marriage.

Likewise, not all cousin marriages are isonymous. An individual's first cousins can be divided into four types, which are labeled as the offspring of either their (i) father's brother, (ii) father's sister, (iii) mother's brother, or (iv) mother's sister. In a patrilineal society, where children take the surname of their father, only marriages between the first type lead to isonymy.[17] This is illustrated in Online Appendix Figure B.17. In the second type, for example, the father passes down his surname, but his sister's children take their father's name. If all four types are equally likely, one quarter of cousin marriages will be isonymous. Hence, to a first approximation the rate of cousin marriage in a population is four times the rate of isonymy. Multiplying the isonymy rate by the correct factor adjusts for false negatives in calculating cousin marriage rates from isonymy.

The relationship between isonymy and cousin marriage relies on the assumption, alluded to above, that consanguineous relations occur through male and female ancestors in equal proportion. That is, all four types of cousin marriage are equally likely. Globally, this assumption does not always hold, notably in societies that distinguish linguistically between types of first cousins.[18] However, no such preference seems to have existed in the United States at the time (Schneider and Homans 1955; Schneider 1980; Swedlund and Boyce 1983). We test this in Online Appendix B.3 using genealogical data and find that the proportion of each type of cousin marriage is roughly one-quarter and shows no secular trend.[19]

17. Second cousins and more distant relations may also share a surname, of course. One of the contributions of (Crow and Mange 1965) is to show that the degree of inbreeding between two marriage partners is proportional to their probability of isonymy.

18. Most societies where cousin marriage is common show a preference for cross-cousin marriage (Murdock 1949). One notable exception to this is that many Arab societies have a preference for marriage between cousins whose fathers are brothers (Korotayev 2000).

19. Two other relevant assumptions are that naming practices are consistent (a child always receives their father's surname) and that illegitimacy, adoption, and surname changes are negligible (Crow and Mange 1965). Following the literature on isonymy in the United States, we take the first for granted (see Swedlund and Boyce 1983). Illegitimacy and adoption are important to geneticists, as it creates a mismatch between inherited genes and inherited surnames. In our case, this distinction is unimportant if children bear the surname of the family that raised

TABLE II

CALCULATING COUSIN MARRIAGE RATES FROM ISONYMY (1750–1858 MARRIAGE RECORDS)

Surname	Smith	Wallace	Goff	Swan
Individuals with surname	92,299	6,369	1,912	1,856
Married to same surname spouse	810	108	66	14
Observed isonymy P (%)	0.88	1.70	3.45	0.75
Random isonymy P^r (%)	1.10	0.09	0.04	0.03
Nonrandom isonymy P^n (%)	−0.22	1.61	3.42	0.72
Cousin marriage rate (%)	0.00	6.45	13.66	2.88

Notes. Observed isonymy is the fraction of same-surname marriages. Random isonymy is the share of same-surname marriages we would expect if marriages were at random in each state-decade marriage pool. Nonrandom isonymy is the component of observed isonymy in excess of random isonymy. Cousin marriage rates are calculated using the following formula: $CousinMarr = \max\{P^n \times 4, 0\}$.

For concreteness, Table II presents cousin marriage rates for Smiths, Wallaces, Goffs, and Swans from 1750 to 1858. Of the many Smiths in our sample, fewer than would be expected at random married other Smiths (observed isonymy is lower than random isonymy). Thus we infer a 0% rate of cousin marriage for Smiths. Wallaces, Goffs, and Swans are much less common than Smiths, and hence have lower rates of random isonymy. Unlike Smiths, they have more same-surname marriages than we would expect at random. Although we observe roughly the same number of Goffs and Swans in our marriage records, Goffs marry each other at roughly five time the rate of Swans. Over 3% of Goffs are married to another Goff, whereas fewer than 1% of Swans married another Swan. Correspondingly, the rate of cousin marriage for Goffs is 13%, versus 3% for Swans.[20] See Online Appendix B.8 for more details on the procedure we use to calculate cousin marriage rates using our marriage records, including how we aggregate surname-level random isonymy rates from state-decade marriage pools.

them, and hence we do not attempt to correct for them. Surname changes were common for Blacks during our period of interest (many did not have inheritable surnames prior to emancipation) and so, partly for this reason, we exclude Blacks from our analysis.

20. Recall that since only one of the four (roughly equiprobable) types of cousin marriage lead to isonymy, we multiply nonrandom isonymy by four to infer cousin marriage rates. In some cases, such as for Smiths, the number of isonymous marriages observed may be less than predicted by random mating, in which case nonrandom isonymy will be negative. In such cases, we treat the cousin marriage rate as being equal to zero.

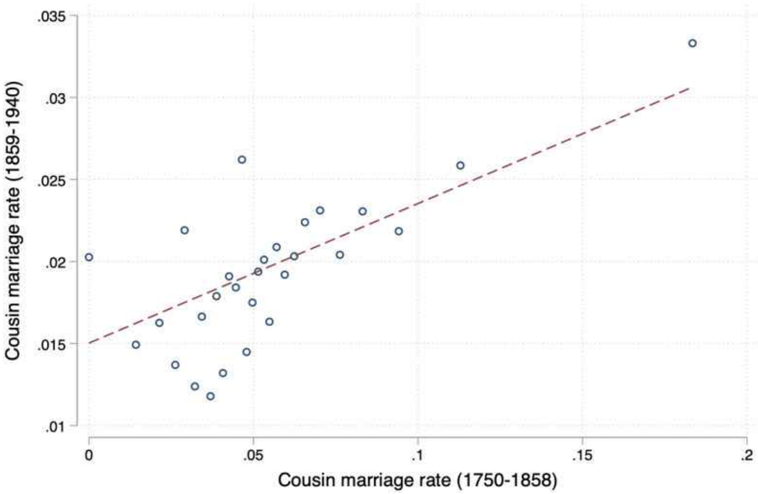

FIGURE I

Persistence in Cousin Marriage Rates by Surname

This figure is a binscatter of surname-level rates of cousin marriage before and after the first state ban on cousin marriage. This figure omits data from state-years where cousin marriage was banned, as well as any surname with fewer than 50 marriages in either period.

For our causal analysis, we would ideally identify individuals in the census who would marry a cousin in the absence of a ban on cousin marriage. These are the individuals most treated by a ban. To approximate this, we identify surnames with high rates of cousin marriage in the period preceding the first ban on cousin marriage in the United States. Men with those surnames are considered to be treated once a ban is enacted in their state of birth. That is, we use pre-1859 surname-level rates of cousin marriage as a proxy of a post-1859 individual's propensity to marry a cousin.

To test whether surnames are a useful marker of the propensity to marry a cousin, we show in Figure I that surname-level rates of cousin marriage are highly persistent. The figure plots on the horizontal axis the average rate of cousin marriage for each surname in our marriage data from 1750 to 1858. This is the period we use in our analysis to identify high-cousin-marriage surnames, since it precedes the first state ban on cousin marriage. The vertical axis plots surname-level rates of cousin marriage from 1859 to 1940, excluding state-year observations in which

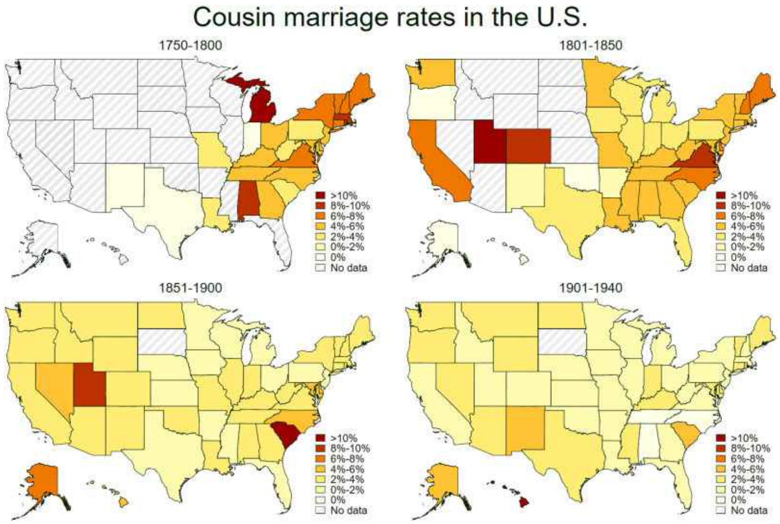

FIGURE II

Cousin Marriage in the United States (1750–1940)

States with fewer than 1,000 records in a given time period are shaded with stripes.

cousin marriage was banned. The upward-sloping relationship is evidence of persistence: surnames with high rates of cousin marriage in the early period also have higher rates of cousin marriage in the later period, despite an overall decline in cousin marriage rates.[21]

III.C. U.S. Cousin Marriage, 1750–1940

How do our estimated rates of cousin marriage vary over time and across the United States? Figure II shows how our measure varies across states over 50-year intervals. The sequence of maps shows the decline in rates over time and its geographic variation. For our main analysis, we classify surnames as having high (or low) rates of cousin marriage using all marriages from 1750 to 1858. Online Appendix Figure A.11 shows state-wide rates for that period, which is the most relevant for our analysis. During

21. The stability of cousin marriage rates is consistent with Giuliano and Nunn (2021), who show persistence of such practices over very long periods of time.

this period, Utah has the highest rate, followed by Virginia, North Carolina, Maine, New Hampshire, Rhode Island, Colorado, and Nebraska. Finally, Online Appendix Figure A.12 shows the estimated rate of cousin marriage for the United States as a whole, from 1750 to 1940. This figure illustrates the decline in cousin marriage rates from about 7% in the late eighteenth century to 1.5% in the early twentieth century.

III.D. Census Data

Data on individual outcomes come from the 1850 to 1940 restricted complete count U.S. Census from IPUMS (Ruggles et al. 2015). We use data on location of residence, occupation, household composition, and medical disability. Online Appendix B.6 provides a complete list of the outcome variables used in our analysis and their definitions.

IV. OLS Estimates from Same-Surname Couples

This section explores how married cousins differ from comparable unrelated couples. We do this by linking married couples in the census to their marriage record. This allows us to directly compare isonymous and nonisonymous couples and study their differences. To focus on comparisons between otherwise comparable couples, we include a stringent set of fixed effects. Our results compare couples living in the same county, with husbands born the same year and in the same state, wives born the same year and in the same state, and observed in the same census round. Comparing these couples provides a credible lower bound on the differences in characteristics of cousin marriages, given two assumptions that we feel are reasonable in this setting.

The first assumption is that a higher proportion of isonymous marriages are cousin marriages than is the case for nonisonymous marriages. Note that we do not require that all isonymous marriages be between cousins. Noncousin isonymous marriages, as long as they are representative of the population of noncousin marriages, will simply lead us to understate the differences between cousin and noncousin marriages in our comparison. Similarly, if nonisonymous marriages contain a large share of cousin marriages, we again understate the differences. It seems likely that a large share of isonymous marriages are between cousins and that the share of nonisonymous marriages that are between cousins is small.

The second assumption is that isonymous cousin marriages, between the children of two brothers who pass down the same surname, are representative of all cousin marriages. We have no direct evidence that these marriages are representative of all cousin marriages, but no reason to believe otherwise. Standard American kinship patterns do not distinguish between cousin types (cross versus parallel, and patrilateral versus matrilateral), unlike in many societies worldwide that have specific taboos or preferences over some type of cousin marriage (Schneider and Homans 1955; Schneider 1980). Kinship in the United States is bilateral, traced through both the father's and the mother's line.

IV.A. Empirical Analysis

Because the census omits married women's premarital surname, we link couples to their marriage record to identify isonymous (same-surname) spouses. To do so, we use questions about the year in which a couple was married, asked only in the 1900, 1910, and 1930 census questionnaires. Combined with the husband's full name and the wife's forename, we use their marriage year to link census couples to their marriage record.[22] We link about one million marriage records to two million census individuals. In Online Appendix Table A.2, we compare the linked data to the entire set of married individuals in the relevant census rounds. About 0.5% of these couples are isonymous—that is, they shared a surname prior to marriage.

Under the assumptions described above, comparing isonymous with nonisonymous couples provides an underestimate of the differences between cousin and noncousin couples. We regress a range of census outcomes on an indicator variable for being in an isonymous marriage and include a range of controls to absorb

22. To allow for recall and administrative errors, we allow links with marriage records that were registered up to two years before or after the year of marriage as stated in the census. We restrict our links to cases where no more than one marriage record matches the data from the census. To allow for slight differences in transcription, we allow for matches where the names are very similar but not identical. We consider a name to match if the Jaro-Winkler distance score is greater than 90%. Both the husband's and the wife's name strings must be above 90% for the couple to be considered a match. To match the analysis in the rest of this article, we restrict this sample to U.S.-born whites ages 18–50.

confounding variation. We use the following specification:

$$y_{ilc} = \beta \text{Isonymous}_i + \alpha_l + \alpha_c + \mathbf{X}_i \delta + \epsilon_{ilc}, \tag{1}$$

where i, l, and c refer to a couple, location of residence (county), and census round, respectively. $Isonymous_i$ is a binary variable that denotes whether the couple are in an isonymous marriage, and β is our coefficient of interest. We include fixed effects for the county of residence and census round (α_l and α_c). $\mathbf{X}_i$ is a vector of couple-level controls that consists of fixed effects for year and state of birth (for husband and wife separately), and (log) surname size.[23] Because some couples are observed more than once across the three census rounds, we cluster standard errors at the level of the couple.

IV.B. Results

Results from Table III show meaningful differences between isonymous and nonisonymous couples. These suggest that married cousins are more rural and less likely to migrate across states and have lower-earning occupations. These differences are substantial in magnitude, even after conditioning on state and year of birth, surname size, and county of residence. We find they are 5 percentage points more likely to live on a farm and 4 percentage points less likely to live in an urban location (more than 2,500 people). They live in locations with 25% fewer residents. Each spouse is 3 percentage points less likely to live in a different state than they were born in.

Men in same-surname marriages have occupations that pay 5%–6% less, measured either using Occupational Income Score (occscore) or a recent alternative that predicts earnings using occupation, industry, and demographics.[24] This result holds even within census enumeration districts, which typically include only a few hundred households (see Online Appendix Table A.3).

Women in same-surname marriages have a slightly higher mean age at marriage, but also a higher variance. Because of this, they also are more likely to marry before the age of 18. Same-surname spouses have slightly larger households and more

23. Surname size is defined as the number of people with that surname. This is to account for more common surnames having higher rates of random isonymy.

24. We do not report the income of women, since their labor force participation is only 5% in this sample.

TABLE III

CORRELATES OF COUSIN MARRIAGE: OLS REGRESSIONS

	Mean of nonisonymous	Isonymous	Std. err.	Observations
Panel A: Residence and migration				
Farm	0.3	0.050***	0.0063	1,017,678
Urban	0.45	− 0.040***	0.0049	1,017,678
Urbanization (log pop'n in location of residence)	3.08	− 0.246***	0.0271	1,017,471
Living outside state of birth (husband)	0.37	− 0.027***	0.0052	1,017,678
Living outside state of birth (wife)	0.34	− 0.028***	0.0053	1,017,678
Panel B: Occupational income (husband)				
Log occscore	3.07	− 0.065***	0.0064	856,585
Log LIDO score	3.04	− 0.045***	0.0057	844,838
Percentile rank	49.84	− 3.44***	0.364	897,692
Panel C: Age at marriage				
Marriage age (husband)	24.4	0.058	0.0566	1,017,678
Marriage age (wife)	20.97	0.095*	0.0572	1,017,678
Husband married before age 20	0.1	0.011**	0.0047	1,017,678
Husband married before age 18	0.02	0.003	0.0023	1,017,678
Wife married before age 18	0.19	0.013**	0.0059	1,017,678
Wife married before age 16	0.05	0.014***	0.0039	1,017,678
Panel D: Household composition				
Family size	4.46	0.19***	0.0308	1,016,367
No. of couples (in unit)	0.07	− 0.001	0.0049	1,017,678
No. of children (under five)	0.64	0.064***	0.012	1,016,367
Multigenerational HH (3+ gens)	0.09	0.001	0.0042	1,017,678
Panel E: Disability				
Deaf or blind (any HH member), per 1,000	1.01	0.453	1.10	329,706

Notes. * $p < .10$, ** $p < .05$, *** $p < .01$. Each observation represents one married couple in one census round. Standard errors are clustered at the couple level, since a marriage record can be linked to more than one census round. All regressions include fixed effects for census year, husband's birth year, wife's birth year, husband's birth state, wife's birth state, and county of residence. Regressions also control for (log) surname size to account for higher random isonymy for more common names. For the analysis in this table, we link married couples in the 1900, 1910, and 1930 U.S. census rounds to their marriage record (as described in Section IV.A) to identify isonymous couples. The outcome variables are defined in Online Appendix Section B.6. Panel B excludes individuals with no reported occupation, and Panel E excludes individuals from the 1900 and 1930 census rounds where these data were not collected.

coresident children under the age of five, perhaps indicative of higher fertility. They are no more likely to live in a multigenerational household (three or more coresident generations), nor do they live with more (related) couples. Household members (including children) have higher rates of disability, but this difference is not statistically significant, in part because this outcome is so rare in this sample.

Consistent with our conceptual framework, cousin marriage is associated with less geographic mobility. In an era of U.S. history undergoing large-scale urbanization and industrialization, as expected this is also associated with lower-paying occupations. In the next section we investigate whether cousin marriage causes lower mobility, as suggested in Section II.A, or whether remoteness, poverty, or some related characteristics lead to higher rates of in-marriage.

V. Event study: Causal Effect of Cousin Marriage

V.A. *Data and Empirical Strategy*

The goal of this analysis is to study the causal effect of cousin marriage on individual-level economic outcomes. To do so, we use variation induced by state-level bans on cousin marriage. We identify surnames with high rates of cousin marriage in the period prior to the first state ban (up to 1858) and treat these high-cousin-marriage surnames as being exposed to a potential ban.[25] Individuals born in a state where a ban on cousin marriage has been passed would be considered treated if they have a high-cousin-marriage surname. Their key comparison group are individuals from the same state-decade birth cohort, but with a surname not associated with high rates of cousin marriage. Our treatment, then, is at the level of a *surname* $\times$ *birth state* $\times$ *birth decade*.

An event study specification allows us to study the dynamic effects of reductions in cousin marriage rates across decadal birth cohorts. This allows us both to visually inspect trends in outcomes prior to bans on cousin marriage and to inspect the short-, medium-, and longer-term consequences of the bans.

Our event study uses stacked individual-level data from all available full-count census rounds, from 1850 to 1940. Since

25. This is justified in part by the high level of persistence of surname-level cousin marriage rates, as shown in Figure I.

women in this period generally change surnames once married, and we assign treatment status using surnames, we only include men in the analysis. We also restrict our sample to 18–50-year-olds to focus on working-age people and to limit the selection out of sample due to adult mortality. We also restrict our sample to whites since we cannot reliably trace surname-level rates of cousin marriage for Blacks, many of whom took new surnames after emancipation (Litwack 1979; Byers 1995).

We link each person's census outcomes to their surname-level cousin marriage rate, as described in Section III.B. This allows us to classify each individual as belonging to a high- or a low-cousin-marriage surname, where we define a high surname as having a cousin marriage rate of 10% or higher before the first state ban (1859).

The specification we use for our event study analysis is

$$(2)\quad y_{ihstc} = \alpha_{st} + \alpha_{sh} + \alpha_c + \sum_{\substack{\tau=-6 \\ \tau\neq-1}}^{6} \beta_\tau \text{HighCM}_h \times \mathbf{1}[K_{st} = \tau] + \epsilon_{ihstc},$$

where i is an individual, h denotes high (or low) cousin marriage surnames, s is a state of birth, $t \in \{1800–1809, 1810–1819, ... , 1920–1929\}$ is a decade of birth, and $c \in \{1850, 1860, ... , 1940\}$ is a census round.[26] HighCM$_h$ is an indicator equal to 1 for surnames classified as having high rates of cousin marriage (above 10% in the preperiod), and each state-and-decade-of-birth cohort is assigned a relative time indicator K_{st}, defined relative to the timing of a ban on cousin marriage in a given state.[27] For our analysis, we restrict the sample to individuals born in states that banned cousin marriage before 1940, which is the last census from which we use outcomes.[28]

26. Anyone born before 1800 would be older than 50 in our first census, and hence not part of our sample, and anyone born after 1929 would be too young in the 1940 census.

27. Seven percent of the census individuals included in our analysis have a high-cousin-marriage surname. In Section V.E we show that our results are robust to alternative thresholds for defining high-cousin-marriage surnames.

28. This means that there are three states (Texas, Kentucky, and Maine) that eventually banned cousin marriage but are not part of our sample. In practice this makes little difference, because these states have very few people in the 1940 census who are young enough to show up in the "six decades prior" relative time period.

The *birth state* $\times$ *birth decade* (α_{st}) fixed effects absorb all variation in exposure to the state-level bans on cousin marriage. The β coefficients on treatment therefore can only be estimated because the relative time indicators are interacted with the indicator for having a high-cousin-marriage surname (HighCM_h). These fixed effects absorb much of the potentially confounding variation in our analysis, as anything that affects states differentially around the passage of a ban is accounted for. Our baseline specification also includes *birth state* $\times$ *high-cousin-marriage surname* (α_{sh}) and census round (α_c) fixed effects, which control for potential persistent differences in outcomes for high-cousin-marriage surnames in some states and time-varying changes in outcomes unrelated to treatment status, respectively. We cluster standard errors at the birth state level.

To allow for treatment effects that differ according to the timing of bans, we implement the "interaction-weighted" estimator proposed in Sun and Abraham (2021) for our event study coefficients. This procedure corrects for treatment effect heterogeneity across birth cohorts and states. In our baseline specification, our treatment group's outcomes are estimated relative to same-cohort individuals from low-cousin-marriage surnames. We show that our results are insensitive to using the last-treated cohort instead. We also present results using OLS to show that this choice of estimator is not driving our results.

The Sun and Abraham (2021) estimator is robust to treatment effect heterogeneity, but it does require two assumptions. The first is the presence of parallel trends, which we explore by reporting six decades of pretreatment coefficients. The second assumption is that there be no anticipatory behavior prior to treatment. In our specification this requires that the relative-time treatment indicators be defined in such a way that cohorts assigned to pretreatment periods not be affected by the bans. Because cohorts born before a ban may be treated, we assign relative-time indicator $K_{st} = 0$ to the cohort born in the decade preceding a ban. Correspondingly, a cohort born in the same decade as a ban is passed in their state is assigned a relative-time value of $K_{st} = 1$.

In the following section, we use this event study estimator to study the dynamic causal effect of cousin marriage bans on a range of outcomes. In doing so, we follow the hypotheses laid out in our conceptual framework. Namely, a decline in cousin marriage may lead to a shift away from intensive kinship practices and an increase in both geographic and occupational mobility. Given the

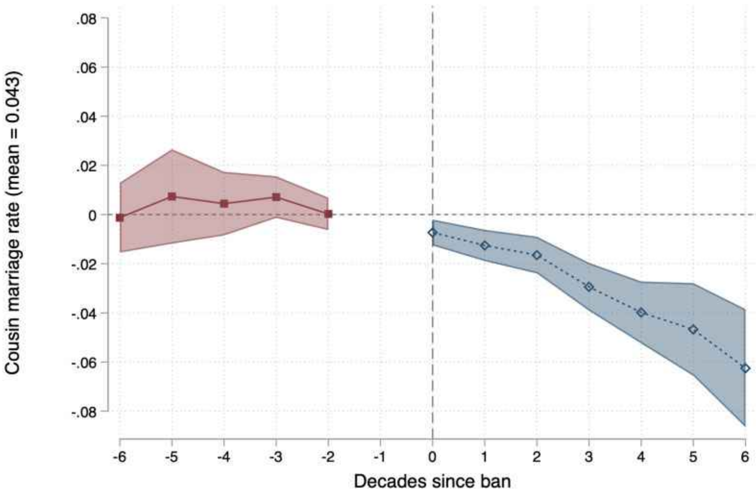

FIGURE III

Impact of Cousin Bans on Cousin Marriage Rates

This figure shows point estimates and 95% confidence intervals of the coefficients β_τ in specification (2), with modifications as described in Section V.B. The outcome variable is cousin marriage rate at the *state* $\times$ *year* $\times$ *preperiod cousin marriage level (high* versus *low)*. The main regressors are relative-time indicators (decades) with respect to the ban in a state, interacted with an indicator variable denoting a surname with high preperiod cousin marriage. The specification includes fixed effects for *census year*, *state* $\times$ *decade*, and *state* $\times$ *high-cousin-marriage surname*. The coefficients represent the differential effect of a ban on high- versus low-cousin-marriage surnames. Standard errors are clustered at the birth state level.

growing returns to urban occupations relative to farm work in this period, this would likely also lead to higher-paying occupations. A decline in the strength of kinship ties should also lead to changes in household composition toward more isolated nuclear households.

V.B. Effect of Bans on Cousin Marriage Rates

Our first result is to show that bans on first-cousin marriage did decrease rates of cousin marriage, as measured using our marriage data set. Specifically, we find that surnames with higher rates of cousin marriage prior to 1859 see a disproportionate fall in cousin marriage rates after a state ban.

Figure III shows the differential effect of cousin marriage bans on high- and low-cousin-marriage surnames.[29] The specification used is similar to equation (2) with some minor modifications. The outcome variable is cousin marriage rate at the *state* × *year* × *high-cousin-marriage surname* level.[30] The main regressors are relative-time indicators (decades) with respect to the ban in a state, interacted with an indicator variable denoting a surname with high preperiod cousin marriage (as in specification (2)). We include *year*, *state* × *decade*, and *state* × *high-cousin-marriage surname* fixed effects. The results show that the bans caused a disproportionate drop in cousin marriage rates for high-cousin-marriage surnames. The effects are observed as early as the decade of the ban and grow over time.

The finding that cousin marriage bans were effective in reducing rates of cousin marriage does not depend on our method for calculating cousin marriage rates or on our empirical strategy. Online Appendix B.3 replicates this result using an entirely distinct data set derived from genealogical records to directly identify cousin marriages, rather than inferring them from same-surname marriage records. Since it does not include names or other identifiers, that genealogical data set does not lend itself to our main analysis because we cannot link it to outcomes of interest. It does however allow us to validate the effect of cousin marriage bans on rates of cousin marriage. Any potential problems with this alternative data set should be orthogonal to the potential issues of measuring cousin marriage using isonymy in marriage records.[31]

V.C. Main Results: Residential and Occupational Mobility

Results from Section IV show that cousin marriage in the United States is associated with lower geographic mobility and lower-paying occupations. Here we consider the causal effect of a decline in cousin marriage on residential and occupational outcomes. We start with residential outcomes that speak to whether the decline of cousin marriage contributed to the dramatic shift

29. The corresponding tables for the figures in this section are presented in Online Appendix A.

30. We drop cells with fewer than 50 observations for this analysis to reduce noise.

31. Specifically, one potential issue is that bans disproportionately affected marriages between cousins that share a surname, which would lead us to overstate the reduction in cousin marriage. We show in Online Appendix B.5 that this was not the case.

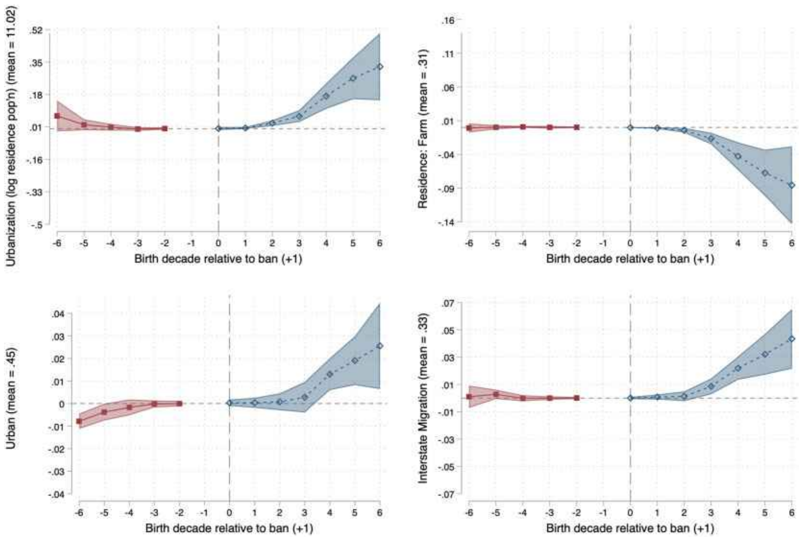

FIGURE IV

Impact of Cousin Marriage Bans on Rural-Urban and Interstate Migration

This figure shows point estimates and 95% confidence intervals of the coefficients β_τ in specification (2). The outcome variables are defined in Online Appendix B.6. The main regressors are relative-time indicators (decades) denoting the birth decade of an individual relative to a cousin marriage ban in their birth state, interacted with an indicator variable denoting a surname with high preperiod cousin marriage. The specification includes fixed effects for *census year*, *state* × *decade*, and *state* × *high-cousin-marriage surname*. The coefficients represent the differential effect of a ban on high- versus low-cousin-marriage surnames. Standard errors are clustered at the birth state level.

during our period out of farms and into urban areas. As per census records, the proportion of the population living in urban areas went from 17% in 1850 to 58% in 1940. Figure IV shows the effect of cousin marriage bans on four related outcomes: log of the population size in an individual's location of residence and indicators for living on a farm, living in an urban area, and living in a state other than one's state of birth. For each outcome, the relevant subfigure's horizontal axis denotes an individual's decade of birth relative to the ban on cousin marriage in their birth state. The vertical axis shows coefficient values denoting the differential effect of cousin marriage bans on individuals with high- versus low-cousin-marriage surnames born in the same *state* × *decade*. The omitted relative-time coefficient at −1 represents individuals born two decades before a ban. Those born the decade before a ban are assigned relative time value 0 since they are the first birth cohort plausibly affected by a ban.

The first of the four outcomes in Figure IV is the log of the population size of an individual's location of residence, as coded by the Census Bureau. Because it captures in a continuous way the transition from farm, to town, to city, we think of this as our best measure of the process of rural-urban migration. Note that because we are always comparing across surnames in state and decade birth cohorts, we are not simply capturing the overall increase in population density in a given state. Rather, a nonzero effect size implies differential movement to higher-density locations by people with high-cousin-marriage surnames in the decades following a ban. Our results suggest that bans on cousin marriage lead people from high-cousin-marriage surnames to migrate to higher-population locations, after a lag of a few decades. The estimated effect sizes are substantial: cousin marriage bans lead treated men born three decades after a ban (relative time value of +4) to live in locations with about 20% larger population.

Consistent with this finding, the second and third subfigures show that treated men become less likely to live on a farm and more likely to live in an urban location.[32] Those born three decades after a ban are 5 percentage points less likely to live on a farm and 1 percentage point more likely to live in an urban area.

Rather than simply using an individual's location of residence, we would ideally measure actual migration. Unfortunately, prior to 1940 the only direct measure of migration in the census comes from comparing an individual's state of residence to their state of birth. This is imperfect because it captures, for example, moves made when a person was an infant, and it treats return migrants identically to those who never leave their state of birth. Given these caveats, we present results for (lifetime) interstate migration in the fourth subfigure. Here again we find that cousin marriage bans had a positive effect on geographic mobility. Treated men born three decades after a ban are 2 percentage points more likely to live in a state other than their state of birth.

Cousin marriage bans also seem to have led to higher incomes, as shown in Figure V. Before 1940, the census does not

32. Cities and incorporated places of 2,500 inhabitants or more are classified as urban by the Census Bureau. Other local subdivisions with population of 10,000 and population density above 1,000 per square mile are also included. See Online Appendix B.6 for more details on this classification.

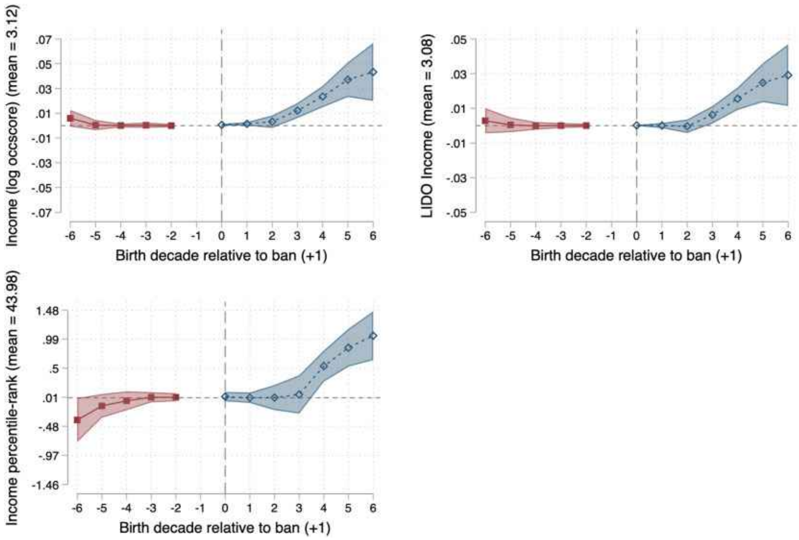

FIGURE V

Impact of Cousin Marriage Bans on Income

This figure shows point estimates and 95% confidence intervals of the coefficients β_τ in specification (2). The outcome variables are defined in Online Appendix B.6. The main regressors are relative-time indicators (decades) denoting the birth decade of an individual relative to a cousin marriage ban in their birth state, interacted with an indicator variable denoting a surname with high preperiod cousin marriage. The specification includes fixed effects for *census year*, *state* $\times$ *decade*, and *state* $\times$ *high-cousin-marriage surname*. The coefficients represent the differential effect of a ban on high- versus low-cousin-marriage surnames. Standard errors are clustered at the birth state level.

include individual information on wages or income, so we use a variety of imputations that use an individual's occupation to infer their income. The first is the log of an occupation's score, defined as the median income for that occupation in 1950 (Sobek 1995). Although commonly used, this measure assumes that all individuals with the same occupation earn the same amount across time, space, sex, and race. To some extent, our research design makes these problems less severe than they might otherwise be. Fixed effects for *birth state* $\times$ *birth decade* account for most of the variation in an occupation's earnings over time and space. Also, by restricting our analysis to white men, we need not account for differential wages by sex or race. Nevertheless, we present results in Figure V for three other methods that use more than just occupation to impute income. We presents results using a cohort-specific occupational percentile rank from Song et al. (2020), and an adjusted income score using industry, occupation, and demographics from Saavedra and Twinam (2020). Each measure is described in Online Appendix B.6. Using any of these measures, we find that a ban on cousin marriage caused movement up the occupational ladder. For men from high-cousin-marriage surnames born three decades postban (relative time value of +4), the ban led to a 2% increase in income.

V.D. Channels

Why did bans on cousin marriage lead to migration off farms and into cities and to higher-paying occupations? We consider evidence for four potential channels: weaker kinship ties, migration induced by having to find a noncousin spouse, changes in inheritance patterns, and fewer genetic disorders. Unfortunately our evidence does not allow us to make a conclusive judgment on the importance of these potential channels, though we do provide suggestive evidence in support of weaker kinship ties and against the latter three.

1. *Kinship Ties.* Our conceptual framework (Section II.A) describes how a decline in cousin marriage may have led to weaker kinship ties. This loosening of ties would have allowed people to be more mobile, both geographically and occupationally, leading to the outcomes described above. In the absence of direct measures of kinship ties, we present results from three types of outcomes that would be affected by weaker kinship bonds: geographic and occupational dispersion, household composition, and the likelihood of living in a (noncorrectional) institution rather than with one's relatives. We do not think of any of these as mechanisms but as ancillary outcomes which would also have been affected if kinship ties were weakened by bans on cousin marriage.

i. *Dispersion.* Although we have shown that bans on cousin marriage led to greater vertical mobility (living in more dense locations and moving up the occupational ladder), our conceptual framework suggests that loosening kinship ties would also lead to horizontal mobility. With weaker attachments to one's family of orientation and extended kin, individuals and nuclear families would become more willing to move to other locations in search of work or housing. Weaker kinship ties would mean that distance from one's kin would be less costly (socially, psychologically, or otherwise). Similarly, horizontal occupational mobility implies greater dispersion in occupations within members of an extended family, without necessarily moving up (or down) the occupational income ladder. This could happen because of an improved ability or willingness to work with and learn from nonkin or because of a greater circle of connections (such as in-laws) to help with finding a job. Furthermore, we should expect these horizontal moves to happen more easily than vertical moves, since they require less of a decline in kinship ties.

To measure horizontal mobility, we present results on geographic and occupational dispersion within *surname* × *birth state* × *birth decade* cohorts. We calculate a geographic analogue to a Herfindahl-Hirschman Index (HHI) where for each individual the outcome is the proportion of men sharing their surname, birth state, and birth decade living in an enumeration district other than their own.[33] Enumeration districts are the smallest geographic units easily available using census data, and are substantially smaller than counties. They represent the area assigned to a single enumerator and typically include at most a few hundred households. This value increases as *surnam* × *birth state* × *birth decade* cohorts disperse across space. Online Appendix Figure A.1 shows that bans on cousin marriage led to an increase in this measure of dispersion. It also shows a similar increase in occupational dispersion, defined in an analogous way but using occupation codes instead of enumeration districts.[34] Notably, we observe an increase in these dispersion measures for earlier treatment cohorts than those which experience rural-urban migration and growth in occupational income ('vertical' mobility). This suggests that reductions in cousin marriage first lead to spreading out, both geographically and across occupations, then movement up the ladder for both.

ii. *Household Composition.* As discussed in our conceptual framework, sociologists have emphasized the importance of the

33. For each individual in our sample, we calculate the following formula: $1 - (\frac{N_{kste}}{N_{kst}})^2$. Here N_{kste} denotes the number of people with the same surname k, birth state s, birth decade t, and living in the same enumeration district e as the person in question, while N_{kst} denotes the total number of people with surname k, birth state s, and birth decade t. The index is defined similarly for occupational categories.

34. The geographic dispersion results hold if we use counties or states instead of enumeration districts. The occupational dispersion results hold if we collapse occupations into 10 one-digit occupation categories from IPUMS.

rise of the conjugal household in the process of industrialization and urbanization in the United States during this period. Weaker kinship ties are closely linked to this change in family patterns, characterized by smaller households and fewer adult relatives cohabitating. We use the following census outcomes to study this transition: household size, number of couples in a household, number of resident children, and an indicator for living in a household that includes three or more generations. (For each of these, we restrict attention to household members to whom one is related, excluding, for example, boarders and servants.) These results are reported in Online Appendix Figure A.2. Bans on cousin marriage led to smaller family size. This is in part because of a reduction in the number of children under the age of five, as well as the number of other coresiding couples.[35] However, we do not see any effect on the probability of being in a multigenerational household.

iii. *Institutionalization.* Although strong kin ties may limit mobility, they provide more than just ties of affection and a sense of belonging. Among other functions, they can be a source of support for the elderly, infirm, or destitute. Weaker kin bonds may mean more such people are either abandoned or placed in an institution. We test for this in a limited way using census data on who lives in institutions for the elderly, handicapped, poor, or mentally ill, as categorized by IPUMS.[36] Weaker kinship ties may lead to an increase in this measure if some of the people who would otherwise have been cared for by their kin are instead placed in one of these institutions. Online Appendix Figure A.3 presents the results. We find a small but significant positive effect on such institutionalization, consistent with a weakening of kinship ties.

In sum, we find increases in horizontal mobility, changes in household composition, and increases in rates of institutionalization that are consistent with weaker kinship ties as a result of bans on cousin marriage. However it is important to note that there are other reasons these outcomes may have changed as they did alongside our main results. Rural-urban migration, caused by

35. IPUMS reports the total number of one's own (coresident) children and children under age five. We use the latter because it abstracts from changes in the age at which children move away from home, and instead measures something closer to fertility. We use the number of coresiding couples to capture clear departures from the definition of a conjugal household: that each married couple establish their own household.

36. For reference, in the 1850 census 53% of people who fit this broad category are in the subcategory "Poor house, almshouse."

some unrelated mechanism, could have led to smaller households and increases in institutionalization. Horizontal geographic mobility could be the result of moves to find a noncousin spouse. We consider prominent potential channels from the literature on the effects of cousin marriage.

2. *Marriage Markets.* Bans on cousin marriage have a direct effect on the marriage markets of people who would have otherwise married a cousin. In rural locations, marriage markets may be thin enough that the inability to marry a cousin means having to move to find a suitable spouse.[37] Similarly, noncousin spouses may simply live further away from each other premarriage, such that now at least one of the spouses must move to join the other. Finally, premarital investments in human capital may have higher returns in the nonkin marriage market. A teenager who expects to marry their cousin may see less need to pursue their education.

Broadly consistent with this channel, we showed an increase in geographic dispersion in Online Appendix Figure A.1 following treatment. To more specifically test whether this represents a direct marriage market effect, we focus here on young, not-yet-married individuals. We first test this by splitting our sample above and below the median age and present results separately for each. Second, we compare effect sizes for married and unmarried individuals above the age of 30. Although this more precisely tests the channel of interest, it is less clean empirically since the choice to marry is endogenous.

We present results from the age heterogeneity analysis in Online Appendix Figure A.4. Note that because the time dimension in our event study refers to birth cohorts, individuals will typically be included more than once, at 10-year intervals, and hence at different ages. The two overlapping coefficients for a given relative-time value therefore are estimated on the same individuals, measured when young (18–30) and again when older (31–50). If migration to more urban areas was driven by the search for a spouse, we should expect that treatment effects should be fully apparent for the younger sample and not grow substantially for the older sample, for whom the ban on cousin marriage imposes no new incentive to migrate. Instead we find that effect sizes are typically larger for the older sample (31–50).

37. As shown in Online Appendix Figure A.1, we do find increased geographic dispersion for men with high-cousin-marriage surnames following a ban.

Online Appendix Figure A.5 provides a comparison of married versus unmarried men age 30 and above. Results suggest that if anything, treatment effects are larger for unmarried men. While of course there is selection in marriage, we might expect that those who remain unmarried past 30 would be less likely to have moved to a more populous location. This result, as well as the one from Online Appendix Figure A.4, holds for all the measures of migration used in our main results (indicators for farm, urban, and lifetime interstate migration).

3. *Inheritance.* The practice of cousin marriage in many cultures is deeply tied to inheritance practices, as emphasized by Bahrami-Rad (2021) in India and the Muslim world. Here cousin marriage matters because it allows wealth (typically land or livestock) to remain in a kinship group.[38] Although the link to inheritance practices is less clear in the nineteenth-century U.S. setting, the inability to consolidate family wealth holdings through cousin marriages may have led to greater rural-urban migration. Rather than having farmland divided into smaller parcels across generations, cousin marriage may have allowed land to be consolidated in fewer branches of the family. As a consequence, more offspring may have become landless and hence found it necessary to leave farming and migrate for work.

Unfortunately we are limited in our ability to test this potential channel. The census does not include any measures of land holdings or wealth after 1870, which means we cannot track how the distribution of farm land or other wealth changed over time. We do find that surname-level cousin marriage rates predict higher 1850 real estate ownership. This is true for both farm and nonfarm households. This supports the idea that intergenerational management of wealth (and especially land) may have played some part in motivating cousins to marry.

To test whether changes in inheritance led to our main results, we turn to heterogeneous effects of the bans on cousin marriage by how much wealth families owned. For families with little wealth to pass on to future generations, changes in inheritance practices should have been less relevant. For these low-wealth

38. Often this arises in societies where daughters share in their father's inheritance. Choosing to marry someone from outside the male line of descent would take away from the clan or kinship group's stock of wealth. In societies where daughters do not inherit, cousin marriage does not affect inheritance outcomes.

families, whether cousins married or not would have had little effect on geographic or occupational mobility.

To study heterogeneity by wealth, we split the sample by each surname's average real estate holdings in the 1850 census, before the first ban on cousin marriage. We assign households to above- or below-median real estate value, which was \$234 (roughly \$9,000 in current value). Online Appendix Figure A.6 shows treatment effects separately for high- versus low-real-estate-value surnames. Migration to more urban areas seems to have been, if anything, slightly greater for surnames with below-median real estate holdings in 1850. This suggests that the consolidation of wealth across generations was not a central channel through which the cousin marriage bans affected geographic mobility.

4. *Individualism.* Cousin marriage may foster communalistic attitudes and lead to lower levels of individualism (Henrich 2020). Our results may be driven by increasing levels of individualism, which might lead to more mobility as highlighted in Knudsen (2022). Following this literature, we use as our measure of individualism the commonness of (given) names parents give their children. Choosing less common first names for one's children is thought to be a reflection of higher levels of individualism (Twenge, Abebe, and Campbell 2010; Varnum and Kitayama 2011).

Following Bazzi, Fiszbein, and Gebresilasse (2020), for each census round we find the top 10 names for boys and for girls under the age of 10. This is done for each census region separately to capture potential regional variation in the list of common names. For each person in our sample with at least one child below 10, we calculate the share of children that do not have one of these common names. Online Appendix Figure A.7 suggests that bans on cousin marriage did not have measurable effects on this measure of individualism, suggesting that in our setting, this does not seem to have been a primary channel through which declines in cousin marriage led to increases in mobility.

5. *Genetic Disability.* Reduced in-marriage may have led to fewer deleterious recessive genes being expressed, and hence better outcomes for descendants of men who would otherwise have married their cousin. The timing of treatment effects is consistent with this potential channel, since we would expect genetic

health to improve slowly, over generations. Only the children and grandchildren of treated men would have improved outcomes.

In the absence of records on genetic disorders, we cannot directly test this potential channel. Instead we propose two measures in the historical census that can serve as proxies. The first is whether an individual was noted as either blind, or "deaf and dumb," the terms used at the time for severe hearing and speech disabilities, respectively.[39] The second is whether an individual was living in a hospital, mental institution, or home for the physically handicapped. Because both proxies are quite rare, we show results for an indicator outcome equal to 1 if an individual is either blind, "deaf and dumb," or living in a medical institution, and report the value per 10,000 people. Online Appendix Figure A.8 presents the results. Although the results are noisy, we do not see a decrease in this measure of disability following a ban on cousin marriage. This suggests that an improvement in genetic health does not explain our findings.

V.E. Robustness and Threats to Identification

1. *Time-Varying Confounders.* The timing of state bans could be correlated with policies or changes that may confound our findings. Because we include *birth decade* × *birth decade* fixed effects in our regressions, confounding variation that affects states differently around the passage of bans is already accounted for—unless they affect high- and low-cousin-marriage surnames differently. We thus control for a number of factors (at the *birth state* × *birth decade* level) that one might be concerned is correlated with the timing of state bans and that may affect high- and low-cousin-marriage families differently. These include whether there was a sterilization law in place, whether there was a compulsory schooling law in place, whether the state had a minimum age of marriage law (above 16), percent of state with railroad coverage, whether the state was already a part of the union (had achieved statehood), percentage of the state that had frontier experience, the share of foreign-born individuals in the state, and finally sex ratios in the state. We interact these variables with the dummy denoting a high-cousin-marriage surname and add them to our main specification—the results are presented in Online Appendix

39. The 1850 and 1860 full-count census rounds also include whether a person was noted as being "idiotic" or "insane." These are too early to provide us with postban outcomes necessary to estimate treatment effects.

Table A.15. Both in terms of magnitude and statistical significance, these estimates are very similar to our main estimates.

2. *Selective Interstate Migration.* One concern with our identification strategy is selective out-migration: families that have a preference for cousin marriage may move to states that do not have a ban. If that were the case, we should see affected individuals preferentially migrating to states that do not (yet) have a ban in place. We rerun the analysis for lifetime interstate migration (living in a state other than your state of birth) but split the outcome into two: migrating to a state that already has a ban in place versus one that does not (Online Appendix Figure A.13). We find that the effects are very similar for both, suggesting there was not a tendency to migrate to marry in a state where it could be done legally.

In a related robustness check, instead of assigning treatment based on an individual's own birthplace, we use their father's birthplace to account for selective migration. Since father's birthplace is missing for some census rounds, our sample shrinks when we do this. Our main results nevertheless remain robust, as shown in Online Appendix Table A.16.

3. *Surname Standardization.* The surnames we use to link census individuals to cousin marriage rates may be subject to spelling variations over time or across space. To account for this, we show that our results are insensitive to standardizing surnames using NYSIIS (New York State Identification and Intelligence System) phonetic codes.[40] This procedure combines, for example, the surnames Smith, Smithe, and Smit and combines Smither and Smithers. We use these standardized phonetic codes to link marriage record surnames to census surnames. Online Appendix Table A.17 shows results for our primary outcomes (rural-urban migration and occupational income) using this transformation. Reported coefficients are almost unchanged.

4. *Time Trends.* We include *high cousin marriage* × *birth decade*-level time trends to account for the possibility that higher-cousin-marriage families may experience faster improvement in outcomes (in terms of income and rural-urban migration) because they start off poorer and more rural, and this may explain the effects we find. While parallel pretrends does alleviate such concerns, we include these controls as an additional robustness

40. These phonetic codes are from Taft (1970). We assign NYSIIS phonetic codes using the following Stata module: Sayers (2018).

check. The results do not change, as reported in Online Appendix Table A.18.

5. *Surname Rates of Cousin Marriage.* Our results are robust to classifying surnames as high or low cousin marriage at the state rather than national level. That is, instead of using country-wide rates to classify surnames into high- and low-cousin-marriage groups, we use state-specific rates to account for the fact that national measures may miss important state-level differences. We do not use this in our baseline specification because this comes at a cost—for most surnames, we only have measures of cousin marriage for a few states, and these states are disproportionately those that were more populated before 1858. Furthermore, we find that surname-level rates of cousin marriage are strongly correlated across states, suggesting that a national surname-level measure of cousin marriage is appropriate. The results in Online Appendix Table A.19 show that our main results are robust to using this alternate (state-specific) measure of pre-1858 cousin marriage rates. Furthermore, since cousin marriage rates may be more precisely measured for surnames with larger number of records, we control for surname-state group sizes or include surname $\times$ state fixed effects when we use this alternate measure—as shown in Online Appendix Table A.20 the results do not change.

Results are not sensitive to the choice of a 10% threshold to classify surnames as having high versus low rates of cousin marriage. We show comparable results using 8% and 12% thresholds in Online Appendix Tables A.21 and A.22. Using 10% as the threshold rate, we classified about 7% of our sample as having high-cousin-marriage surnames. With 8% and 12% as threshold values, the proportions are roughly 12% and 4.5%, respectively.

Finally, our adjustment for random isonymy in our calculation of cousin marriage rates may be overcorrecting for differences in same-surname marriages for those with common surnames. To test for this, we show that our results are robust to the use of observed isonymy instead of nonrandom isonymy to classify surnames into high (observed isonymy $\geqslant$ 2.5%) and low (observed isonymy $<$ 2.5%) cousin marriage groups (Online Appendix Table A.23).

6. *Surname Fixed Effects.* We do not include surname fixed effects in our main analysis because it is computationally intensive with the Sun and Abraham (2021) method. (There are over 400,000 surnames in our final sample.) However, surnames

may capture important cultural traits, and therefore having these fixed effects could account for potentially problematic variation. We control for them in the two-way fixed effects specification and find that our results are unchanged (Online Appendix Table A.24). This table also confirms that our main results are robust to using a standard two-way fixed effects estimator.

7. *Control Group: Last Treated Cohort.* In our baseline specification using the Sun and Abraham (2021) method, we treat all individuals with low-cousin-marriage surnames in the preperiod (the never-treated cohort) as our control group. In Online Appendix Table A.25, we instead treat people in states that banned cousin marriage after 1920 (last treated) as the control group and show that our results are not sensitive to this choice.

8. *Common Surnames.* In Online Appendix Table A.26, we show robustness of the results to dropping the bottom (columns (1) and (2)) as well as the top 25% (columns (3) and (4)) of people in our sample with the most common surnames.

VI. Conclusion

This article uses nineteenth- and twentieth-century U.S. state-level bans on cousin marriage to provide causal micro-evidence of the effect of consanguineous marriages on a range of economic outcomes. Borrowing a method from population genetics, we show that excess rates of same-surname marriages can provide credible estimates of cousin marriage rates by surname, by state, and over time. Bans on first-cousin marriage led to greater geographical and occupational mobility, higher incomes, and increased rural-to-urban migration. These effects do not seem to be driven by the genetic effects of cousin marriage. Instead, we argue that the economic gains we document are driven largely by changes in social relationships that stem from weakened kinship ties.

These effects, while striking in magnitude, are consistent with work in anthropology and sociology that studies the characteristics of strong kinship ties. Henrich (2020), for example, summarizes a large body of ethnographic and historical research showing that tight (intensive) kinship is associated with greater cooperation in a kin group, at the cost of geographic and social mobility and participation in anonymous markets and broader impersonal institutions. The results from this article are consistent with the view that structural transformation can lead to a mismatch between cultural norms and economically optimal behavior. Tight kinship bonds, reinforced by cousin marriage, become less adaptive as societies shift out of agriculture, but marriage practices may adapt only slowly.

We believe that these results are not just of historical significance. They are also relevant for contemporary development outcomes, since intensive kinship is still prevalent in many societies. Online Appendix Figure A.14 shows estimated national contemporary rates of cousin marriage plotted against incomes per capita. These data show high rates of cousin marriage in many countries and a striking cross-country correlation with development and political institutions (Woodley and Bell 2013; Akbari, Bahrami-Rad, and Kimbrough 2019; Schulz 2022). The causal estimates in this article of the effect of kinship are not directly applicable to such societies, where kinship ties may substitute for weak formal institutions. Nevertheless, our results do suggest that as economies undergo structural transformation, leading to the development of better institutions, there could be economic returns from family structure transitions that lead to weaker kinship ties.

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/JOXAFP (Ghosh, Hwang, and Squires 2023).

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