Where Marriage Ends: The Geography of Divorce in America*

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Abstract

From the 1900s till around 1980, the geographic patterns of divorce across America were relatively stable, with persistently higher rates of divorce in the West, the industrial Midwest, Florida, and Maine. I show that these patterns were sustained by three reinforcing mechanisms: selective migration, social interactions, and intergenerational transmission. After 1980, the geography of divorce began to shift, concentrating in the South instead. I propose that changes in the national wage structure, which saw worsening economic circumstances for less-skilled workers, had a role to play. Consistent with this hypothesis, I find that the geography of divorce reorientated around places with fewer college graduates, namely the South.

Keywords: Divorce, Selective Migration, Social Interactions, Intergenerational Transmission, Wage Structure

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1 Introduction

Today, some 40 percent of marriages in America will end in divorce (Bailey et al., 2025). This phenomenon has major repercussions for society. For the couple separating, both sides become more anxious and depressed, effects that linger long-after they have parted (Dasgupta et al., 2025). For children, parental divorce leads to lower adult earnings and college residence, whilst increasing incarceration, mortality, and teen births (Johnston et al., 2025). Across generations, the cycle of divorce perpetuates itself, as children of divorced parents are themselves more likely to go through a divorce (Wolfinger, 2005). Figure 1A traces the evolution of divorce in America, capturing the dramatic transformation of family structure over the past 150 years.[1] For much of history, divorce has been on a steady ascent, with a temporary spike after World War II (WWII) followed by a steep rise from the late 1960s, before trending downward after 1980. Given its prevalence and impact, a large literature in economics has sought to understand the forces driving the trends in divorce (Stevenson and Wolfers, 2007). This paper looks beneath these aggregate trends, focusing instead on the geographic variation in divorce across the country. The period when divorce was rapidly rising (1960s-1980) was characterized by a relatively stable geography of divorce, one dating back to the dawn of modern US history. In contrast, the era of declining divorce (after 1980) was accompanied by a shift in spatial patterns. What explains the *initial persistence* and *subsequent change* in the landscape of divorce?

I begin by documenting the geographic patterns of divorce over the last century. The 1940-1980 US censuses and the 2008-2018 American Community Surveys (ACS) collected information on both current marital status and the number of times a person had been married, allowing one to identify those who had ever been divorced. From these data, I can map the prevalence of divorce across different parts of the US, covering a span of almost 80 years. My findings are twofold. First, relatively consistent patterns are observed between 1940-1980, with higher rates of divorce in the West, the stretch from the industrial Midwest southwestward to Texas, as well as in Florida and Maine. In fact, historical administrative records indicate that these patterns go even further back till at least the 1900s, the dawn of modern US history. This spatial variation in divorce thus persisted for almost century, from the 1900s to 1980. Second, a markedly different picture emerges by 2008-2018, where divorce is now concentrated in the South.

Why was the geography of divorce so persistent before 1980? This paper proposes that the prevailing patterns of divorce were sustained by three reinforcing mechanisms: selective migration, social interactions, and intergenerational transmission. I investigate each of these forces in turn.

The first reinforcing mechanism is selective migration, where those who have been divorced or who have a greater propensity for divorce are systematically more likely to move to high-divorce areas or less likely to leave these places. I test for such dynamics using a large sample of individuals linked across the 1940 and 1950 full count censuses, the only enumerations with the information and size needed for this analysis. Among those starting out in low-divorce areas, persons who have

[1] Figure A.1 shows the corresponding trend in the raw count of divorces.

gone through a divorce are 2 percentage points more likely to migrate to high-divorce locations. Conversely, among those starting out in high-divorce areas, persons that had experienced a divorce are around 2 percentage points less likely to leave for low-divorce places. These differences are sizable, around 16 to 37 percent of the mean divorce rate. Such selective migration into and out of high-divorce localities can help sustain the existing patterns of divorce.

The second reinforcing mechanism are social interactions, where a person's behavior is shaped by who she interacts with. At a time when modern communication tools were not available, interactions among neighbors may have been particularly salient. I investigate whether living close to someone who had gone through a divorce influences an individual's own propensity for divorce. To do this, I adapt the spatial proximity approach in Bayer et al. (2008). Specifically, I examine the likelihood that a pair of neighbors have both experienced a divorce, comparing neighbor-pairs that reside close together versus those that live slightly further apart. By focusing on small geographic spaces, this research design limits the scope for residential sorting, allowing one to estimate the impact of social interactions. I find evidence of such interpersonal dynamics at the neighborhood level: neighbor-pairs living close by are 0.1 to 0.2 percentage points more likely to both have been divorced, about 11 to 13 percent of the mean divorce rate. The cumulative impact of such interactions would be greater in places where divorce was already more prevalent, strengthening the prevailing geography of divorce.

A third reinforcing mechanism is the intergenerational transmission of divorce risk, where children of divorced parents are more likely to go through a divorce themselves, a pattern that has been documented in more recent times (Wolfinger, 2005). Drawing on linked census data from the early 20th century, I show that this parent-child association was also evident in historical America. In the presence of such vertical transmission, areas where divorce is more widespread in one generation will also be where divorce is more common in the next generation, all else equal.[2] This would solidify the existing landscape of divorce.

Having unpacked why the geography of divorce was so persistent before 1980, I then turn to the shift in divorce landscape after 1980—what caused the transformation? This paper argues that broader economic forces may have been at play. Here, I focus on changes in the wage structure. From the 1980s, the college wage premium rose dramatically, driven in part by worsening economic circumstances for those without a college degree. Such adverse effects can increase the likelihood of divorce (Banzhaf, 2018; Charles and Stephens, 2004; Jackson et al., 2017), amplifying the negative association between college and divorce over time. If sufficiently strong, such dynamics could offset the reinforcing mechanisms of selective migration, social interactions, and intergenerational transmission, causing the geography of divorce to reorientate around areas with more unskilled workers. I find empirical support for this prediction: places with relatively more (less) college graduates in 1980 experienced greater declines (increases) in divorce over the ensuing decades. Reassuringly, no systematic patterns are observed before 1980, allaying concerns over pretrends. My results are also robust to applying an instrumental variable (IV) approach, using the local

[2]This assumes that a majority of individuals remain where they grew up, a point that I return to below.

industry composition in the past (1940) to predict the college share in 1980.

With a deeper understanding of the initial persistence and subsequent change in the geography of divorce, I then complete the analysis by diving deeper into history: what gave rise to the original patterns of divorce which the triumvirate of selective migration, social interactions, and intergenerational transmission then reinforced for much of the 20th century? That these patterns were already evident in the 1900s rules out a wide range of explanations, from the mobilization for World War I (WWI), to the passage of the 19th Amendment, the prosperity of the Roaring Twenties, and the chaos of the Great Depression, among other things. What then accounts for the early patterns of divorce? To make progress, I take a step back into the 19th century. At the time, the US was still rapidly expanding its territory westward. Using administrative divorce records, I show that conditions on the western frontier increased the prevalence of divorce in 19th-century America, in line with Bazzi et al. (2024). Frontier conditions may thus have contributed to the original patterns of divorce. In contrast, I find that other 19th-century factors like the expansion of women's economic rights, the influx of immigrants, and differences in state divorce legislation are unlikely to explain the early geography of divorce.

My paper contributes to two broad strands of literature. The first is the large body of work studying the trends in divorce and their underlying drivers. Stevenson and Wolfers (2007) survey the research in this field. Much has been written on the steep rise in divorce from the late 1960s (Figure 1A). A wide range of potential causes have been cited, including the growth in female labor force participation (McKinnish, 2007; Neeman et al., 2008), technological progress in the household sector (Greenwood and Guner, 2008; Greenwood et al., 2016), access to improved contraception (Michael, 1978), and the expansion in legal services (Cunningham and Goodman-Bacon, 2025). One of the most widely-discussed factors is the role of unilateral divorce laws. From the late-1960s through the 1970s, the "no-fault revolution" swept through America as states began to abandon fault-based systems, passing unilateral divorce laws that allowed people to seek a divorce without spousal consent. Friedberg (1998) finds that these laws explain a sixth of the rise in divorce since the 1960s. Wolfers (2006), however, contends that the laws have little impact once the dynamic response of divorce is accounted for. Lee and Solon (2011), in turn, show that Wolfers's (2006) results are sensitive to the method of estimation and functional form. Another line of inquiry looks at the more recent decline in divorce after 1980 (Figure 1A). Possible explanations here include the changing selectivity of marriage (Kennedy and Ruggles, 2014), the rise in age at marriage (Rotz, 2016), Medicaid expansion (Slusky and Ginther, 2021), and the liberalization of divorce laws (Mechoulan, 2006; Rasul, 2006). Amidst the ongoing debate over what caused the rise and fall in divorce, the salience of geography has largely been neglected. This paper highlights the initial persistence and subsequent shift in the geographic patterns of divorce, elucidating the forces at work in each phase.

My work also adds to studies examining the impact of broad economic shocks on labor market and social outcomes in the US. The Great Depression lowered intergenerational income mobility (Feigenbaum, 2015). China's ascension to the World Trade Organization (WTO) led to higher

unemployment, lower labor force participation, reduced wages, and diminished marriage market value for young men in America (Autor et al., 2013, 2019). Progress in computing technology adversely affected workers performing routine job tasks, polarizing the labor market (Autor and Dorn, 2013). More recently, the emergence of Generative AI has been a double-edged sword, improving wages for younger and more educated workers, but harming employment opportunities in sectors where AI can directly substitute for human labor (Johnston and Makridis, 2025). I investigate how the change in wage structure brought about a shift in the geography of divorce, a dynamic that has not been explored.

2 Data

To document the geography of divorce over the long-run, one requires divorce statistics (i) at the local level, (ii) across several decades, and (iii) for a consistently-defined population. This section describes the available data.

2.1 Administrative Sources

Given the salience of divorce in the public discourse, US government agencies have been collecting local divorce statistics since the 19th century.[3] It began in 1887, when Congress directed the Commissioner of Labor to compile retrospective divorce figures for 1867-1886. The resulting county data were published in *A Report on Marriage and Divorce in the United States, 1867 to 1886*. In 1905, another resolution was passed, authorizing the Director of the Census to collate divorce counts from 1887-1906. The county figures from this exercise were released in *Marriage and Divorce, 1887-1906*. A third attempt to assemble divorce statistics was scheduled for 1917, with plans to cover 1907-1916. However, because the US entered WWI in 1917, only data for 1916 could be gathered. These appeared in *Marriage and Divorce, 1916*. After WWI, local divorce statistics were reported annually in the Census Bureau's *Marriage and Divorce* pamphlets. This lasted from 1922-1932, before the Great Depression and WWII led to a pause. Data collection then resumed after WWII, this time spearheaded by the National Office of Vital Statistics. Under the new agency, county divorce numbers were published in the *Vital Statistics of the United States* volumes until 1988.

While administrative divorce data are rich in geographic fineness (county-level) and time span (over a century from 1867-1988), they are not stratified by race, nativity, or age. This matters for three reasons. First, divorce rates vary systematically across demographic groups—these associations can even change over time (Figures A.2 and A.3). For example, divorce was substantially more prevalent among blacks than whites at mid-century, but that gap shrank in later decades (Figures A.2A and B). Second, each demographic group clusters in different parts of the country (Figure A.4). For instance, foreign-born persons tend to settle in the coastal areas (Figure A.4B). Third, throughout history, the US has experienced large movements of people both *within* the country, such as the Great Migration out of the South (Bazzi et al., 2023a; Boustan, 2010; Derenoncourt,

[3]This section draws on the historical account in National Center for Health Statistics (1973).

2022), and *into* the country, such as the post-1965 rise in immigration (Abramitzky and Boustan, 2017). These three patterns imply that without demographic stratification, any observed shift in the geography of divorce could simply reflect changes in the local population composition. This complicates attempts to interpret the spatial patterns of divorce over the long-run. I thus rely on administrative divorce data for supplementary exploration, but not for the main investigation.

2.2 Census and ACS

My core analysis draws on US census and ACS data available from IPUMS (Ruggles et al., 2024, 2025). These report a wide range of socioeconomic characteristics for each respondent, making it possible to isolate a consistent demographic group over time, thereby circumventing the limitations of administrative divorce statistics. To minimize complications from local compositional shifts whilst maximizing the geographic scope for examination, my base population will comprise US-born non-hispanic white women aged 25-50 in each census/ACS.

The census/ACS data also allow one to determine if a person had gone through a divorce. These surveys record an individual's current marital status, which can be used to identify divorcees at a *given point in time*. Since a majority of divorcees later remarry (Stevenson and Wolfers, 2007; Wolfers, 2006), this measure alone will understate the true prevalence of divorce. Fortunately, in several years, the census/ACS enumerations posed a further question to ever-married persons: how many times have they been married? From this, one can infer whether a person has remarried and, by extension, gone through a divorce. This additional information is available for sample-line females in the 1940-1950 censuses as well as for all respondents in the 1960-1980 censuses and the 2008 ACS onward (Table A.1).[4] In practice, I use the full count censuses for 1940-1950 and rely on the IPUMS samples for the remaining years.[5] I end my study period in 2018 to avoid any distortions from the Covid-19 pandemic. My main analysis thus spans 1940-1980 and 2008-2018.

2.3 Defining Divorce

With the necessary data, I can then define my measure of divorce. I identify individuals (i) who are divorced in a given survey year or (ii) who are married, separated, or widowed at the time but have been married more than once—together, they comprise the *ever-divorced* population (Stevenson and Wolfers, 2007; Wolfers, 2006).[6] Figure A.5 breaks down the ever-divorced population by current

[4]Sample-line respondents are those whose names are recorded on specific lines of the census form—they are asked additional questions. In the 1940 census, two lines on each 40-line census page are designated as sample lines, comprising 5 percent of the population. In the 1950 census, every fifth line on a 30-line census page is designated a sample line but only the last sample-line individual on the page is asked about marital history, making up 3.3 percent of the population.

[5]The 1910 census also asked about the number of times married. However, this information is only available in the 1 percent sample but not the full count, leaving a much smaller dataset to work with compared to later decades. The different variables in the sample and full count reflect the different transcribing organizations. The 1 percent sample was transcribed by social scientists at the University of Pennsylvania and the University of Michigan. In contrast, the full count was transcribed by Ancestry.com, a genealogy platform.

[6]The ever-divorced measure implicitly assumes that currently married, separated, or widowed persons who have been married more than once were previously divorced rather than widowed. Wolfers (2006) demonstrates that this

marital status—some 50 to 76 percent of ever-divorced women have remarried, with the proportion increasing as one goes further back in time.[7] Current marital status alone would thus understate the extent of divorce substantially, underscoring the importance of using information on *both* current marital status and the number of times married.

Figure 1B plots the share of ever-divorced women since 1940 for both the general population (solid line) and for whites (dashed line).[8] I observe a rise in ever-divorced rates over the 20th century, similar to the more conventional measure of new divorces depicted in Figure 1A. These rates then decline in the 21st century, somewhat lagging behind the corresponding reversal in new divorces. The difference in timing is a mechanical artifact of the two divorce concepts. My ever-divorced metric captures the *stock* of divorcees whereas new divorces represent a *flow*—changes in the latter take time to be reflected in the former, as the stock is an accumulation of both current and past flows.[9]

Finally, I aggregate the ever-divorced counts from the census/ACS microdata to the local level. The geographic detail in each census/ACS year varies, ranging from counties to county groups to Public Use Microdata Areas (PUMAs), among others. To facilitate intertemporal comparisons, I match the respective units in each year to the corresponding Commuting Zones (CZs), clusters of counties with strong commuting ties (Autor and Dorn, 2013; Tolbert and Sizer, 1996).[10] For greater accuracy, I restrict my analysis to the set of CZs with at least 50 sample-line persons from the base population in the 1940 full count. This leaves a baseline sample of 679 CZs. I then compute the share of women in each CZ who have ever been divorced.

3 The Geography of Divorce

Figure 2 maps the geography of divorce from 1940-2018.[11] Two distinct phases can be observed. Between 1940-1980, higher rates of divorce are observed in the West, a wide stretch from the industrial Midwest southwestward to Texas, as well as in Florida and to a lesser extent in Maine.[12] By the 21st century, however, a different pattern emerges, with a greater concentration of divorce

is a reasonable assumption, using data from the June 1995 Current Population Survey.

[7] The "separated" category was only introduced in 1950; before that, persons in this group would likely have been counted under "married, spouse absent" (Ruggles et al., 2025).

[8] Stevenson and Wolfers (2007) also plot the ever-divorced trend, but their series ends in 2001 and is based on a mix of data rather than a consistent source. Specifically, they use census data for 1910 and 1940-1980, Current Population Survey data for 1990 and 1995, as well as Survey of Income and Program Participation data for 2001.

[9] Figure A.6 offers supporting evidence for this explanation by splitting the ever-divorced trend by age group. I find an earlier decline in ever-divorced rates for younger ages, those less affected by the cumulative effect.

[10] The conversion to CZs is carried out in three steps. First, I map counties in the 1940 and 1950 full count to CZs by adapting the county-to-CZ correspondence in Chetty and Hendren (2018). Second, I assign PUMAs in the 1960 sample to CZs using the crosswalks developed by Evan Rose. Third, I match county groups in the 1970 and 1980 samples as well as PUMAs from the 2008-2018 ACS to the respective CZs using the crosswalks from Autor and Dorn (2013). In all cases, I use the 1990 vintage of CZ definitions.

[11] For parsimony, I display the maps for 1940-1980, 2008, and 2018. The subsequent analysis will utilize the full range of data between 2008-2018.

[12] The patterns in 1950 appear somewhat noisier. This could reflect the smaller underlying sample (sample-line respondents comprise 3.3 percent of the population) or the large spike in divorces at the close of WWII (Figure 1A), which might have temporarily distorted the spatial patterns of divorce.

in the South.[13]

To capture the initial persistence and subsequent change in divorce landscape, I compute the rank-rank correlation between the spatial patterns of divorce in 1940 and the corresponding distribution in later census/ACS years.[14] Figure 3 plots these correlations. The associations are relatively high between 1940-1980, typically over 0.7—this indicates substantial persistence in the geography of divorce, consistent with Figure 2.[15] Moving into the 21st century, the rank-rank correlations drop sharply to around 0.2, pointing to a sizable shift in geographic patterns as depicted in the maps above. This theme of initial persistence and subsequent change is robust to alternative permutations: using *ever-married* women as the base population instead of all women to account for spatial and temporal changes in marriage patterns (Figure A.7A), distinguishing between younger and older age groups to check for age effects (Figures A.7B and C), focusing on those residing in their birth states to limit the influence of internal migration (Figure A.7D), and doing a similar analysis for men in the subset of years where ever-divorced status can be determined for both genders (Figure A.7E).[16]

Could the persistence in geographic patterns observed between 1940-1980 go even further back in time? To shed light on this, I supplement my main analysis with administrative county divorce statistics (Section 2.1). Unlike the census/ACS data, these records pool all demographic groups together and reflect the flow of new divorces rather than the stock of divorcees. Nonetheless, they have the advantage of time span, going back to 1867. Because the share of counties with missing information rises sharply the earlier one goes (Figure A.8), I focus on the period when around 80 percent of more counties had valid information, thereby maintaining a broad geographic scope. In practice, this restricts my analysis to 1900-1932. I digitize county administrative data for 1900-1906 from Bureau of the Census (1908) while data for 1916 and 1922-1932 come from BGSU Marriage and Divorce Data Compass (2018). Where possible, I aggregate the divorce counts by decade to reduce the influence of year-to-year fluctuations and for parsimony: the 1900s (1900-1906), 1910s (1916), 1920s (1922-1929), and 1930s (1930-1932). I then match the county divorce statistics to CZs and scale them by the respective population of women.[17] Figure 4 presents the results. Mirroring the patterns between 1940-1980 (Figure 2), I find that divorce in the 1900s-1930s was more prevalent in the West, the stretch from the industrial Midwest to Texas, as well as in Florida and Maine.

[13] Divorce rates in Nevada remain elevated in both the 20th and 21st centuries.

[14] Rank-rank correlations are less susceptible to year-to-year fluctuations in the variance of divorce, whereas simple correlations are directly affected by the standard deviation of divorce rates in the years being compared.

[15] As alluded to previously, the lower rank-rank correlation with 1950 could reflect the greater noise from having a smaller sample that year or the divorce spike after WWII.

[16] I make three points here. First, for each permutation, I restrict the sample to CZs with at least 50 sample-line persons in the respective base population as measured in 1940 (or 1960 in the case of men). Second, when looking at the state of residence and birth, I exclude 1970. My original analysis uses the metro files of the 1970 census, which can be matched to CZs but do not report the state of residence for all persons—only five states have completely identifiable records (Ruggles et al., 2025). Third, for men, the earliest census with information on the number of times married is the 1960 enumeration (Table A.1). My starting point for this permutation is thus a comparison between 1960 and 1970.

[17] To obtain the denominator for scaling (number of women in each CZ and period), I map county census data from Manson et al. (2023) to the CZ level, linearly interpolate these decadal data, and then aggregate the figures for each decade interval: the 1900s (1900-1906), 1910s (1916), 1920s (1922-1929), and 1930s (1930-1932).

The spatial patterns of divorce thus persisted for almost a century from 1900-1980, before changing dramatically thereafter. The rest of this paper attempts to explain the initial persistence and subsequent change in the geography of divorce.

4 Before 1980: Persistence

What explains the persistence in divorce patterns before 1980? This section explores three reinforcing mechanisms. The first is selective migration, where divorcees or persons with a greater propensity for divorce are attracted to locations that have higher divorce rates. The second are social interactions, where living close to divorcees influences a person's behavior, increasing the likelihood of divorce. The third concerns intergenerational transmission, where children of divorced parents are themselves more likely to go through a divorce later in life—places with more divorce in the parent's generation may thus be where divorce is more common in the child's generation. I examine each reinforcing mechanism in turn, finding evidence for all three.

4.1 Selective Migration

To examine the role of selective migration, information on a person's origin, destination, and pre-move characteristics is needed. One can then check if individuals who had previously gone through a divorce are more likely to move to high-divorce areas or less likely to leave these places. Such patterns of selection would reinforce the existing landscape of divorce.

4.1.1 Linked Sample

My analysis draws on the 1940 and 1950 full count censuses. These record the names of all individuals, allowing me to link each person across the two censuses in order to track their movement over time. This is not possible after 1950 as the later full counts have not been released.[18] In addition, none of the full counts before 1940 have information on the number of times married, which is key to identifying those who have ever been divorced. Hence the focus on 1940 and 1950.

From the full count data, I build my baseline sample in two steps. First, I define the population of interest in 1940: sample-line US-born non-hispanic white women aged 25-40, some of whom have gone through a divorce while others have not (pre-move characteristics).[19] Second, I link these individuals forward to 1950, using the census linkages from the IPUMS Multigenerational

[18] The census full counts, which contain individual names, are released 72 years after a given enumeration. At the time of writing, the 1950 full counts are thus the latest that are available. After 1950, only census samples are available—these do not include names. An alternative approach that does not involve linking is to exploit differences in the birth and current state of respondents to identify internal migrants. However, this approach is less precise, both in terms of the geographic unit across which moves occur (states instead of CZs) and timing (whether one moved during childhood or adulthood).

[19] As noted above, only sample-line respondents in the 1940 census are asked about the number of times they have been married—combining this information with current marital status identifies the pool of ever-divorced.

Longitudinal Panel (MLP) (Helgertz et al., 2024).[20] Comparing one's location of residence in 1940 versus 1950 allows me to distinguish between stayers (same CZ) and migrants (different CZ).

4.1.2 Approach

I then split my 1940-1950 linked sample based on where individuals lived in 1940. The first group comprises those who started in *low-divorce* CZs—by 1950, some will still be in the same CZ (stayers) while others would have left for *high-divorce* CZs (migrants).[21] The second group consists of persons who began in *high-divorce* CZs—come 1950, a number will remain in the same CZ (stayers) while their counterparts would have moved to *low-divorce* CZs (migrants).[22] These classifications require a precise definition of low- and high-divorce CZs. In theory, the definition could be anchored to the 1940 or 1950 census, since both years have the information needed to identify ever-divorced persons.[23] In practice, the geographic patterns of divorce in 1940 and 1950 do not overlap perfectly (Figures 2 and 3)—the choice of year thus matters.[24] Here, I proceed as follows. Ranking CZs based on the ever-divorce rate in each year, I define (i) low-divorce CZs as those with below-median ever-divorced rates in *both* 1940 and 1950, while (ii) high-divorce areas have above-median rates in *both* periods.[25] By this definition, low- and high-divorce CZs each comprise about 27 percent of the CZ sample. Figure A.9 maps these areas. Mirroring Figure 2, low-divorce CZs tend to be found in the non-industrial Midwest while high-divorce CZs are typically located in California as well as the stretch from the industrial Midwest down to Texas.

Having described my setup, I then compare the pre-move (1940) ever-divorced status of migrants and stayers:

$$y_i = \alpha + \gamma \; Migrant_i + FE + u_i \tag{1}$$

where i denotes an individual. The outcome, y, is an indicator for those who were ever divorced, as measured in 1940. The explanatory variable, $Migrant$, is a dummy that takes the value 1 for persons who left their initial CZ for a CZ of the opposite type by 1950 (low- to high-divorce CZs; high- to low-divorce CZs).[26] Also included are cohort and the initial (1940) CZ fixed effects, FE.

[20] To construct the census links in the IPUMS MLP, Helgertz et al. (2024) use a three-step probabilistic procedure with supervised machine learning. First, they link individuals across a pair of censuses. Second, they exploit the links from the previous step to match any remaining household members. Third, they search for single women who married and changed their names between censuses.

[21] A third group, those who left their initial low-divorce CZs for other low-divorce CZs, is excluded from my sample. This mirrors the setup in Bazzi et al. (2020), who study migrants to and from frontier counties.

[22] Similarly, those who left their initial high-divorce CZs for other high-divorce CZs are dropped from this sample.

[23] A fixed definition must be applied to both origins and destinations. Mixed definitions are difficult to interpret. For example: those who moved from a low-divorce CZ based on the 1940 definition to a destination CZ that would be considered high-divorce in 1940 but low-divorce in 1950—it is unclear what direction of selection this should be classified as. I thus avoid anchoring origins and destinations to different years.

[24] Ideally, a 1940 definition should be applied to those who moved around 1940 while a 1950 definition would suit those that migrated near 1950. The timing of move, however, is not readily available in historical censuses.

[25] To reduce the mechanical influence of internal migration, I base the CZ ever-divorced rates on US-born non-hispanic white women aged 25-40 in 1940 (35-50 in 1950) who can be linked between 1940-1950 and that *stayed* in the same CZ between 1940-1950. For greater accuracy, only CZs with at least 50 of such stayers are used.

[26] As noted previously, those who moved to a CZ of the same type (low- to low-divorce CZs; high- to high-divorce CZs) are excluded from the analysis.

4.1.3 Evidence

I find evidence that selective migration helped sustain the geographic patterns of divorce at mid-century. Table 1 presents the γ estimates from regression (1). Columns 1 and 4 display the results without fixed effects, columns 2 and 5 include cohort fixed effects, while columns 3 and 6 further add initial CZ fixed effects. Consider first the group that started from low-divorce CZs in 1940 (columns 1-3). Positive and statistically significant point estimates are observed. In words, individuals who had previously experienced a divorce are more likely to move to a high-divorce CZ. The difference of just under 2 percentage points is sizable at about 33 to 37 percent of the mean divorce rate. Next, I look at the group that originated from high-divorce CZs in 1940 (columns 4-6). Negative and significant estimates are found. That is, those who had gone through a divorce are less likely to leave a high-divorce CZ for a low-divorce CZ. Again, the selection effects are relatively large, with the 2-percentage-point gap translating to 16 to 26 percent of the mean divorce rate. Under both scenarios, the direction of selection would reinforce the existing geography of divorce.

4.2 Social Interactions

Beyond selective migration, does living in a place where divorce is more/less prevalent also change a person's behavior, specifically her propensity for marital dissolution? One way this could happen is through social interactions—interpersonal contact between individuals. At a time when modern communication devices were not available, social interactions among neighbors may have been particularly salient. A growing economics literature highlights the pervasive influence of social interactions at the neighborhood level. Such dynamics can shape a person's culture (Bazzi et al., 2025), political behavior (Brown et al., 2021), labor market outcomes (Hellerstein et al., 2011, 2014; Schmutte, 2015), rate of naturalization (Biavaschi et al., 2022), and attitude toward foreigners (Bursztyn et al., 2024). What about divorce? Intuitively, having a neighbor that has gone through a divorce could alter an individual's own divorce propensity via peer influence (e.g. divorcees encouraging their neighbors to leave abusive partners) or greater awareness (e.g. learning about prevailing divorce legislation and options).[27] Studying Framingham in Massachusetts, McDermott et al. (2013) find that divorce can spread between friends, extending to two degrees of separation in a network. However, aside from such descriptive accounts, the nexus of social interactions and divorce remains largely unexplored. This section examines if neighbor interactions have a causal impact on divorce risk.

[27] Could differences in stigmatization be another way that social interactions operate? For example, the presence of more divorcees in a neighborhood might reduce the stigma of divorce, lowering the barriers to divorce for others in the area. While I do not test for this channel directly, the social interactions effects derived in this section are unlikely to reflect changes in stigmatization. This is because my approach utilizes small geographic spaces—the degree of stigmatization may not differ significantly between neighbors living nearby versus those living slightly further apart. Importantly, this does not imply that stigmatization did not play a role, only that my analysis is not designed to tease out such effects.

4.2.1 Research Design

A key challenge to identifying social interaction effects at the neighborhood level is the non-random sorting of residents. That is, instead of people in the same area influencing each other and thus having similar outcomes, it may well be that those with similar outcomes choose to reside in the same place. To address this, I adopt the spatial proximity research design pioneered by Bayer et al. (2008). The idea is to compare the similarity of outcomes among neighbors living close by *relative* to the similarity of outcomes for neighbors living slightly further apart. By focusing on small geographic spaces, this setup narrows the scope for residential sorting, allowing one to isolate the impact of social interactions. Implementing such an approach to study divorce in historical America requires information on (i) a person's ever-divorced status (outcome) and (ii) residential proximity between neighbors. Again, only the 1940 and 1950 full count censuses have the necessary data.[28] For consistency with the previous sections, I continue to focus on sample-line US-born non-hispanic white women aged 25-50 in these enumerations.

To operationalize the spatial proximity research design, I start by defining two levels of geography: (i) small *local areas* that capture neighbors living close by and (ii) broader *neighborhood reference groups* comprising several nearby local areas that to approximate neighbors living slightly further apart. The choice of local areas is constrained by the geographic detail available in historical censuses. Bayer et al. (2008) use census blocks for their contemporary analysis, but these did not exist historically.[29] Instead, I turn to enumeration districts (EDs), the smallest administrative units in historical censuses that are small enough to be be surveyed by a single enumerator on enumeration day. I use a single ED to define the local area of residence.[30] Next, I delineate the larger neighborhood reference group—how many local areas constitute a neighborhood? The more local areas I include, the more precise my regression estimates, but the greater the margin for sorting. As a baseline, I use three nearby EDs to define a neighborhood reference group, where "nearby" is determined by consecutive ED numberings.[31] Figure A.11 shows that EDs with similar numberings tend to be closer together in space.[32] Finally, to avoid potential complications from racial segregation or clustering, I restrict my analysis to neighborhoods with no black residents.

The two levels of geography allow me to estimate social interaction effects among neighbors.

[28] By construction, census samples cannot be used to identify neighbors. Hence the need for full count data.

[29] Given this limitation, the economic history literature has developed other means of identifying neighbors. A common approach is to exploit how censuses were taken in the past. Before 1960, census enumerators went door-to-door surveying households in the order with which their dwelling units were physically situated (Logan and Parman, 2017). This setup allows researchers to infer next-door neighbors from consecutive lines on a census page (Bazzi et al., 2025; Brown et al., 2021) or those who live close by as proxied by a single census page (Tan, 2022). Such methods are not feasible here as my analysis only includes sample-line respondents, leaving large gaps in the spatial continuum of residential locations. Furthermore, each page in the 1940 (1950) census has at most two (one) sample-line respondents, too few to operationalize the spatial proximity research design.

[30] Figures A.10A and B show the distribution of the number of individuals residing in a given local area (ED), for the full and base populations.

[31] Figures A.10C and D display the distribution of people across neighborhood reference groups (three nearby EDs), for the full and base populations.

[32] I check this for 11 US cities from 1900-1930, using the ED maps digitized by Shertzer et al. (2016). These data allow me to explore how (i) differences in ED numbering relate to (ii) the spatial distance between EDs. Figure A.11 shows a positive association, indicating that EDs with similar numberings are also closer in space.

Specifically, I examine the likelihood that a pair of neighbors have both experienced a divorce, comparing neighbor-pairs that reside in the same local area (ED) versus pairs in nearby local areas of the same neighborhood reference group (three nearby EDs). This is implemented with the following regression:

$$y_{ii'n} = \Omega \; SameArea_{ii'n} + \eta_n + v_{ii'n} \tag{2}$$

where i and i' denote a pair of individuals from different households living in the same neighborhood reference group n. The outcome, y, is a dummy that takes the value 1 if both members of a neighbor-pair have ever been divorced. The main explanatory variable, $SameArea$, is an indicator for pairs residing in the same local area. Finally, η is a vector of neighborhood reference group fixed effects, designed to control for any correlation in attributes among individuals in the same reference group. A positive and significant Ω coefficient can be interpreted as the impact of social interactions, under the assumption that residents do not sort across local areas within narrowly-defined reference groups.

4.2.2 Identifying Assumption

Do residents sort across local areas in a given reference group? For this to happen, the search period of buyers/renters looking for a specific unit type in a particular area must overlap with when sellers/landlords of those units are selling/renting—this match becomes less likely the smaller the geographic space due to the limited options available. Bayer et al. (2008) note that housing markets tend to be relatively thin at small geographic scales. This suggests that individuals may not have complete control over where they live within narrowly-defined neighborhoods. That is, they might be able to choose the broader neighborhood, but which local area they settle in within that neighborhood depends on the few options they have at the time. The identifying assumption thus appears plausible. In fact, an even stronger case can be made in the context of divorce: it is much easier for people to pick a local area based on the observable characteristics of neighbors than to do so on the basis of more private matters like divorce status.

Empirically, I assess the identifying assumption by checking for any systematic associations between the characteristics of individuals and their neighbors, following Bayer et al. (2008). This exercise proceeds in three steps. First, taking my population of interest, I draw a random person from each local area (ED). Second, I use the remaining individuals in the local area who are not from the same household as the random person to measure the average characteristics of an area. Third, I compare the attributes of the randomly selected person with the average characteristics of her neighbors, keeping the analysis to local areas with at least five people excluding the randomly chosen person and members of her household. Figure 5 shows the resulting correlations across a range of socioeconomic traits, unconditional (grey diamond markers) and with neighborhood reference group (three nearby EDs) fixed effects (black circle markers). Substantial residential sorting is observed, but much of this occurs between rather than within reference groups—the

correlations are much smaller (and low) with reference group fixed effects.[33] This indicates limited sorting within reference groups, giving weight to the identifying assumption.

4.2.3 Evidence

I find evidence that social interactions helped reinforce the existing geography of divorce in mid-century America. Columns 1 and 5 of Table 2 present the Ω estimates from regression (3), based on the 1940 and 1950 full count censuses, respectively. Positive and significant effects are observed. The odds that a pair of neighbors have both experienced a divorce is 0.2 percentage points higher among pairs in the same local area (ED) compared with pairs living in different local areas within the same neighborhood reference group (three nearby EDs). These differences are sizable at 11 to 13 percent of the mean ever-divorced rate.[34] In words, living close to people who have gone through a divorce increases the likelihood of a person ending a marriage. Extending these results from neighborhoods to CZs, the cumulative impact of such social interactions could be greater in CZs with more divorcees to begin with, strengthening the prevailing patterns of divorce.

To probe the validity of my findings, I conduct a suite of robustness checks. First, I address the residual correlation between various individual and neighbor traits (Figure 5), even though these correlations are already low within neighborhood reference groups. Specifically, I add individual fixed effects to regression (2) for each member of a neighbor-pair, following the approach in Bayer et al. (2008) and Schmutte (2015).[35] These further control for sorting on observable and unobservable attributes. While this specification is demanding on the underlying data, similar results can be obtained (columns 2 and 6, Table 2). Second, I hone in on situations where it may be more difficult for people with similar characteristics to sort across local areas in a neighborhood and co-locate, settings in which the identifying assumption may be more plausible. Here, I restrict my sample to neighbor-pairs where one person has lived in the same house for at least 5 years while the other was from a different county 5 years ago, based on retrospective 5-year migration details available in the 1940 full count. As alluded to previously, the location choices of recent arrivals are partly constrained by the local supply of housing units available for sale/rent at the point in which they enter a neighborhood—households may not be able to select their preferred local areas if their search period does not overlap with when sellers/tenants in those areas move out. Such restrictions may be even more salient when looking at narrow geographic areas, as is the case here. This exercise leaves me with under 5 percent of the original sample. Nonetheless, I continue to find a positive and significant coefficient (column 3, Table 2). I can go even further by honing in on neighbor-pairs where *both* members were residing in a different county 5 years ago, constraining the

[33] The implicit assumption here is that the non-zero correlations reflect the failure of the research design to account for sorting. An alternative explanation is that the grouping of EDs to form neighborhood reference groups has some error. As noted previously, these groups are constructed from EDs with adjacent numberings—these EDs are closer in space on average but not all the time. Consequently, not every grouping of three EDs reflects adjacent spaces, increasing the scope for selection.

[34] The mean ever-divorced rate is measured using resident pairs in different local areas of a given neighborhood reference group.

[35] Identification is possible because each person appears in more than one neighbor-pair.

location choices of both members rather than just one. Again, I find evidence of social interaction effects (column 4, Table 2).

4.3 Intergenerational Transmission

Besides selective migration and social interactions, a third reinforcing mechanism that could explain the persistence in geographic patterns before 1980 is the intergenerational transmission of divorce risk. Such vertical transmission might come through the passing down of particular traits or socioeconomic circumstances which are associated with divorce. In the presence of such intergenerational transmission, places where divorce is more prevalent in one generation may also display higher divorce rates in the next generation, reinforcing the existing patterns of divorce.[36] Contemporary studies have documented intergenerational links in divorce risk (Amato, 1996; Gaspard and Clifford, 2016; Wolfinger, 2005)—was this also the case historically?

4.3.1 Data and Empirics

To shed light on the intergenerational transmission of divorce risk in historical America, information on (i) the parent's marital status when the child was still residing with them and (ii) the child's own marital status by adulthood is required. This necessitates the use of historical linked data. Here, I link US-born non-Hispanic white girls aged 0-18 in the 1910 census to their older selves in 1940, restricting the analysis to sample-line respondents in the end year.

Three features of the 1910-1940 linked sample are worth noting. First, the end year (1940) contains the details needed to determine a child's ever-divorced status in adulthood. Second, the base year (1910) reports a parent's current divorce status, but not ever-divorced status. This limitation is unlikely to pose a major problem here. Given the aim of identifying children who were exposed to divorced parents, rather than children born after their parents had remarried, the current divorce status of parents offers greater certainty that a child did live with a parent who was still divorced. Third, I match individuals between 1910 and 1940 using the links created by the Census Tree Project (Price et al., 2023), instead of those from the IPUMS MLP (Helgertz et al., 2024). The Census Tree Project provides the largest collection of historical census links available, for all pairwise combinations of historical full count censuses—size is particularly salient here as my sample has already been reduced by the restriction to sample-line respondents in the end year and the lower match rates over longer durations (30-year interval).[37] However, the Project does not yet cover the 1950 census.[38] Hence the use of a 1910-1940 linked sample over a 1920-1950 dataset.

Having assembled the necessary data, I then compare the adult outcomes of daughters who

[36] This assumes that a majority of individuals remain in their childhood locations into adulthood. Based on the linked sample of daughters used in the analysis that follows, I calculate that almost 60 percent stayed in the same CZ between 1910 and 1940.

[37] For my base population of interest, links from the Census Tree Project produce samples that are 10 times as large compared with the IPUMS MLP Helgertz et al.'s (2024) links.

[38] Hence the use of Helgertz et al.'s (2024) links when combining the 1940 and 1950 censuses to study selective migration (Section 4.1).

grew up with and without a divorced parent, using the regression:

$$y_{ih}^{child} = \alpha + \rho\ y_{h}^{parent} + \text{FE} + u_{ih} \tag{3}$$

where the subscripts i and h denote an individual and household. The outcome, y, is an indicator for daughters who had gone through a divorce by 1940 (based on current marital status and number of times married). The main explanatory variable is a dummy for those whose parents were still divorced in 1910 (based on current marital status only). Fixed effects for the child's cohort, the parent's cohort, and the CZ of residence in 1910 are also included in FE. The parameter of interest ρ captures the intergenerational transmission of divorce risk.

4.3.2 Evidence

I find evidence of a divorce cycle even in historical America. Table 3 presents the ρ estimates from regression (3), separately for daughters residing with their mothers (column 1) or their fathers (column 2) in the base year. The corresponding treated groups are daughters living with divorced mothers (column 1) or divorced fathers (column 2).[39] I find positive and statistically significant associations between the divorce risk of parents and children. Daughters with divorced parents in 1910 were 6.2 to 7.9 percentage points more likely to go through a divorce themselves by 1940. The observed associations are sizable, comparable to the mean ever-divorced rate. Similar results can be obtained when restricting the base population to daughters who were ever-married by 1940 (Table A.2). My findings here suggest that the intergenerational transmission of divorce risk helped strengthen existing geographic patterns of divorce.

5 After 1980: Times of Change

Even as divorce rates climbed sharply from the 1960s to 1980, the underlying geography of divorce held steady, exhibiting a persistent pattern that traces back to the dawn of modern US history; yet the decline in divorce after 1980 was accompanied by a shift in the landscape of divorce (Figures 1-3)—what caused the change in geographic patterns? This section proposes that broader economic forces have a role to play, particularly the change in wage structure.

5.1 Timing

To narrow the set of possible explanations for the change in spatial patterns, it is useful to know when the geography of divorce began to evolve. The census/ACS data in Sections 2 and 3 allowed me to track the patterns of divorce from 1940-1980 and 2008-2018, leaving a 30-year gap in-between. It is thus unclear if the changes observed between 1940-1980 and 2008-2018 are a recent phenomenon (the 2000s) or an earlier occurrence (the 1980s).

[39] There is some overlap in the control group used for both cases, as daughters whose parents are not divorced are likely to be residing with both mother and father.

Here, I turn to Current Population Survey (CPS) data available from Flood et al. (2025). Between 1978-2009, the CPS regularly asked ever-divorced respondents about the year of most recent divorce or separation, making it possible to infer the ever-divorced population. These data fill the time gap in the census/ACS series. For greater comparability with the preceding analysis, I restrict the CPS data to 25-50-year-old non-Hispanic white women. From 1993 onward, the sample can be further narrowed to US-born persons, fully consistent with the previous sections. A caveat here is that the CPS microdata can only be aggregated to the state rather than the CZ level. I thus reconstruct the geography of divorce at the state level using census/ACS data for 1940-1980 and 2008-2018, along with state-level CPS estimates from 1978-2009.

Figure 6 shows the rank-rank correlations between the patterns of divorce across states in 1940 (census-based) and the corresponding distribution in later years, the latter of which come from the census/ACS (solid line) or CPS (dashed lines). I find that the shift in the geography of divorce was no recent event. Instead, the landscape of divorce was changing throughout the 1980s to 2000s. The forces driving this shift must thus have emerged in the 1980s, lasting several decades thereafter.

5.2 Changes in Wage Structure

Given the timing of change in geographic patterns, could fundamental shifts in the wage structure be a potential driver? Changes in the college wage premium are often used to trace developments in the wage structure (Acemoglu and Autor, 2011; Katz and Murphy, 1992). Drawing on CPS data from 1963-2018, I regress the log annual wages of individuals on an indicator for those with a 4-year college degree or higher, controlling for potential experience—the coefficient on the college variable captures the college wage premium. To minimize any bias induced by selection into the workforce, I focus on white male full-time full-year wage and salary workers aged 25-50.[40] Figure 7A traces the college wage premium over time. It rose in the 1960s, fell in the 1970s, and then surged from the 1980s, as others have documented (Acemoglu and Autor, 2011; Katz and Murphy, 1992). Decomposing this aggregate trend, Figure 7B shows that the post-1980 trajectory reflects a combination of growing real earnings among college graduates and falling real earnings for those without a college degree, a pattern previously noted by Acemoglu and Autor (2011).[41] Skilled workers have thus pulled ahead since the 1980s while the less-skilled face worsening economic circumstances. Importantly, this evolution in wage structure coincides with when the geography of divorce began to transform (Figure 6).

Why might a change in wage structure affect divorce? On average, college graduates are less likely to be divorced (Isen and Stevenson, 2013; Neeman et al., 2008). I refer to this as the "college divorce discount". A growing literature in economics reveals how the erosion of economic status

[40] I make two point here. The first relates to the data. I use the 1964-2019 March Annual Demographic Files of the CPS, which correspond to the years 1963-2018. The CPS provides the longest high-frequency data series enumerating labor force participation and earnings. CPS sample weights are used for all computations. The second pertains to my sample. I define full-time full-year workers as those who worked at least 40 weeks in the previous year and at least 35 hours in the previous week. Only workers between the 5th and 95th percentiles of the wage distribution are kept, as earnings outside this range are less accurately measured (Acemoglu and Autor, 2011).

[41] For consistency with Figure 7A, I residualize the log real wages here by potential experience.

increases the risk of divorce (Banzhaf, 2018; Charles and Stephens, 2004; Jackson et al., 2017). Consequently, as the wage structure changes in a way that disadvantages the low-skilled, one would expect an increasingly negative association between skill level and divorce status. That is, a widening of the college divorce discount. Exploring the college-divorce link over time is complicated by the changing association between college and marriage or remarriage as well (Figure A.13). To make progress, I proceed in three steps. First, drawing on census/ACS data, I begin by tracing the college ever-divorced discount for my base population, documenting a downward trend (Figure 8A). Second, to account for changes in the college-marriage nexus, I repeat the exercise for ever-married women; similar patterns are observed, with slightly more pronounced changes after 1980 (Figure 8B). Third, to address the evolving link between college and remarriage, I redo the analysis using current divorce status instead of ever-divorce status—a sharp decline is now evident after 1980 (Figure 8C). All three cases depict a growing college divorce discount, with the sharpest changes found in current divorce status.

The increasing association between skill level and divorce has geographic implications. As these dynamics play out, parts of the US with relatively more (less) college graduates will experience greater declines (increases) in divorce. If these forces are sufficiently strong, offsetting the reinforcing mechanisms of selective migration, social interactions, and intergenerational transmission, the geography of divorce will begin to change. Specifically, the landscape of divorce will reorientate around places with fewer college graduates. Figure A.14 offers preliminary evidence for this by mapping the share of college graduates across CZs in 1980, prior to the aforementioned changes—drawing comparisons with Figure 2, I observe that areas with fewer college graduates in 1980, primarily located in the South, are the places that later emerged as divorce centers.

5.3 Empirics

To formally test whether the geography of divorce reorientated around areas with fewer skilled workers, I run the following event study on census/ACS data:

$$Divorce_{zt} = \sum_{j=1940}^{2018} \mu_j \; College_{z,1980} \;\; \text{x} \;\; 1[t = j] + \theta_z + \theta_t + \zeta_{zt} \tag{4}$$

where much of the notation follows from regression (1). For consistency with the previous sections, the outcome is the share of US-born non-hispanic white women aged 25-50 in a CZ who have ever been divorced. The main explanatory variable is the interaction between the share of college graduates in each CZ as of 1980 (Figure A.14), *College*, and a vector of year dummies. The model also includes CZ and year fixed effects, the θs. If my hypothesis is correct, I would expect to find negative μ estimates *after* 1980, as places with more (less) college graduates experience relative declines (increases) in divorce.

Figure 9A plots the coefficients from regression (4), with 1980 as the omitted year. No distinct trends are observed before 1980, suggesting parallel trends. In contrast, negative and significant

effects are found in later decades—CZs with relatively more (less) skilled labor see a greater fall (rise) in divorce. That such effects are only observed after 1980 is consistent with a story where the distribution of college graduates becomes more relevant when the wage structure begins to change (Figure 7). My results are robust to including state-by-year fixed effects, as depicted in Figure 9B. Broader economic forces may thus explain the shifting landscape of divorce.

5.4 IV Approach

While the absence of pretrends in Figure 9 is reassuring, concerns might still linger over the potential endogeneity in the spatial distribution of college graduates. To address this, I propose an IV approach in the spirit of Autor and Dorn (2013). Specifically, I use the local industry composition in 1940 to predict the corresponding supply of college graduates in 1980, exploiting two historical sources of variation: (i) national cross-industry differences in skill dependence and (ii) cross-CZ differences in industry composition.[42] Intuitively, some industries are more dependent on skilled workers than others due to the type of work involved—areas with more (less) skill-intensive industries would attract more (fewer) college graduates, raising (lowering) the local supply of skilled labor.[43] Importantly, I use the local industry composition in 1940, when college education was still relatively uncommon.[44] This limits the scope for reverse causality, where the supply of skilled workers in an area attracts skill-intensive industries instead. The 1940 industry composition also predates other potential confounders, such as (i) the rapid expansion in college education from the 1960s (Figure A.12), (ii) the impact of the GI Bill on both the supply and distribution of college graduates (Barrera et al., 2025; Bound and Turner, 2002), and (iii) the effects of WWII on the local industry structure (Garin and Rothbaum, 2025; Greenspon and Hanson, 2025).

Formally, I construct my IV as:

$$IV_z = \sum_{e=1}^{I} E_{e,z,1940} \text{ x } College_{e,-s,1940} \tag{5}$$

The first component, E, is the employment share of industry e in CZ z in 1940. The second piece, $College$, is the share of workers in industry e who are college graduates in 1940, measured across all US states except the state s containing CZ z. As a whole, the instrument thus gives the expected share of college graduates in an area based on the local industry composition.

Is the IV valid? I assess both instrument relevance and exogeneity in turn. To gauge relevance, Figure A.15 shows how the 1940-based IV relates to the share of college graduates 40 years later in 1980. A positive and statistically significant association is observed. Locations with relatively more skill-intensive industries in the past thus have a larger pool of college workers in subsequent decades, demonstrating instrument relevance. Next, I examine instrument exogeneity. This is

[42] Autor et al. (2013) use the local industry composition in 1950 to predict the share of routine-intensive jobs in a CZ 30 years later in 1980.

[43] For example, 43 percent of workers in education services are college graduates while almost none of those providing personal services to private households have a bachelor's degree, based on the 1950 census.

[44] This is the earliest year one can go as education attainment was first recorded in the 1940 census.

done by replacing the college share variable in regression (4) with my IV—are there pretrends in this reduced form setup? Figure A.16 shows the resulting estimates, without and with state-by-year fixed effects. No systematic patterns are detected before 1980, indicating a lack of pretrends; negative effects then follow thereafter. Taken together, the exercises here suggest that my IV is likely to be both relevant and exogenous.

Having established instrument validity, I now revisit the link between college share and divorce rates. This is done in two steps. First, I collapse my event study (regression (4)) into a simple difference-in-differences (DD) specification, substituting the year interactions with a post-1980 indicator. Columns 1 and 2 of Table 4 show negative and significant DD effects, without and with state-by-year fixed effects, in line with the original results. Second, I instrument the college-post-1980 interaction with an interaction between my IV and the post-1980 dummy. The 2SLS estimates in columns 3 and 4 are negative, with first-stage F-statistics over 4,000. These effects are sizable: moving from a CZ with the median college share in 1980 to a CZ at the 90th percentile reduces the ever-divorced rate by 5 to 6 percentage points, around 20 to 24 percent of the outcome mean.

6 Deep Origins

This paper set out to understand why the geography of divorce was so persistent before 1980 and so different after 1980. A natural extension is to dive deeper into the past. Before 1980, the forces of selective migration, social interactions, and intergenerational transmission reinforced the existing geography of divorce—but where did the spatial variation being reinforced originate from? This section provides an exploratory account of what might have mattered. Since the observed patterns were already around in the 1900s, I focus on potential explanations from the 19th century. In particular, I consider four potential factors: frontier conditions, the expansion of women's economic rights, immigration, and differences in divorce legislation.

6.1 Frontier Conditions

I begin with frontier conditions. Throughout the 19th century, America was rapidly expanding its territory, with successive waves of pioneering settlers heading westward. Following Porter et al. (1895) and Turner (1893), Bazzi et al. (2020) define the frontier as the line dividing places with population densities above and below two people per square mile. This precise definition allows them to locate and track the movement of the frontier from 1790-1890.[45] An area could be part of the frontier in one year but no longer so several years later, as the frontier continued its westward march. Rather than a fixed locality, the frontier represents a form of society in its nascent stage of development, one characterizied by extreme isolation (remote and sparsely populated). Such conditions offered new opportunities to those who were able to thrive in harsh environments. Populations on the frontier thus displayed distinctive characteristics: greater individualism, male-biased sex ratios, younger cohorts, more marriages, higher fertility, and fewer gainfully-employed

[45] The frontier was deemed to have closed by 1890.

women (Bazzi et al., 2020, 2024). Beyond descriptives, recent work has also examined the impact of frontier conditions on individualism and gender norms (Bazzi et al., 2020, 2024).

Did frontier conditions affect divorce rates? With more children to care for and less help from extended families (Bazzi et al., 2024), households on the frontier could come under greater strain, leading to more marital dissolution. At the same time, frontier exposure instilled religiosity (Bazzi et al., 2023b), which might discourage divorce. The net impact of frontier conditions on divorce is thus unclear, with no consensus in the literature. Contemporaries viewed the high divorce rates in the West as a western rather than a frontier phenomenon (Bureau of the Census, 1908; Riley, 1991).[46] Empirically, Bazzi et al. (2024) find *lower* female divorce rates on the frontier when using county-level census data but *higher* male divorce rates when using linked individual census data from 1870-1880, suggesting an outmigration of divorced women from frontier locations.

I revisit the link between frontier conditions and divorce using administrative divorce records from the 19th century (Section 2.1). The county data for 1867-1886 have been digitized by BGSU Marriage and Divorce Data Compass (2018), while I digitize the 1887-1906 numbers from *Marriage and Divorce, 1887-1906*. Taking these data, I then implement a DD regression:

$$Divorce_{ct} = \alpha + \varphi\ Frontier + \theta_c + \theta_t + u_{ct} \tag{6}$$

where the subscripts c and t denote a county and year, respectively. I focus on the decades 1870, 1880, and 1890 to match the decadal frontier data in Bazzi et al. (2020). I also restrict my sample to counties located between the 1790 frontier line in eastern US and the 1890 frontier line in the heartland (Bazzi et al., 2020). The outcome, *Divorce*, is the number of divorces per 1,000 women, constructed in several ways: (i) using single years (1870, 1880, 1890), (ii) averaging across 3-year intervals (1869-1871, 1879-1881, 1889-1891), and (iii) averaging across 5-year intervals (1868-1872, 1878-1882, 1888-1892), where the averaging attempts to reduce the influence of year-to-year fluctuations. The explanatory variable, *Frontier*, is an indicator for frontier status. Bazzi et al. (2020) define a frontier county as one that is within 100 km of the frontier line with less than six people per square mile. Finally, I include county and year fixed effects in the θs.

I find suggestive evidence that frontier conditions increased the prevalence of divorce in 19th-century America. Table 5 presents the frontier effects from regression (6) with different outcomes (1-year, 3-year-average, 5-year-average), for an unbalanced (Panel A) and balanced (Panel B) panel of counties. Even columns further include state-by-year fixed effects. Positive and statistically significant point estimates are observed in all cases—being on the frontier thus raised the propensity for divorce.[47] This effect comes through both the remoteness (100 km of frontier line) and sparseness (under six people per square mile) of frontier counties (Table A.3). Taken as a whole, frontier conditions may have contributed to the original geography of divorce, which selective migration,

[46] Contemporaries argued that if frontier conditions were the cause, the elevated divorce rates in the West should fade over time, yet the rates in this region kept rising instead (Bureau of the Census, 1908; Riley, 1991).

[47] The baseline analysis uses standard errors clustered at the county level. Similar conclusions are observed when clustering standard errors within 60-by-60 square-mile grid cells.

social interactions, and intergenerational transmission then reinforced.[48]

6.2 Women's Economic Rights

Besides frontier conditions, did the uneven expansion of women's economic rights across states during the 19th century also contribute to the original geography of divorce? Beginning with Massachusetts in 1848, states started to abandon the laws of coverture, granting married women rights over their own property and labor earnings. Recent work has established the impact of these early reforms: Hazan et al. (2019) find a change in household portfolios now that investor protection was extended to women, while Hazan et al. (2025) show a reduction in fertility as bargaining power shifted from husbands to wives.

Did the rise of women's economic rights also affect divorce? To investigate this, I exploit the staggered introduction of legislative reforms that expanded women's property and earnings rights, collectively referred to as economic rights.[49] Adapting the approach in Hazan et al. (2025), I compare divorce rates in contiguous county-pairs on either side of the border between states that granted such rights at different times. Importantly, the final timing when economic rights were granted is plausibly exogenous, as these rights were often overturned due to unforeseen technicalities before eventually being accepted (Hazan et al., 2025).[50] Drawing on administrative divorce data from the 19th century (Section 2.1), I run the following event study:

$$Divorce_{cst} = \sum_{k=-10}^{10} \delta_k \; 1[t = k]_s + \theta_{c,b(c)} + \theta_s + \theta_t + u_{cst} \tag{7}$$

where the subscripts c, s, and t denote the county, state, and year (1867-1906), respectively.[51] The outcome, *Divorce*, is the number of divorces per 1,000 women. The main explanatory variable is a vector of indicators for the number of years since women's economic rights were effectively granted in the state a county is located in, k.[52] For simplicity, I code periods more than 10 years before or after a state changed its laws as -10 and 10. Finally, three sets of fixed effects are included in the θs: county-pair c-$b(c)$ along a state border b, state, and year.

I find no evidence that the expansion of women's economic rights shaped the early geography

[48] Is the persistence in the geography of divorce between 1900-1980 due to the reinforcing mechanisms of selective migration, social interactions, and intergenerational transmission, or is it an artifact of path dependence arising from 19th-century frontier conditions? Bazzi et al. (2020) measure the long-run impact of frontier conditions using the duration with which a county was exposed to frontier conditions. I find no robust relation between this metric and divorce in later decades, even when using historical migration shocks to instrument for frontier exposure (Table A.4). This suggests a greater role for the forces of selective migration, social interactions, and intergenerational transmission.

[49] Hazan et al. (2025) make the case for looking at property and earnings rights together rather than separately, as the absence of one hampers the effectiveness of the other. They provide real-life examples to illustrate this point.

[50] Anticipatory effects are unlikely to pose a serious problem as legislatures passed laws that were conservatively interpreted by the courts initially, making the precise timing of effective rights uncertain (Hazan et al., 2025). Other legal changes are also unlikely to confound the estimated impact of economic rights—randomly picking a year for women's rights in each state yields effects centered around zero (Hazan et al., 2025).

[51] Some 33 states granted economic rights to married women over my study period.

[52] I follow the dates in Hazan et al. (2025), who in turn draw on Geddes and Lueck (2002). These are not necessarily the dates when the laws were *first* passed as the laws can be challenged and overturned.

of divorce. Figure 10 presents the estimates from regression (7), with confidence bands based on standard errors double-clustered on state and county-pair. No statistically significant estimates are observed either before or after the legislative reforms. To probe these results further, I conduct five additional exercises. First, I add county-pair-specific linear trends (Figure A.17A). Second, I drop counties along the border of a former Confederacy and a non-Confederacy state (Figure A.17B). Third, I exclude counties on the border of a community property state and the neighboring state (Figure A.17C). Fourth, recognizing the potential bias in fixed effects models with staggered treatment, I implement the two-step estimator proposed by Gardner et al. (2025) (Figure A.17D).[53] No discernible effects are found under any scenario. The original geography of divorce is thus unlikely to reflect early differences in women's economic rights.[54]

6.3 Immigration

Beyond America's westward expansion and the rise in women's rights, the 19th century also ushered in the Age of Mass Migration (circa 1850-1920), when some 50 million Europeans left their home countries, almost 30 million of whom settled in the US (Hatton and Williamson, 1998)—did the influx of foreigners at the time give rise to the early divorce patterns in America? Historically, divorce was less common in Europe than America (Figure A.19, Bureau of the Census (1908)). A greater inflow of immigrants to an area could thus bring down the overall rate of divorce mechanically, if immigrants hold on to their "low-divorce culture".[55] However, as Bazzi and Fiszbein (2025) explain, cultural *persistence* is not the only option for immigrants; they can also *influence* native culture (further reducing divorce rates in this case) or *assimilate* into it (limiting any changes in divorce rates). In addition, the male-biased composition of immigrants might distort local sex ratios (Angrist, 2002), changing the pool of potential mates one is exposed to and possibly leading to more divorces (Kuroki, 2013; McKinnish, 2007; Svarer, 2007). Finally, the process of moving to the US could strain existing unions, causing more breakups among immigrants (Caarls and Mazzucato, 2015; Hu, 2018). Given the myriad of dynamics at play, the net impact of immigration on divorce is unclear.

[53] This approach first estimates the non-event-study coefficients on non-treated observations, before using the predicted values to run the original regression.

[54] In related work, Roehrkasse (2019) finds that the expansion of married women's *property* rights in the 19th century led to a substantial rise in divorce rates. This appears to contradict my findings. Two issues are salient here. First, Roehrkasse (2019) only looks at property rights, rather than economic rights comprising both property and earnings rights. As Hazan et al. (2025) note, these should be viewed together since the absence of one hampers the effectiveness of the other. I check that switching from the property rights dates to the economic rights dates eliminates all significant estimates in Roehrkasse's (2019) setup. Going the other way, had my analysis used the property rights dates instead of the economic rights dates, I would still find a null effect, as shown in Figure A.18. Second, specification choice matters. Roehrkasse (2019) primarily uses county-level regressions with county and year fixed effects. The impact of property rights on divorce disappears if one further includes state-specific linear trends, a specification that does not feature in his paper.

[55] Recall that the county divorce statistics pool all demographics together, whether native or immigrant—hence the mechanical result. Another assumption is that migrants have similar characteristics with those that stayed back in their origin countries. This need not be the case. Knudsen (2024) finds that 19th-century emigrants from Scandinavia to the US were more individualistic than those who remained. Covering a similar period, Abramitzky et al. (2012) show that Norwegians with poorer economic prospects at home were more likely to move to the US.

To shed light on the role of immigration, I compare the change in divorces rates between places receiving more and less immigrants, drawing once again on administrative county divorce records from the 19th century. Formally, I run the following regression:

$$\frac{\triangle \#Divorce_{ctt'}}{Women_{ct}} = \alpha + \Gamma \frac{\triangle \#Foreign_{ctt'}}{Pop_{ct}} + \theta_c + \theta_t + u_{ct} \tag{8}$$

where c is a county, t is the base year (1870, 1880, 1890), and t' is the end year a decade later (1880, 1890, 1900).[56] The outcome is the change in the number of divorces in county c between t and t', $\triangle \#Divorce$, scaled by the corresponding number of women in the base year, $Women$. Mirroring the frontier analysis, the outcome can be measured using single years (1870, 1880, 1890) or by averaging across 3-year (1869-1871, 1879-1881, 1889-1891) and 5-year (1868-1872, 1878-1882, 1888-1892) intervals to limit the influence of year-to-year fluctuations. The explanatory variable is the change in foreign-born persons in county c between t and t', $\triangle \#Foreign$, scaled by the local population in the base year, Pop. County and year fixed effects, the θs, are also included.

A natural concern with regression (8) is the endogeneity of migrant settlement patterns across the US. To address this, I adapt the IV approach developed by Sequeira et al. (2019). Departing from traditional shift-share methods, Sequeira et al. (2019) isolate plausibly exogenous variation in the distribution of immigrants by combing two sources of variation. The first is the expansion of railway networks, which enabled arriving immigrants to travel inland. The second is the aggregate inflow of immigrants, which fluctuated greatly from decade to decade. Putting the two together: a county that is connected to the railway network during an immigration boom will receive more immigrants, and vice versa. This *interaction* between the timing of railway connection and immigration boom, rather than each variable alone, is unlikely to affect divorce rates through other channels besides migration flows—Sequeira et al. (2019) show that counties connected just before an immigration boom are similar in initial characteristics to places without railway connection. I thus use the interaction between railway access in the base year and European migrant inflows over the ensuing decade to instrument the relative change in foreign-born persons in each county, controlling for railway access in the initial period.[57]

While Sequeira et al. (2019) rigorously probe the validity of their IV approach, applying this method to study historical divorces brings additional constraints. My analysis is restricted by the availability of county divorce data, which limits both the set of counties in the sample (due to missing divorce statistics, Figure A.8) and the time horizon (1867-1906). Here, I can only examine the decades beginning 1870, 1880, and 1890.[58] The narrow sample introduces several constraints. First, while Sequeira et al. (2019) allow the impact of railway connection to vary by GDP growth

[56] These years are chosen to match the decadal immigration and IV data in Sequeira et al. (2019).

[57] I deviate from Sequeira et al. (2019) by using the interaction of railway access and immigration booms to instrument the *relative change* in foreign-born persons, rather than the *share* of foreign-born. The latter approach yields a weak first stage in my setting.

[58] Recall that these years are selected to match the decadal data in Sequeira et al. (2019). In addition, while divorce statistics are more readily available from the 1920s onward, the railway network was largely established by then and the Age of Mass Migration had ceased, rendering the instrument irrelevant.

to account for business cycles, this is not possible here as immigration and GDP growth are almost perfectly correlated over my short study window (Table A.5). Second, with a smaller sample, fewer robustness checks are feasible. The results in this section should thus be viewed as suggestive.

Table 6 indicates that immigration is unlikely to explain the early geography of divorce. Panel A uses an unbalanced panel of counties while Panel B uses a balanced panel. Divorce in columns 1-3 is measured using single years while divorce in the remaining columns is averaged across 3- (columns 4-6) or 5-year (columns 7-9) intervals. Simply running regression (8) yields a positive association between immigration and divorce (columns 1, 4, 7). Adding an indicator for railway access in the base year does not alter this relation substantially (columns 2, 5, 8). Importantly, the railway indicator is never significant at the 5 percent level or lower—without the immigration booms, railway access per se does not affect divorces. When applying the railway-immigration IV, I observe changes in the sign, magnitude, or significance of the immigration coefficient (columns 3, 6, 9), particularly with the balanced panel.[59] All 2SLS estimates are insignificant. Similar patterns can be observed when using aggregate immigration flows predicted from weather shocks in Europe (Table A.6). Taken as a whole, my results suggest that immigration was not a key contributor to the early geography of divorce.

6.4 Legislation

Finally, I investigate if differences in 19th-century state divorce legislation might have given rise to the early geography of divorce. In *Marriage and Divorce, 1887-1906* (Bureau of the Census, 1908), the Census Bureau published a detailed summary of each state's divorce legislation at the time, covering a wide range of issues including jurisdiction, causes, alimony, refusal of divorce, and limitations of time, among other things. However, the intricacies and specifics of individual state laws make direct cross-state comparisons challenging.

To make progress, I construct a summary measure of how strict a state's divorce laws are, in terms of the difficulty of obtaining a divorce. This is done in two steps. First, I feed the raw text of each state's divorce legislation, as presented by the Bureau, to OpenAI's GPT 5.1 model. Second, I ask the model to score each state's laws from 0-100, where a higher score denotes stricter divorce rules. Figure 11 maps these strictness scores across the country. There is some overlap with the geography of divorce in the 1900s-1930s (Figure 4). For example, Michigan and Florida, states with less strict laws, are where divorce rates are relatively higher. However, the overlap is not perfect. California, for instance, has both strict divorce laws and high divorce rates. This inconsistency could stem from differences in enforcement (or the lack thereof)—what happens on the ground differs from what is written in law.

An alternative approach is to consider the nature of divorces in each state. To do this, I digitize 19th-century state-level divorce data from the Bureau of the Census (1908), stratified along three dimensions. First, who initiated the divorce, the wife or the husband (Figure A.20)? Second, what share of divorces are caused by abandonment/desertion, adultery, cruelty, habitual

[59] The first-stage is usually strong, with an F-statistic over 10 in most cases.

drunkenness/intoxication, or neglect to provide (Figures A.21 and A.22)? Third, what proportion of divorces involve children (Figure A.23)? If legislative differences are the root cause of the early divorce landscape, these dimensions of divorce should vary in a way that matches the geography of divorce. Yet none match the spatial patterns of divorce tightly. While this does not completely rule out a role for legislative differences, it suggests that these laws are unlikely to be the main explanation for the early patterns of divorce.

7 Conclusion

Much has been written on the rise and fall of divorce in America. When divorce was rapidly rising from the 1960s-1980s, the underlying geography of divorce remained relatively stable. In fact, the patterns observed over this period date back to at least the 1900s. The persistence in geography was sustained by the reinforcing mechanisms of selective migration, social interactions, and intergenerational transmission. After 1980, as divorce rates started to fall, the landscape of divorce also began to change. Behind this shift was the evolving wage structure—worsening economic circumstances for less-skilled workers raised their likelihood of divorce, causing the geography of divorce to reorientate around places with fewer skilled workers.

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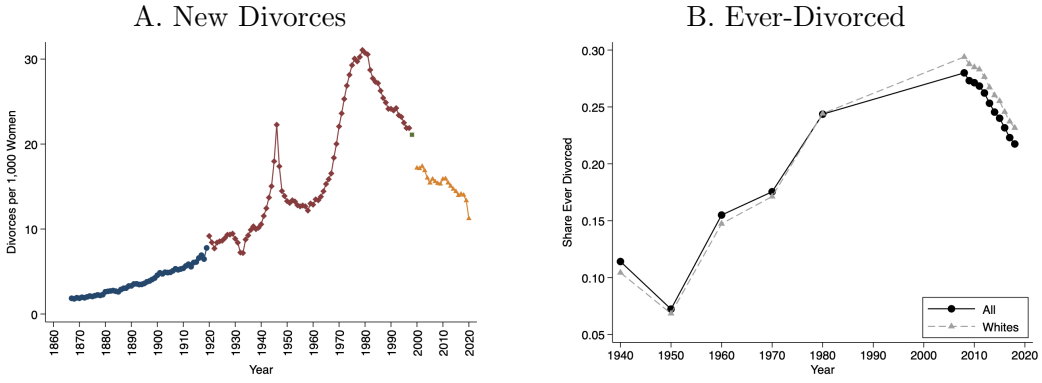
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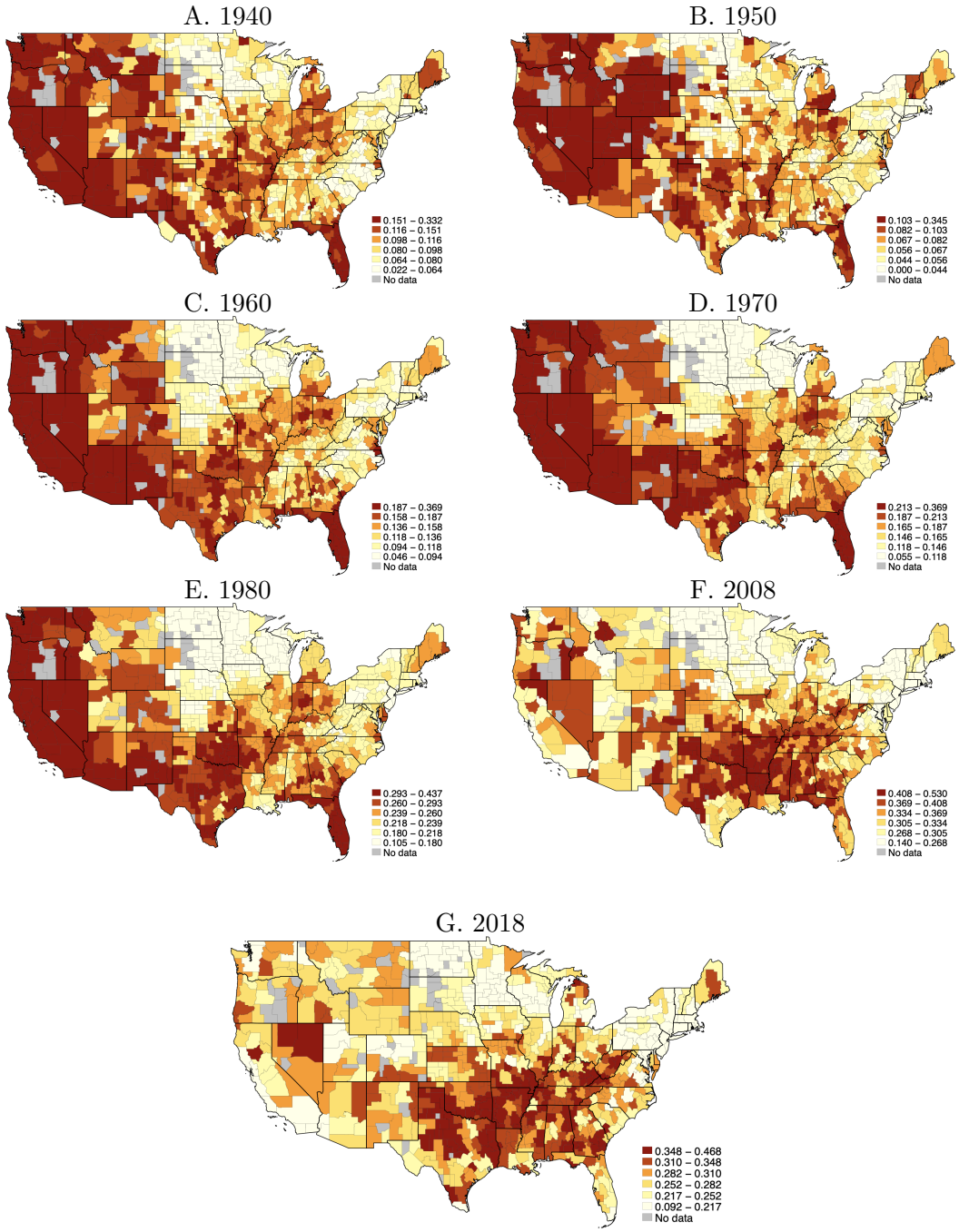
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Figure 1: Divorce Trends



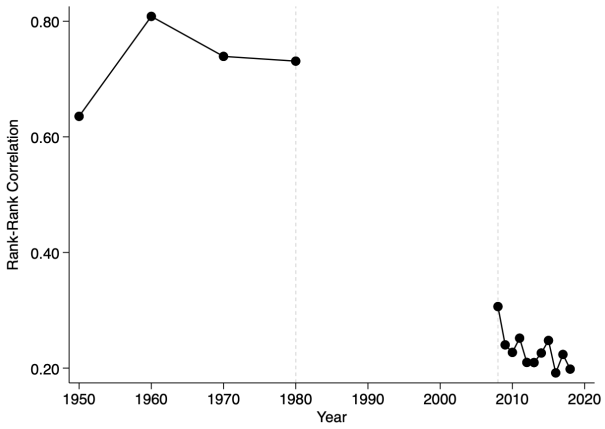
Notes - A: Data are from the National Center for Health Statistics (1973) for 1867-1919, Carter et al. (2006) for 1920-1997, Centers for Disease Control and Prevention (1999) for 1998, Centers for Disease Control and Prevention (2025) for 2000-2020, Manson et al. (2023), and Ruggles et al. (2025). The figure shows the number of new divorces per 1,000 women aged 25-50, by year. B: Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figure shows the share of US-born non-hispanic women aged 25-50 who were ever divorced, as a whole (solid line) or for whites (dashed line), by year.

Figure 2: Geography of Divorce, 1940-2018



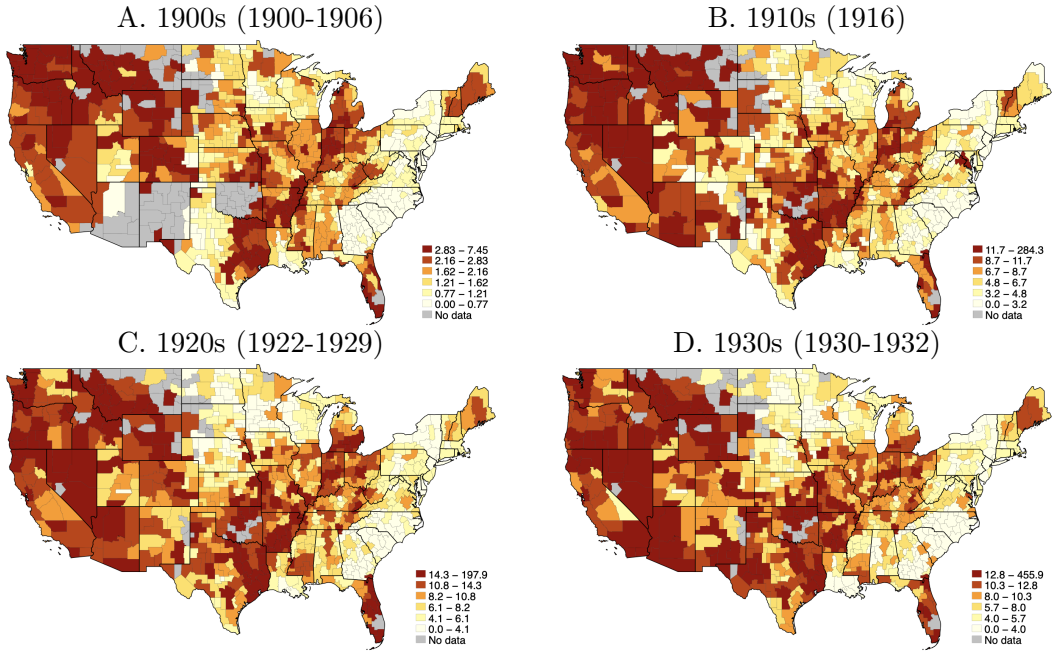
Notes - Data are from the 1940-1980 censuses as well as the 2008 and 2018 ACS (Ruggles et al., 2024, 2025). The figures show the share of US-born non-hispanic white women aged 25-50 in each CZ who were ever divorced, by year. The sample is restricted to CZs with at least 50 sample-line persons from the base population in 1940. The sample size is 679 CZs.

Figure 3: Persistence and Change in Geographic Patterns, Rank-Rank Correlations



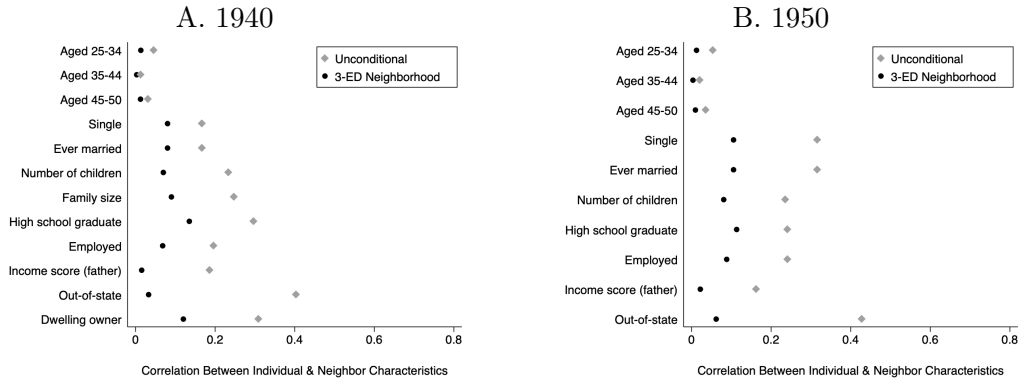
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). Each marker denotes the CZ-level rank-rank correlation between the share of US-born non-hispanic white women aged 25-50 in each CZ who were ever divorced as of 1940 and the corresponding share in a subsequent year given along the x-axis. The sample size is 679 CZs.

Figure 4: Geography of Divorce, 1900s-1930s



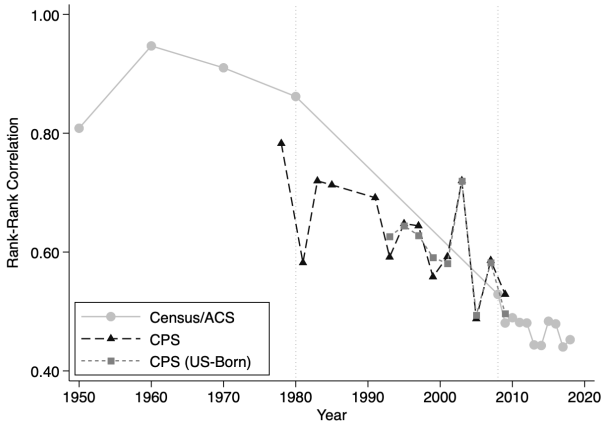
Notes - Data are from the Bureau of the Census (1908) and BGSU Marriage and Divorce Data Compass (2018). The figures show the number of divorces per 1,000 women in each CZ during the 1900s (1900-1906) (A), the 1910s (1916) (B), the 1920s (1922-1929) (C), and the 1930s (1930-1932) (D). The sample sizes are 662 (A) and 685 (B-D) CZs.

Figure 5: Individual and Neighbor Characteristics



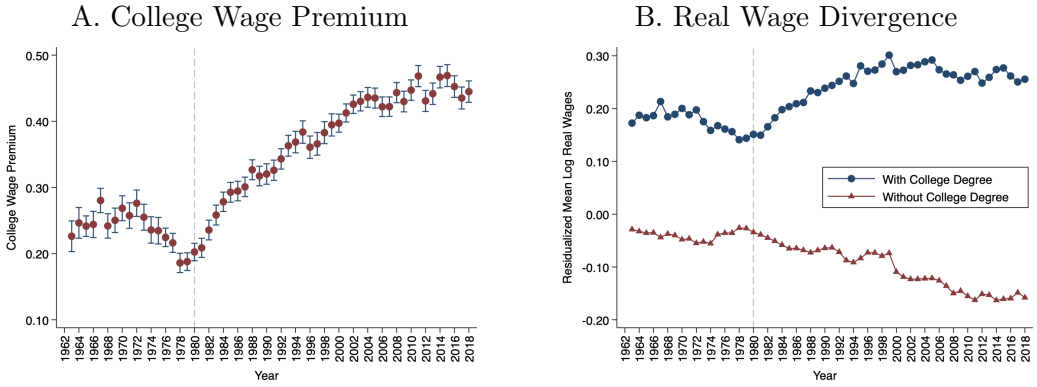
Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024). The underlying sample comprises sample-line US-born non-hispanic white women aged 25-50 residing in neighborhoods with no black residents. The figures show the correlations between the characteristics (along the y-axis) of individuals and their neighbors. These correlations are obtained in three steps. First, a random person is drawn from each local area (an ED). Second, the remaining residents who are not from the same household as the random person are used to compute the average characteristics of a given area. Third, the characteristics of the randomly selected individual and the average characteristics of neighbors (which are shares or means of the characteristics along the y-axis) are compared, using only local areas with at least five residents other than the randomly chosen person and members of the same household. The grey diamond markers denote the unconditional correlations while the black circle markers denote the correlations within neighborhood reference groups (three nearby EDs).

Figure 6: Timing of Change in the Geography of Divorce



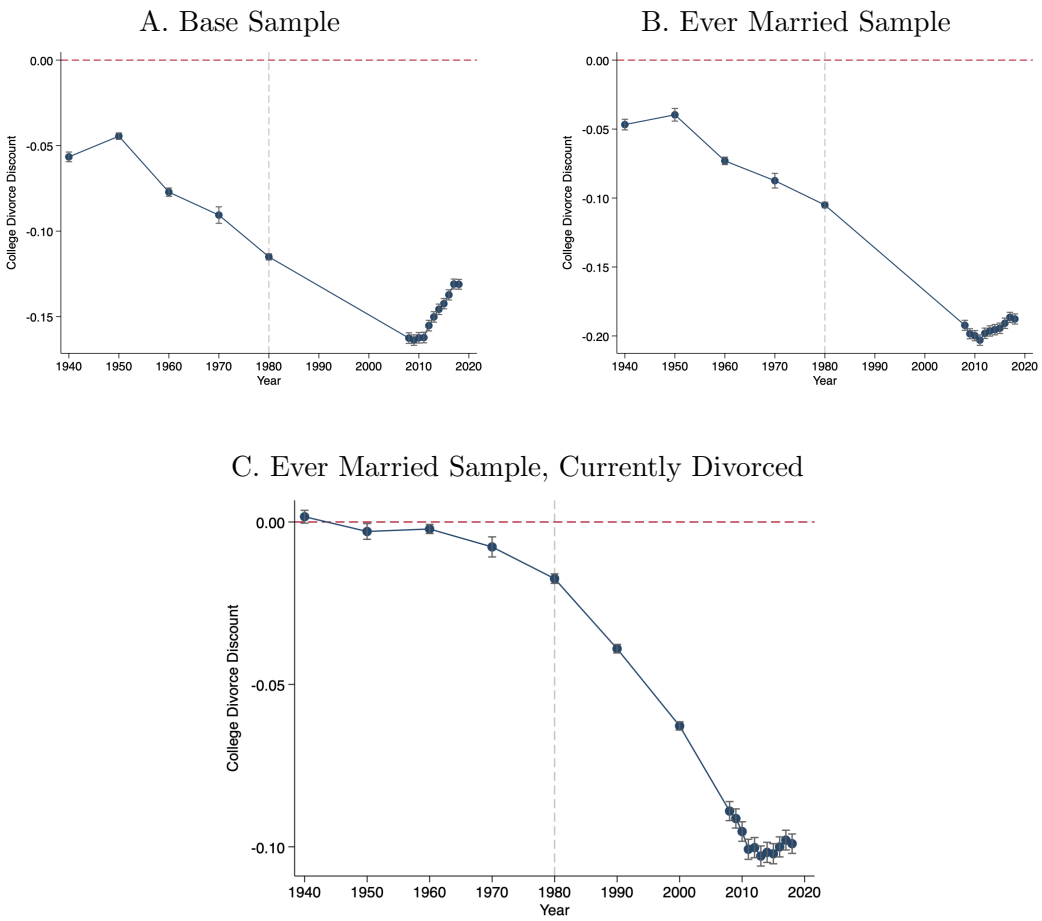
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025), as well as the 1978-2009 CPS (Flood et al., 2025). Each marker denotes the state-level rank-rank correlation between the share of US-born non-hispanic white women aged 25-50 in each state who were ever divorced as of 1940 and the corresponding share in a subsequent year given along the x-axis, separately using census/ACS data (solid line) or CPS data (dashed lines). The CPS sample comprises non-hispanic white women aged 25-50 (triangle markers) or US-born non-hispanic white women aged 25-50 (square markers). The sample size is 49 states.

Figure 7: Changes in Wage Structure



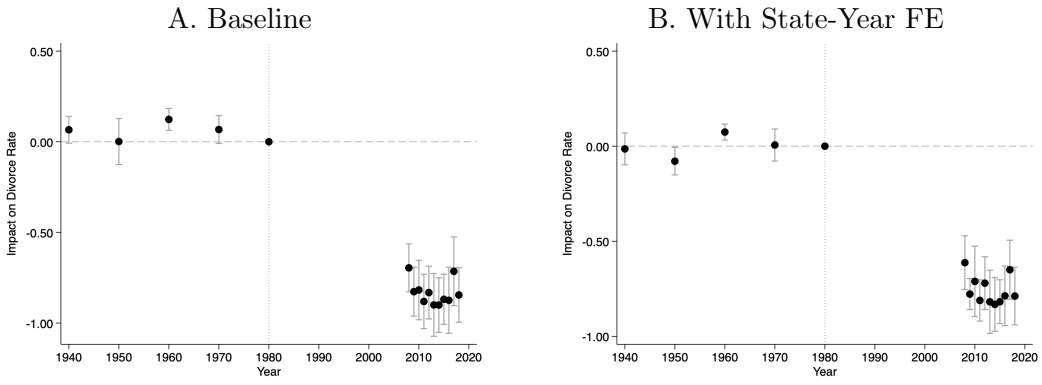
Notes - Data are from the 1963-2018 CPS (Flood et al., 2025). The sample is restricted to white male full-time full-year wage and salary workers aged 25-50. A: The figure shows the coefficients from regressions of individual log annual wages on an indicator for those with a 4-year college degree or higher, controlling for potential experience, run separately for each year. Sample weights are used in each regression. 95 percent confidence bands are displayed. B: The figure shows the mean log real wage for those with (circle markers) and without (triangle markers) a 4-year college degree or higher, residualized on potential experience, by year.

Figure 8: College-Divorce Association, Trend



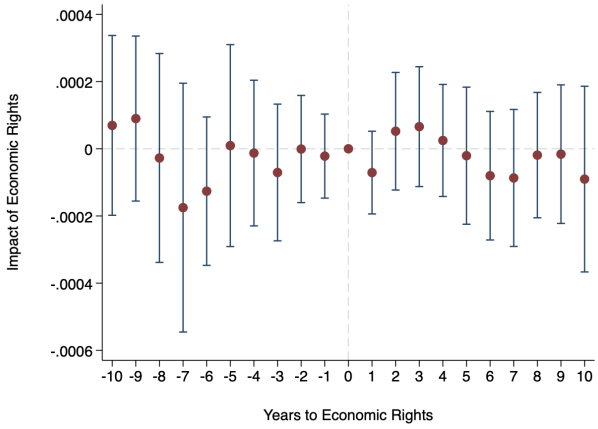
Notes - Data are from the 1940-2000 censuses and the 2008-2018 ACS (Ruggles et al., 2025). The base sample refers to US-born non-hispanic white women aged 25-50, while the ever-married sample is further restricted to those who had ever been married. The figures show the coefficients from regressions of an indicator for ever-divorced (A and B) or currently divorced (C) persons on an indicator for those with a 4-year college degree or higher, controlling for a quadratic in age. Sample weights are used in each regression. 95 percent confidence bands are displayed.

Figure 9: Impact of College Share on Divorce



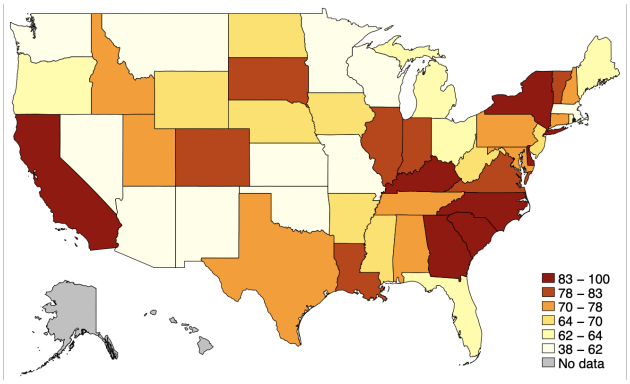
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures show the interaction coefficients from a regression of the share of US-born non-hispanic white women aged 25-50 in a CZ who were ever divorced on the interaction between the share of college graduates in 1980 and year dummies, controlling for CZ and year fixed effects. B further includes state-by-year fixed effects. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 10,864 CZ-year observations.

Figure 10: Impact of Women's Economic Rights on Divorce



Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The unit of observation is the county-year. Each marker denotes the coefficient from a regression of the number of divorces per 1,000 women on indicators for the number of years since women's economic rights were effectively granted in the state a county is located in, controlling for three sets of fixed effects: county-pair along a state border, state, and year. Confidence bands based on standard errors double-clustered on state and county-pair are displayed. The sample size is 67,710 county-year observations.

Figure 11: Strictness of Historical Divorce Legislation



Notes - Data are from the Bureau of the Census (1908). The figure shows the strictness score of each state's divorce legislation between 1887-1906, determined by feeding the raw text of every state's legislation into OpenAI's GPT 5.1 model. Darker colors indicate stricter divorce rules.

Table 1: Selective Migration by Ever-Divorced Status

Omitted group	Outcome: Ever-Divorced Prior to Moving					
	Stayers in Low-Divorce CZs			Stayers in High-Divorce CZs		
	(1)	(2)	(3)	(4)	(5)	(6)
Migrant from low- to high-divorce CZ	0.016***	0.018***	0.017***			
	(0.004)	(0.004)	(0.004)			
Migrant from high- to low-divorce CZ				-0.023***	-0.019***	-0.014**
				(0.007)	(0.007)	(0.006)
Cohort fixed effects		Y	Y		Y	Y
Initial CZ fixed effects			Y			Y
Outcome mean	0.048	0.048	0.048	0.088	0.088	0.088
Adjusted R-squared	0.000	0.004	0.005	0.000	0.005	0.009
N	86,559	86,559	86,559	69,215	69,215	69,215

Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024) as well as the IPUMS MLP (Helgertz et al., 2024). The base sample comprises sample-line US-born non-hispanic white women aged 25-40 in the 1940 census who can be linked forward to the 1950 census. Low-divorce CZs are those with below-median ever-divorced rates (based on stayers) in both 1940 and 1950; high-divorce CZs have above-median ever-divorced rates (based on stayers) in both 1940 and 1950. Columns 1-3 focuses on those who started in low-divorce CZs, some of whom stayed in the same CZ (stayers) while others left for high-divorce CZs (migrants). Columns 4-6 focuses on those who started in high-divorce CZs, some of whom stayed in the same CZ (stayers) while others left for low-divorce CZs (migrants). Each cell shows the coefficient from a regression of an indicator for ever-divorced persons as measured in 1940 on a dummy that takes the value 1 for those who left their initial CZ by 1950 (migrant). Columns 2 and 5 include cohort fixed effects; columns 3 and 6 further add fixed effects for the initial CZ of residence in 1940. Standard errors clustered by initial CZ are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table 2: Social Interactions and the Propensity for Divorce

	1940				1950	
	(1)	(2)	(3)	(4)	(5)	(6)
Social interaction effect	0.002***	0.001***	0.001**	0.002***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Sample	All	All	One mover	Both movers	All	All
Individual fixed effects		Y				Y
Outcome mean	0.013	0.013	0.011	0.026	0.009	0.009
N	7,123,949	7,123,949	323,663	216,524	7,231,327	7,231,327

Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024). The baseline sample comprises sample-line US-born non-hispanic white women aged 25-50 residing in neighborhoods with no black residents. The unit of observation is a pair of residents. Each cell shows the coefficient from a regression of a dummy that takes the value 1 if both members of a pair have ever been divorced on an indicator for pairs residing in the same local area (an ED), controlling for neighborhood reference group fixed effects (three nearby EDs). Columns 2 and 6 include individual fixed effects for each member of a resident pair. Column 3 restricts the sample to resident pairs where one person has lived in the same house for at least 5 years while the other was from a different county 5 years ago. Column 4 restricts the sample to resident pairs where both members were residing in a different county 5 years ago. The outcome mean is measured using resident pairs in different local areas of a given neighborhood reference group. Standard errors clustered by neighborhood reference group are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table 3: Intergenerational Transmission of Divorce Risk

Parent	Mother	Father
	(1)	(2)
Parent divorced in 1910	0.062***	0.079***
	(0.010)	(0.023)
Daughter cohort fixed effects	Y	Y
Parent cohort fixed effects	Y	Y
1910 CZ fixed effects	Y	Y
Outcome mean	0.084	0.084
Adjusted R-squared	0.011	0.010
N	294,994	286,984

Notes - Data are from the 1910 and 1940 full count censuses (Ruggles et al., 2024) as well as the Census Tree Project (Price et al., 2023). The base sample comprises 0-18-year-old US-born non-hispanic white daughters in the 1910 census who can be linked forward to 1940. Only sample-line respondents in 1940 are kept. Column 1 looks at the relation between mothers and daughters while column 2 looks at the relation between fathers and daughters. Each cell shows the coefficient from a regression of an indicator for daughters who were ever-divorced by 1940 on a dummy for having a parent who was divorced in 1910, controlling for the daughter's cohort, the parent's cohort, and the CZ of residence in 1910. Standard errors are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table 4: Impact of College Share on Divorce

	(1)	(2)	(3)	(4)
Share of college graduates, 1980	-0.884***	-0.754***	-1.28***	-1.02***
	(0.079)	(0.049)	(0.026)	(0.028)
Estimator	OLS	OLS	2SLS	2SLS
State-by-year fixed effects		Y		Y
Outcome mean	0.262	0.262	0.262	0.262
First stage F-statistic			8,896	7,427
Adjusted R-squared	0.880	0.910		
N	10,864	10,864	10,864	10,864

Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). Each cell shows the interaction coefficient from a regression of the share of US-born non-hispanic white women aged 25-50 who were ever divorced on the interaction between the share of college graduates in 1980 and a post-1980 indicator, controlling for CZ and year fixed effects. Even columns further include state-by-year fixed effects. Columns 3 and 4 use an IV approach, where the instrument is the college share predicted by the industry composition in 1940, interacted with a post-1980 indicator. Standard errors clustered at the state level are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table 5: Impact of Frontier Conditions on Divorce

	1-Year		3-Year-Avg		5-Year-Avg	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Unbalanced Panel					
Frontier county	0.584***	0.541***	1.09***	0.897***	1.07***	0.900***
	(0.169)	(0.185)	(0.143)	(0.143)	(0.148)	(0.168)
Adjusted R-squared	0.269	0.283	0.320	0.345	0.333	0.354
Outcome mean	1.15	1.15	1.13	1.13	1.13	1.13
N	4,284	4,284	4,687	4,687	4,792	4,792
	Panel B: Balanced Panel					
Frontier county	0.657***	0.565***	1.16***	0.934***	1.17***	0.981***
	(0.181)	(0.187)	(0.155)	(0.154)	(0.160)	(0.184)
Adjusted R-squared	0.245	0.262	0.297	0.326	0.301	0.325
Outcome mean	1.10	1.10	1.08	1.08	1.08	1.08
N	4,088	4,088	4,460	4,460	4,550	4,550
State-by-year fixed effects		Y		Y		Y

Notes - Data are from Bazzi et al. (2020), the BGSU Marriage and Divorce Data Compass (2018), and the Bureau of the Census (1908). The unit of observation is the county-year. Each cell shows the coefficient from a regression of the number of divorces per 1,000 women on an indicator for frontier status, controlling for county and year fixed effects. Even columns further include state-by-year fixed effects. Columns 1-2 measure the outcome in a single year (1870, 1880, 1890); columns 3-4 average the outcome across 3-year intervals (1869-1871, 1879-1881, 1889-1891); columns 5-6 average the outcome across 5-year intervals (1868-1872, 1878-1882, 1888-1892). Panel A uses an unbalanced panel of counties; Panel B uses a balanced panel. Standard errors clustered by county are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

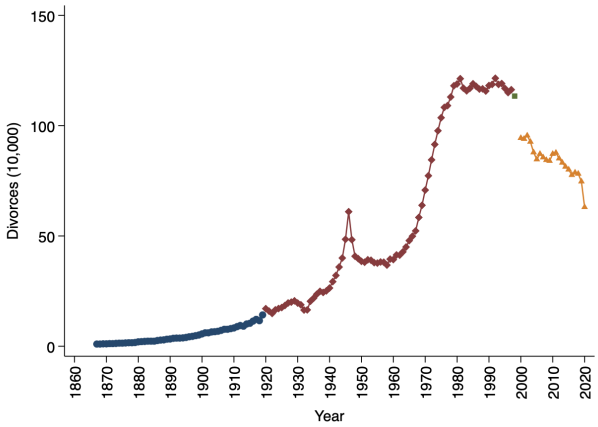
Table 6: Impact of Immigration on Divorce

	1-Year			3-Year-Avg			5-Year-Avg		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Unbalanced Panel									
Relative change in immigrants	0.004***	0.004***	0.004	0.003***	0.003***	0.003	0.003***	0.003***	0.002
	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.002)
Estimation	OLS	OLS	2SLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Railway access in base year		Y	Y		Y	Y		Y	Y
Outcome mean	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
Adjusted R-squared	0.163	0.164		0.311	0.311		0.353	0.353	
First stage F-statistic			24.1			17.4			17.5
N	5,085	5,085	5,085	5,813	5,813	5,813	6,006	6,006	6,006
Panel B: Balanced Panel									
Relative change in immigrants	0.004***	0.004***	-0.005	0.003***	0.003***	0.003	0.003***	0.003***	0.001
	(0.001)	(0.001)	(0.006)	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.003)
Estimation	OLS	OLS	2SLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Railway access in base year		Y	Y		Y	Y		Y	Y
Outcome mean	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
Adjusted R-squared	0.146	0.147		0.297	0.297		0.359	0.360	
First stage F-statistic			8.16			12.3			13.9
N	3,540	3,540	3,540	4,434	4,434	4,434	4,704	4,704	4,704

Notes - Data are from Sequeira et al. (2019), the BGSU Marriage and Divorce Data Compass (2018), and the Bureau of the Census (1908). The unit of observation is a county-year. Each cell shows the coefficient from a regression of the change in the number of divorces in a county scaled by the corresponding number of women in the base year on the change in the number of foreign-born in a county scaled by the corresponding population in the base year, controlling for county and year fixed effects. Changes are measured between the base year (1870, 1880, 1890) and the end year (a decade later). Divorce in columns 1-3 is measured using single years (1870, 1880, 1890); divorce in columns 4-6 is averaged across 3-year intervals (1869-1871, 1879-1881, 1889-1891); divorce in columns 7-9 is average across 5-year intervals (1868-1872, 1878-1882, 1888-1892). Columns 3, 6, and 9 instrument the relative change in immigrants using the interaction between an indicator for railway access in the base year and the inflow of European immigrants in the ensuing decade. Panel A uses an unbalanced panel of counties; Panel B uses a balanced panel. Standard errors clustered by county are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

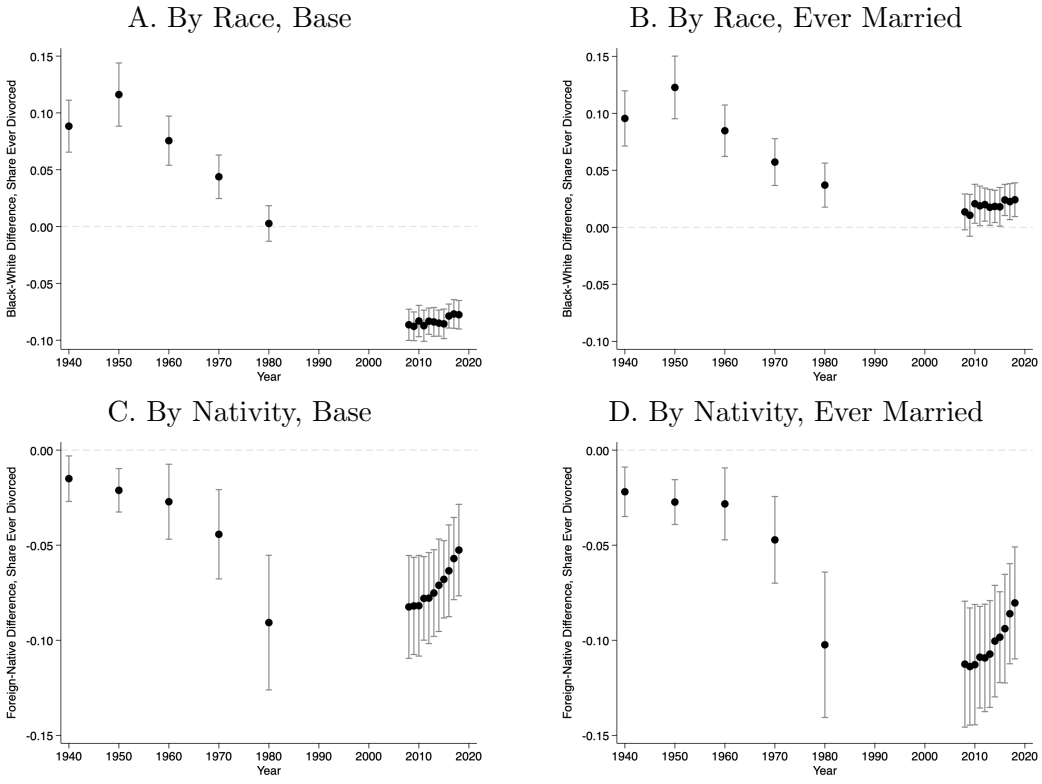
For Online Publication

Figure A.1: Raw Divorce Trend



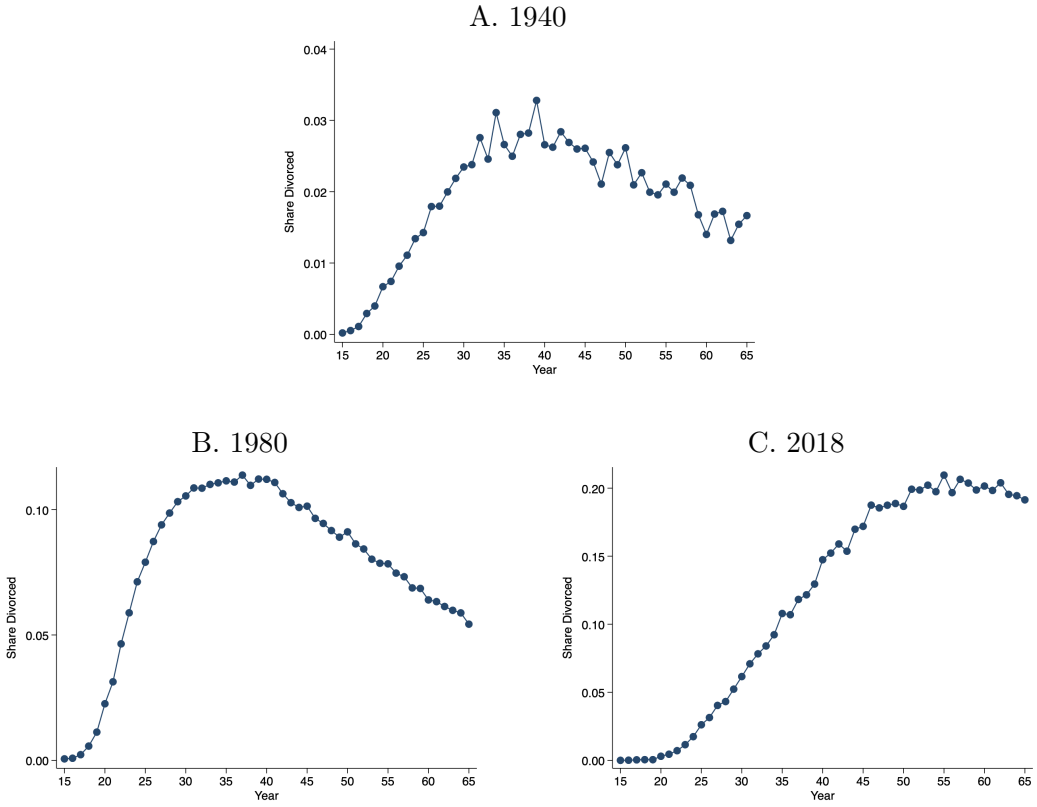
Notes - Data are from the National Center for Health Statistics (1973) for 1867-1919, Carter et al. (2006) for 1920-1997, Centers for Disease Control and Prevention (1999) for 1998, and Centers for Disease Control and Prevention (2025) for 2000-2020. The figure shows the number of new divorces (10,000), by year.

Figure A.2: Divorce Rates by Race and Nativity



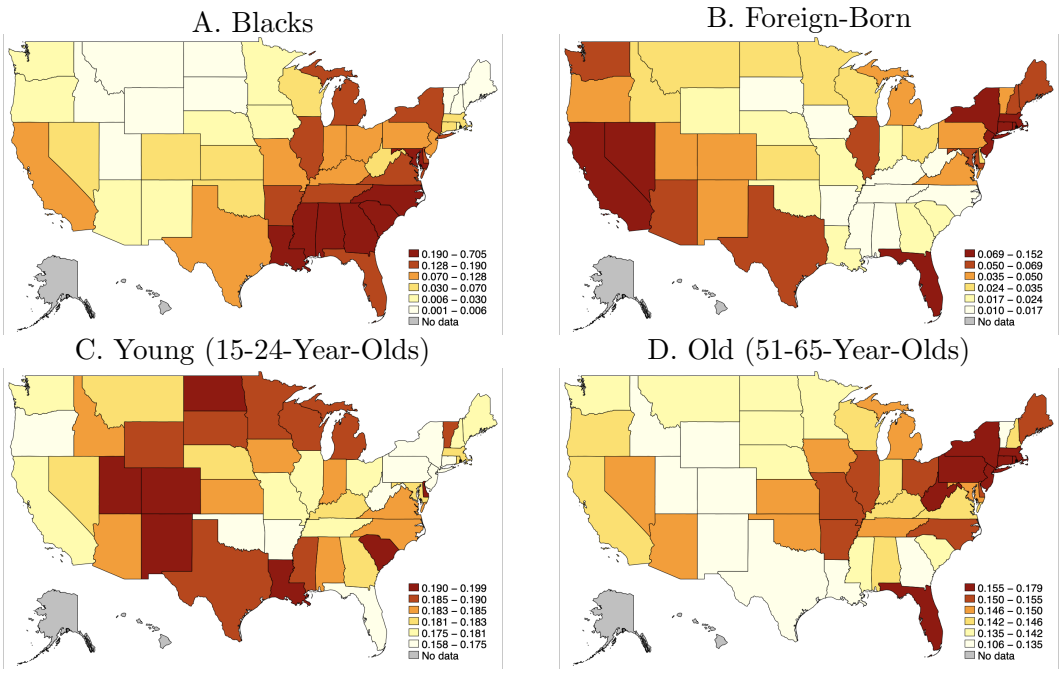
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2025). A and B: The base sample comprises US-born non-hispanic white and black women aged 25-50. The figures show the coefficients from regression of an indicator for ever-divorced persons on an indicator for blacks, controlling for a quadratic in age and state fixed effects, for the base sample (A) or the ever-married subset (B). C and D: The base sample comprises white women aged 25-50. The figures show the coefficients from regression of an indicator for ever-divorced persons on an indicator for foreign-born individuals, controlling for a quadratic in age and state fixed effects, for the base sample (C) or the ever-married subset (D). A-D: Sample weights are used for all estimates. 95 percent confidence bands based on standard errors clustered at the state level are displayed.

Figure A.3: Divorce Rates by Age



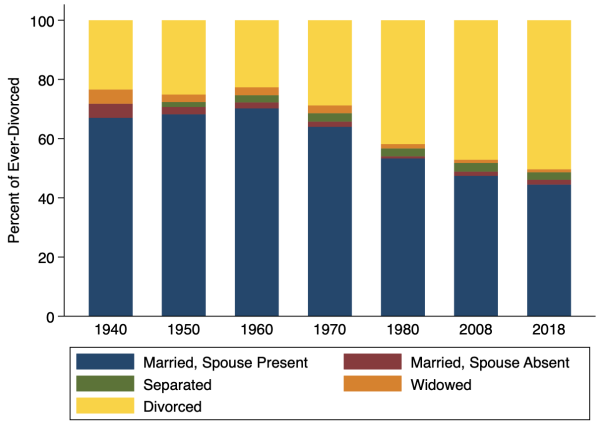
Notes - Data are from the 1940 and 1980 censuses as well as the 2018 ACS (Ruggles et al., 2025). The figures shows the share of US-born non-hispanic white women who are divorced as of 1940 (A), 1980 (B), or 2018 (C), by age.

Figure A.4: Distribution of Demographic Groups



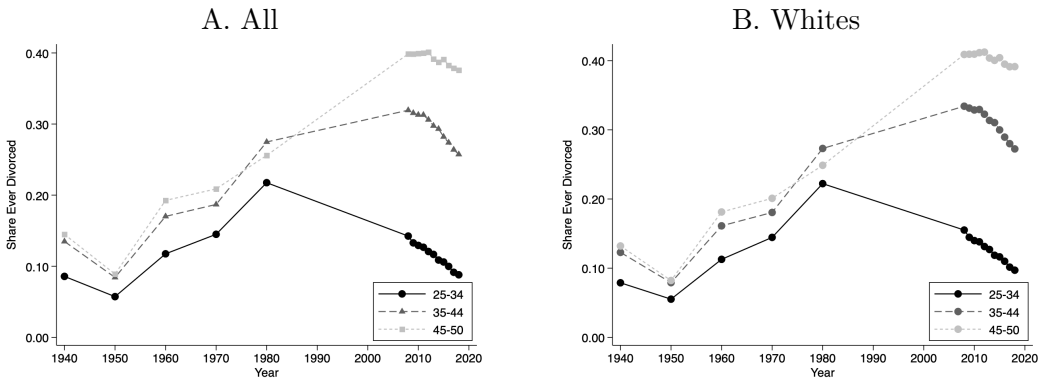
Notes - Data are from the 1980 census (Ruggles et al., 2025). The base population comprises all females in contiguous US. The figures show the share of non-hispanc blacks (A), foreign-born persons (B), young persons (15-24-year-olds) (C), and old persons (51-65-year-olds) in each state.

Figure A.5: Current Marital Status of Ever-Divorced Women



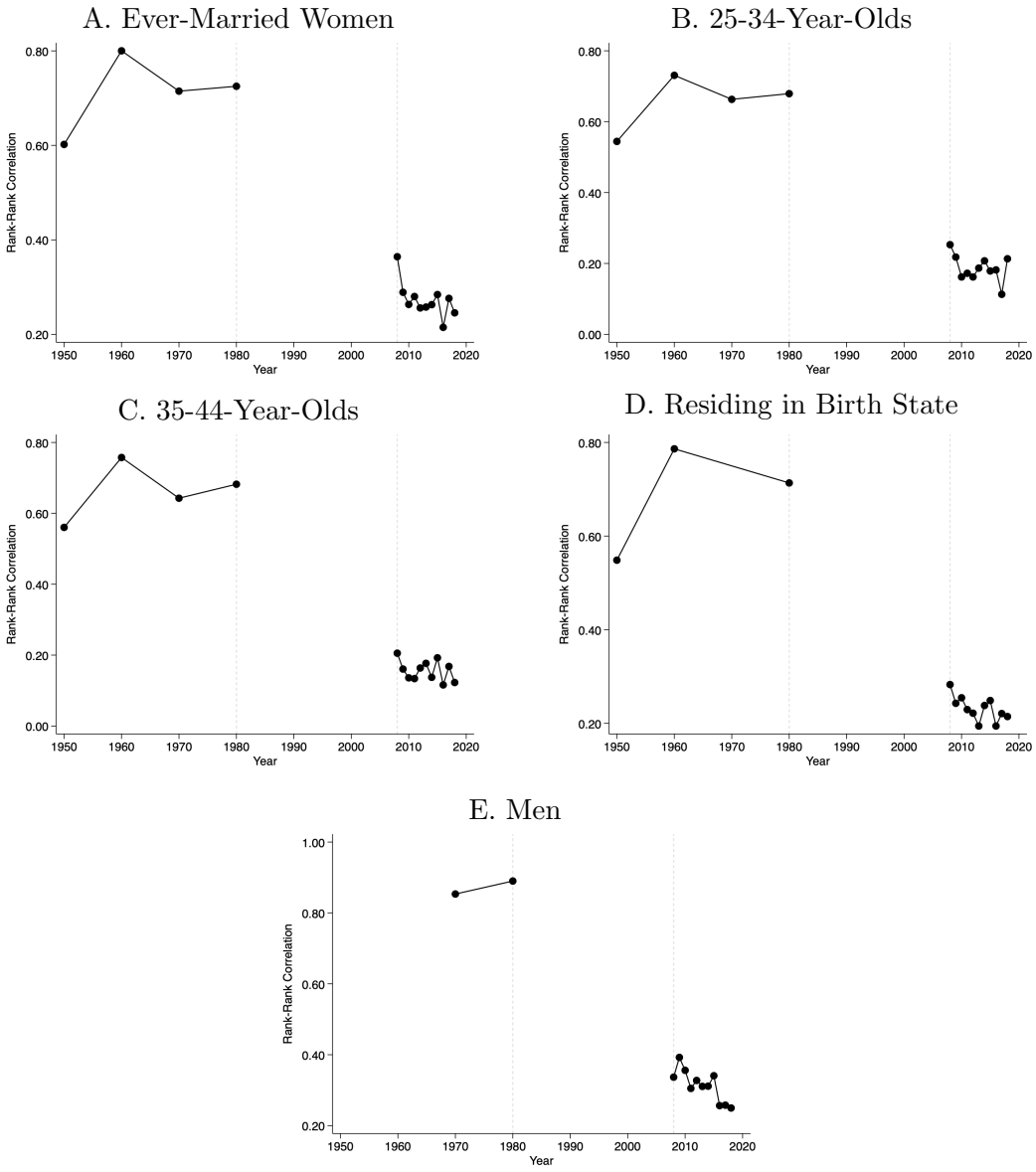
Notes - Data are from the 1940-1980 censuses as well as the 2008 and 2018 ACS (Ruggles et al., 2024, 2025). The figure show the current marital status of ever-divorced US-born non-hispanic white women aged 25-50 (percentage breakdown), by year.

Figure A.6: Ever-Divorced Trends, by Age Group



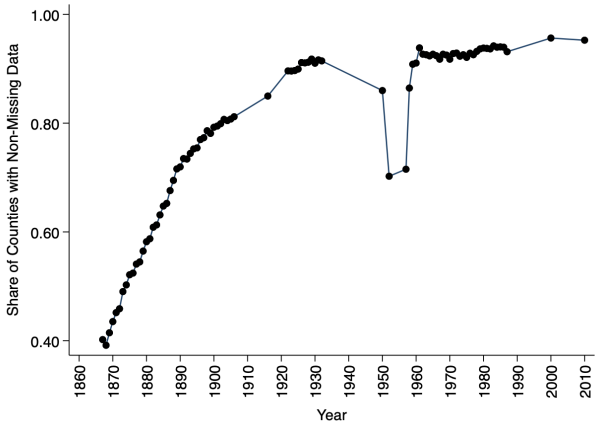
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures show the share of US-born non-hispanic women aged 25-50 who were ever divorced, by age group (25-34, 35-44, 45-50) and year. B further restricts the sample to whites.

Figure A.7: Persistence and Change in Geographic Patterns, Alternative Rank-Rank Correlations



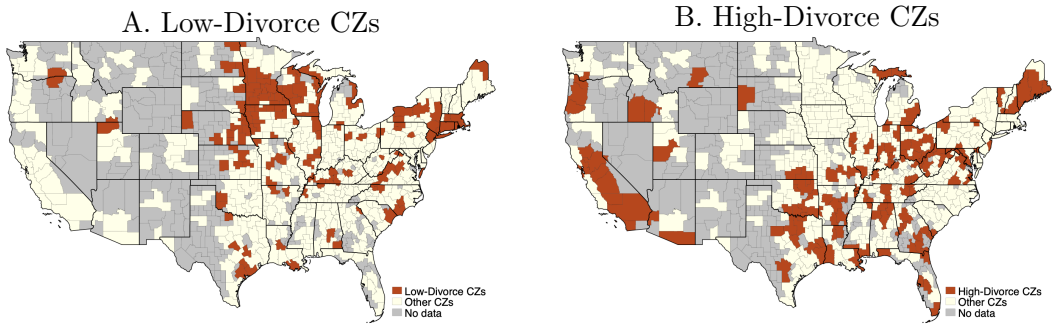
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The baseline for comparison is Figure 3, which displays the CZ-level rank-rank correlations between the share of US-born non-hispanic white women aged 25-50 in each CZ who were ever divorced as of 1940 and the corresponding share in subsequent years. The figures here display the rank-rank correlations under alternative permutations: using ever-married women as the base population instead of all women (A), distinguishing between younger and older age groups (B and C), focusing on those residing in their birth states (D), and redoing the analysis for men (E). The sample sizes range from 593 (C) to 722 (E) CZs.

Figure A.8: Counties with Non-Missing Administrative Divorce Statistics



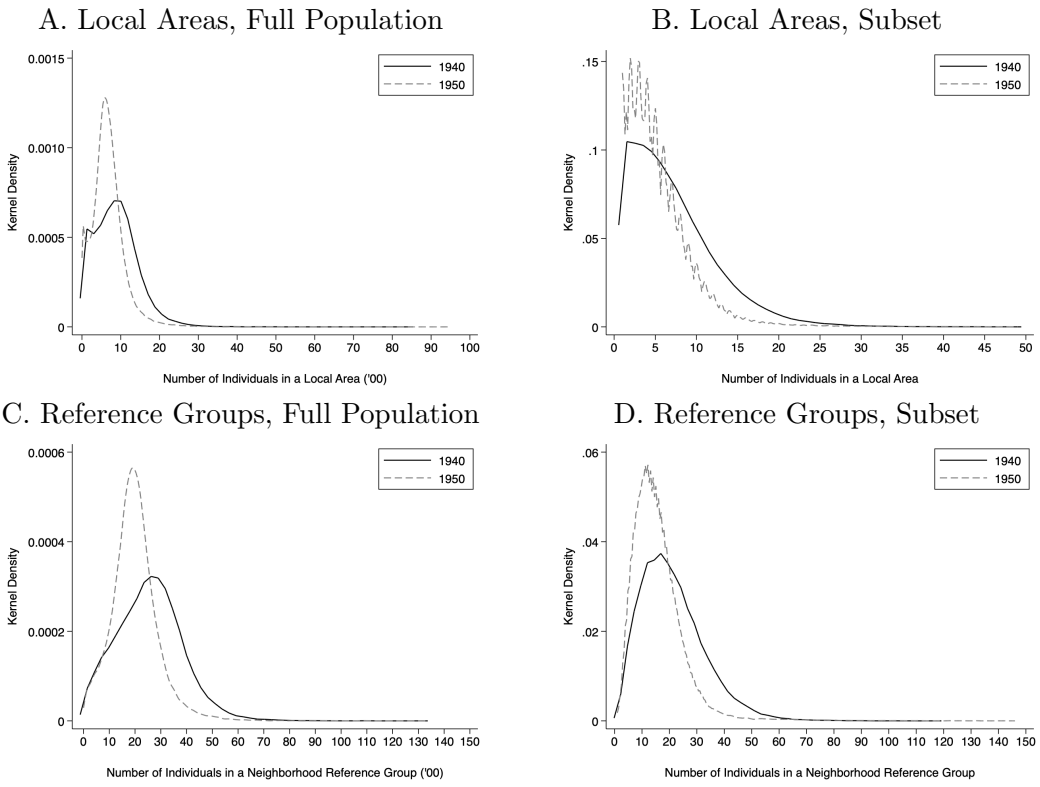
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018), supplemented with divorce statistics from 2000 and 2010 collected by the BGSU Marriage and Divorce Data Compass (2012, 2016), and 1887-1906 statistics digitized from the Bureau of the Census (1908). The figure shows the share of counties with non-missing divorce statistics, by year.

Figure A.9: Low- and High-Divorce CZs



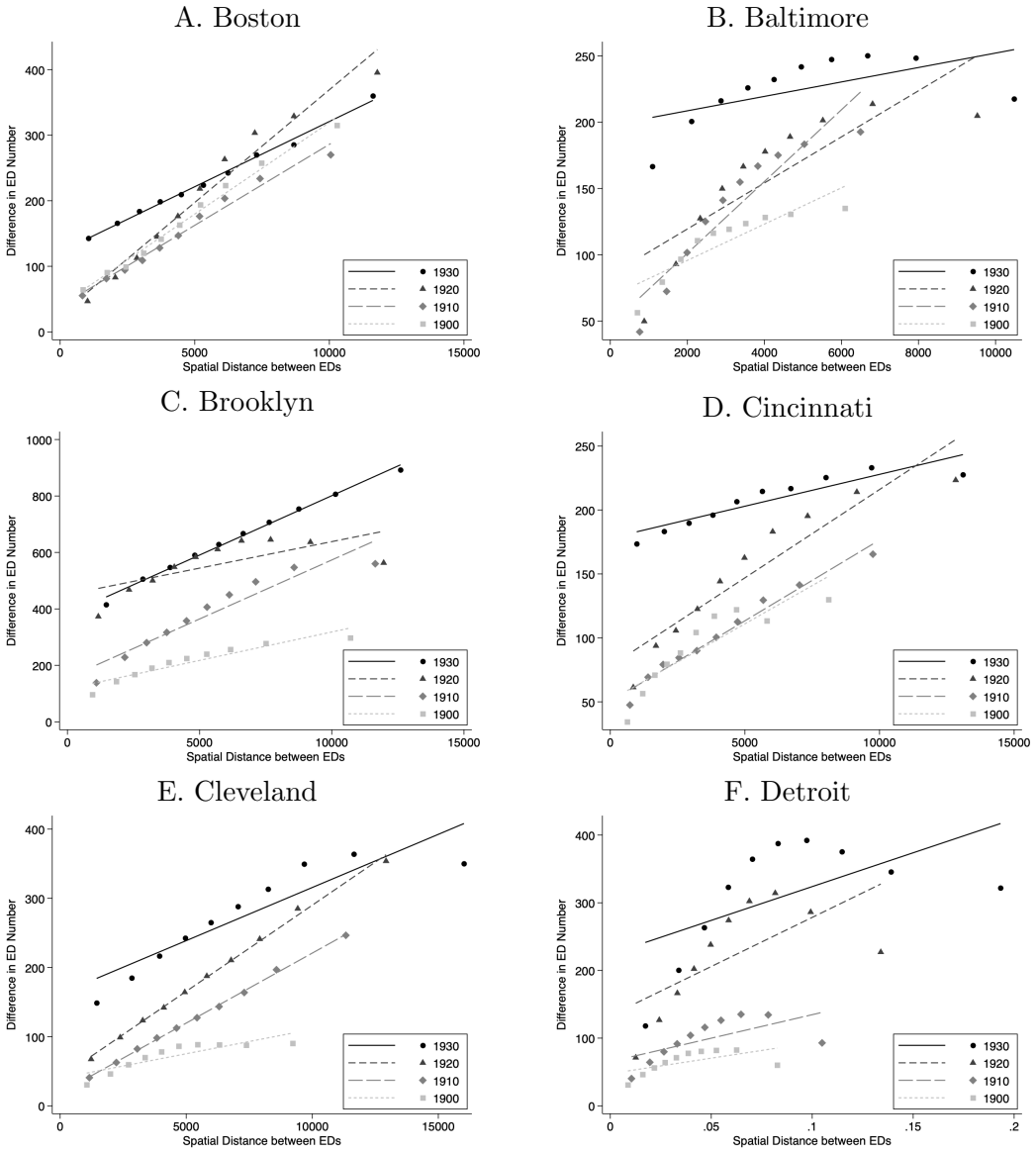
Notes - Data are from the 1940 and 1950 censuses (Ruggles et al., 2024) as well as the IPUMS MLP (Helgertz et al., 2024). The base sample comprises sample-line US-born non-hispanic white women aged 25-40 in the 1940 census who can be linked to the 1950 census, and who remained in the same CZ over this decade interval. Low-divorce CZs are those with below-median ever-divorced rates in both 1940 and 1950; high-divorce CZs are those with above-median ever-divorced rates in both 1940 and 1950. Each figure shows a map of low- (A) or high-divorce (B) CZs. The sample size is 457 CZs.

Figure A.10: Local Areas and Neighborhood Reference Groups, Population Distribution



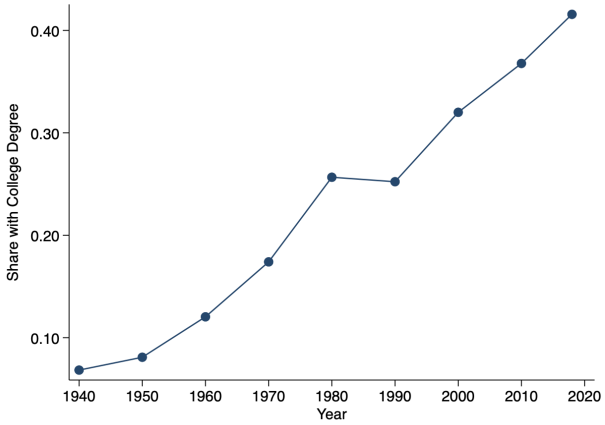
Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024). The figures show the distribution of the number of persons (A and C) or sample-line US-born non-hispanic white women aged 25-50 (B and D) residing in a local area (an ED, A and B) or neighborhood reference group (three nearby EDs, C and D).

Figure A.11: Differences in ED Numbering and Distance



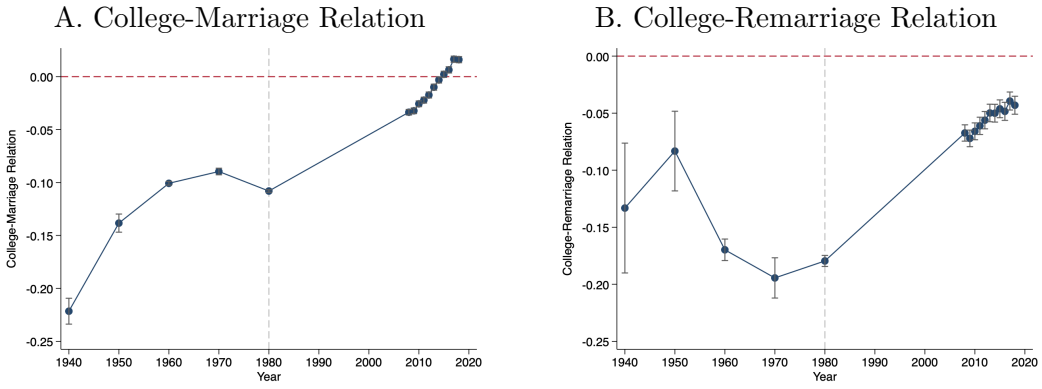
Notes - Data are from the 1900-1930 ED shapefiles for 11 US cities (Shertzer et al., 2016). The figures plot the relation between the difference in ED numbering (y-axis) and the spatial distance between EDs (x-axis), by city and year.

Figure A.12: Trend in College Education



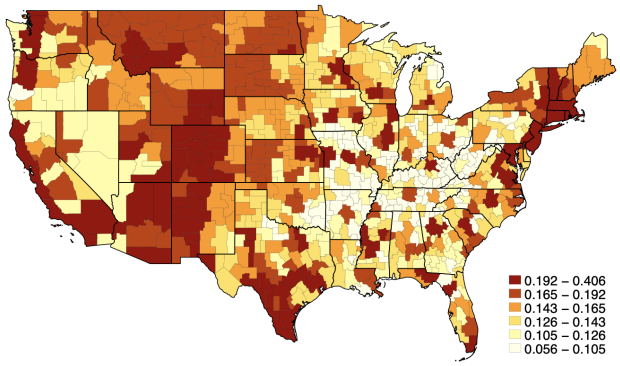
Notes - Data are from the 1940-2000 censuses as well as the 2010 and 2018 ACS (Ruggles et al., 2025). The figure shows the share of US-born non-hispanic whites aged 25-34 with a 4-year college degree or higher, by year. Sample weights are used for all estimates.

Figure A.13: College, Marriage, and Remarriage



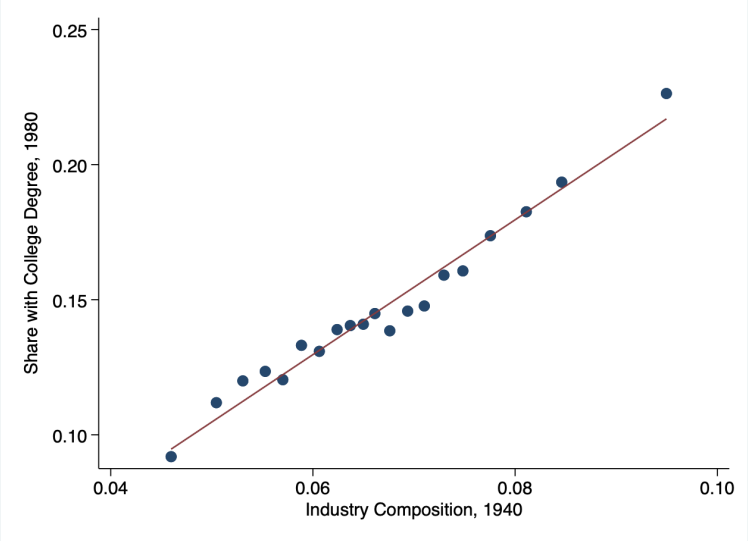
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2025). The sample is restricted to US-born non-hispanic white women aged 25-50 (A) and the subset of ever-divorced women (B). The figures show the coefficients from regressions of an indicator for ever-married (A) or remarried (B) persons on an indicator for those with a 4-year college degree or higher, controlling for a quadratic in age. Sample weights are used for all estimates. 95 percent confidence bands are displayed.

Figure A.14: Geography of Skilled Labor, 1980



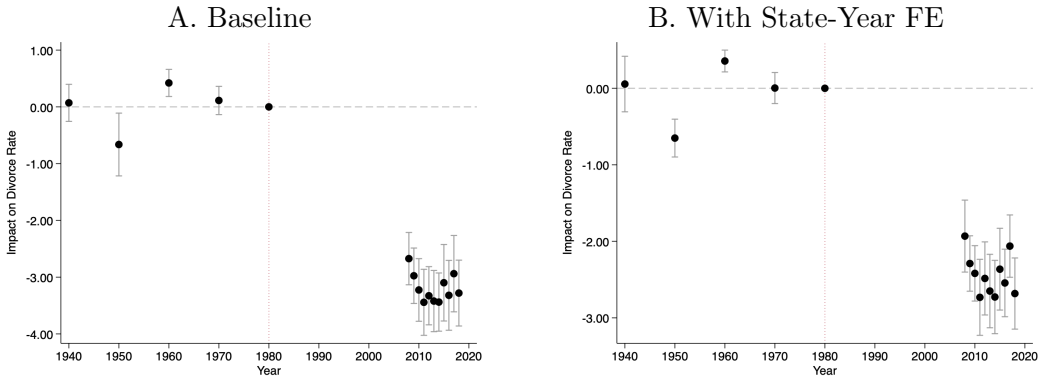
Notes - Data are from the 1980 census (Ruggles et al., 2025). The figure shows the share of US-born non-hispanic white women aged 25-50 in each CZ with a 4-year college degree or higher. The sample size is 679 CZs.

Figure A.15: Instrument Relevance



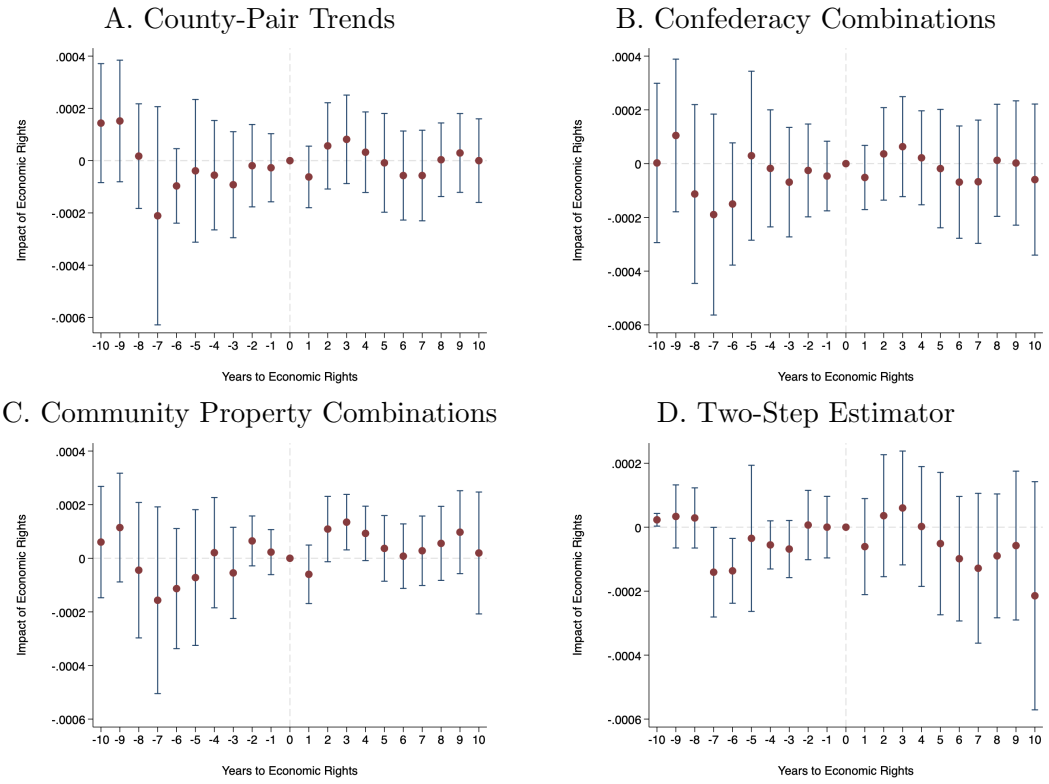
Notes - Data are from the 1940 and 1980 censuses (Ruggles et al., 2024, 2025). The figure shows a binscatter of the share of college graduates in a given CZ as of 1980 (y-axis) against the 1940-based industry instrument (x-axis). The sample size is 679 CZs.

Figure A.16: Impact of College Share on Divorce, Reduced Form



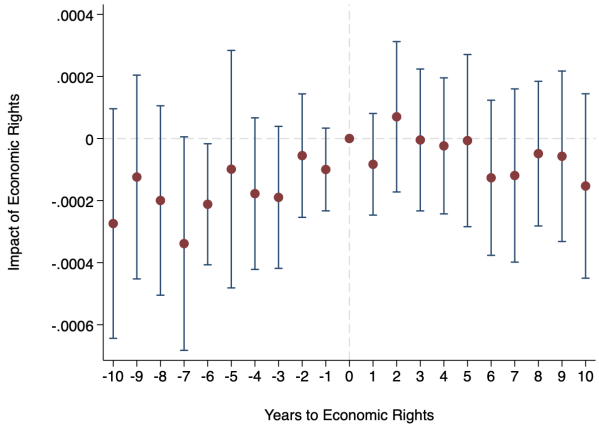
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures show the interaction coefficients from a regression of the share of US-born non-hispanic white women aged 25-50 in each CZ who were ever divorced on the interaction between the 1940-based industry IV and year dummies, controlling for CZ and year fixed effects. B further include state-by-year fixed effects. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 10,864 CZ-year observations.

Figure A.17: Impact of Women's Economic Rights on Divorce, Robustness Checks



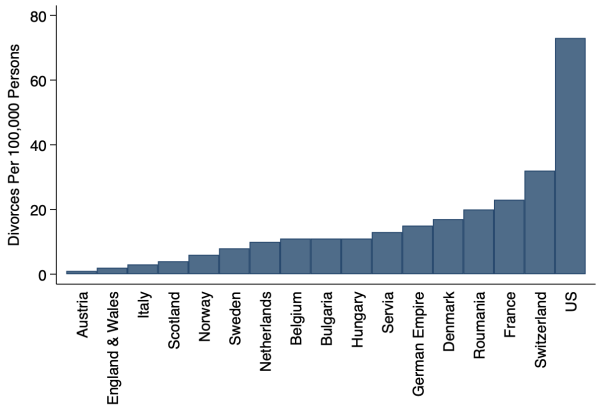
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The unit of observation is the county-year. In the baseline (Figure 10), each marker denotes the coefficient from a regression of the number of divorces per 1,000 women on indicators for the number of years since women's economic rights were effectively granted in the state a county is located in, controlling for three sets of fixed effects: county-pair along a state border, state, and year. A adds county-pair-specific linear trends. B drops counties along the border of a former Confederacy and a non-Confederacy state. C excludes counties on the border of a community property state and the neighboring state. D implements the two-step estimator proposed by Gardner et al. (2025). Confidence bands based on standard errors double-clustered on state and county-pair are displayed. The sample sizes are 67,710 (A and D), 60,416 (B), and 60,040 (C) county-year observations.

Figure A.18: Impact of Women's Property Rights on Divorce



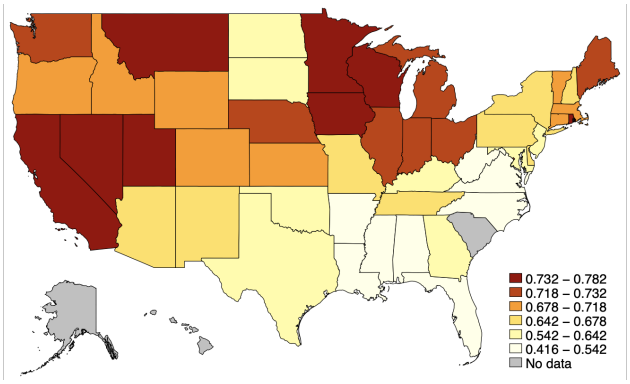
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The unit of observation is the county-year. Each marker denotes the coefficient from a regression of the number of divorces per 1,000 women on indicators for the number of years since women's property rights were effectively granted in the state a county is located in, controlling for three sets of fixed effects: county-pair along a state border, state, and year. Confidence bands based on standard errors double-clustered on state and county-pair are displayed. The sample size is 67,710 county-year observations.

Figure A.19: Historical Divorce Rates, Europe Versus the US



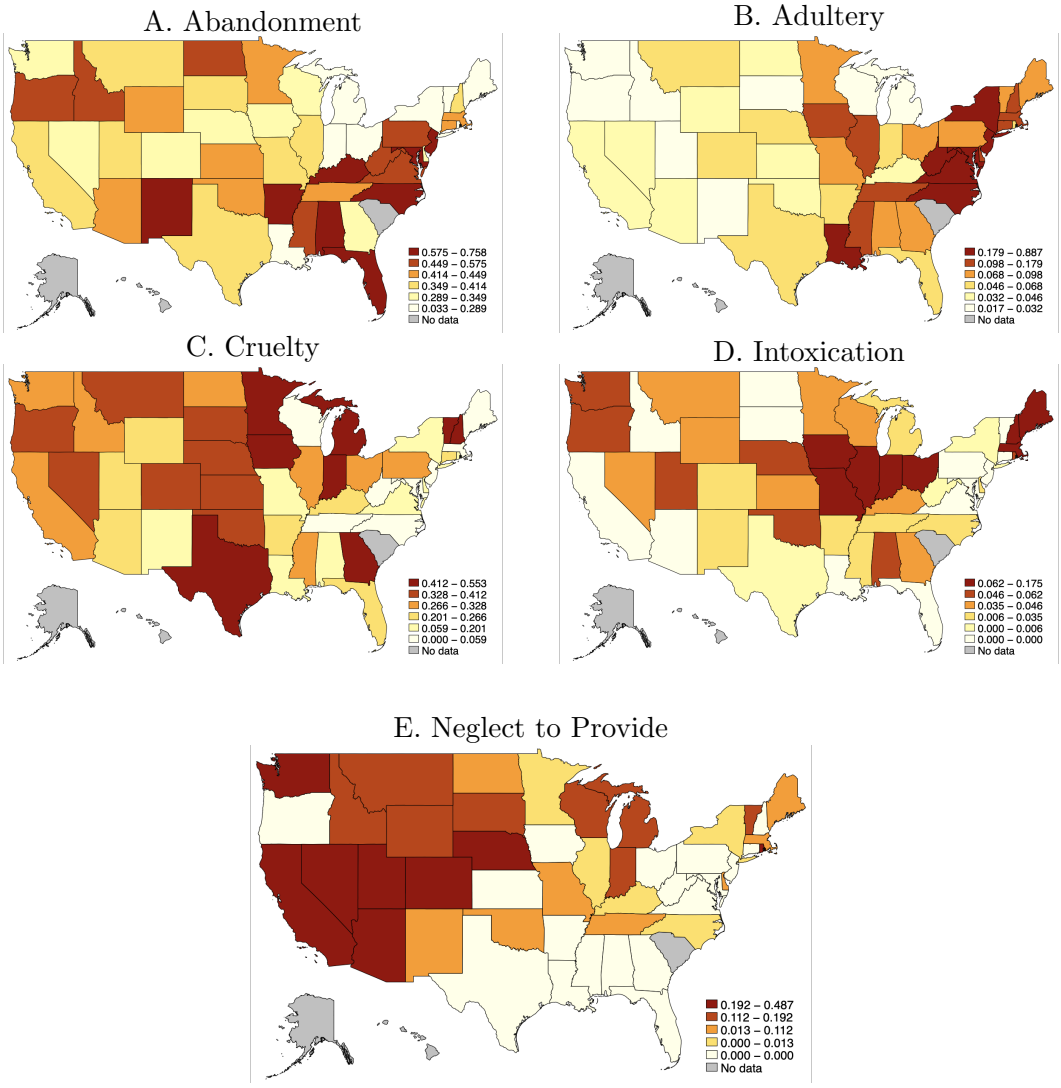
Notes - Data are from the Bureau of the Census (1908). The figure shows the number of divorces per 100,000 persons, for a set of European countries and the US. The statistics for each country are drawn from a single census year, either 1899, 1900, or 1901.

Figure A.20: Share of Divorces Initiated by Wife



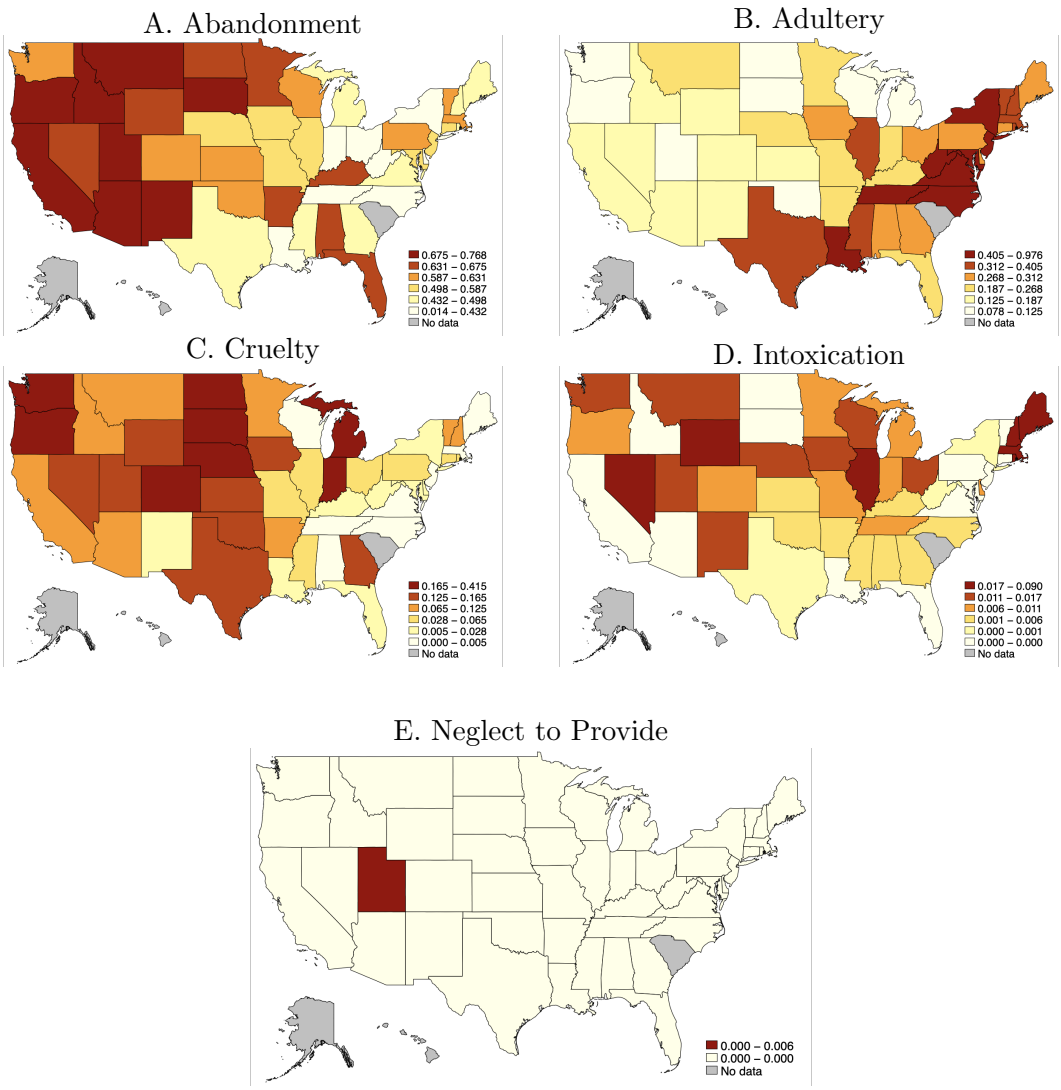
Notes - Data are from the Bureau of the Census (1908). The figure shows the share of divorces in each state that were initiated by the wife.

Figure A.21: Causes of Divorces Initiated by Wife



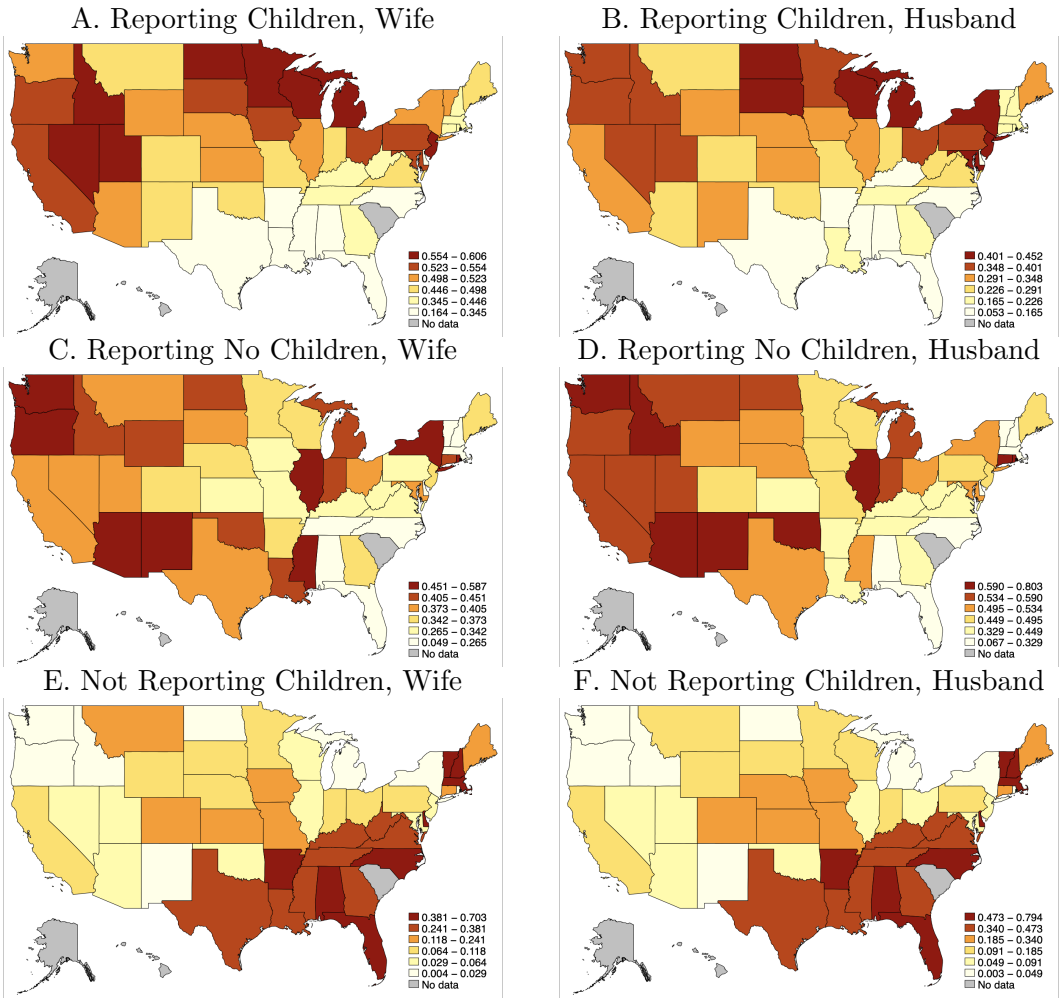
Notes - Data are from the Bureau of the Census (1908). The figures show the share of divorces initiated by the wife in each state that are caused by the factors listed in the subheadings.

Figure A.22: Causes of Divorces Initiated by Husband



Notes - Data are from the Bureau of the Census (1908). The figures show the share of divorces initiated by the husband in each state that are caused by the factors listed in the subheadings.

Figure A.23: Divorces Involving Children



Notes - Data are from the Bureau of the Census (1908). The figures show the share of divorces initiated by the wife (A, C, E) or husband (B, D, F) in each state that report children (A and B), that report no children (C and D), or that do not report children (E and F).

Table A.1: Number of Times Married, Universe

Year/Data	Universe
1940 census	Ever-married sample-line females
1950 census	Ever married sample-line females
1960 census	Ever-married persons, age 14+
1970 census	Ever-married persons, age 14+
1980 census	Ever-married persons, age 15+
2008-2018 ACS	Ever-married persons, age 15+

Notes - Source: Ruggles et al. (2025). The table shows the population universe in each census/ACS enumeration for which information on the number of times married was collected.

Table A.2: Intergenerational Transmission of Divorce Risk, Ever-Married Daughters

Parent	Mother (1)	Father (2)
Parent divorced in 1910	0.073***	0.078***
	(0.012)	(0.026)
Daughter cohort fixed effects	Y	Y
Parent cohort fixed effects	Y	Y
1910 CZ fixed effects	Y	Y
Outcome mean	0.104	0.103
Adjusted R-squared	0.009	0.008
N	238,635	232,626

Notes - Data are from the 1910 and 1940 full count censuses (Ruggles et al., 2024) as well as the Census Tree Project (Price et al., 2023). The base sample comprises 0-18-year-old US-born non-hispanic white daughters in the 1910 census who can be linked forward to 1940. Only ever-married sample-line respondents in 1940 are kept. Column 1 looks at the relation between mothers and daughters while column 2 looks at the relation between fathers and daughters. Each cell shows the coefficient from a regression of an indicator for daughters who were ever-divorced by 1940 on a dummy for having a parent who was divorced in 1910, controlling for the daughter's cohort, the parent's cohort, and the CZ of residence in 1910. Standard errors are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table A.3: Impact of Frontier Conditions on Divorce, Unbundling Frontier Isolation

	1-Year		3-Year-Avg		5-Year-Avg	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Unbalanced Panel			
Near frontier line	0.193**	0.151	0.367***	0.263***	0.354***	0.213**
	(0.079)	(0.103)	(0.074)	(0.095)	(0.088)	(0.104)
Low population density	0.331**	0.331**	0.702***	0.628***	0.696***	0.618***
	(0.142)	(0.140)	(0.104)	(0.100)	(0.085)	(0.092)
Adjusted R-squared	0.268	0.282	0.319	0.345	0.331	0.351
Outcome mean	1.15	1.15	1.13	1.13	1.13	1.13
N	4,284	4,284	4,687	4,687	4,792	4,792
			Panel B: Balanced Panel			
Near frontier line	0.189**	0.128	0.352***	0.237**	0.389***	0.236**
	(0.082)	(0.106)	(0.075)	(0.099)	(0.092)	(0.109)
Low population density	0.416***	0.406***	0.752***	0.675***	0.707***	0.633***
	(0.146)	(0.139)	(0.112)	(0.106)	(0.089)	(0.095)
Adjusted R-squared	0.245	0.262	0.297	0.327	0.297	0.321
Outcome mean	1.10	1.10	1.08	1.08	1.08	1.08
N	4,088	4,088	4,460	4,460	4,550	4,550
State-by-year fixed effects		Y		Y		Y

Notes - Data are from Bazzi et al. (2020), the BGSU Marriage and Divorce Data Compass (2018), and the Bureau of the Census (1908). The unit of observation is the county-year. Each cell shows the coefficient from a regression of the number of divorces per 1,000 women on an indicator for counties within 100 km of the frontier line (near frontier line) and an indicator for counties with less than six people per square mile (low population density), controlling for county and year fixed effects. Even columns further include state-by-year fixed effects. Columns 1-2 measure the outcome in a single year (1870, 1880, 1890); columns 3-4 average the outcome across 3-year intervals (1869-1871, 1879-1881, 1889-1891); columns 5-6 average the outcome across 5-year intervals (1868-1872, 1878-1882, 1888-1892). Panel A uses an unbalanced panel of counties; Panel B uses a balanced panel. Standard errors clustered by county are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table A.4: Long-Run Impact of Frontier Conditions on Divorce

	(1)	(2)	(3)	(4)	(5)
	Panel A: Share Ever-Divorced in 1940				
Total frontier experience (decades)	0.002	-0.001	0.000	-0.002	0.000
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Estimator	OLS	OLS	OLS	2SLS	2SLS
Geoclimatic controls		Y	Y	Y	Y
State fixed effects			Y	Y	Y
Outcome mean	0.092	0.092	0.092	0.092	0.092
Adjusted R-squared	0.002	0.102	0.244		
First stage F-statistic				554	507
N	1,730	1,730	1,730	1,730	1,730
	Panel B: Share Ever-Divorced in 1950				
Total frontier experience (decades)	0.003***	0.003***	0.004***	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Estimator	OLS	OLS	OLS	2SLS	2SLS
Geoclimatic controls		Y	Y	Y	Y
State fixed effects			Y	Y	Y
Outcome mean	0.064	0.064	0.064	0.064	0.064
Adjusted R-squared	0.010	0.057	0.131		
First stage F-statistic				472	446
N	1,519	1,519	1,519	1,519	1,519

Notes - Data are from Bazzi et al. (2020), the BGSU Marriage and Divorce Data Compass (2018), and the Bureau of the Census (1908). The unit of observation is the county. Each cell shows the coefficient from a regression of the share of US-born non-hispanic white women aged 25-50 in a county who were ever divorced by 1940 (Panel A) or 1950 (Panel B) on total frontier experience (expressed in decades). Column 2 adds the following geoclimatic county controls: latitude, longitude, area, rainfall, temperature, elevation, potential agriculture yield, and distance to rivers, lakes, and the coast. Column 3 includes state fixed effects. Column 4 instruments total frontier experience with the log of the average national annual migration inflow in the 30 years subsequent to the frontier arriving within 110 km of the county. Column 5 instruments total frontier experience with annual migration inflows predicted by weather shocks in Europe. Standard errors clustered by state are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table A.5: Correlation Between Immigration Flows and GDP Growth

	End Period					
Base Period	1860s	1870s	1880s	1890s	1900s	1910s
1850s	-1	-0.603	0.119	0.235	0.317	0.172
1860s		-1	0.715	0.650	0.580	0.436
1870s			1	0.983	0.852	0.556
1880s				1	0.820	0.470
1890s					1	0.244
1900s						-1

Notes - Data are from Sequeira et al. (2019). Each cell shows the correlation between the aggregate immigration inflow to the US and US GDP growth between two periods. The base periods are given in the left-most column. The end periods are shown in the column headings.

Table A.6: Impact of Immigration on Divorce, Weather-Predicted Flows

	Unbalanced Panel			Balanced Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
	1-year	3-year-avg	5-year-avg	1-year	3-year-avg	5-year-avg
Relative change in immigrants	0.005	0.003	0.002	-0.010	0.000	-0.002
	(0.004)	(0.003)	(0.003)	(0.010)	(0.004)	(0.004)
Estimation	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Railway access in base year	Y	Y	Y	Y	Y	Y
Outcome mean	0.001	0.001	0.001	0.001	0.001	0.001
First stage F-statistic	16.1	11.9	12.9	4.31	7.08	8.96
N	5,085	5,813	6,006	3,540	4,434	4,704

Notes - Data are from Sequeira et al. (2019), the BGSU Marriage and Divorce Data Compass (2018), and the Bureau of the Census (1908). The unit of observation is a county-year. Each cell shows the coefficient from a regression of the change in the number of divorces in a county scaled by the corresponding number of women in the base year on the change in the number of foreign-born in a county scaled by the corresponding population in the base year, controlling for an indicator for railway access in the initial year as well as county and year fixed effects. The explanatory variable is instrumented by the interaction between an indicator for railway access in the base year and the inflow of European immigrants predicted by weather shocks in Europe in the ensuing decade. Changes are measured between the base year (1870, 1880, 1890) and the end year (a decade later). Divorce in columns 1 and 4 is measured using single years (1870, 1880, 1890); divorce in columns 2 and 5 is averaged across 3-year intervals (1869-1871, 1879-1881, 1889-1891); divorce in columns 3 and 6 is average across 5-year intervals (1868-1872, 1878-1882, 1888-1892). Columns 1-3 use an unbalanced panel of counties; columns 4-6 use a balanced panel. Standard errors clustered by county are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.