Divorce: The Geography of an American Tradition

Hui Ren Tan*

Abstract

Until 1980, the geography of divorce across the US was relatively stable, with higher rates in the West, industrial Midwest, Florida, and Maine. These patterns were sustained by selective migration as well as place effects through social interactions. The divorce landscape can be traced back to the 19th century. Frontier conditions contributed to these patterns in the early years. After 1980, the geography of divorce shifted, concentrating in the South. This was driven by the rising return to skill—areas with more skilled workers in 1980 experienced relative declines in divorce over the following decades.

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*Department of Economics, National University of Singapore, email: huiren@nus.edu.sg

1 Introduction

Today, some 40 percent of marriages in America will end in divorce (Bailey et al., 2025). The repercussions of marital dissolution are far-reaching. At separation, both parties become more anxious and depressed, effects that linger well-after the couple has parted (Dasgupta et al., 2025). Parental divorce also lowers children's adult earnings and college residence while 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 new divorces in the US over the past 150 years, showcasing the dramatic transformation of American family structure.[1] For much of history, divorce has been on a steady ascent, with a temporary spike after World War II (WWII) followed by a larger sustained acceleration from the late 1960s, before trending downward after 1980. Given its prevalence and consequences, a large literature in economics has sought to understand the forces driving the trends in divorce (Stevenson and Wolfers, 2007). This paper focuses on a hitherto unexplored feature beneath these trends: geography. Across the country, divorce is more prevalent in some areas than others. The period of rapidly rising divorce from the 1960s was characterized by a relatively stable geographic pattern of divorce dating back to the dawn of modern US history. In contrast, the era of declining divorce after 1980 was accompanied by a shift in the spatial distribution of divorce. What explains the geography of divorce and its changing nature?

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 me to identify women who had ever been divorced. From these data, I can then map the prevalence of divorce across different parts of the US, covering a span of almost 80 years. My findings are twofold. First, a relatively consistent pattern is 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. Second, moving forward to 2008-2018 reveals a markedly different picture, where divorce is now concentrated in the South. The story is thus one of initial persistence and subsequent change.

What explains the landscape of divorce between 1940-1980? Put differently, why is divorce more widespread in some parts of the country but less common in others? One possible explanation is selective migration, where those who have been divorced or who have a greater propensity for divorce are systematically more likely to move to particular locations. I test for such dynamics using a large sample of women linked between the 1940 and 1950 full count censuses, the only enumerations with the information and size needed for this analysis. Among women starting out in low-divorce areas, those who have gone through a divorce are 2 to 5 percentage points more likely to migrate to high-divorce areas, about 25 to 47 percent of the mean divorce rate. This direction of selection would reinforce the geographic patterns of divorce. On the other hand, no

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

consistent migration patterns are observed among persons initially residing in high-divorce areas. Selective migration *into* high-divorce areas, rather than selective migration *out of* these places, thus contributed to the prevalence of divorce in some locations but not others.

Beyond selective migration, I consider another mechanism: whether living in a place where divorce is more (less) widespread changes a person's behavior, raising (lowering) one's own propensity for divorce. That is, are there place effects? Such effects could come about through social interactions among residents living close by, where those who have gone through a divorce influence others that have not. To investigate this channel, I adapt the spatial proximity approach in Bayer et al. (2008) by examining the likelihood that a pair of neighbors have both experienced a divorce, comparing neighbor-pairs that reside in the same versus nearby area. I find substantial social interaction effects at the neighborhood level: neighbor-pairs in the same area are 0.1 to 0.3 percentage points more likely to both have been divorced, about 10 to 15 percent of the outcome mean.[2] The cumulative impact of such interactions would be greater in places with more divorcees to begin with, reinforcing existing geographic patterns of divorce. Complementing this horizontal transmission in divorce propensity, I also document a vertical transmission in divorce behavior from one generation to the next among whites. Since a majority of individuals remain where they grew up, the intergenerational transmission of divorce propensity will further strengthen the existing landscape of divorce. Finally, I ask if the divorce environment one is exposed to as a child affects the likelihood of divorce in adulthood, tying in with the literature on childhood environment (Chetty et al., 2016; Chetty and Hendren, 2018; Chetty et al., 2020; Chyn, 2018). Comparing siblings born before and after their households migrated from a low- to high-divorce region, I observe similar divorce propensities for the two sets of siblings when they grow up. Childhood exposure effects on divorce are thus unlikely to be a key mechanism.

Having examined the geography of divorce between 1940-1980, a natural question is what things were like further back in time. Drawing on various historical sources including published reports ordered by Congress, I reconstruct the landscape of divorce before 1940, going back to the 1860s at the dawn of modern US history. Interestingly, the broad patterns observed between 1940-1980 can be found almost a century earlier. The root causes underlying the geography of divorce thus precede a wide range of events, from industrial growth in the Gilded Age, to the passage of the 19th Amendment, the prosperity of the Roaring Twenties, and the Great Depression, among other things. What then shaped the map of divorce at such an early stage of American society? During the 19th century, the US was rapidly expanding westward and settling new land. Exploiting the movement of the western frontier, I show that frontier conditions increased the prevalence of divorce at the time. However, these effects did not persist after the frontier closed, paving the way for selective migration and place effects to take over and sustain the geographic patterns into later decades. I find that other contemporaneous factors like the expansion of women's economic rights, the influx of immigrants, and differences in state divorce legislation are unlikely to account for the

[2]This gap can be attributed to social interactions under the assumption that residents do not sort across nearby areas. I discuss the validity of this assumption in the analysis that follows.

patterns of divorce in the 19th century.

After diving deep into America's past, I then move forward in time to investigate the changes after 1980, when declining divorce rates were accompanied by a shift in the geography of divorce—what caused this transformation in spatial patterns? I propose that the growing return to skill played a role. The college wage premium rose steeply from the 1980s, a phenomenon associated with worsening economic circumstances for unskilled workers (Acemoglu and Autor, 2011). Such adverse effects can increase the likelihood of divorce (Banzhaf, 2018; Charles and Stephens, 2004; Jackson et al., 2017). This leads to the following prediction: after 1980, the geography of divorce should reorientate around areas with more unskilled workers, the group worse-hit by the changing value of skills. I show that places with relatively more (less) graduates in 1980 indeed experienced greater declines (increases) in divorce over the ensuing decades. Importantly, no systematic associations are detected before 1980, allaying concerns over pretrends. My findings are also robust to applying an instrumental variable (IV) approach, using the local industry composition in 1950 to predict the college share in 1980.

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 examined, 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 state after state abandoned fault-based systems, passing unilaterial divorce laws that allowed people to seek a divorce without the consent of their spouses. Friedberg (1998) finds that these laws accounted for a sixth of the rise in divorce rates since the 1960s. Wolfers (2006), however, contends that the laws had 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. As I show below, the geography of divorce matters—past spatial patterns are highly predictive of subsequent trends in divorce.

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 mobility for sons (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 of young men (Autor et al., 2013, 2019). Labor markets were polarized by the introduction of computers, which adversely affected workers performing routine job tasks (Autor and Dorn, 2013). More recently, the emergence of Generative AI has been a double-edged sword, leading to wage gains for younger and more educated workers, but reducing employment in sectors where AI can directly substitute for human labor (Johnston and Makridis, 2025). I investigate how the rising labor market return to skill changed the geography of divorce, a relation that has not been explored.

2 The Geography of Divorce

I begin by documenting the geography of divorce over the last century. While these spatial patterns have been explored in existing work, the analysis is often limited to state-level variation (Glass and Levchak, 2014; Manning et al., 2014; Westrick-Payne et al., 2022) or narrow timeframes, whether in the past (Bureau of the Census, 1908) or present (Payne and Manning, 2018; Manning et al., 2016). My paper provides a more comprehensive survey of the divorce landscape, examining a finer level of geography across a wider span of almost 80 years.

2.1 Data

To trace the geographic patterns of divorce over the long-run, I draw on US census and ACS data, available from IPUMS (Ruggles et al., 2024, 2025).[3] The census/ACS records contain a wide range of socioeconomic characteristics for each respondent. This includes an individual's current marital status, which can be used to identify divorcees at a *given point in time*. However, this measure understates the true prevalence of divorce as a majority of divorcees later remarry (Stevenson and Wolfers, 2007; Wolfers, 2006). Fortunately, in a number of 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. Such details are primarily available for sample-line females in the 1940-1950 censuses as well as all respondents in the 1960-1980 censuses and the 2008 ACS onward (Table A.1).[4] Because the universe of interest is much smaller in 1940-1950, I use the full count censuses for those decades, whilst relying on the IPUMS samples for the remaining years.[5] Finally, I end my study period in 2018 to avoid potential complications arising from the COVID-19 pandemic. My analysis thus spans 1940-1980 and 2008-2018.

[3] While other sources of local divorce statistics are available for the 20th and 21st centuries, these tend to pool all divorces together regardless of demographics, making it difficult to account for changes in the local population composition. In contrast, the demographic characteristics recorded in the census/ACS data allow one to measure divorce for a consistently defined population of interest.

[4] The 1910 census also asked about the number of times married. However, as I note below, this information is only available in the 1 percent sample and not the full count, leaving a much smaller dataset to work with compared to later decades. I return to the 1910 census again in the subsequent analysis.

[5] In the 1940 census, two lines on each census page of 40 individuals are designated as sample lines, comprising 5 percent of the population. In the 1950 census, every fifth line on a census page of 30 individuals 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.

With the necessary data, I then define my measure of divorce. My primary target population are US-born non-hispanic women aged 25-50 in each census/ACS. 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] This serves as my primary divorce measure. Figure A.2 breaks down the ever-divorced population by current marital status.[7] Some 32 to 88 percent of ever-divorced women have remarried, with the proportion increasing as one goes further back in time. Current marital status alone would thus understate the extent of divorce, reinforcing the importance of using information on *both* current marital status and the number of times married.

For comparison with the more conventional measure of new divorces in Figure 1A, Figure 1B plots the share of ever-divorced women since 1940.[8] For the general population (solid line, Figure 1B), I observe a rise in ever-divorced rates over the 20th century, similar to the trend in new divorces. 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 different 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] Stratifying the population by race, I find a comparable trajectory for whites (dashed line, Figure 1B); among blacks, the initial rise in divorce is less pronounced while the subsequent decline happens earlier (dotted line, Figure 1B), consistent with Stevenson and Wolfers (2007).[10]

2.2 Spatial Patterns

Taking my ever-divorced measure, I construct the geography of divorce in two steps. In the first step, I fix the unit of analysis. 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).[11] For greater accuracy, I restrict my analysis to CZs with at least 50

[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, an assumption that is well-supported in real-world data (Wolfers, 2006).

[7] By definition, none of the ever-divorced population in 1940 are currently separated. This is because the "separated" classification was only introduced in 1950; before that, persons in this category 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 uses a mix of data rather than a consistent source (census/ACS). 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.3 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. This is evident for the population as a whole and by race.

[10] The earlier decline in ever-divorced rates for blacks is driven by the younger age group, as shown in Figure A.3.

[11] The matching to CZs is done in three steps. First, I aggregate counties in the 1940 and 1950 full count to CZs by adapting the county-to-CZ correspondence in Chetty and Hendren (2018). Second, I match PUMAs in the 1960 sample to CZs using the crosswalks developed by Evan Rose. Third, I convert county groups in the 1970 and 1980

sample-line persons from the target population by race, as measured in the 1940 full count. This leaves a baseline set of 679 CZs for whites and 235 CZs for blacks. In the second step, I compute the share of women in each CZ who have ever been divorced.

Figures 2 and 3 map the geography of divorce for whites and blacks across 1940-2018.[12] I start with the patterns for whites (Figure 2). From 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.[13] By the 21st century, however, a different pattern emerges, with a greater prevalence of divorce in the South.[14] For blacks (Figure 3), the geographic scope is much narrower due to their concentration in the South and subsequent Great Migration to other regions, primarily the Northeast. Nonetheless, over 1940-1980, I continue to find higher divorce rates stretching from the industrial Midwest southwestward to Texas, as well as in Florida. Thereafter, the distribution becomes more dispersed across the South. A similar narrative is thus evident for both whites and blacks: relative stability in the geography of divorce from 1940-1980, followed by a shift sometime between the late-20th and early-21st centuries.

To capture the initial persistence and subsequent change in the landscape of divorce, I compute the rank-rank correlation between the spatial patterns of divorce in 1940 and the corresponding distribution in later census/ACS years.[15] Figure 4 plots these correlations, by race. The associations are relatively high between 1940-1980, typically hovering over 0.6—this indicates substantial persistence in the geography of divorce, consistent with Figures 2 and 3. Moving into the 21st century, the rank-rank correlations drop sharply to around 0.2 for whites and even lower for blacks, 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.4A), distinguishing between younger and older age groups to check for age effects (Figures A.4B and C), focusing on those residing in their birth states to limit the influence of internal migration (Figure A.4D), and doing a similar analysis for men in the subset of years that have the necessary information (Figure A.4E).[16]

samples as well as PUMAs from the 2008 ACS onward to the respective CZs using the crosswalks from Autor and Dorn (2013). In all cases, I use the 1990 vintage of CZ definitions.

[12] 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.

[13] 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 distorted the spatial patterns of divorce.

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

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

[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 target populations 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 for all residents—only five states have completely identifiable records (Ruggles et al., 2025). Third, for men, the earliest year with information on the number of times married is 1960 (Table A.1). My starting point for this permutation is thus a comparison between 1960 and 1970. With men, the shift in geographic patterns appears to be less stark among blacks.

2.3 Relevance

Why study the geography of divorce? While a large literature in economics has sought to understand the trends in divorce, the role of geography has largely been neglected. I argue that the spatial patterns of divorce matter because they are highly predictive of subsequent changes in divorce rates, both the rise in the second half of the 20th century and the fall thereafter. To illustrate this, I run a simple regression using the census/ACS data:

$$Divorce_{zt} = \sum_{j=1950}^{2018} \beta_j \; Divorce_{z,1940} \;\; \text{x} \;\; 1[t = j] + \theta_z + \theta_t + \varepsilon_{zt} \tag{1}$$

where the subscripts z and t denote a CZ and year, respectively. The outcome, $Divorce$, is the share of US-born non-hispanic women aged 25-50 in a given CZ and year who have ever been divorced (Figures 2 and 3). The main explanatory variable is the interaction between the corresponding share of ever-divorced women in 1940 and a vector of year dummies. Also included are CZ and year fixed effects, the θs. The β parameters then capture the relation between the local prevalence of divorce in 1940 and the divorce rate in later years. This setup does not imply causality—the goal is simply to document the association between past spatial patterns and future trends.

Figure 5 plots the β coefficients from regression (1) for whites and blacks, with 1950 as the omitted year.[17] I find that places where divorce was more widespread in 1940 are the areas that experienced a steeper rise in divorce rates through the 1960s-1980s, as depicted by the positive point estimates. This is particularly evident for whites. Such places are also where divorce subsequently fell more sharply in the 21st century, as indicated by the negative point estimates across 2008-2018. The historical geography of divorce thus predicts both the rise and fall in divorce over the ensuing decades. Any attempt to fully understand the trends in divorce must thus contend with its distribution across space.

2.4 Summary

Circling back to the evolution in new divorces depicted in Figure 1A, this section highlighted two lesser-known facts. First, the period of rapidly rising divorce rates from the 1960s-1980s was associated with a relatively stable geography of divorce, with higher rates in the West, industrial Midwest, Florida, and Maine. Second, the decades of falling divorce after 1980 coincided with a shift in the landscape of divorce, with the South emerging as a divorce hotspot. In the remainder of this paper, I investigate both the factors behind the consistent geographic patterns in the 20th century and the forces driving the changes into the 21st century.

[17] Figure A.5 shows similar patterns when using ever-married women as the base population.

3 1940-1980: A Persistent Geography of Divorce

What explains the geographic differences in divorce rates between 1940-1980, a pattern that appears to be relatively persistent (Figure 4)? I explore two potential mechanisms. The first is selective migration, where divorcees or persons with a greater propensity for divorce are attracted to particular locations, shaping the geography of divorce in the image of its inhabitants. The second is how living in a given locality changes a person's behavior, namely, the likelihood of divorce—I refer to this as "place effects" for ease of exposition. This section examines each mechanism in turn.

3.1 Selective Migration

Can selective migration explain the geography of divorce from 1940-1980? To shed light on this, information on a person's origin, destination, and pre-move characteristics is needed. One can then check if individuals who have previously gone through a divorce are more likely to move to high-divorce areas, and vice versa. This direction of selection reinforces the landscape of divorce.

I use the 1940 and 1950 full count censuses to assess the role of selective migration. Why these years? To track the movement of people over time in a historical setting, individuals need to be linked across different census records—this can only be done with the full counts, which are available up to 1950.[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. These factors confine my analysis to 1940-1950. From these 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 women aged 25-40, some of whom have gone through a divorce while others have not.[19] Second, I link these individuals forward to 1950, using the census linkages created by Helgertz et al. (2024).[20] Comparing one's location of residence in 1940 and 1950 allows me to distinguish between stayers (same CZ) and migrants (different CZs).

Next, I divide the 1940-1950 linked sample into two groups, based on where a person lived in 1940. The first group comprises those who started in *low-divorce* CZs—by 1950, some of them 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

[18] The census full counts are released 72 years after a given enumeration. These contain the names of respondents that can be used to link them across historical census records. Such information is not available in the IPUMS samples from 1960. In theory, one could also exploit differences in the birth and current state of respondents to identify internal migrants, without the need for linking. 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.

[20] To construct their census links, 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 link 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 way Bazzi et al. (2020) setup their study on migrants to and from frontier counties.

continue on in the same CZ (stayers) while their counterparts would have moved to *low-divorce* CZs (migrants).[22] To operationalize this division, a precise definition of low- and high-divorce CZs is required. For parsimony, I consider CZs with below-median ever-divorced rates to be low-divorce CZs and vice versa for high-divorce CZs, separately for each race.[23] These ever-divorced rates can be measured with the 1940 or 1950 census, for a different set of (sample-line) individuals. I thus use both censuses to create alternative definitions of low- and high-divorce CZs, henceforth referred to as the "1940 definition" and the "1950 definition."[24]

Having described my setup, I then compare the ever-divorced status of migrants and stayers in 1940, prior to any moves:

$$y_i = \alpha + \gamma \; Migrant_i + FE + u_i \tag{2}$$

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).[25] Also included are fixed effects, FE, for cohort and, in extensions, the initial CZ of residence in 1940.

I find some evidence of selective migration. Table 1 presents the γ estimates from regression (2), separately for whites (Panel A) and blacks (Panel B), under the 1940 (columns 1-4) and 1950 (columns 5-8) definitions of low- and high-divorce CZs. Odd columns include cohort fixed effects; even columns have both cohort and initial CZ fixed effects. Consider first the group that started from low-divorce CZs in 1940 (columns 1-2, 5-6). Positive point estimates are observed for both races and under each definition, albeit with some imprecision for blacks under the 1940 definition. In words, individuals who had previously experienced a divorce are more likely to move to a high-divorce CZ. The gap is around 2 percentage points for whites and between 2 to 5 percentage points for blacks, about 25 to 47 percent of the respective mean divorce rates. This direction of selection is consistent with the geographic patterns of divorce (Figures 2 and 3), thereby reinforcing it. Next, I look at the group that originated from high-divorce CZs in 1940 (columns 3-4, 7-8). No consistent patterns are observed here, with the coefficients varying in sign and significance across race and definition. Taken as a whole, my results suggest that selective migration *into* high-divorce CZs, rather than selective migration *out of* these places, helped sustain the existing geography of divorce.

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

[23] To reduce the influence of internal migration, I base the CZ ever-divorced rates on US-born non-hispanic 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 stayers are used.

[24] Recall that information on the number of times married is only available for sample-line respondents in 1940 and sample-line respondents in 1950, two groups with little overlap. When measuring ever-divorced rates from the 1940 census (1940 definition), I use stayers who are sample-line respondents in 1940. Similarly, when measuring ever-divorced rates from the 1950 census (1950 definition), I use stayers who are sample-line respondents in 1950.

[25] 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.

3.2 Place Effects

Beyond selective migration, does living in a place with more/less divorce also change a person's propensity for marital dissolution? This section investigates if such place effects helped shape the geography of divorce.

As motivation, consider the likelihood of divorce among individuals who spent their lives in a high- versus low-divorce area. I start with 0-10-year-old US-born non-hispanic girls in the 1910 census who can be linked forward to the 1940 census via the Census Tree links (Price et al., 2023), keeping only those who remained in the same CZ between 1910 and 1940.[26] To capture the divorce environment these persons are exposed to, I use the 1910 1 percent IPUMS census sample. While the 1910 full count does not have information on the number of times married, the 1910 sample does, allowing me to tabulate the mean ever-divorced rate for US-born non-hispanic women aged 25-50 in each CZ.[27] Naturally, using a sample rather than the full count has several limitations: (i) not every area is covered, (ii) places that are covered may not have sufficient observations for precise or reliable inference, and (iii) the sample was not meant to be representative at the sub-state level. For greater accuracy, I thus limit the analysis to CZs with at least 25 observations in the 1910 sample. Is an individual's probability of divorce *by 1940* associated with their divorce environemnt as measured *in 1910*?[28]

Figure A.6 plots the average ever-divorced rate for persons in the 1910-1940 linked sample when they are adults (1940, y-axis) against the ever-divorced rate of their CZs (1910, x-axis). A positive relation is observed, where a one standard deviation increase in the surrounding divorce rate is associated with a 1.70-percentage-point higher in the likelihood of divorce, around 22 percent of the mean divorce rate. That is, those exposed to high (low) divorce environments are more (less) likely to go through a divorce. While not causal, this pattern hints at the possibility that where one lives affects personal behavior.

3.2.1 Social Interactions

Why might living in a high/low divorce area affect a person's propensity for divorce? One possible mechanism is the social interaction between individuals, where people with particular traits or behavior influence the corresponding attributes of others. While social interactions can operate along various dimensions, the absence of social media and modern communication divorces in a historical setting make interactions between people living close by particularly salient. A growing

[26] To build their census links, Price et al. (2023) exploit genealogical records and machine learning. The result is the largest database of linked census records at the time of writing. These are available for all pairwise combinations of census full counts, except those involving the 1950 census—they were thus not used in the previous 1940-1950 analysis. I pool both whites and blacks together.

[27] The raw data underlying the 1 percent IPUMS sample and full count are the same, but there are some differences in the set of variables available across the two datasets. This reflects the different transcribing organizations. The 1 percent sample was transcribed and coded 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.

[28] I measure the divorce outcomes of individuals and divorce environment in different years to avoid any mechanical association between the outcome and explanatory variables.

literature in economics 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 attitudes toward foreigners (Bursztyn et al., 2024). What about divorce? Studying Framingham in Massachusetts, McDermott et al. (2013) find that divorce can spread between friends, extending to two degrees of separation in a network. Beyond such descriptive accounts, however, the nexus of social interactions and divorce remains largely unexplored. This paper examines if social interactions at the neighborhood level have a causal impact on the probability of divorce.

A key challenge to identifying such social interaction effects is the non-random sorting of residents. To address this, Bayer et al. (2008) exploit differences in the spatial proximity of neighbors. Specifically, they compare the propensity of working in the same location for neighbors living in the same census block versus neighbors residing in nearby census blocks. Applying this approach to Boston in 1990, Bayer et al. (2008) find that those in the same block are more likely to work in the same place, a pattern they attribute to job referrals through social interactions. I adapt Bayer et al.'s (2008) spatial proximity research design to study the link between social interactions and divorce in historical America. This requires data on (i) a person's ever-divorced status and (ii) her location of residence at a granular (sub-county) level. Only the 1940 and 1950 full count censuses have the necessary information, again restricting my analysis to this period. In each of these years, I focus on sample-line US-born non-hispanic women aged 25-50.

To operationalize the spatial proximity approach, I define two levels of geography: (i) small *local areas* where social interactions are potentially strong and (ii) broader *neighborhood reference groups* comprising several local areas that are nearby. I start by defining local areas. Unlike contemporary records, the census blocks used in Bayer et al. (2008) are not available in historical census data.[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 used, the greater the precision in regression analyses, but the larger the margin for sorting. For the purposes of this paper, I use three nearby EDs to define a neighborhood reference group, where consecutive ED numberings are used to determine "nearness."[31] On average, EDs with more

[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 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, 2022b). 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.7A and B show the distribution of the number of individuals residing in a given local area or ED, for the full and target populations.

[31] Figures A.7C and D display the distribution of people across neighborhood reference groups, for the full and target populations.

similar numberings are closer together in space (Figure A.8).[32]

The two levels of geography allow me to isolate social interaction effects. I examine the likelihood that a pair of neighbors have both experienced a divorce, comparing neighbor-pairs that reside in the same local area versus pairs in nearby local areas of the same neighborhood reference group. This is implemented with the following regression:

$$y_{ii'n} = \Omega \; SameArea_{ii'n} + \eta_n + v_{ii'n} \tag{3}$$

where i and i' denote a pair of individuals from different households residing 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 if residents do not sort *within* reference groups—is this a reasonable assumption?

To probe the validity of the identifying assumption, I check if there is any systematic association between the characteristics of individuals and their neighbors. Following Bayer et al. (2008), this is operationalized in three steps. First, taking my population of interest, I draw a random person from each local area. 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 6 shows the resulting correlations across a range of socioeconomic traits, unconditional (grey diamond markers) and with neighborhood reference group fixed effects (black circle markers). Substantial sorting is observed, but much of this occurs between rather than within reference groups—the correlations for most characteristics are much smaller (and low) with reference group fixed effects.[33] This suggests limited residential sorting within reference groups, giving weight to the identifying assumption. The main exception is race. Even with reference group fixed effects, the correlation between whether a person is black and the share of black neighbors is over 0.4. This is not surprising given the highly segregated residential patterns at the time (Cutler et al., 1999), prior to the Fair Housing Act of 1968. In the subsequent analysis, I address the correlations that still remain between individual and neighbor characteristics even within reference groups.

I find evidence that social interactions may have helped shape the geography of divorce in

[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.8 shows a positive association, indicating that EDs with similar numberings are also closer in space.

[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 induces 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.

historical America. Columns 1 and 6 of Table 2 present the Ω estimates from regression (3), based on the 1940 or 1950 census. Positive and significant effects are observed. The odds that a pair of neighbors have both experienced a divorce is 0.1 to 0.3 percentage points higher among pairs in the same local area compared with pairs living in different areas within the same neighborhood reference group. These differences are sizable at 10 to 16 percent of the mean divorce rate among neighbor-pairs in different local areas. Social interactions at the neighborhood level thus appear to affect the likelihood of divorce. Zooming out to CZs, the cummulative effect of social interactions will be greater in places with more divorcees to begin with, thereby helping to sustain the existing geographic patterns of divorce.

Finally, I conduct a suite of robustness checks to rule out potential confounders. First, to address racial sorting, I limit the analysis to neighborhood reference groups with no black residents in the population of interest. This reduces my sample size by 37 to 44 percent, but the point estimates remain largely unchanged (columns 2 and 7, Table 2). Second, I tackle the residual correlation between various individual and neighbor traits. While these correlations are relatively low when conditioning on reference group, they are typically not zero (Figure 6). I thus add individual fixed effects to regression (3) for each member of a neighbor-pair, following Bayer et al. (2008) and Schmutte (2015).[34] These further control for sorting on observable and unobservable attributes. Even with such a demanding specification, similar social interaction effects can still be detected (columns 3 and 8, Table 2). Third, I hone in on circumstances where it may be more difficult for people with similar traits to sort across local areas and co-locate, settings where the identifying assumption may be more plausible. Here, I restrict the 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. The location choices of recent arrivals are partially constrained by the local supply of housing units available for sale or rent at the point in which they enter a county—if the search period of potential renters or buyers does not coincide with when existing renters or owners move out, households may not be able to select their preferred local areas.[35] Such constraints may be even more salient when looking at narrow geographic areas, as is the case here. This restriction leaves me with under 5 percent of the original sample but still yields a positive and significant coefficient (column 4, 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 location choices of both members rather than just one. Again, I find evidence of social interactions (column 5, Table 2).

3.2.2 Intergenerational Transmission

While the discussion on social interactions centered on the *horizontal* transmission of divorce propensity from one adult to another, could there also be a *vertical* transmission across gener-

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

[35] Location choices are also constrained by heterogeneous tastes among households for particular housing attributes.

ations from parents to children?[36] If so, places where divorce is common in one generation will continue exhibiting high levels of divorce in the next generation, sustaining existing geographic patterns of divorce and contributing to the persistence between 1940-1980.[37] Contemporary studies document that children with divorced parents are themselves more likely to go through a divorce in later years (Amato, 1996; Gaspard and Clifford, 2016; Wolfinger, 2005). Was this true historically?

To quantify the historical parent-child relation in divorce status, I proceed in five steps. First, I link US-born non-hispanic girls aged 0-18 from the 1910 census to their older selves in the 1940 enumeration, again using the Census Tree links (Price et al., 2023).[38] Second, I look at the marital status of parents in 1910. As mentioned previously, the 1910 full count does not have information on the number of times married, making it impossible to tell if currently-married parents had previously been divorced. The only parents for whom there is certainty over their divorce history are divorcees who had not yet remarried when the 1910 census was taken. I thus focus on this group, bearing in mind the noise arising from an imperfect knowledge of divorce status. Third, turning to the children, I identify those who had ever been divorced by 1940. Fourth, for greater comparability, I divide my linked sample into (i) daughters whose mothers were the household head and (ii) daughters whose fathers were the household head. Fifth, I compare the adult outcomes of daughters who grew up with and without a divorced parent, using the regression:

$$y_{ih}^{child} = \alpha + \rho\ y_{h}^{parent} + \text{FE} + u_{ih} \tag{4}$$

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 divorced in 1910 (based on current marital status only). Fixed effects for child and parent cohort as well as childhood CZ in 1910 are included in FE. The parameter of interest ρ captures the intergenerational transmission of divorce propensity.

I find some evidence of a divorce cycle in historical America. Table 3 presents the ρ estimates from regression (4), separately for whites (columns 1-2) and blacks (columns 3-4), distinguishing between daughters of female (columns 1 and 3) and male (columns 2 and 4) household heads.[39] For whites, I find a positive and statistically significant association between the divorce status of parents and children. Daughters with divorced parents in 1910 were 5.5 to 8.8 percentage points more likely to have gone through a divorce themselves by 1940. This intergenerational transmission is particularly strong between mothers and daughters. For blacks, however, no transmission in

[36] Such intergenerational transmission could come about through the passing down of particular traits or socioeconomic circumstances that are associated with divorce.

[37] This assumes that a majority of individuals remain in their childhood locations into adulthood. Based on the sample of linked daughters in Figure A.6, I calculate that almost 60 percent stayed in the same CZ from 1910-1940.

[38] I do not use a 1920-1950 linked sample here as the Census Tree links do not cover the 1950 full count yet. The links from Helgertz et al. (2024) can be used to build a 1920-1950 dataset, but these links comprise a much smaller set, which matters especially when looking at relatively small groups like female-headed households.

[39] When implementing the analysis by race, I consider the race of both the daughter and parent (e.g. white daughters with white mothers).

divorce status is observed across generations, with insignificant estimates of ρ in all cases.[40] These results hold when restricting the base population to daughters who were ever-married by 1940 (Table A.2). The evidence is thus mixed—there is vertical transmission in divorce status among whites but not blacks.

3.2.3 Childhood Exposure Effects

Living in a high/low-divorce area affects a person's propensity for divorce through social interactions between adults and (for whites) intergenerational transmission from parents to children—are children themselves directly influenced by the divorce environment of the places they grow up in? That is, are there childhood exposure effects on divorce outcomes? A growing literature in economics emphasizes the lasting impact of childhood environment. For example, children raised in better environments tend to earn more when they enter the workforce (Chetty et al., 2016; Chetty and Hendren, 2018; Chetty et al., 2020, 2025; Chyn, 2018), while those exposed to more innovation in the locations they grew up in are more likely to become inventors themselves (Bell et al., 2019). Along these lines, I investigate if childhood exposure to high-divorce (low-divorce) environments raises (lowers) the probability of divorce in adulthood.

To isolate childhood exposure effects, I exploit the movement of households across regions with varying levels of divorce. The use of internal migrants has become a popular means of triangulating the impact of place on various outcomes (Bazzi et al., 2020; Bronnenberg et al., 2012; Chetty and Hendren, 2018; Raz, 2025; Deryugina and Molitor, 2020). Here, I compare the likelihood of divorce in adulthood between siblings born before and after their households migrated from a low- to high-divorce region—the former would have less exposure to a high-divorce environment during their formative years while the latter would have been more exposed. Differences in divorce rates between the two sets of siblings can be attributed to the change in childhood environment if there are no systematic trends in divorce across siblings.

I start by defining the base set of households. Of particular interest here are households that migrated to a high-divorce area by 1910. As alluded to previously, the 1910 1 percent IPUMS sample (not the full count) has the variables needed to calculate the mean ever-divorce rate in an area.[41] For transparency, I use alternative thresholds to define high-divorce areas: the top-15 or top-20 states in terms of 1910 ever-divorced rates.[42] This captures the divorce environment that a child is exposed to while growing up. I restrict my sample to households in 1910 (i) with two or more children, (ii) where at least one child was born in a low-divorce region and at least one other child was born in a high-divorce divorce region, and (iii) where there is a monotonic transition from being born in a low-divorce region to being born in a high-divorce region going from the

[40] The insignificant positive coefficient for black female-headed households does not simply reflect a lack of precision. The point estimates are sensitive—switching from CZ to county fixed effects can reverse the sign.

[41] For my computations in 1910, I use the population of US-born non-hispanic women aged 25-50.

[42] The top-15 states by ever-divorced rate in 1910 are: Alabama, Arizona, Arkansas, California, Colorado, Florida, Louisiana, Mississippi, Montana, New Hampshire, Nevada, Oklahoma, Oregon, Washington, and Texas. The next five states are: Georgia, Idaho, Kentucky, Maine, and Tennessee.

oldest to youngest child. These criteria identify are households that migrated once from a low- to high-divorce region by 1910, a group for which the movers strategy can be applied to.

A key aspect of the movers setup involves distinguishing between siblings born before and after their households migrated—this requires information on when households moved. While not directly recorded in the 1910 census, the timing of moves can be inferred by exploiting differences in the birth place and birth year of siblings. Ordering siblings by age, I pick out the adjacent siblings who have different birth regions (low- or high-divorce), and then take the midpoint of their birth years as the household's year of move. For concreteness, consider the following household with six children in the 1910 census: Thurman (born 1907 in Tennessee), Bebas (born 1909 in Tennessee), Charles (born 1911 in Arizona), Curtis (born 1914 in Arizona), Fay (born 1916 in Arizona), and Robert (born 1918 in Arizona), where Tennessee is a low-divorce state while Arizona is a high-divorce state. In this example, Bebas and Charles are the adjacent siblings born in different regions. I thus impute the household's year of move as 1910, the midpoint of their birth years. Similar imputations have been used in the economic history literature (Atack and Bateman, 1987; Bazzi et al., 2020, 2023a; Collins and Zimran, 2019; Raz, 2025; Tan, 2022a).

To complete my setup, I require information on the divorce status of children when they reach adulthood. Taking my sample of households, I link 0-to-18-year-old daughters from the 1910 census to the 1940 census, using the Census Tree links (Price et al., 2023). While the variables needed to determine ever-divorced status are reported in the 1940 census, these are only available for sample-line respondents. Having already lost a large number of observations when restricting the sample to households that moved across states once and from linking daughters between censuses, further imposing a sample-line restriction in 1940 would leave a sample too small for meaningful analysis. This necessitates adopting a less precise measure of divorce: persons that are currently divorced in 1940, which is collected for the whole population. In theory, this means that one is not restricted to divorce outcomes in 1940, since current marital status is recorded in each census. In practice, I choose 1940 as the end year to mitigate the lifecycle dynamics of divorce, where the probability of marital dissolution increases with age until the late-30s and 40s (Figure A.9). Daughters in my sample would be around 30-48 years old in 1940, a range where the such lifecycle dynamics are less pronounced, thus reducing any mechanical bias.

Having set up the movers research design, I then use an event study to determine if childhood exposure to a high-divorce environment affects divorce outcomes later in life:

$$y_{ih} = \sum_{m=-10}^{10} \kappa_m \, 1[YSM = m]_{ih} + \zeta_{ih} + \theta_h + v_{ih} \tag{5}$$

where the subscripts i and h denote an individual and household, respectively. The outcome, y, is an indicator for whether a person was divorced in 1940, based on current marital status. The main explanatory variable is a set of indicators for the number of years since a household moved to a high-divorce state, YSM, at the time the individual was born, m, calculated as the difference between the person's birth year and the household's year of migration. Births before the household

migrated have negative values of m, while births after the move have positive values. For simplicity, births occurring more than 10 years before or after the household migrated are coded as $m = -10$ or 10. I also control for birth cohort in ζ.[43] Key to regression (5) is the vector of household fixed effects, θ, which ensures that only variation between siblings is used for estimation. Under the assumption that sibling outcomes would have been similar had the household not moved, positive κ coefficients post-migration can then be interpreted as the impact of childhood environment.

I find little evidence of childhood exposure effects on divorce status. Figure 7 plots the event study coefficients from regression (5), with 95 percent confidence bands based on standard errors clustered at the household level. No statistically significant estimates are observed with either the top-15 (A) or top-20 (B) definition of high-divorce areas. This suggests that the divorce environment a person grows up in does not affect her chances of divorce later in life. Importantly, the apparent null effect does not simply reflect the less precise nature of my divorce measure (currently divorced rather than ever-divorced). As evidence for this, I redo the analysis by linking my sample of daughters from 1910 to 1930 instead of 1940, re-examining their divorce outcomes in this earlier decade. These daughters would be 20-38 years of age in 1930, when the lifecycle dynamics of divorce are still pronounced (Figure A.9). One should thus see a negative trend in this setting if current divorce status is a sufficiently precise measure, as divorce rates mechanically decline from the oldest to youngest sibling. Figure A.10 shows that this is indeed the case. The initial null result is thus unlikely to be driven by noise.

4 Before 1940: Deep Roots

The preceding analysis showed that selective migration and place effects (through social interactions) contributed to the geographic patterns of divorce between 1940-1980—what were things like before 1940? In particular, does the geography of divorce before 1940 mirror the landscape in 1940-1980? If so, what gave rise to these patterns? I explore these questions in turn.

4.1 Back to the Past

I start by reconstructing the geography of divorce as far back in time as possible, drawing on various historical sources. America experienced a wide range of events in the 19th and early-20th centuries, from global wars to women empowerment to economic depression, any of which could have shaped the landscape of divorce. The earlier one can observe the geographic patterns of divorce, the more precisely one can rule out or triangulate potential causes.

Taking 1940 as the reference point, I begin by moving back to the 1930s and 1920s. With the close of World War I (WWI), the Census Bureau began collecting annual divorce statistics at the county level (National Center for Health Statistics, 1973). These were published in the pamphlet *Marriage and Divorce* from 1922-1932, ending when the Great Depression hit America.[44] The

[43] I use an indicator for 13-18-year-olds, categorizing my sample into pre-teens and teens in 1910.

[44] Divorce statistics were also collected in 1939, but only for 12 states (National Center for Health Statistics, 1973).

published records have been digitized by researchers at Bowling Green State University (BGSU) (BGSU Marriage and Divorce Data Compass, 2018). Unlike the census/ACS-based measure of divorce (Section 2), the county divorce data here are not stratified by demographic group, pooling all ages, races, and nativities together.[45] In addition, while the census/ACS ever-divorced measure captures the stock of divorcees, the county divorce numbers reflect the flow of new divorces. Bearing these differences in mind, I proceed by (i) aggregating the county data to CZs, (ii) averaging the CZ divorce counts over the 1930s (1930-1932) and 1920s (1922-1929) separately, and then (iii) scaling these averages by the respective population of women.[46] Figures 8A and B map the resulting county divorce rates for the 1930s and 1920s. Divorce is more prevalent in the West, the stretch from the industrial Midwest to Texas, as well as in Florida and Maine, mirroring 1940-1980. That similar patterns can be observed almost two decades earlier rules out events like the Great Depression, the New Deal, and the Dust Bowl as root causes of the divorce landscape.

Turning back the clock to the 1910s, two sources of local divorce statistics are available. One is *Marriage and Divorce, 1916*, a Census Bureau publication reporting county divorce statistics for 1916.[47] Again, these have been digitized by researchers at BGSU (BGSU Marriage and Divorce Data Compass, 2018). I aggregate the county divorce numbers to the CZ level and scale them by the corresponding population of women to obtain the local divorce rate in 1916. Another data source is the 1910 1 percent IPUMS sample used in the previous sections. For this year, I tabulate the share of US-born non-hispanic women aged 25-50 in each CZ who had ever been divorced, limiting the scope to CZs with at least 25 observations for greater accuracy.[48] Figures 8C and D then display the geography of divorce in 1916 and 1910. The patterns here are somewhat noisier, possibly due to the use of a single year (in the case of 1916) or the smaller underlying sample (in the case of 1910).[49] Nonetheless, a broadly familiar picture emerges, with higher rates of divorce in the West, industrial Midwest, Florida, and Maine. This rules out even more events as root causes: WWI, Prohibition, the 19th Amendment, and the economic prosperity of the Roaring Twenties.

To go even deeper, I turn to two landmark investigations ordered by Congress in the late-19th and early-20th centuries. Public concerns over rising divorce rates had led to growing calls for the federal government to collect divorce statistics.[50] These efforts first bore fruit in 1887, when Congress directed the Commissioner of Labor to compile retrospective divorce numbers for

[45] Without demographic stratification, observed shifts in the geography of divorce could mechanically result from changes in the local population composition.

[46] I average the divorce counts across the 1930s and 1920s both for parsimony and to reduce the influence of year-to-year fluctuations in new divorces. To obtain the denominator for scaling, the number of women in each county and period, I linearly interpolate decadal census data from Manson et al. (2023) and then average these figures across 1930-1932 and 1922-1929.

[47] This data collection exercise was scheduled for 1917, with the aim of covering 1906-1917. Unfortunately, the US entered WWI by 1917 and only data for 1916 was compiled.

[48] For consistency with the maps in this section, I pool both whites and blacks together. Figure A.11A shows the corresponding map for whites, which can be compared with Figure 2. I do not provide a similar map for blacks due to the small underlying sample.

[49] One way to enlarge the sample size in 1910 is to calculate the ever-divorced rate for both men and women combined. Figure A.11B shows the resulting map.

[50] Growing concerns over divorce led to the formation of the New England Divorce Reform League in 1881. The League played a key role in coordinating petitions to Congress calling for the collection of divorce statistics.

all states, covering the period 1867-1886.[51] The results were published in *A Report on Marriage and Divorce in the United States, 1867 to 1886* and the county data therein have been digitized by BGSU (BGSU Marriage and Divorce Data Compass, 2018). Nearly two decades later in 1902-1905, a new wave of petitions were sent to Congress, urging the government to update the earlier divorce statistics. Congress passed a resolution in 1905 authorizing the Director of the Census to collect divorce counts across the country, this time from 1887-1906. The findings were released in *Marriage and Divorce, 1887-1906*. I digitize the county data in this publication for my analysis.[52] Together, the two collections make it possible to push the geography of divorce back by another 40 years, to the dawn of modern US history. While both reports covered most counties, the share of counties with missing information in a given year increases substantially as one goes further back in time, as shown in Figure A.12. Consequently, aggregating counties to CZs can often leave the larger units incomplete. For simplicity, I focus on county-level variation instead. Figure 9 shows the number of divorces per 1,000 women by county, averaged across the 1900s (1900-1906), 1890s (1890-1899), 1880s (1880-1889), 1870s (1870-1879), and 1860s (1867-1869), separately.[53] While the patterns here are noisier, I continue to find higher rates of divorce in the West, the stretch from the industrial Midwest southwestward to Texas, as well as in Florida and Maine. Put differently, the geography seen in 1940-1980 can be found almost a century earlier, ruling out major events like the industrial growth of the Gilded Age and the Panic of 1893 as originating factors.

4.2 Origins

What gave rise to the distinctive geography of divorce so early in US history? I consider four potential causes in this section: frontier conditions, the expansion of women's economic rights, immigration, and differences in divorce legislation.

4.2.1 Frontier Conditions

I begin with frontier conditions. America was rapidly expanding throughout the 19th century, with successive waves of pioneering settlers heading westward. To describe this phenomenon, the Census Bureau introduced the concept of the frontier, a line demarcating the edge of the population settlement. 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, allowing them to locate and track the movement of the frontier from 1790-1890.[54] Because it was moving westward continually, the frontier was not a fixed area—rather, it was a form of society at an early stage of development. These nascent societies were characterizied by extreme isolation, both in terms of low population density and remoteness. While challenging, such conditions offered new opportunities to those attracted by land abundance and who were able to

[51] Special agents were sent to collect these statistics from courts with divorce jurisdiction. In some of the more sparsely settled or distant counties, local authorities compiled the necessary statistics and returned them by mail.

[52] BGSU only digitized two years of data from this publication: 1890 and 1900.

[53] As before, the averaging is done separately for the numerator and denominator before combining them.

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

thrive in harsh environments. Populations on the frontier thus featured distinctive demographics: greater individualism, male-biased sex ratios, younger populations, more marriages, higher fertility, and fewer gainfully-employed women (Bazzi et al., 2020, 2024). Beyond descriptives, recent work has also examined the impact of frontier conditions: Bazzi et al. (2020) show that frontier exposure amplified individualism while Bazzi et al. (2024) contend that it shaped conservative gender norms.

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 the literature failing to reach a consensus. Contemporaries viewed the higher divorce rates in the West as a western rather than a frontier phenomenon (Bureau of the Census, 1908; Riley, 1991).[55] Yet Bazzi et al. (2024) find that men from frontier counties are more likely to get a divorce.[56]

To investigate the link between frontier conditions and divorce, I combine county divorce data from the reports authorized by Congress (Section 4.1) with a difference-in-differences (DD) setup:

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

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 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 different 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. As defined in Bazzi et al. (2020), a frontier county is one (i) that is within 100 km of the frontier line (ii) with less than six people per square mile. Because the frontier is fluid, a county can be on the frontier in one decade but not in the next. Finally, I include county and year fixed effects in the θs.

I find suggestive evidence that frontier conditions contributed to the patterns of divorce in the 19th century. Table 4 presents the frontier effects from regression (6), with different permutations of the outcome (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 as a robustness check. In all cases, positive and statistically significant point estimates are observed.[57] Being on

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

[56] Bazzi et al. (2024) use a sample of men linked between 1870-1880 to study the association between frontier origins and divorce. Besides this *individual*-level analysis, they also conduct a *county*-level analysis, finding lower divorce rates among women on the frontier. To reconcile these differences, the authors postulate that divorced women were more likely to leave frontier counties.

[57] 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.

the frontier thus raised the propensity for divorce. To unpack the frontier effect, I separate the two dimensions of frontier isolation, replacing the frontier indicator in regression (6) with a dummy for proximity to the frontier line (within 100 km) and a dummy for low population density (below six people per square mile). Table A.3 shows that both aspects of frontier isolation matter: counties within 100 km of the frontier line have 0.1 to 0.4 more divorces per 1,000 women than those east of the 100 km cutoff, while counties with under six people per square mile have around 0.3 to 0.8 more divorces per 1,000 women.

Although frontier conditions helped shape the patterns of divorced in the 19th century, these effects are unlikely to have persisted after the nation had settled. To illustrate this, I follow Bazzi et al. (2020) in using variation in the duration with which counties were exposed to frontier conditions between 1790-1890. Specifically, Bazzi et al. (2020) calculate the Total Frontier Experience (TFE) of each county, based on its frontier status in each period. I relate this TFE measure to the share of ever-divorced persons in a county, tabulated using full count census data from 1940 and 1950 (after the frontier closed). This comparison gives the long-run effect of frontier conditions. Table A.4 presents the results. Column 1 indicates a positive relation between TFE and later rates of divorce, without controls. Column 2 then adds a set of geoclimatic covariates, eliminating the association in 1940.[58] Little else changes when state fixed effects are included in column 3. To further address concerns over the endogeneity of TFE, I also adopt the IV strategy in Bazzi et al. (2020), exploiting historical shocks to settlement patterns driven by immigration. The instrument for TFE takes two forms: (i) the log of the average annual migration inflow in the 30 years subsequent to the frontier arriving within 110 km of the county (column 4) and (ii) and a similar measure using annual migration inflow predicted by weather shocks in Europe (column 5). The 2SLS point estimates are negative in 1940 and insignificant in 1950. Similarly fragile results are observed when looking at divorce rates by race (Tables A.5 and A.6). In summary: while frontier conditions contributed to the initial variation in divorce, its impact did not last, leaving selective migration and place effects to sustain the patterns of divorce into later decades.

4.2.2 Women's Economic Rights

Could the expansion of women's economic rights across states during the 19th century explain the geographic patterns of divorce at the time? Beginning with Massachusetts in 1848, state after state abandoned the laws of coverture, granting married women rights over their own property and labor earnings. Recent work has established the impact of such 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 expansion 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, together referred to as economic rights.[59] Adapting the approach in Hazan et al. (2025),

[58] The geoclimatic controls include: latitude, longitude, area, rainfall, temperature, elevation, potential agriculture yield, and distance to rivers, lakes, and the coast.

[59] Hazan et al. (2025) make the case for looking at both property and earnings rights together, instead of studying

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 unlikely to be endogenous, as these rights were often overturned due to unforeseen technicalities before eventually being accepted (Hazan et al., 2025).[60] Formally, I use the an event study to quantify the impact of women's economic rights, drawing on county divorce data from the reports authorized by Congress (Section 4.1):

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

where the subscripts c, s, and t denote the county, state, and year (1867-1906), respectively. Over my study period, 33 states granted economic rights to married women. 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.[61] 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, state, and year.

I find that the expansion of women's economic rights did not shape the geographic patterns 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 before the legislative reforms, suggesting parrallel trends. However, no significant effects are detected after the reforms either. To probe these results further, I conduct five additional exercises. First, I add county-pair-specific linear trends (Figure A.13A). Second, I drop counties along the border of a former Confederacy and a non-Confederacy state (Figure A.13B). Third, I exclude counties on the border of a community property state and the neighboring state (Figure A.13C). 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.13D).[62] No discernible effects are found under any scenario. The geography of divorce is thus unlikely to have emerged from the heterogeneous expansion of women's economic rights.[63]

each separately, as the absence of one hampers the effectiveness of the other. They provide several real-life examples to illustrate this point.

[60] 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).

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

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

[63] 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

4.2.3 Immigration

As the western frontier expanded and women's rights grew, 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)—could the influx of foreigners affect divorce patterns in America? Historically, divorce rates in Europe were much lower than America (Figure A.15, Bureau of the Census (1908)). Parts of the US that received more immigrants, a relatively low-divorce group, could then experience a mechanical decline in overall divorce rates.[64] However, this conjecture implicitly assumes that immigrants hold on to their "low-divorce culture."[65] As Bazzi and Fiszbein (2025) explain, *persistence* in culture is not the only way immigrants can behave; they can also *influence* native culture (further reducing divorce rates in this case) or *assimilate* into native culture (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 a foreign land (the US in this setting) could strain existing unions, causing more breakups among immigrants (Caarls and Mazzucato, 2015; Hu, 2018). Given the complex and conflicting array of dynamics, the net impact of immigration on divorce is unclear.

To shed light on the role of immigration, I compare the relative change in divorces between places receiving more and less immigrants, drawing once again on county divorce data from the reports authorized by Congress (Section 4.1). 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).[66] 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.

As with other causal studies on immigration, the endogeneity of migrant settlement patterns

property rights dates instead of the economic rights dates, I would still find a null effect, as shown in Figure A.14. 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.

[64] Recall that the county divorce statistics pool all demographics together, whether native or immigrant.

[65] 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.

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

across the US is a key issue here. 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 to their final destinations. The second is the aggregate inflow of immigrants to the US, 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. Importantly, this *interaction* between the timing of railway connection and immigration booms, rather than each variable alone, is unlikely to affect divorce rates through other channels besides migration flows. Indeed, 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 for the relative change in foreign-born persons in each county, controlling for railway access in the initial period.[67]

While Sequeira et al. (2019) rigorously probe the validity of their IV approach, there are additional constraints when using this method to study historical divorces. 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.12) and the time horizon (1867-1906). Here, I can only examine the decades beginning 1870, 1880, and 1890.[68] The narrow sample introduces several constraints. First, while Sequeira et al. (2019) allow the impact of railway connection to vary by GDP growth to address the potential confounding effect of business cycles, this is not possible here as immigration and GDP growth are almost perfectly correlated over my short study window (Table A.7). Second, with a smaller sample, fewer robustness checks are feasible. The results in this section should thus be viewed as suggestive.

Table 5 indicates that immigration is unlikely to be a key driver of the geographic variation in 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-year (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. In words, 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.[69] All 2SLS

[67] 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.

[68] 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.

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

estimates are insignificant. Similar patterns can be observed when using aggregate immigration flows predicted from weather shocks in Europe (Table A.8). Together, my results suggest that immigration was not the primary explanation for the spatial patterns of divorce in the 19th century.

4.2.4 Legislation

Finally, I investigate if state differences in divorce legislation might explain the geographic variation in marital dissolution during the 19th century. In *Marriage and Divorce, 1887-1906* (Bureau of the Census, 1908), the Census Bureau published a detailed summary of divorce legislation in each state up until 1907, covering a wide range of issues such as jurisdiction, causes, alimony, refusal of divorce, and limitations of time, among other things. While comprehensive, the intricacies and specifics of individual state laws make direct cross-state comparisons unwieldy.

To make progress, I construct a summary measure of how strict a state's divorce laws are, based on 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 the state laws from 0-100, where a higher score denotes stricter rules for divorce. Figure 11 maps the resulting strictness scores across the country. Compared with the prevailing geography of divorce (Figures 8 and 9), some overlap is observed. 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.

Another way to check if differences in state divorce legislation are driving the spatial patterns of divorce is to look at the characteristics of divorces in each state. To do this, I digitize 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.16)? Second, what share of divorces are caused by abandonment/desertion, adultery, cruelty, habitual drunkenness/intoxication, or neglect to provide (Figures A.17 and A.18)? Third, what proportion of divorces involve children (Figure A.19)? If legislative differences are the root cause of the divorce landscape, these characteristics should vary systematically across states in a way that matches the geography of divorce. Yet none match the picture of divorce tightly. While this does not completely rule out a role for legislative differences, it suggests that differences in these laws are unlikely to be the main reason for the divorce patterns.

5 After 1980: Times of Change

The steep rise in divorce from 1960-1980 was characterized by a relatively stable geography stretching back to the 19th century, but this changed after 1980 when falling divorce rates were accompanied by a shift in the landscape of divorce—what caused the transformation in geographic patterns? This section proposes a role for broader economic forces, particularly the rising return to skill.

5.1 Stylized Facts

To set the stage, I begin by presenting three stylized facts.

First, I document when the geography of divorce began to change. The census/ACS data in Section 2 allowed me to track the spatial patterns of divorce from 1940-1980 and from 2008 onward, but not in the intervening years, leaving a three-decade gap in the timeline. It is thus unclear if the changes in geographic patterns are a recent phenomenon or something that began much earlier. Knowing the precise timing of change can help narrow the set of potential causes. To make progress, I turn to county divorce data published annually by the National Center for Health Statistics (NCHS) from 1960-1988.[70] These have been digitized by BGSU (BGSU Marriage and Divorce Data Compass, 2018). In addition, BGSU has collected its own county divorce statistics for 2000 and 2010 (BGSU Marriage and Divorce Data Compass, 2012, 2016). Put together, the combined series (1960-1988, 2000, 2010) can plug the gap between 1980 and 2008.[71] For consistency with the previous sections, I aggregate the county divorce data to CZs and scale these figures by the local population of women. I then compute the CZ rank-rank correlation between the geography of divorce in the 1930s (Figure 8A) or 1920s (Figure 8B) and the corresponding rates from 1960-2010.[72] Figures 12A and B display these correlations. Interestingly, the transformation in geographic patterns was no recent event—it was a process long in the making, accelerating from the 1980s. The factors driving this shift must thus have emerged in the 1980s.

Second, I trace how the wage premium paid to skills in the labor market has evolved. A common measure of how the market values skills is the college wage premium (Acemoglu and Autor, 2011; Katz and Murphy, 1992). Using Current Population Survey (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, focusing on male full-time full-year wage and salary workers aged 25-50.[73] Figures 12C and D depict the trends in college wage premium. The premium rises in the 1960s, falls in the 1970s, and then explodes from the 1980s onward, as others have documented (Acemoglu and Autor, 2011; Katz and Murphy, 1992).[74] Importantly, Acemoglu and Autor (2011) show that the rising college premium reflects a combination of growing real earnings at the top of the income distribution and falling real earnings at the bottom. Over the 20th century, skilled workers have thus pulled ahead while unskilled labor face worsening circumstances. The inflexion around 1980 appears to coincide with when the shift in the geography of divorce began to accelerate—could there be a connection?

[70] County divorce data are also available for several years in the 1950s, but the coverage is poorer (Figure A.12).

[71] Figure A.12 shows that this series is comprehensive, covering over 91 percent of counties each year.

[72] I use the 1930s and 1920s as anchor points because similar data are not available in the 1940s.

[73] I make two point here. The first concerns 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).

[74] The trend for blacks is substantially noisier. This could reflect a combination of the small sample of blacks in the CPS as well as the increasing share of non-working black men over time, driven in part by rising incarceration rates (Bayer and Charles, 2018).

Third, I follow the relation between education and divorce over time. On average, college graduates are less likely to be divorced (Isen and Stevenson, 2013; Neeman et al., 2008). Figures 12E and F show the evolution of this "college divorce discount" by race, using the 1940-1980 censuses and the 2008-2018 ACS when ever-divorced status can be determined. To account for changing trends in marriage, education, and their interaction (Greenwood et al., 2016; Stevenson and Wolfers, 2007), I focus on *ever-married* US-born non-hispanic women aged 25-50 as the base population in this exercise. For whites, the college divorce discount has grown throughout the 20th century, with some acceleration after 1980. For blacks, the patterns before 1980 are noisier, but a widening can still be observed after 1980. As before, the census/ACS data leave a 30-year gap in the timeline—exactly when between 1980 and 2008 did the college divorce discount begin to grow more rapidly? The CPS is available annually but lacks information on the number of times married, which is key to identifying the ever-divorced population. Nonetheless, some progress can be made in the case of whites. Using census/ACS data, Figure A.20 shows a broadly similar college-divorce profile for whites when using the share of ever-married persons that are *currently* divorced, but not for blacks. This suggests that one can study the college divorce discount for whites using the less precise measure of divorce in the CPS. Doing so, Figure A.21 reveals that the college divorce discount began to grow around the 1980s, concurrent with the rise in college wage premium (Figure 12C and D) and the shift in the geography of divorce (Figure 12A and B).

Piecing the three stylized facts together tell a story. Why did the geography of divorce begin to shift from the 1980s (stylized fact 1)? Broader economic forces dramatically increased the return to skill after 1980 (stylized fact 2), characterized by a sustained decline in the economic circumstances of unskilled workers. This erosion in economic status can increase the risk of divorce (Banzhaf, 2018; Charles and Stephens, 2004; Jackson et al., 2017). The result is a growing association between skill level and divorce (stylized fact 3). As these phenomena play out, the parts of the US with relatively more (less) college graduates will experience greater declines (increases) in divorce. If these dynamics are stronger than the forces of selective migration and place effects, the geography of divorce will change. Figure A.22 offers preliminary evidence for this narrative by mapping the share of college graduates across CZs in 1980, prior to the aforementioned changes. Drawing comparisons with Figures 2 and 3, I find that areas with fewer college graduates in 1980, primarily located in the South, are the places that later emerged as centers of divorce.

5.2 Empirics

To formally test if the geography of divorce reorientated around areas with fewer skilled workers after 1980, I run the following event study using census/ACS data:[75]

$$Divorce_{zt} = \sum_{j=1940}^{2018} \mu_j \; College_{z,1980} \;\; \text{x} \;\; 1[t = j] + \theta_z + \theta_t + \zeta_{zt} \tag{9}$$

[75] To facilitate the IV analysis in the next section, I pool both non-hispanic whites and blacks together here.

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 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.22), *College*, and a vector of year dummies. The model also includes CZ and year fixed effects, the *theta*s. Under the predicted direction of change, negative μ estimates are expected *after* 1980, as places with more (less) college graduates experience relative declines (increases) in divorce.

Figure 13A plots the coefficients from regression (9), 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 the effects are only observed after 1980 is consistent with a story where the distribution of college graduates matters for the geography of divorce because of the rising return to skills since the 1980s. My results are robust to including state-by-year fixed effects, as depicted in Figure 13B. Changes in the return to skill may thus explain the shifting landscape of divorce.

5.3 IV Approach

While the absence of pretrends in the above event study is reassuring, concerns might still linger over the potential endogeneity in the spatial distribution of college graduates. To isolate plausibly exogenous variation, I propose an IV approach in the spirit of Autor and Dorn (2013). Specifically, I use the local industry composition in 1950 to predict the corresponding supply of college graduates in 1980, exploiting two sources of variation: (i) cross-industry differences in skill dependence and (ii) cross-CZ differences in industry composition. Intuitively, some industries are more dependent on skilled workers than others due to the type of work involved—areas with a more (less) skill-intensive industries would attract more (fewer) college graduates, raising (lowering) the local supply of skilled labor.[76] Importantly, I use the local industry composition in 1950, prior to the rapid expansion in college education from the 1960s-1970s and from the 1990s onward (Figure A.23). This limits the scope for reverse causality, where the supply of skilled workers in an area attracts skill-intensive industries instead. As a robustness check, I also use the local industry structure in 1940 to construct an alternative IV.[77]

Formally, my IV can be written as:

$$IV_z = \sum_{e=1}^{I} E_{e,z,1950} \text{ x } College_{e,-s,1950} \tag{10}$$

The first component, E, is the employment share of industry e in CZ z in 1950. The second piece, *College*, is the share of workers in industry e who are college graduates in 1950, measured across all

[76] 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.

[77] Using the industry composition in 1940 addresses concerns that the GI Bill might have affected both the supply and distribution of college graduates (Barrera et al., 2025; Bound and Turner, 2002). This is the earliest year one can go as the 1940 census was the first Federal Census to record the education attainment of all respondents.

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 exogeniety in turn. To gauge relevance, Figure A.24A shows how the IV relates to the share of college graduates 30 years later. 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 later years, evidence of instrument relevance. Similar patterns can be found when using the local industry composition in 1940 to construct the IV (Figure A.24B). Next, I examine instrument exogeneity. This is done by replacing the college share variable in regression (9) with the IV—are there pretrends in this reduced form setup? Figures A.25A and B show the resulting estimates, without and with state-by-year fixed effects. Some negative and significant estimates are detected in the early decades, but these fade by 1960, well-before 1980. If anything, an upward rather than a downward trend is observed in the early years. Moreover, the negative associations post-1980 are almost double the magnitude of the pre-1980 coefficients. Using an IV based on the industry structure in 1940 yields similar results (Figures A.25C and D), allaying concerns that WWII would have changed the industry structure across the country (Garin and Rothbaum, 2025; Greenspon and Hanson, 2025). Taken together, my results suggest that the IV is likely to be both relevant and exogenous.

I now use the IV to study the link between college share and divorce rates. This is done in two steps. First, I collapse my event study (regression (9)) into a simple DD specification, substituting the year interactions with a post-1980 indicator. Columns 1 and 2 of Table 6 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 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. Results are similar with the 1940-based IV (Table A.9).

5.4 Skill-Biased Technical Change

The analysis thus far sought to connect the shifting geography of divorce to the presence (or absence) or skilled labor in an area—could other factors correlated with the distribution of skilled workers be driving this relation instead? Recall that what ties the changing spatial patterns to skilled labor is the rising return to skill. The factors behind this phenomenon could thus help rule out alternative channels. A large economics literature identifies skill-biased technical change as a major cause of the rising skill premium (Berman et al., 1994; Doms et al., 1997; Krueger, 1993). Key to this transformation is the personal computer (PC), which can substitute for tasks performed by unskilled workers but complement the work of their skilled counterparts. The introduction of the IBM PC in 1981, along with rapidly falling computing prices, fueled a growing demand for skilled workers that outstripped the corresponding increase in supply (Acemoglu and Autor, 2011), raising the college wage premium. Importantly, Doms and Lewis (2006) show that the supply of skilled

workers in an area determines the rate of PC adoption. This leads to the following prediction: if the shifting divorce landscape is tied to the presence of skilled workers through the rising return to skill, places with more college graduates in 1980 should not only see a decline in divorce but also experience a greater uptake of PCs. Using data on computer adoption across CZs from Autor and Dorn (2013) and Doms and Lewis (2006), Figure A.26 plots the relation between college share in 1980 and computer use over 1980-2008. A strong positive association is observed, reinforcing the link between geographic changes in divorce and the distribution of skilled labor.

6 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. These patterns were sustained by selective migration and place effects, with the latter operating through social interactions among neighbors. But the geographic patterns of divorce at mid-century go much further back in time, at least until the 1860s. Conditions on the rapidly expanding western frontier helped shape these patterns at the time. After 1980, as divorce rates started to fall, the landscape of divorce also began to change. Driving this change was the rising return to skill—the worsening economic circumstances of unskilled workers raised their likelihood of divorce, causing the geography of divorce to reorientate around places with fewer skilled workers.

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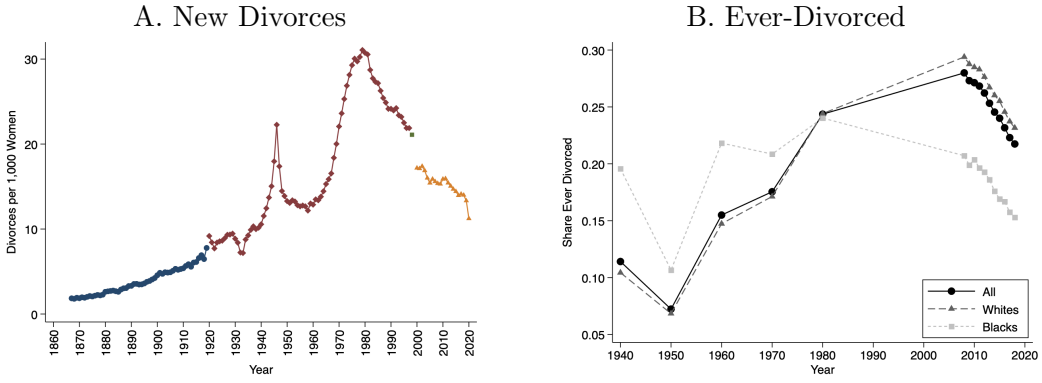
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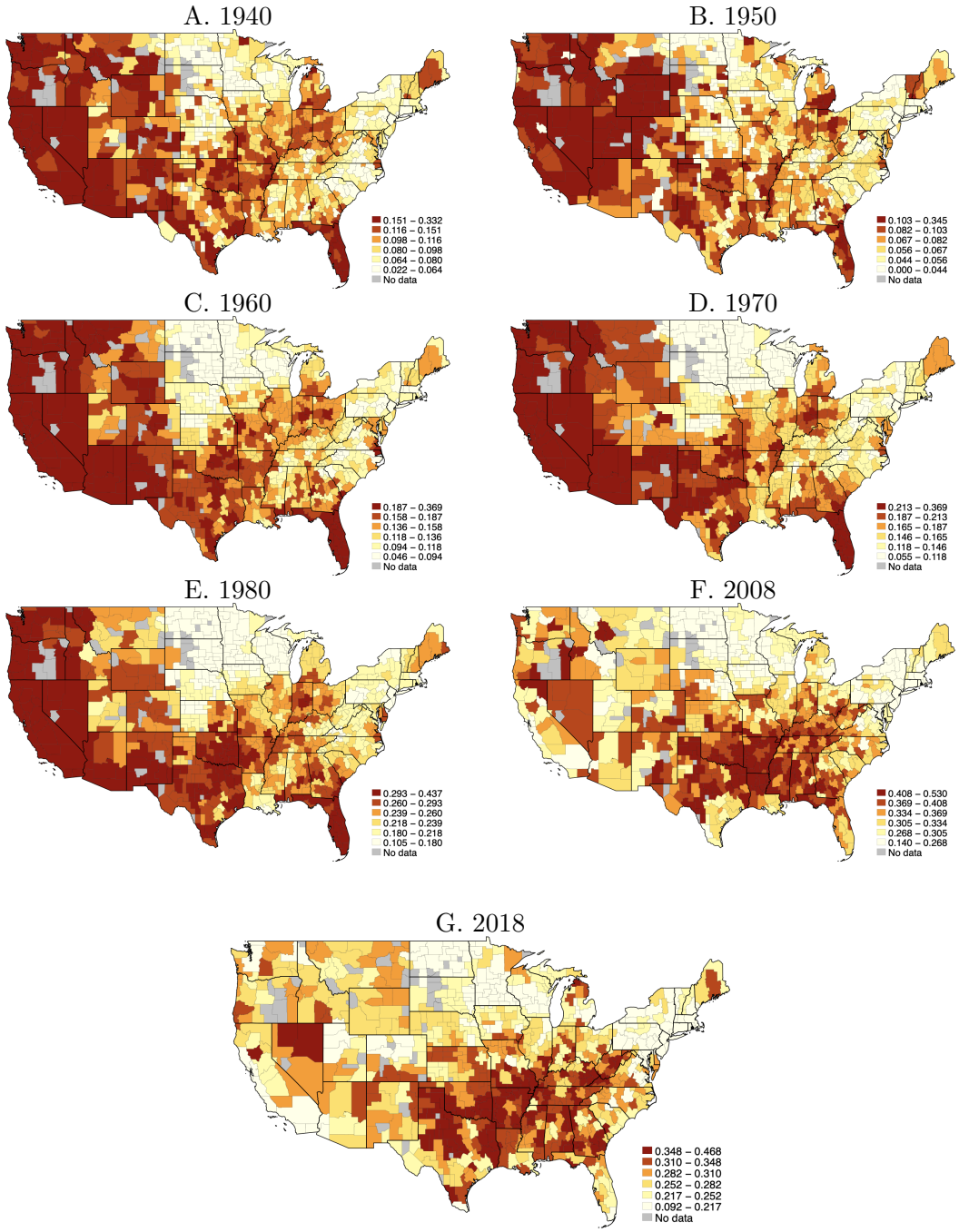
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Figure 1: Divorce Trends



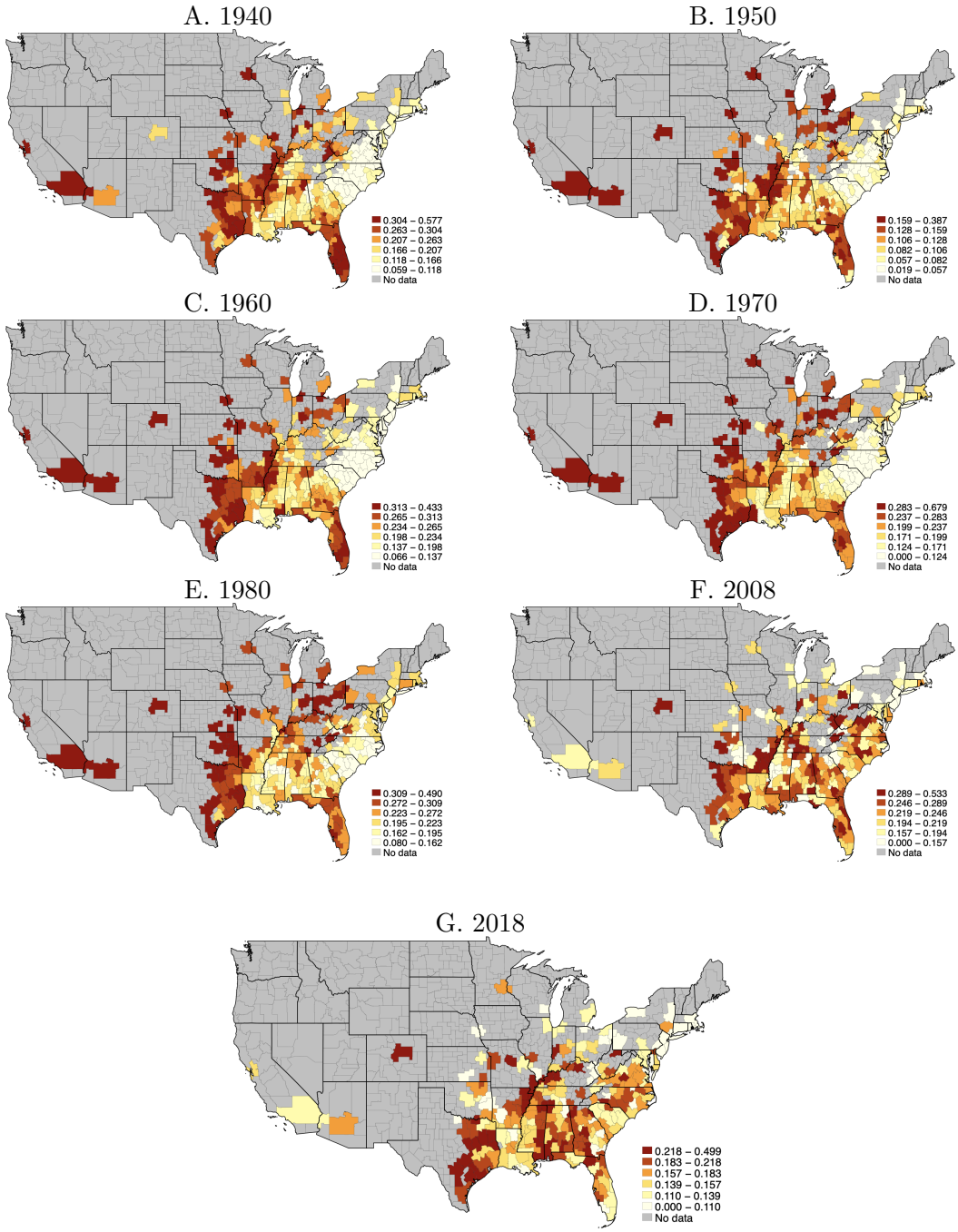
Notes - A: Data are from the National Center for Health Statistics (1973) for 1867-1919, Carter et al. (2006) for 1920-1997, Centers for Disease Control and Prevention (1999) for 1998, Centers for Disease Control and Prevention (2025) for 2000-2020, Manson et al. (2023), and Ruggles et al. (2025). The 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, by race (all, whites, blacks) and year.

Figure 2: Geography of Divorce, Whites



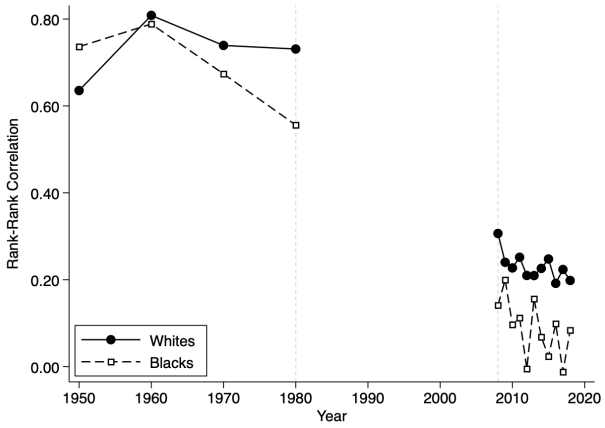
Notes - Data are from the 1940-1980 censuses as well as the 2008 and 2018 ACS (Ruggles et al., 2024, 2025). The figures show the share of US-born non-hispanic white women aged 25-50 in each CZ who were ever divorced, by year. The sample is restricted to CZs with at least 50 sample-line persons from the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

Figure 3: Geography of Divorce, Blacks



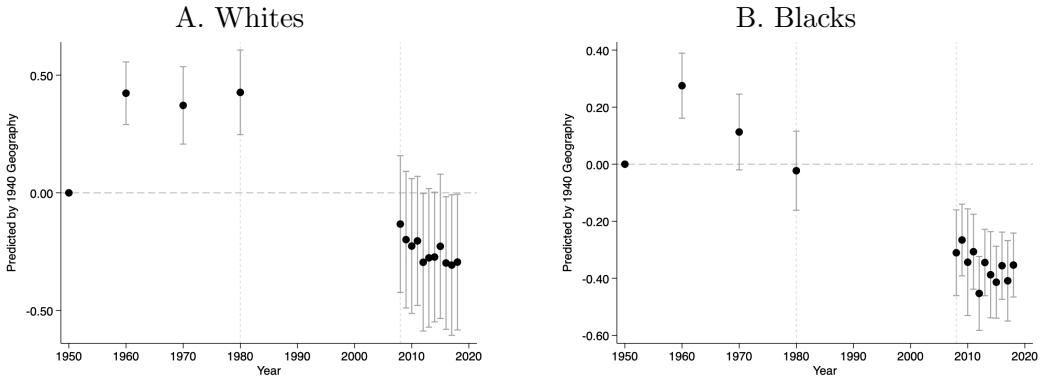
Notes - Data are from the 1940-1980 censuses as well as the 2008 and 2018 ACS (Ruggles et al., 2024, 2025). The figures show the share of US-born non-hispanic black women aged 25-50 in each CZ who were ever divorced, by year. The sample is restricted to CZs with at least 50 sample-line persons from the target population in 1940. The sample size is 235 CZs.

Figure 4: Persistence and Change in Geographic Patterns, Rank-Rank Correlations



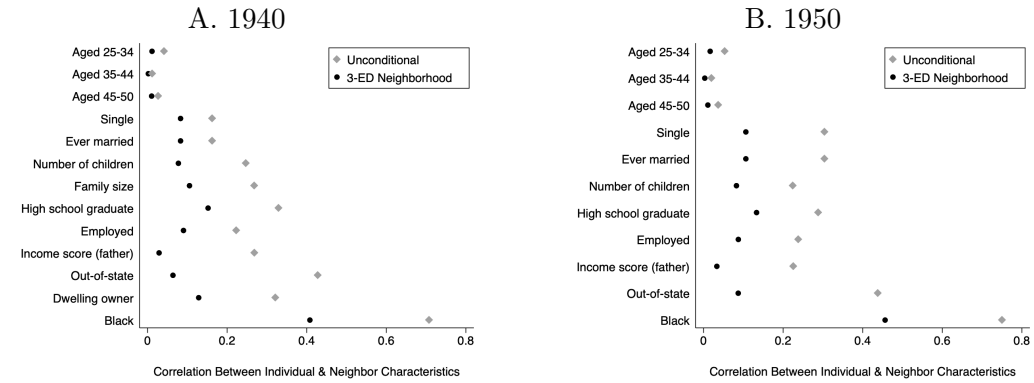
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). Each marker denotes the CZ-level rank-rank correlation between the share of US-born non-hispanic women aged 25-50 in each CZ who were ever divorced as of 1940 and the corresponding share in a subsequent year given along the x-axis, by race. The sample sizes are 679 (for whites) and 235 (for blacks) CZs.

Figure 5: Relevance of the Geography of Divorce



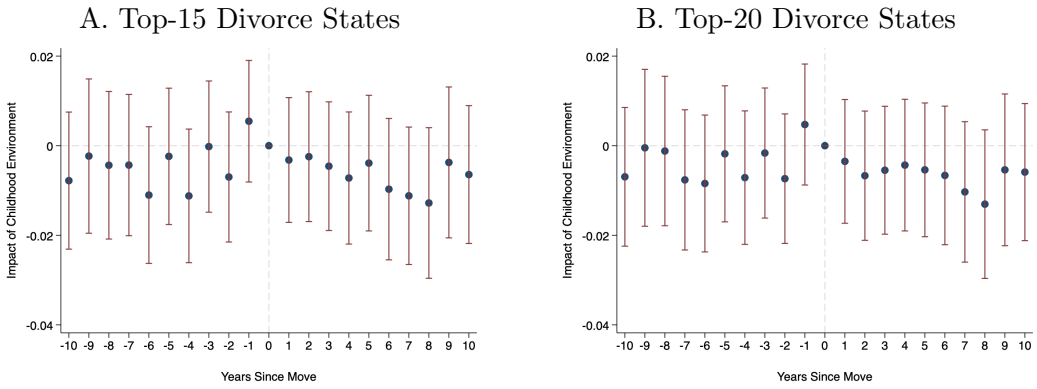
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures show the interaction coefficients from a regression of the share of US-born non-hispanic women aged 25-50 in each CZ who were ever divorced on the interaction between the corresponding share in 1940 and year indicators, controlling for CZ and year fixed effects. This is done separately for whites (A) and blacks (B). The omitted year is 1950. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample sizes are 10,185 (A) and 3,525 (B) CZ-year observations.

Figure 6: Individual and Neighbor Characteristics



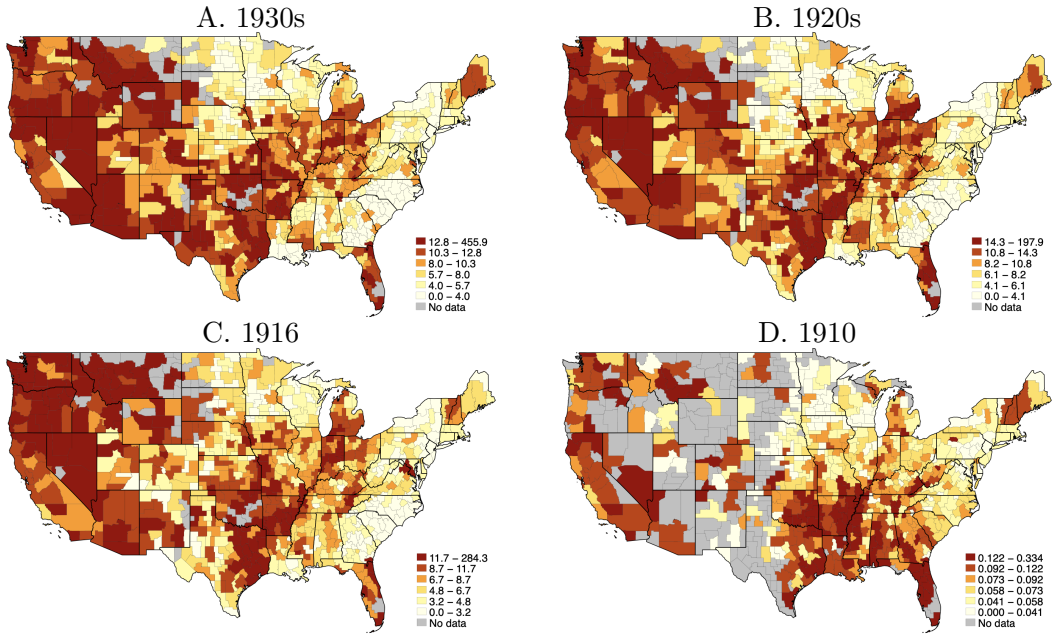
Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024). The underlying sample comprises sample-line US-born non-hispanic women aged 25-50. The figures show the correlations between the characteristics (along the y-axis) of individuals and their neighbors. These correlations 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 7: Childhood Environment and Divorce Propensity



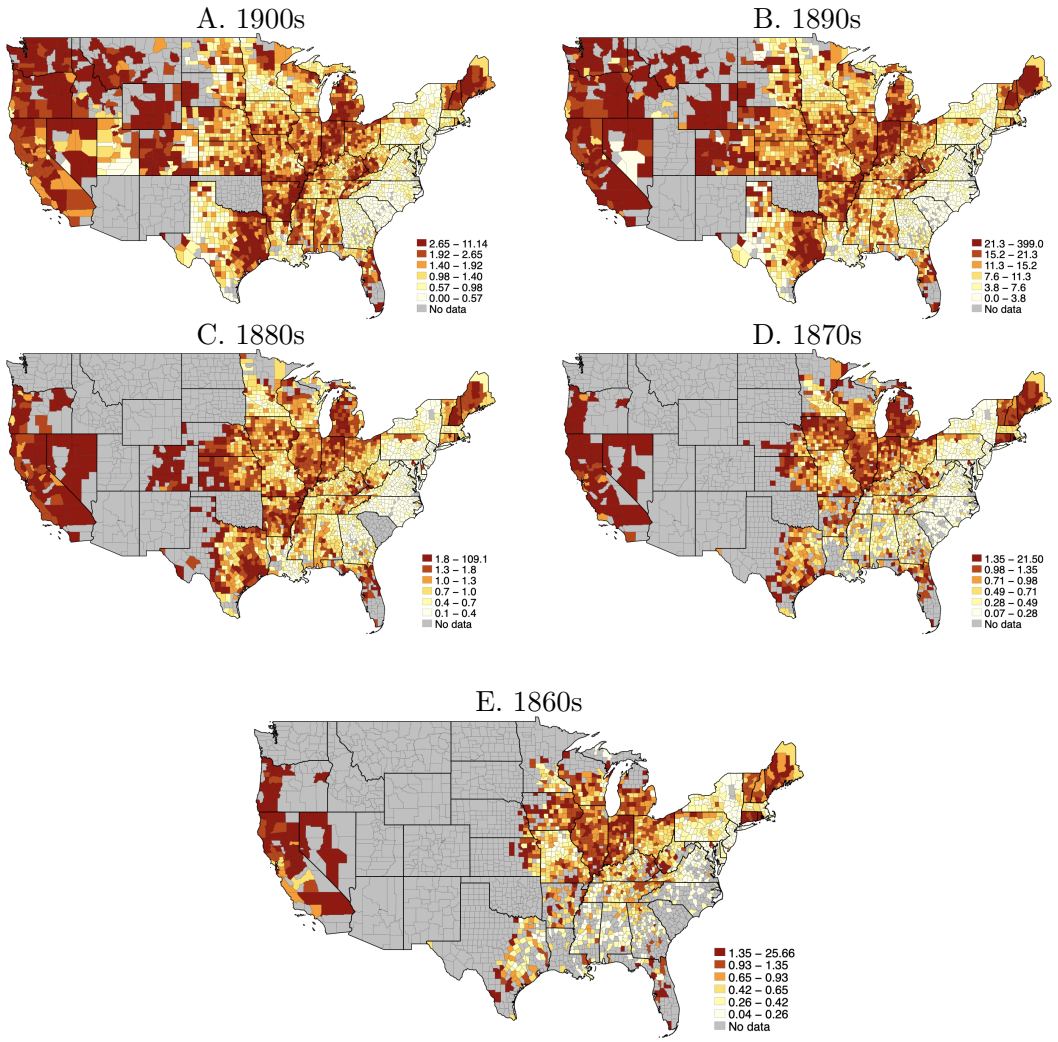
Notes - Data are from the 1910 and 1940 full count censuses (Ruggles et al., 2024) and the Census Tree links (Price et al., 2023). The underlying sample comprises 0-18-year-old daughters from households that moved to a top-15 (A) or top-20 (B) divorce state by 1910, and who can be followed to 1940. The figures show the coefficients from a regression of an indicator for being divorced in 1940 on a set of indicators for the number of years since a household moved to a top-15 (A) or top-20 (B) divorce state at the time the daughter was born, controlling for household and cohort fixed effects. 95 percent confidence bands based on standard errors clustered at the household level are displayed. The sample sizes are 69,976 (A) and 68,031 (B) observations.

Figure 8: Geography of Divorce, Early-20th Century



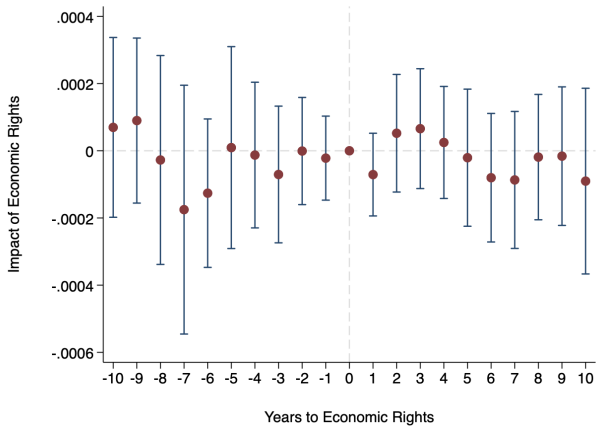
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the 1910 1 percent IPUMS sample (Ruggles et al., 2025). A-C: The figures show the number of divorces per 1,000 women in the 1930s (1930-1933) (A), the 1920s (1920-1929) (B), and in 1916 (C). D: The figure shows the share of US-born non-hispanic women aged 25-50 in each CZ who were ever divorced by 1910. The sample sizes are 685 (A-C) and 554 (D) CZs.

Figure 9: Geography of Divorce, Reports Authorized by Congress



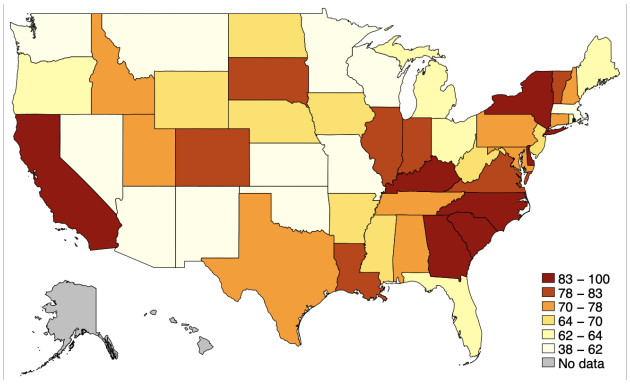
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The figures show the number of divorces per 1,000 women in the 1900s (1900-1906) (A), 1890s (1890-1899) (B), 1880s (1880-1889) (C), 1870s (1870-1877) (D), and 1860s (1867-1869) (E). The sample sizes range from 1,526 (E) to 2,768 (A) counties.

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



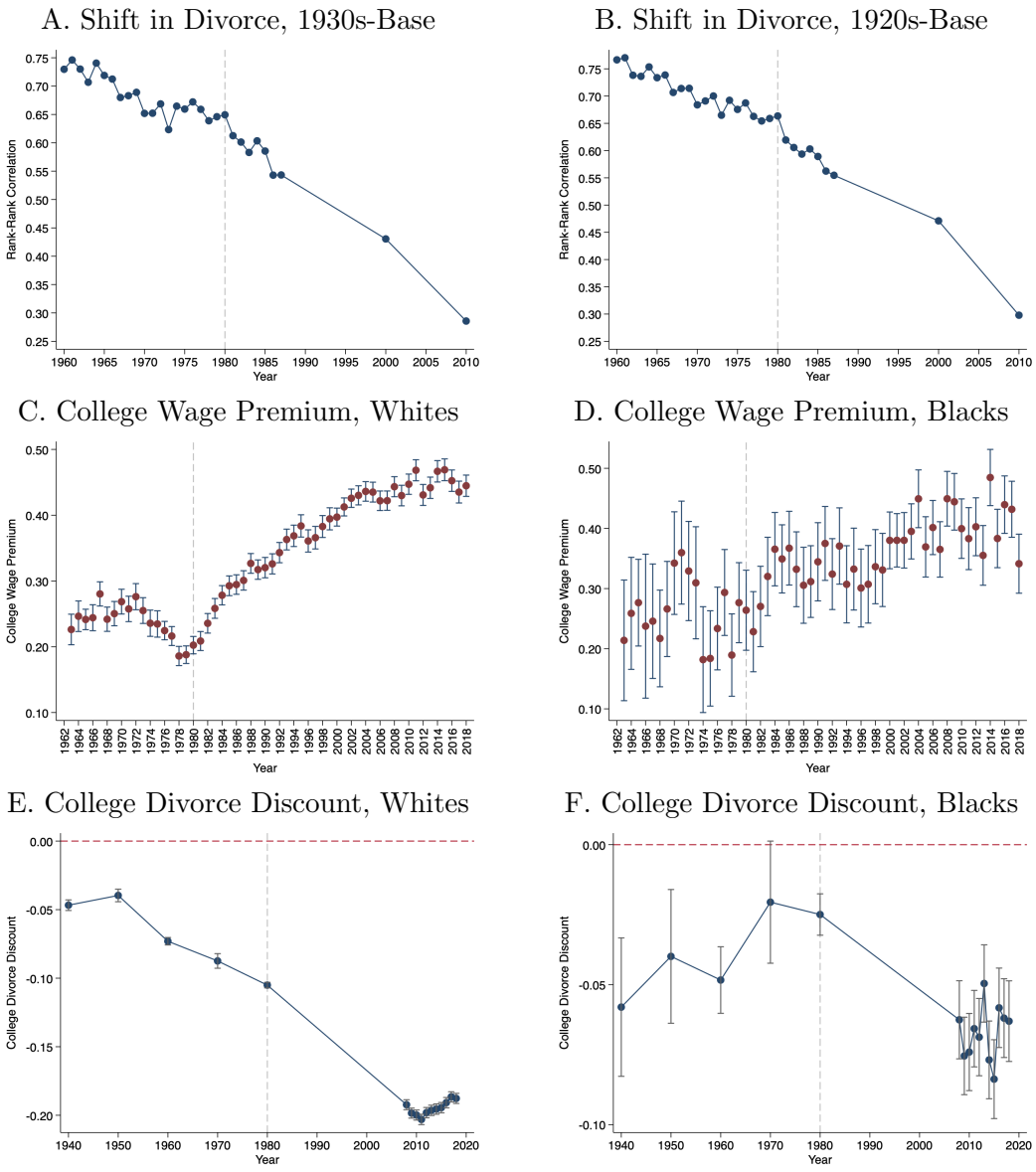
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The unit of observation is the county-year. Each marker denotes the coefficient from a regression of the number of divorces per 1,000 women on indicators for the number of years since women's 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 Divorce Legislation



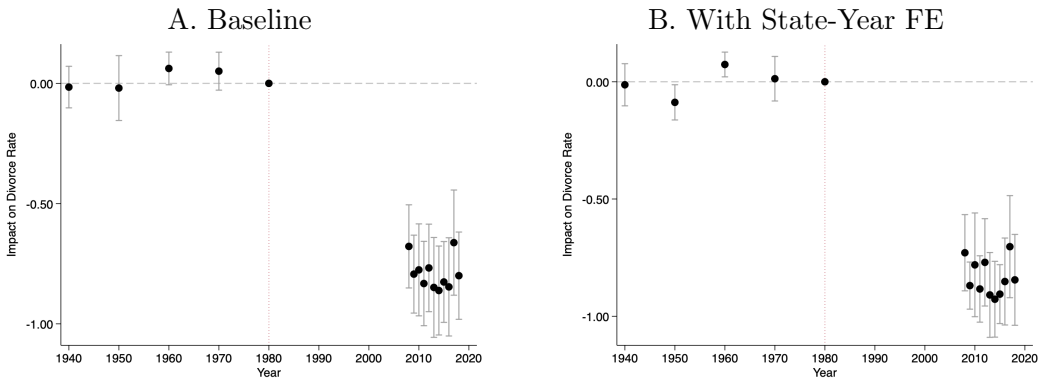
Notes - Data are from the Bureau of the Census (1908). The figure shows the strictness score of each state's divorce legislation up until 1907, determined by feeding the raw text of every state's legislation into OpenAI's GPT 5.1 model. Darker colors indicate stricter divorce rules.

Figure 12: Divorce and Skill Level, Stylized Facts



Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018), the BGSU Marriage and Divorce Data Compass (2012, 2016), the Bureau of the Census (1908), the 1964-2019 CPS (Flood et al., 2025), as well as the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). Three stylized facts are presented. A and B: The figures show the CZ rank-rank correlations between the average divorce rate in each CZ during the 1930s (A) or 1920s (B) and the corresponding divorce rate in subsequent years. C and D: The figures show 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 and race. Sample weights are used in each regression. E and F: The figures show the coefficients from regressions of an indicator for ever-divorced persons on an indicator for those with a 4-year college degree or higher, controlling for a quadratic in age, among ever-married US-born non-hispanic white (E) or black (F) women aged 25-50. C-F: 95 percent confidence bands are displayed.

Figure 13: 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 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.

Table 1: Selective Migration by Ever-Divorced Status

Definition of low/high-divorce CZs	1940				1950			
Initial CZ	Low-Divorce		High-Divorce		Low-Divorce		High-Divorce	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Whites				
Migrant	0.023***	0.021***	-0.013***	-0.009***	0.023***	0.015***	0.014***	0.012***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Outcome mean	0.048	0.048	0.087	0.087	0.061	0.061	0.072	0.072
Adjusted R-squared	0.004	0.005	0.005	0.008	0.004	0.012	0.004	0.014
N	141,861	141,861	119,014	119,014	135,837	135,837	121,962	121,962
				Panel B: Blacks				
Migrant	0.033	0.019	0.002	0.006	0.052***	0.043**	0.015	0.016
	(0.026)	(0.027)	(0.045)	(0.049)	(0.019)	(0.019)	(0.041)	(0.036)
Outcome mean	0.076	0.076	0.178	0.178	0.134	0.134	0.113	0.113
Adjusted R-squared	0.012	0.011	0.019	0.028	0.021	0.063	0.013	0.029
N	7,109	7,109	6,208	6,208	4,777	4,777	6,639	6,639
Cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Initial CZ fixed effects		Y		Y		Y		Y

Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024) and the IPUMS Multigenerational Longitudinal Panel (Helgertz et al., 2024). The base sample comprises sample-line US-born non-hispanic women aged 25-40 in the 1940 census who can be linked forward to the 1950 census. CZs are split into low- and high-divorce areas based on the corresponding ever-divorced rate of US-born non-hispanic women aged 25-40 in 1940 who stayed in the same CZ between 1940 and 1950, divided along the median. These rates are either measured using information from the 1940 (1940 definition) or 1950 (1950 definition) census. Columns 1-2 and 5-6 compare 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 3-4 and 7-8 compare those who started in high-divorce CZs, some of whom stayed in the same CZ (stayers) while others left for low-divorce CZs (migrants). The results are arranged separately for whites (Panel A) and blacks (Panel B). 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). All specifications include cohort fixed effects; even columns further add fixed effects for the initial CZ of residence in 1940. Standard errors clustered by the 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)	(7)	(8)
Social interaction effect	0.003***	0.002***	0.002***	0.002***	0.002***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Sample	All	White groups	All	One mover	Both movers	All	White groups	All
Individual fixed effects			Y					Y
Outcome mean	0.016	0.013	0.016	0.013	0.027	0.011	0.009	0.011
N	12,703,706	7,123,947	12,703,706	565,790	395,823	11,362,968	7,231,327	11,362,968

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 women aged 25-50. 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 7 restrict the sample to reference groups with no black residents in the population of interest. Columns 3 and 8 include individual fixed effects for each member of a resident pair. Column 4 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 5 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 residing in different local areas. 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 Status

	Whites		Blacks	
Parent (household head)	Mother	Father	Mother	Father
	(1)	(2)	(3)	(4)
Parent divorced in 1910	0.082***	0.061***	0.100	-0.137
	(0.014)	(0.026)	(0.123)	(0.168)
Daughter cohort fixed effects	Y	Y	Y	Y
Parent cohort fixed effects	Y	Y	Y	Y
1910 CZ fixed effects	Y	Y	Y	Y
Outcome mean	0.089	0.084	0.141	0.129
Adjusted R-squared	0.018	0.010	0.093	0.036
N	11,767	283,989	566	6,973

Notes - Data are from the 1910 and 1940 full count censuses (Ruggles et al., 2024) and the Census Tree links (Price et al., 2023). The base sample comprises 0-18-year-old US-born non-hispanic daughters in the 1910 census who can be linked forward to 1940. Only sample-line respondents in 1940 are kept. Columns 1-2 look at white daughters with white mothers while columns 3-4 look at black daughters with black mothers. Columns 1 and 3 focus on daughters of female household heads, while columns 2 and 4 focus on daughters of male household heads. 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 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 5: 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.

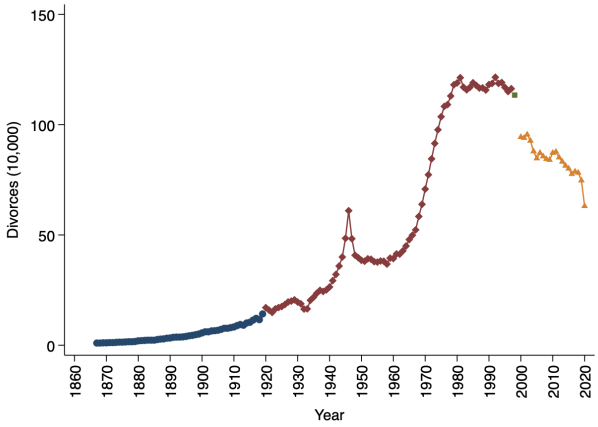
Table 6: Impact of College Share on Divorce

	(1)	(2)	(3)	(4)
Share of college graduates, 1980	-0.806***	-0.831***	-0.983***	-0.870***
	(0.094)	(0.067)	(0.032)	(0.035)
Estimator	OLS	OLS	2SLS	2SLS
State-by-year fixed effects		Y		Y
Outcome mean	0.254	0.254	0.254	0.254
First stage F-statistic			4,696	4,881
Adjusted R-squared	0.857	0.893		
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 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 1950, 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.

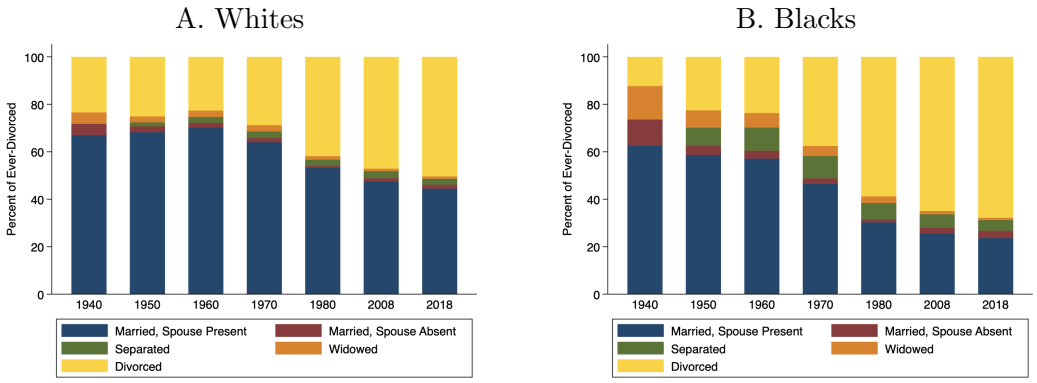
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Figure A.1: Raw Divorce Trends



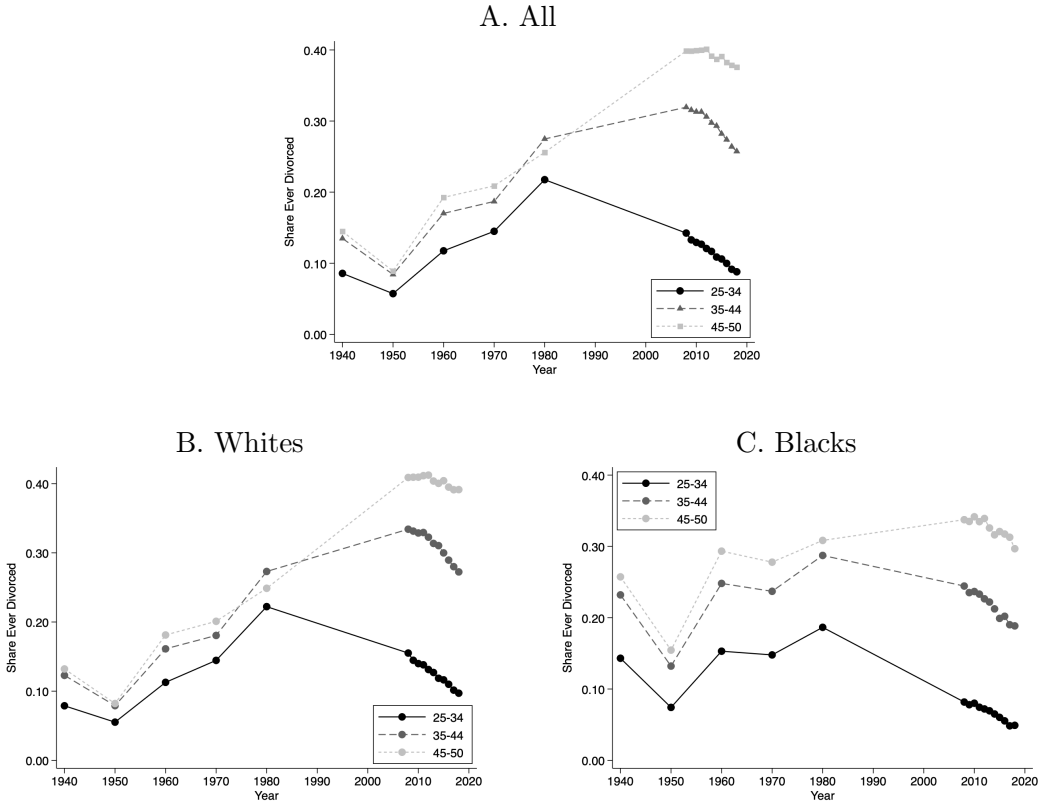
Notes - Data are from the National Center for Health Statistics (1973) for 1867-1919, Carter et al. (2006) for 1920-1997, Centers for Disease Control and Prevention (1999) for 1998, and Centers for Disease Control and Prevention (2025) for 2000-2020. The figure shows the number of divorces (10,000),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

Figure A.2: Current Marital Status of Ever-Divorced Women



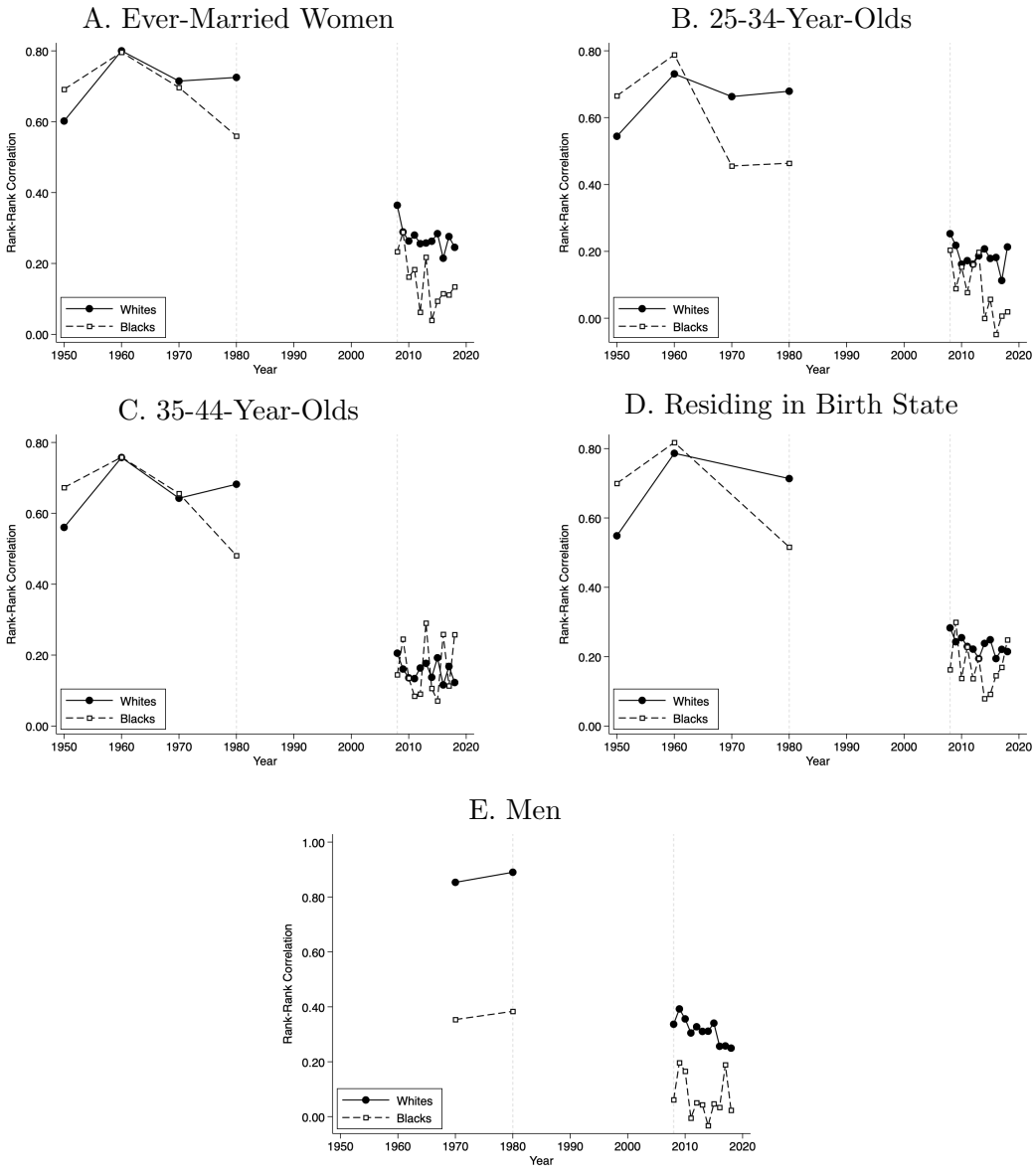
Notes - Data are from the 1940-1980 censuses as well as the 2008 and 2018 ACS (Ruggles et al., 2024, 2025). The figures show the current marital status of ever-divorced US-born non-hispanic women aged 25-50 (percentage breakdown), by race and year.

Figure A.3: Ever-Divorced Trends, by Age Group



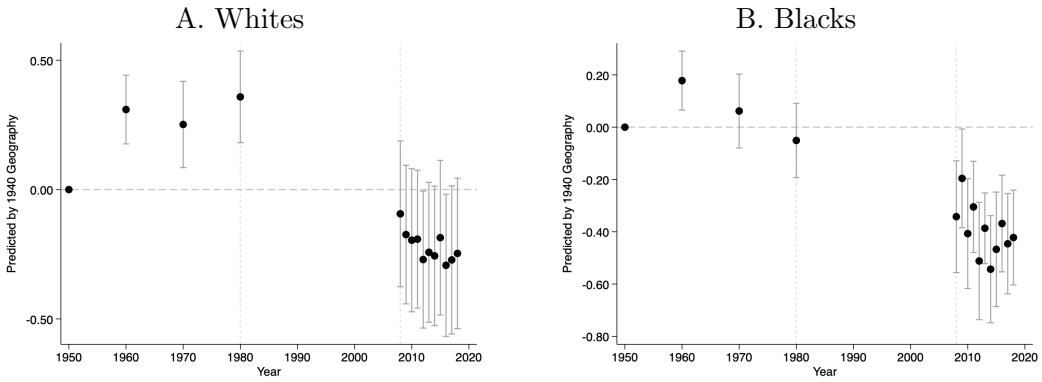
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures shows the share of US-born non-hispanic women aged 25-50 who were ever divorced, by race (all, whites, blacks), age group (25-34, 35-44, 45-50), and year.

Figure A.4: Persistence and Change in Geographic Patterns, Alternative Rank-Rank Correlations



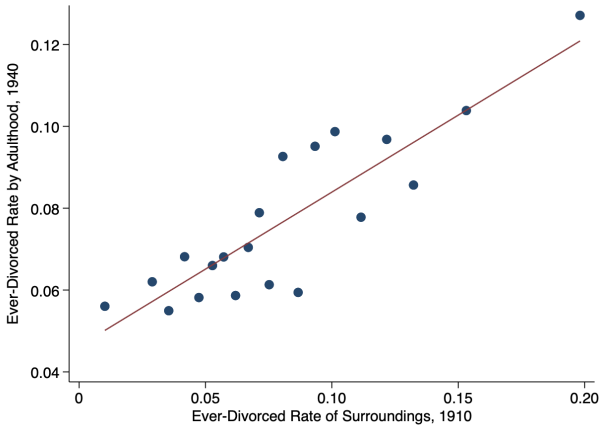
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The baseline for comparison is Figure 4, which displays the CZ-level rank-rank correlations between the share of US-born non-hispanic women aged 25-50 in each CZ who were ever divorced as of 1940 and the corresponding share in subsequent years, by race. The figures here display the corresponding 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 for whites, and from 165 (C) to 454 (E) CZs for blacks.

Figure A.5: Relevance of the Geography of Divorce, Alternative Base



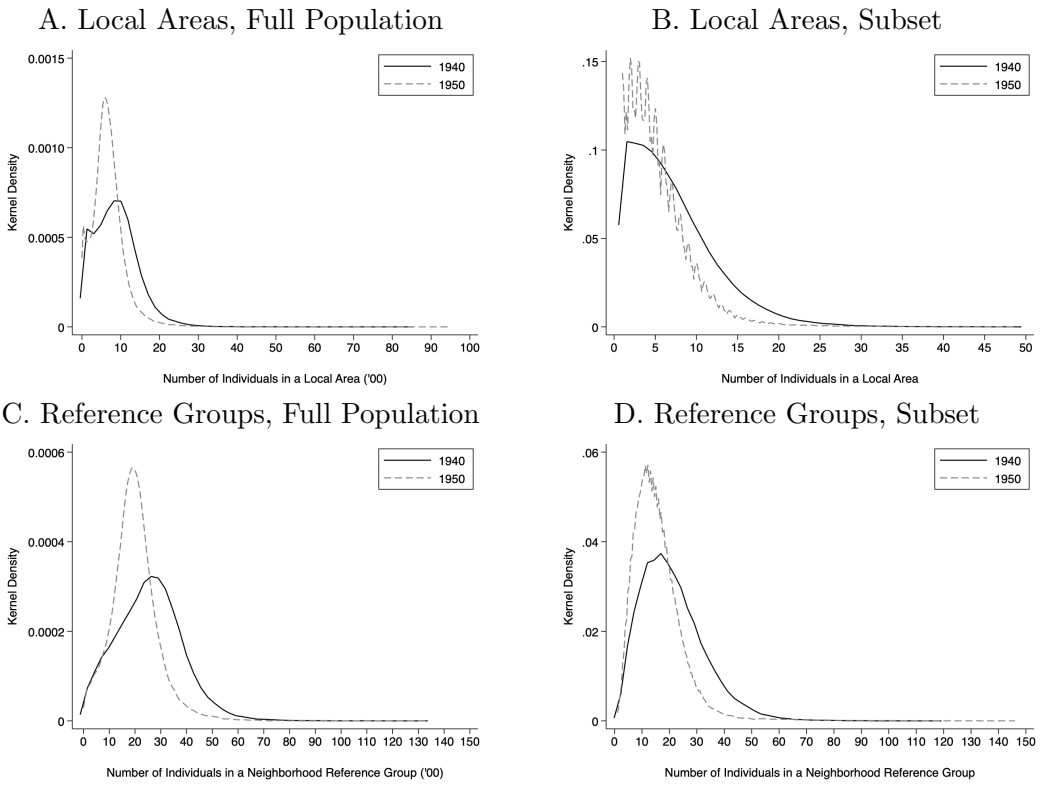
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures show the interaction coefficients from a regression of the share of ever-married US-born non-hispanic women aged 25-50 in each CZ who were ever divorced on the interaction between the corresponding share in 1940 and year indicators, controlling for CZ and year fixed effects. This is done separately for whites (A) and blacks (B). The omitted year is 1950. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample sizes are 10,140 (A) and 3,374 (B) CZ-year observations.

Figure A.6: Place of residence and Divorce Outcomes



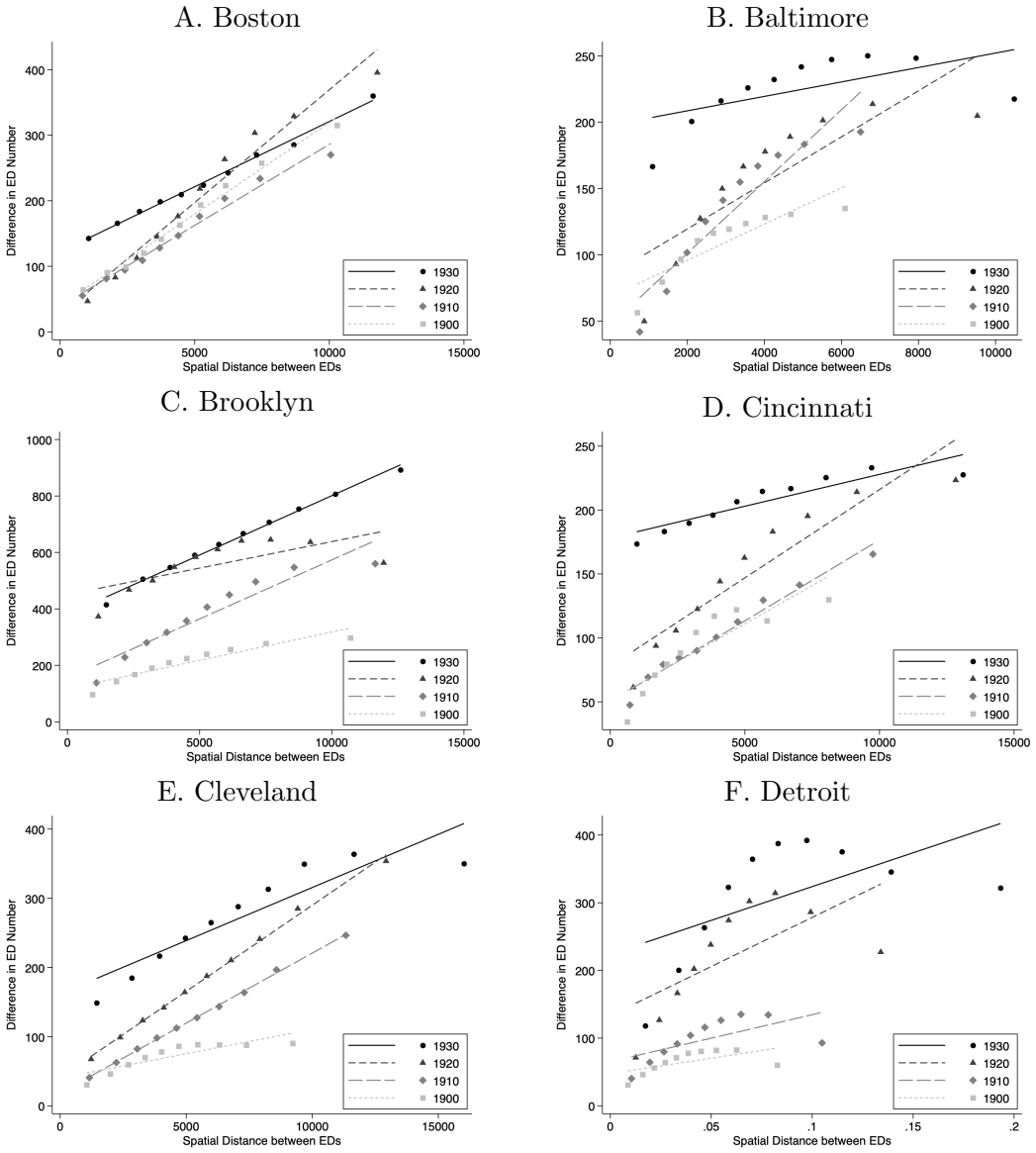
Notes - Data are from the 1910 and 1940 full count censuses (Ruggles et al., 2024), the 1910 IPUMS sample (Ruggles et al., 2025), and the Census Tree links (Price et al., 2023). The underlying sample comprises 0-10-year-old US-born non-hispanic girls in the 1910 census who can be linked forward to the 1940 census and who remained in the same CZ between 1910 and 1940. Only sample-line respondents in 1940 are kept. The figure show a binscatter of the mean ever-divorced rate for individuals in the linked sample when they are adults in 1940 (y-axis) against the ever-divorced rate of their CZs in 1910 (x-axis). The sample size is 554 CZs.

Figure A.7: Local Areas and Neighborhood Reference Groups, Population Distribution



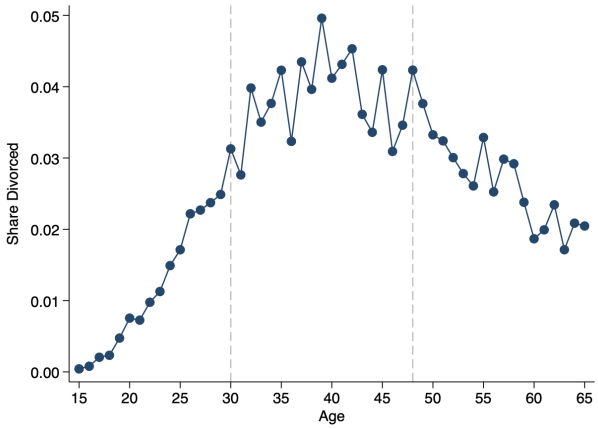
Notes - Data are from the 1940 and 1950 full count censuses (Ruggles et al., 2024). The figures show the distribution of the number of persons (A and C) or sample-line US-based non-hispanic women aged 25-50 (B and D) residing in a local area (A and B) or neighborhood reference group (C and D).

Figure A.8: Differences in ED Numbering and Distance



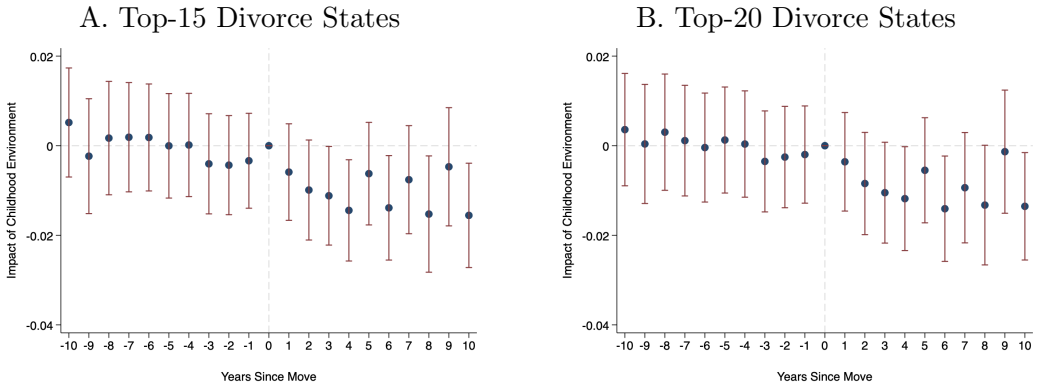
Notes - Data are from the 1900-1930 ED shapefiles for 11 US cities (Shertzer et al., 2016). The figures plot the relation between the difference in ED numbering (y-axis) and the spatial distance between EDs (x-axis), by city and year.

Figure A.9: Lifecycle of Divorce



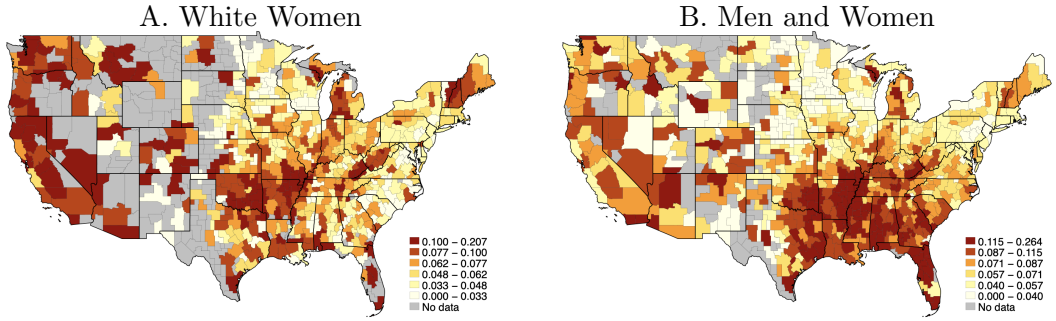
Notes - Data are from the 1940 1 percent IPUMS sample (Ruggles et al., 2025). The figure shows the share of US-born non-hispanic women who are divorced as of 1940, by age.

Figure A.10: Childhood Environment and Divorce Propensity in 1930



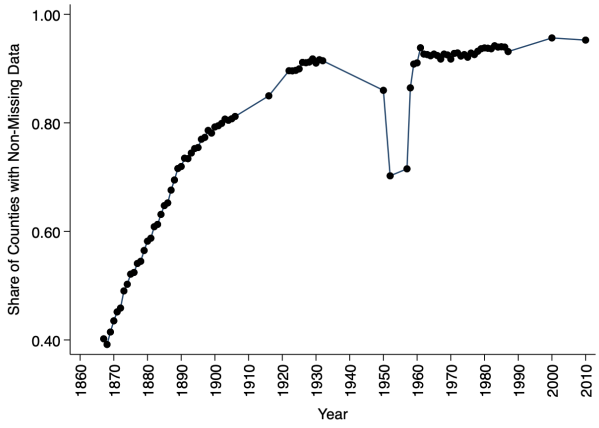
Notes - Data are from the 1910 and 1930 full count censuses (Ruggles et al., 2024) and the Census Tree links (Price et al., 2023). The underlying sample comprises 0-18-year-old daughters from households that moved to a top-15 (A) or top-20 (B) divorce state by 1910, and who can be followed to 1930. The figures show the coefficients from a regression of an indicator for being divorced in 1930 on a set of indicators for the number of years since a household moved to a top-15 (A) or top-20 (B) divorce state at the time the daughter was born, controlling for household and cohort fixed effects. 95 percent confidence bands based on standard errors clustered at the household level are displayed. The sample sizes are 82,167 (A) and 79,756 (B) observations.

Figure A.11: Alternative Geography of Divorce, 1910



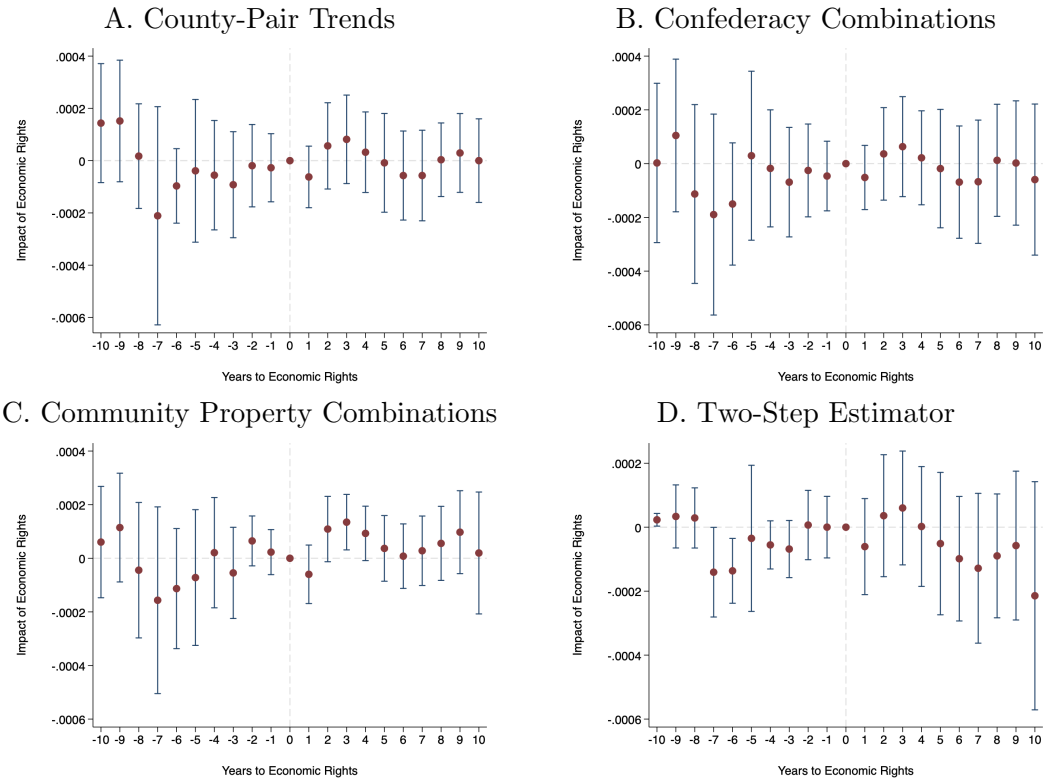
Notes - Data are from the 1910 1 percent IPUMS sample (Ruggles et al., 2025). The figures show the share of US-born non-hispanic white women (A) or men and women (B) aged 25-50 in each CZ who were ever divorced by 1910. The sample sizes are 540 (A) and 631 (B) CZs.

Figure A.12: Counties with Non-Missing Divorce Statistics



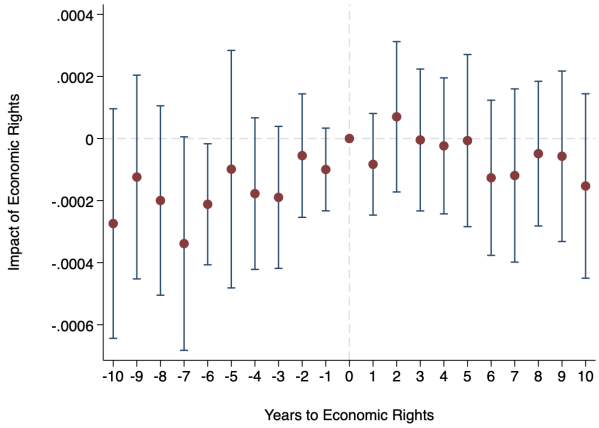
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018), supplemented with divorce statistics from 2000 and 2010 collected by the BGSU Marriage and Divorce Data Compass (2012, 2016), and 1887-1906 statistics digitized from the Bureau of the Census (1908). The figure shows the share of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

Figure A.13: Impact of Women's Economic Rights on Divorce, Robustness Checks



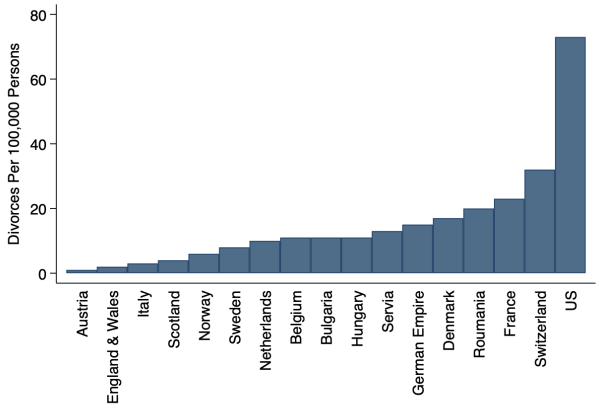
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The unit of observation is the county-year. In the baseline (Figure 10), each marker denotes the coefficient from a regression of the number of divorces per 1,000 women on indicators for the number 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.14: Impact of Women's Property Rights on Divorce



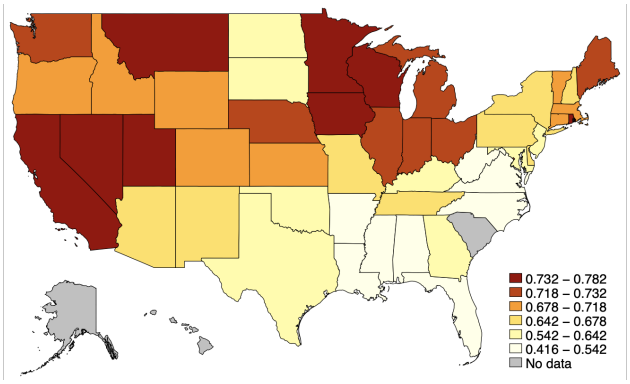
Notes - Data are from the BGSU Marriage and Divorce Data Compass (2018) and the Bureau of the Census (1908). The unit of observation is the county-year. Each marker denotes the coefficient from a regression of the number of divorces per 1,000 women on indicators for the number of years since women's 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.15: Historical Divorce Rates in 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.

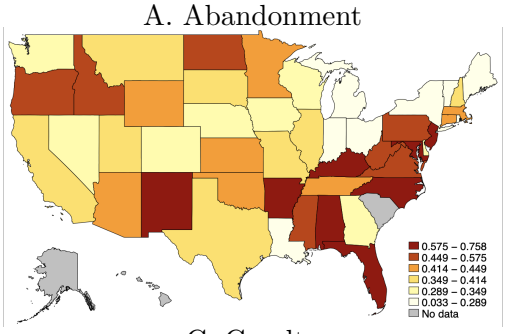
Figure A.16: Share of Divorces Initiated by Wife



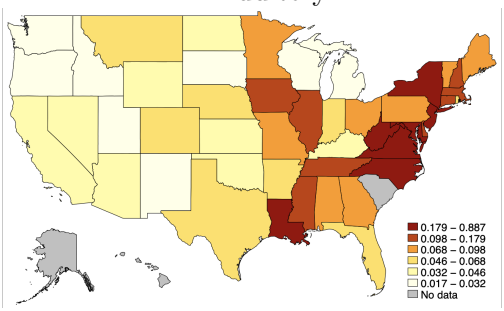
Notes - Data are from the Bureau of the Census (1908). The figure shows the share of divorces in each state that were initiated by the wife.

Figure A.17: Causes of Divorces Initiated by Wife

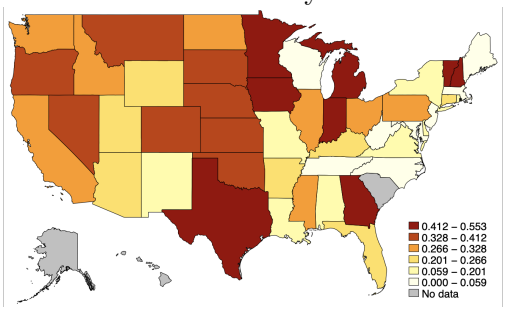
A. Abandonment



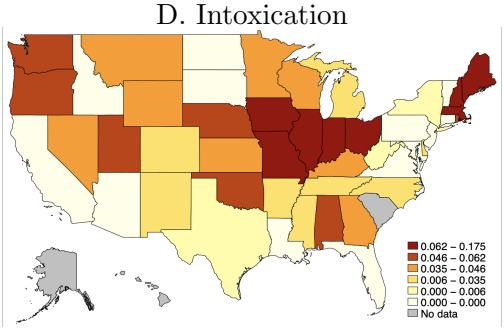
B. Adultery



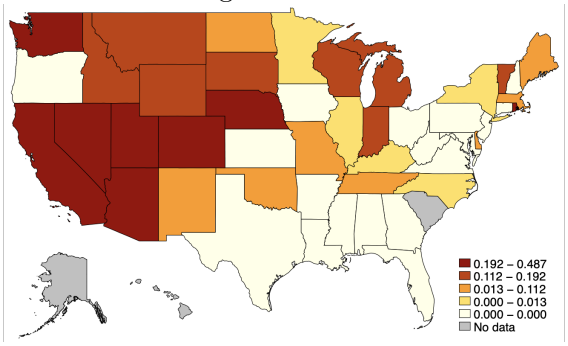
C. Cruelty



D. Intoxication

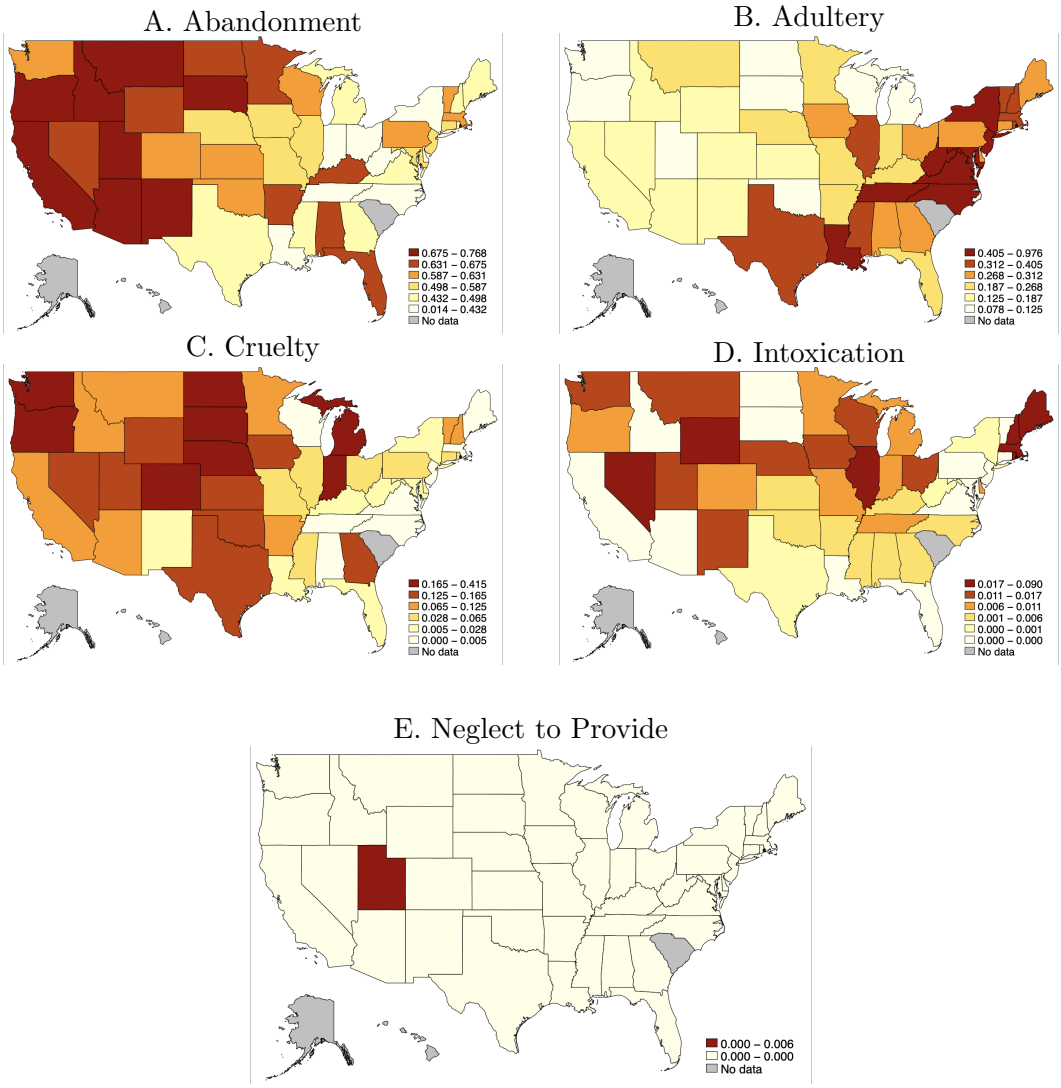


E. Neglect to Provide



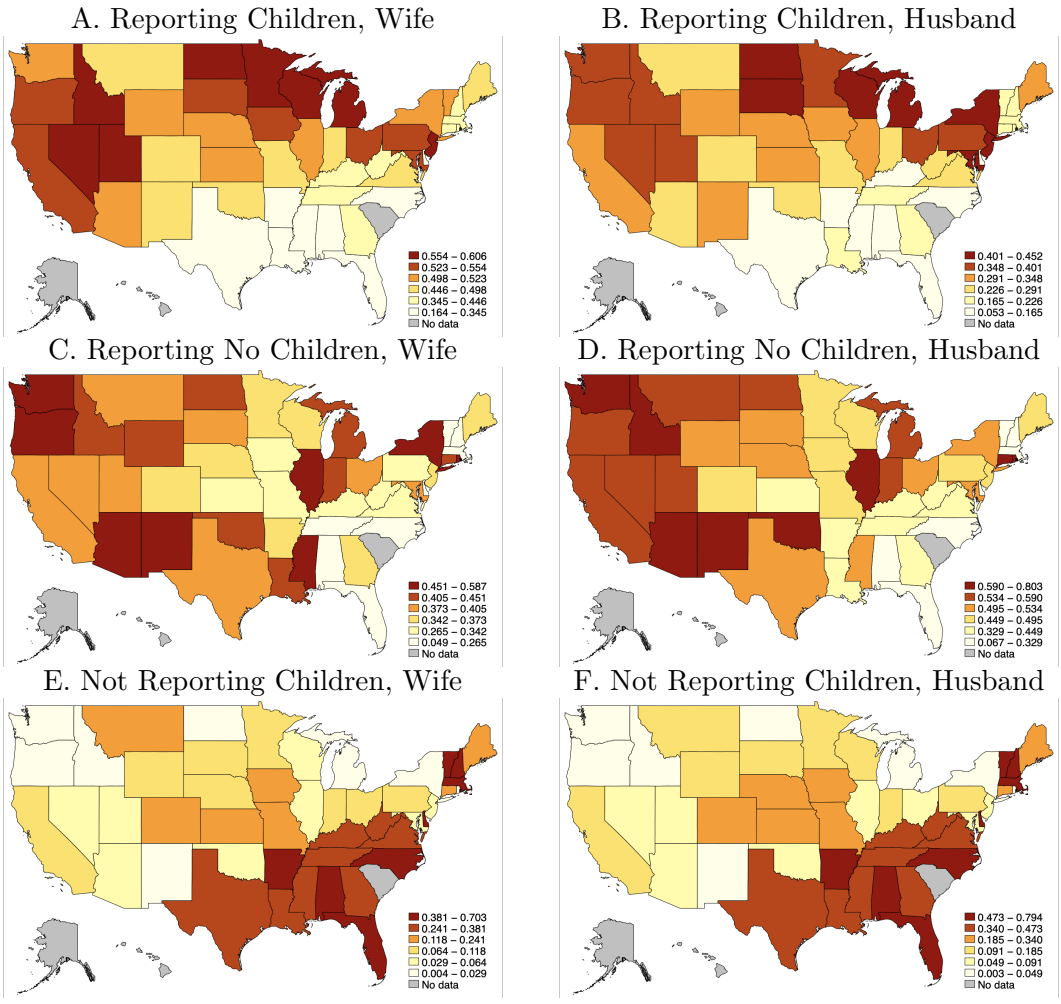
Notes - Data are from the Bureau of the Census (1908). The figures show the share of divorces initiated by the wife in each state that are caused by the factors listed in the subheadings.

Figure A.18: Causes of Divorces Initiated by Husband



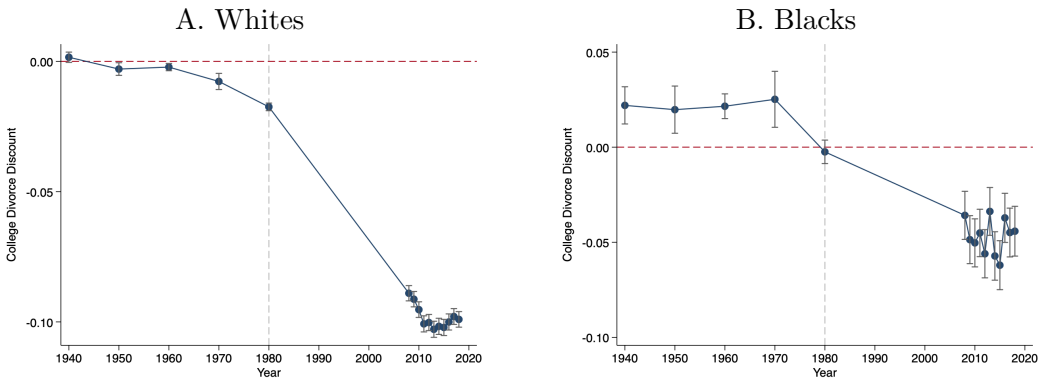
Notes - Data are from the Bureau of the Census (1908). The figures show the share of divorces initiated by the husband in each state that are caused by the factors listed in the subheadings.

Figure A.19: Divorces Involving Children



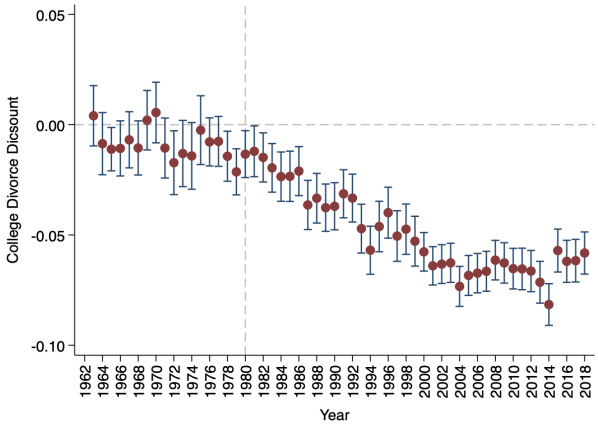
Notes - Data are from the Bureau of the Census (1908). The figures show the share of divorces initiated by the wife (A, C, E) or husband (B, D, F) in each state that report children (A and B), that report no children (C and D), or that do not report children (E and F).

Figure A.20: College Divorce Discount, Alternative Measure



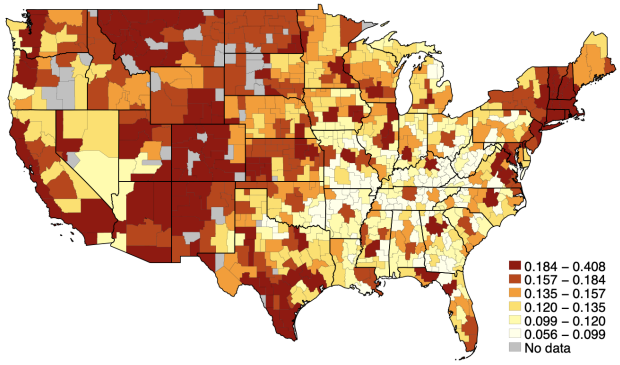
Notes - Data are from the 1940-1980 censuses and the 2008-2018 ACS (Ruggles et al., 2024, 2025). The figures show the coefficients from regressions of an indicator for currently-divorced persons on an indicator for those with a 4-year college degree or higher, controlling for a quadratic in age, among ever-married US-born non-hispanic white (A) or black (B) women aged 25-50. 95 percent confidence bands are displayed.

Figure A.21: College Divorce Discount for Whites, CPS



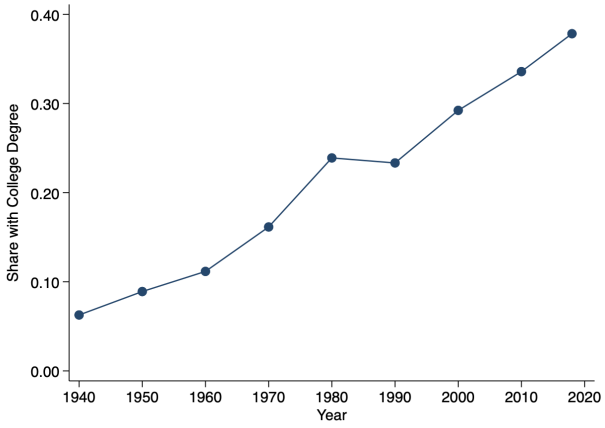
Notes - Data are from the 1964-2019 CPS (Flood et al., 2025). Each marker denotes the coefficient from a regression of an indicator for being currently divorced on an indicator for persons who completed a 4-year college degree or higher, controlling for a quadratic in age, among ever-married white women. 95 percent confidence bands are displayed. Sample weights are used in each regression.

Figure A.22: 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 women aged 25-50 in each CZ with a 4-year college degree or higher. The sample size is 679 CZs.

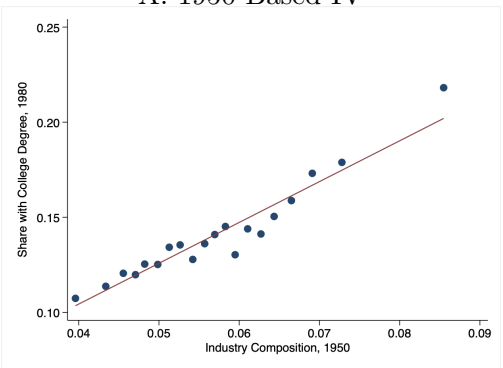
Figure A.23: Trend in College Education



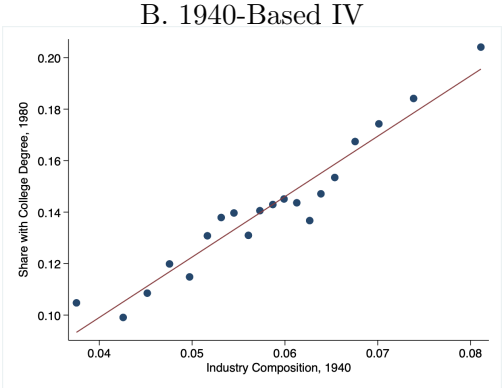
Notes - Data are from the 1940-2000 censuses as well as the 2010 and 2018 ACS (Ruggles et al., 2025). The figure shows the share of US-born non-hispanic whites and blacks aged 25-50 with a 4-year college degree or higher, by year.

Figure A.24: Instrument Relevance

A. 1950-Based IV

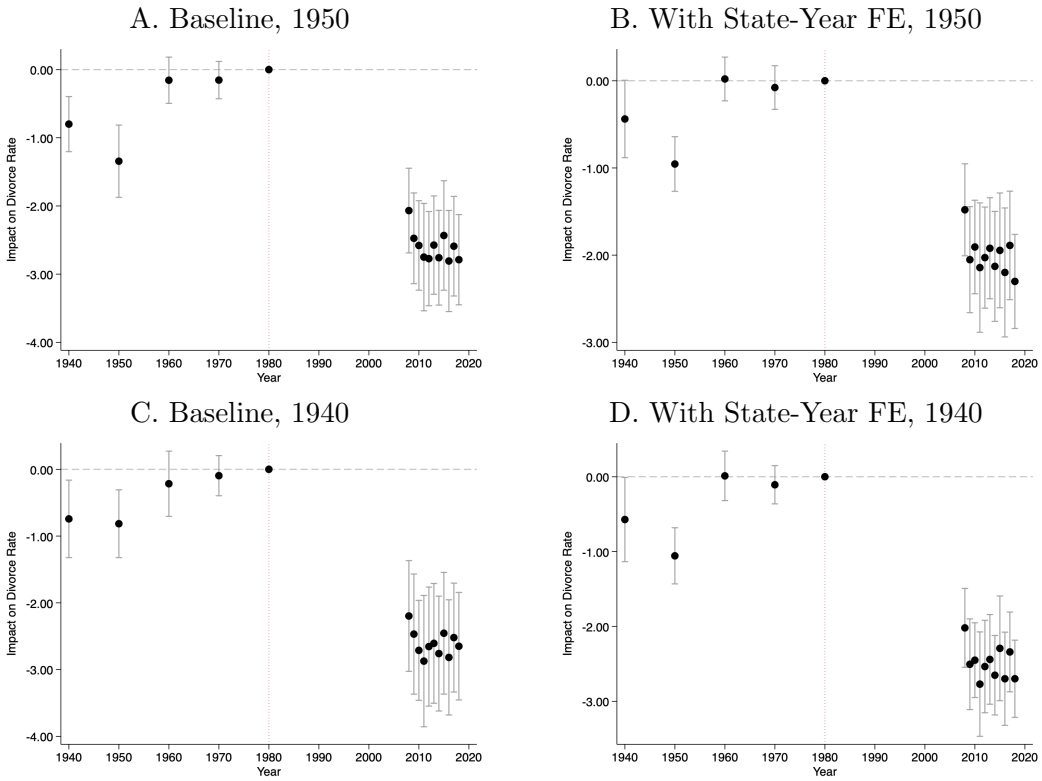


B. 1940-Based IV



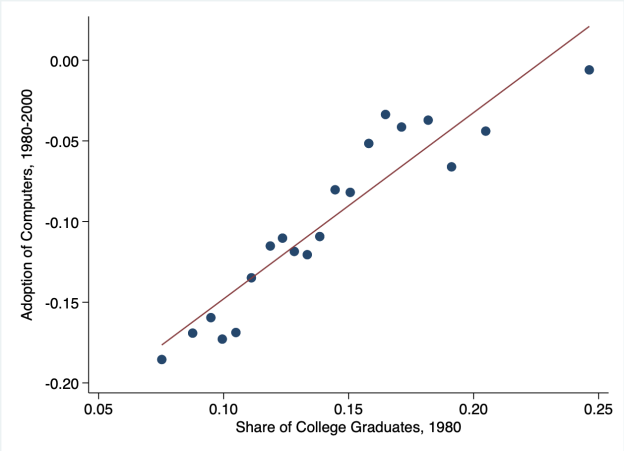
Notes - Data are from the 1940, 1950, and 1980 censuses (Ruggles et al., 2024, 2025). The figures show binscatters of the share of college graduates in a given CZ as of 1980 (y-axis) against the 1950- (A) or 1940-based (B) industry instrument (x-axis). The sample size is 679 CZs.

Figure A.25: 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 women aged 25-50 in each CZ who were ever divorced on the interaction between the 1950- (A and B) or 1940-based (C and D) industry IV and year dummies, controlling for CZ and year fixed effects. B and D 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.26: College Share and Computer Adoption



Notes - Data are from Autor and Dorn (2013) and the 1980 census (Ruggles et al., 2025). The figure shows a binscatter of the adoption of PCs between 1980-2008 in a CZ (y-axis) against the share of US-born non-hispanic women aged 25-50 in a CZ with a 4-year college degree or higher in 1980 (x-axis). The sample size is 679 CZs.

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 Status, Ever-Married Daughters

	Whites		Blacks	
Parent (household head)	Mother	Father	Mother	Father
	(1)	(2)	(3)	(4)
Parent divorced in 1910	0.091***	0.059***	0.017	-0.166
	(0.017)	(0.030)	(0.160)	(0.276)
Daughter cohort fixed effects	Y	Y	Y	Y
Parent cohort fixed effects	Y	Y	Y	Y
1910 CZ fixed effects	Y	Y	Y	Y
Outcome mean	0.110	0.103	0.208	0.182
Adjusted R-squared	0.012	0.008	0.174	0.042
N	9,379	230,303	385	4,897

Notes - Data are from the 1910 and 1940 full count censuses (Ruggles et al., 2024) and the Census Tree links (Price et al., 2023). The base sample comprises 0-18-year-old US-born non-hispanic daughters in the 1910 census who can be linked forward to 1940. Only sample-line respondents who were ever-married in 1940 are kept. Columns 1-2 look at white daughters with white parents while columns 3-4 look at black daughters with black parents. Columns 1 and 3 focus on daughters of female household heads, while columns 2 and 4 focus on daughters of male household heads. 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, Unblundling 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)	0.006***	0.000	0.000	-0.008***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Estimator	OLS	OLS	OLS	2SLS	2SLS
Geoclimatic controls		Y	Y	Y	Y
State fixed effects			Y	Y	Y
Outcome mean	0.105	0.105	0.105	0.105	0.105
Adjusted R-squared	0.015	0.242	0.332		
First-stage F-statistic				621	584
N	1,806	1,806	1,806	1,806	1,806
	Panel B: Share Ever-Divorced in 1950				
Total frontier experience (decades)	0.004***	0.003***	0.003**	0.000	0.000
	(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	0.069	0.069	0.069	0.069	0.069
Adjusted R-squared	0.015	0.096	0.175		
First stage F-statistic				494	472
N	1,621	1,621	1,621	1,621	1,621

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 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: Long-Run Impact of Frontier Conditions on Divorce, Whites

	(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.6: Long-Run Impact of Frontier Conditions on Divorce, Blacks

	(1)	(2)	(3)	(4)	(5)
	Panel A: Share Ever-Divorced in 1940				
Total frontier experience (decades)	0.004	0.014**	0.017***	-0.004	-0.003
	(0.007)	(0.006)	(0.005)	(0.009)	(0.009)
Estimator	OLS	OLS	OLS	2SLS	2SLS
Geoclimatic controls		Y	Y	Y	Y
State fixed effects			Y	Y	Y
Outcome mean	0.232	0.232	0.232	0.232	0.232
Adjusted R-squared	-0.001	0.267	0.361		
First stage F-statistic				80.3	76.8
N	308	308	308	308	308
	Panel B: Share Ever-Divorced in 1950				
Total frontier experience (decades)	-0.002	0.007*	0.003	-0.014*	-0.010
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)
Estimator	OLS	OLS	OLS	2SLS	2SLS
Geoclimatic controls		Y	Y	Y	Y
State fixed effects			Y	Y	Y
Outcome mean	0.129	0.129	0.129	0.129	0.129
Adjusted R-squared	-0.004	0.212	0.326		
First stage F-statistic				59.8	59.7
N	226	226	226	226	226

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 black 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.7: 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.8: 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.

Table A.9: Impact of College Share on Divorce, 1940-Based IV

	(1)	(2)	(3)	(4)
Share of college graduates, 1980	-0.806***	-0.831***	-0.953***	-1.03***
	(0.094)	(0.067)	(0.030)	(0.038)
Estimator	OLS	OLS	2SLS	2SLS
State-by-year fixed effects		Y		Y
Outcome mean	0.254	0.254	0.254	0.254
First stage F-statistic			5,619	3,910
Adjusted R-squared	0.857	0.893		
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 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.