Frontier Culture: The Roots and Persistence of "Rugged Individualism" in the United States*

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Abstract

The presence of a westward-moving frontier of settlement shaped early U.S. history. In 1893, the historian Frederick Jackson Turner famously argued that the American frontier fostered individualism. We investigate the Frontier Thesis and identify its long-run implications for culture and politics. We track the frontier throughout the 1790–1890 period and construct a novel, county-level measure of total frontier experience (TFE). Historically, frontier locations had distinctive demographics and greater individualism. Long after the closing of the frontier, counties with greater TFE exhibit more pervasive individualism and opposition to redistribution. This pattern cuts across known divides in the U.S., including urban–rural and north–south. We provide evidence on the roots of frontier culture, identifying both selective migration and a causal effect of frontier exposure on individualism. Overall, our findings shed new light on the frontier's persistent legacy of rugged individualism.

Keywords: Culture, Individualism, Preferences for Redistribution, American Frontier, Persistence

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1 Introduction

Rapid westward expansion marked the early history of the United States. According to the influential historian Frederick Jackson Turner, the presence of "a continually advancing frontier line" at the "edge of free land" strongly influenced American culture (Turner, 1893). The frontier fostered the development of distinctive cultural traits, including individualism and opposition to government intervention. The combination of these two traits characterizes "rugged individualism," a term popularized by Republican Herbert Hoover in his 1928 presidential campaign.

This paper shows that the American frontier shaped a persistent culture of rugged individualism. First, using Census data from the 18th and 19th century, we establish the distinctive demographics and higher levels of individualism that characterized frontier locations historically. Then, using more recent Census, survey, and electoral data, we show that locations exposed to frontier conditions for a longer period exhibit higher contemporary levels of individualism, lower desired and actual levels of redistribution, and stronger opposition to government regulation. Finally, we explore the origins of frontier culture. Rugged individualism became ingrained through selective migration of individualists to the frontier and cultural change toward individualism in response to frontier conditions. Both forces were arguably responses to the differential socioeconomic returns to individualism on the frontier.

We determine the position of the frontier and track its evolution over time using population data from the Census and applying Geographic Information System (GIS) techniques. Following Turner's classic essay and the *Progress of the Nation* report from the 1890 Census, we define the frontier line as the boundary at which population density dropped below two people per square mile. We identify the frontier as comprised of counties with low population density in close proximity to the frontier line. This time-varying measure of frontier status is consistent with Turner's view of the frontier as "a form of society" rather than a fixed area. We measure total frontier experience (TFE) as the time spent on the frontier between 1790 and 1890. This precise and comprehensive measure of frontier history at a fine geographic level makes it possible to characterize previously unidentified cultural patterns.

We provide systematic evidence on the demographic and cultural distinctiveness of frontier locations. Consistent with historical narratives, frontier settlers were disproportionately male, prime-age, and foreign-born. These traits are strongly correlated with each of the two defining features of the frontier: sparse population and remoteness. Semiparametric regressions and structural break tests point to a qualitative difference occurring close to the density cutoff defining the frontier line in historical accounts.

Frontier locations also exhibit greater prevalence of individualism as captured by naming patterns. Name choices are a primordial act of intergenerational cultural transmission and offer a unique opportunity to measure individualism historically. We consider two proxies for individualism: (i) infrequent names, and (ii) absence of parents passing down their given names to children (patronymic for father-to-son, matronymic for mother-to-daughter). The frontier differential is not an artifact of immigrant prevalence: it holds for children with native-born parents and in matched pairs of counties with nearly identical foreign-born shares. Moreover, the difference in infrequent names is robust to adjusting for misspellings and to considering alternative geographic reference groups and degrees of infrequency, while the patronymic/matronymic measure does not require such additional checks.

A rich social science literature motivates our names-based measures of individualism. The informational content of names has been emphasized in economics (e.g., Abramitzky et al., forthcoming; Bertrand and Mullainathan, 2004) as well as psychology and sociology (e.g., Gerrit and Onland, 2011; Lieberson and Bell, 1992). We adopt infrequent names from social psychology, a field that characterizes individualism as central to understanding cross-country variation in culture (Heine, 2010). The foundational contributions of Hofstede (1991) and Triandis (1995) associate individualism with several related traits: a view of the self as independent rather than interdependent, emphasis on self-reliance, primacy of self-interest, and regulation of behavior by personal attitudes rather than social norms. Consistent with these traits, infrequent names reflect a desire to stand out, as opposed to common names, which reflect a desire to fit in (Twenge et al., 2010). Patronymic/matronymic names, which reflect a non-individualistic emphasis on interdependence, provide a useful complementary measure (Brown et al., 2014).

Using our novel measure of TFE, we investigate its long-run effects on individualism and opposition to government intervention. First, we show that TFE positively correlates with individualistic naming patterns in the mid-20th century. Second, we find a robust association of TFE with opposition to redistribution and public spending based on contemporary surveys that measure different aspects of government intervention. TFE is also associated with actual policy differences such as lower property tax rates. The results are robust to state fixed effects as well as geoclimatic controls including area, latitude and longitude, rainfall and temperature, distance to waterways, and potential agricultural productivity.

These long-run differences in preferences have translated into stronger contemporary support for the Republican Party. Each decade of TFE is associated with 3.5 percent more votes for the Republican Party in presidential elections between 2000 and 2016, a period of rising political polarization in which Republicans adopted increasingly strong positions against tax redistribution. We also show that this association ratchets up over the 2000s as each election exhibits a significantly larger effect of TFE. We relate this finding to preferences over contentious policies that animate the growing partisan divide: health insurance, minimum wage, gun control, and environmental protection. Republican positions on these issues can be linked to salient aspects of frontier culture described in the historical literature, including opposition to the welfare state, strong belief in effort versus luck in reward, necessity of self-defense, and notions of "manifest destiny". Locations with greater TFE exhibit stronger opposition to each of these policies. We provide further evidence on the historical trajectory of voting behavior back to the early 1900s, linking TFE to greater support for candidates who, like Hoover, appeal to values associated with frontier culture.

Importantly, the persistent effects of TFE are not merely a reflection of low population density today or simply a history of low density. It was not only low density but also remoteness that created the challenges and opportunities defining the frontier experience. We rule out the confounding effects of density in several ways, the most demanding of which involves a pairwise-matching exercise in which we estimate the effects of TFE across counties (within the same state) with nearly identical contemporary density. We also account for other potential confounders, including mineral resource abundance, rainfall risk, access to railroads, slavery, and birthplace diversity.

Further results support the proposed link between frontier experience and contemporary culture. First, African Americans' preferences today are unaffected by TFE, consistent with the fact that mechanisms fostering rugged individualism on the frontier—selective migration, prospects for upward mobility—were of limited historical relevance for blacks, especially in the antebellum period. Second, racial resentment toward blacks does not explain the effect of TFE on opposition to redistribution among the white population. Third, preferences over policies less directly connected with frontier culture (e.g., international sanctions) exhibit little variation with TFE and may be viewed as placebos.

We find similar effects of TFE across different regions of the U.S. This includes (i) expanding to regions exposed to frontier conditions after 1890, the year in which the frontier closed according to the Census Bureau, and (ii) adding the West Coast, including California, which experienced its own frontier expansion eastward in the second half of the 19th century. These results point to a common underlying persistence in rugged individualism despite large regional differences in preferences.

Despite myriad controls, the effects of TFE may still be confounded by unobservable location-specific fundamentals (e.g., market access) that shaped the speed of settlement historically and culture today. We develop an IV based on emigration shocks in Europe that aims to address this place-based endogeneity. For each county, the IV captures weather-induced emigration flows to the U.S. starting just before the onset of local frontier settlement. The ebb and flow of arrivals to the eastern seaboard from 1790 to 1890 shaped the pace of westward expansion and hence the time it took for frontier locations to become established settlements. These time-varying, national population shocks are unrelated to local conditions of frontier counties and help move us closer to a causal interpretation.

Frontier culture may have persisted through various mechanisms long after the end of frontier conditions. Culture can be sticky and converge at very slow speeds, or not at all. In the presence of path dependence, initial conditions affect long-run outcomes. In this sense, the earliest stages of development on the frontier were likely a critical juncture in the formation of local culture.[1]

Having established a persistent cultural legacy of the frontier with policy implications today, we then explore how this culture of rugged individualism arose historically. First, we show that frontier locations attracted individualistic people. Second, we present evidence pointing to causal effects of frontier exposure on individualism. These results suggest cultural change in response to frontier conditions, a mechanism consistent with theories of utilitarian intergenerational transmission (e.g., Doepke and Zilibotti, 2008) and with Turner's (1893) suggestion that frontier life fostered "a modification of the original stock". These two forces—selective migration and cultural change—may be explained by the unique threats and opportunities on the frontier, which rewarded behaviors associated with individualism.

We identify selective migration using complete-count Census data from the 1800s. Looking at children's names prior to moving, we find that families that move from settled areas to the frontier have children with more individualistic names than families that remain in settled areas. Similarly, families that move from frontier areas to settled ones have children with a lower prevalence of individualistic names. In other words, individualists selectively move to the frontier, and non-individualists selectively move away from the frontier. These patterns are consistent with the characterization of frontier migrants as individualists willing and able to give up their social environment to settle in remote and isolated contexts (see Kitayama et al., 2006).

Frontier conditions not only attracted people with individualistic traits but also shaped the evolution

[1] Turner (1893) noted that "traits [of frontier society] have, while softening down, still persisted as survivals in the place of their origin, even when a higher social organization succeeded." According to the so-called "doctrine of first effective settlement" advanced by Zelinsky (1973), when "an empty territory undergoes settlement [...] the specific characteristics of the first group able to effect a viable, self-perpetuating society are of crucial significance for the later social and cultural geography of the area, no matter how tiny the initial band of settlers may have been."

of individualism among frontier settlers. We identify this exposure effect on culture using two complementary approaches: (i) within-parent changes in children's names pre- and post-move to the frontier, and (ii) cross-sibling variation in frontier experience based on age-at-move. Both approaches, like our long-run analysis of TFE, emphasize that the length of frontier exposure determines the scope for cultural change. While the two strategies rely on different sources of exposure across the life-cycle, they both point to significant effects on individualism that are distinct from selective migration.

Approach (i) shows that individualism among frontier migrants increased over time following arrival to the frontier. In particular, an event-study design with household fixed effects reveals that children's names become more individualistic as parents reside in the frontier for a longer period of time. Moreover, the growth in individualism begins only after arrival to the frontier, as evidenced by the lack of pre-trends in individualistic names for kids born prior to moving. In other words, while there is selective migration on individualism in levels, frontier migrants do not appear to be self-selected on a prior trend in individualism.

Approach (ii) identifies childhood exposure effects by exploiting variation in age at move among the children of frontier migrants. Consider brothers that moved to the frontier with their parents before 1850. We find these brothers in the 1880 Census when they are household heads with children of their own. Conditional on 1850 family fixed effects, the younger brother's children in 1880 exhibit more individualistic names than the older brother's children. These differences capture the effect of living on the frontier for a longer period during childhood. Given the lack of pre-trends in approach (i), the brothers would have plausibly been exposed to similar degrees of parental individualism were it not for their subsequent frontier exposure.

These cultural dynamics on the frontier are consistent with differential returns to individualism. We provide suggestive evidence along these lines using Census data from the 1800s with occupational scores to proxy for economic status. Conditional on county fixed effects, fathers giving their children more individualistic names exhibit relatively higher occupational standing in frontier counties than in settled counties. These patterns resonate with historical narratives emphasizing that, on the frontier, independence and self-reliance were key to survival and success. Frontier settlers faced many challenges (e.g., weather shocks, attacks by Native Americans) with little or no social infrastructure to turn to (Edwards et al., 2017). As Overmeyer (1944) put it, "life was rough, crude, hard, and dangerous." On the other hand, the abundance of land offered profit opportunities (Stewart, 2006), and in an uncharted environment, individualists' non-conformism and inventiveness made them more resourceful (Shannon, 1977).

While individualists may generally oppose interference in the pursuit of self-interest, there were complementary forces nurturing a culture of opposition to redistribution on the frontier. In particular, the frontier's land abundance created expectations of upward mobility through effort. As suggested by political economy theories (see, e.g., Alesina and Angeletos, 2005; Piketty, 1995), prospects for upward mobility and the importance of effort versus luck in income generation would make tax-based redistribution both more unfair and inefficient. Turner (1893) observed that on the frontier the "tax-gatherer is viewed as a representative of oppression," since the environment "produces antipathy to control."[2]

[2] Billington (1974), a notable follower of Turner, argued that on the frontier "every man was a self-dependent individual, capable of caring for himself without the fostering care of society," which "seemed just in a land that provided equal opportunity for all to ascend the social ladder."

Related Literature. This paper contributes to a growing literature on the historical origins and persistence of cultural traits. Our findings make a distinct addition to the economics literature on individualism (e.g., Gorodnichenko and Roland, 2016; Hoang-Anh et al., 2018; Olsson and Paik, 2016) and preferences for redistribution (see Alesina and Giuliano, 2010, for a survey). We use a wealth of subnational data spanning two centuries to connect the American frontier with the origins and persistence of rugged individualism. Complete-count Census data, record linking, and names-based measures of individualism provide a unique opportunity to examine not only persistence but also the historical inception of this trait at the granular level of individuals and communities.

Differences in rugged individualism across the U.S. have suggestive implications for cultural differences with Europe. The forces we emphasize—selective migration, cultural change, an advantage of individualism, and prospects for upward mobility—were arguably important in distinguishing American from European culture. According to Turner (1893), "the Atlantic coast. . . was the frontier of Europe." Comparing the U.S. and Europe, Alesina et al. (2001) conjecture that "American anti-statism" may be partly linked to the frontier, which "strengthened individualistic feelings and beliefs in equality of opportunities rather than equality of outcomes." We provide the first evidence on this hypothesis at the subnational level. Moreover, the deep roots of rugged individualism shed new light on a puzzle in American political economy: the relative stability of preferences for redistribution over the last 40 years despite significant increases in inequality (Ashok et al., 2015).

Finally, we make a novel contribution to the large social science literature inspired by Turner's ideas. Many historians and sociologists describe life on the frontier. We provide systematic empirical evidence on the distinctive features of frontier settlements, measuring the historical prevalence of individualism for the first time. The only study of the Frontier Thesis in economics is García-Jimeno and Robinson (2011), which links the quality of democratic institutions in the Americas to historical variation in land abundance across countries. Our paper examines subnational variation in individualism and preferences for redistribution in the U.S. using a new measure of historical frontier experience. Social psychologists use state-level data to study variation in contemporary individualism, comparing demographic features (Vandello and Cohen, 1999) or infrequent names (Varnum and Kitayama, 2011) between the western U.S. and the rest of the country. We go beyond these broad geographic correlations by providing a historically-consistent definition of the frontier and ruling out potential confounders of settlement history. Furthermore, we disentangle the effects of selective migration and place-based exposure in shaping frontier culture historically.

The paper is organized as follows. Section 2 explains how we locate the frontier and measure total frontier experience. Section 3 documents the distinctive features of frontier populations. Section 4 provides estimates of the long-run effects of frontier experience on culture. Section 5 offers evidence on the roots of frontier culture. Section 6 concludes with key lessons and caveats about extrapolating to other countries or even the U.S. as a whole.

2 Mapping the History of the Frontier

This section presents our method for mapping the history of the frontier. We explain how to use U.S. Census data and GIS techniques to determine the position of the frontier line. We then define frontier counties and our new measure of total frontier experience.

From colonial times until the late 19th century, America underwent rapid population growth and a massive westward expansion. Historical sources document this process, and the noteworthy 1890 Census report on the *Progress of the Nation* (Porter et al., 1890) provides a key source of inspiration for Turner's classic 1893 essay. The authors observe that the Thirteen Colonies, already settled communities by 1790, were "the sources of supply for a great westward migration," as people "swarmed from the Atlantic coast to the prairies, plains, mountains, and deserts by millions during the last century." The report describes in great detail the decade-by-decade push westward and includes detailed maps of population density very similar to ours (see Appendix Figure A.1). From 1790 to 1890, as the nation's total population increased from 3.9 million to 62.6 million, the extent of settled area went from under 240,000 square miles to almost 2,000,000, and the mean center of population shifted westward over 500 miles, from just east of Washington D.C. to Decatur, Indiana.

The *Progress of the Nation* report considered that the frontier had closed by 1890. In a passage quoted in Turner's essay, it stated that "up to and including 1890 the country had a frontier of settlement, but at present the unsettled area has been so broken into by isolated bodies of settlement that there can hardly be said to be a frontier line." As one of the authors put it elsewhere, "the frontier line has disappeared ... the settled area has become the rule and the unoccupied places the exception" (Gannett, 1893).

2.1 Locating the Frontier and Tracking its Movements

Prior research on the American frontier adopted simplifying definitions. In a study of westward migrants in 1850 and 1860, Steckel (1989) identifies the frontier with the states of Minnesota, Iowa, Kansas, Texas, and those farther west. Ferrie (1997) studies migration to the frontier between 1850 and 1870 and defines 90° west longitude as the frontier's eastern boundary. Kitayama et al. (2010) simply associate the frontier with the Western United States.

We take a different approach. Following the *Progress of the Nation* report from 1890 and Turner's classic essay, we define the frontier line as the line dividing settlements with population density of two or more per square mile from those with less.[3] We therefore define frontier counties as those (i) in close proximity to the frontier line (100 kilometers in our baseline) so as to capture Turner's notion of the "frontier belt", and (ii) with population density below six people per square mile, a cutoff stipulated by Porter et al. as the beginning of established, post-frontier settlement. While these cutoffs are necessarily arbitrary, we find empirical support for these definitions in Section 3.2, and our results are robust to alternative distance and density thresholds.

This definition allows us to precisely locate the frontier at every point in time and trace its movement. As Turner noted, the frontier was "a form of society rather than an area." Its two defining features implied distinct forms of isolation. Low density meant isolation from other people within a given location. Proximity to the frontier line meant isolation from population centers to the east, and in many cases limited interaction with agents of the federal government. Both dimensions entailed (i) a lack of social infrastructure that made frontier life rough and dangerous, and (ii) relative resource abundance

[3] Turner (1893) notes, "The most significant thing about the American frontier is, that it lies at the hither edge of free land. In the census reports it is treated as the margin of that settlement which has a density of two or more to the square mile. ... We shall consider the whole frontier belt including the Indian country and the outer margin of the "settled area' of the census reports."

that implied favorable prospects for upward mobility and attracted pioneering settlers in search of opportunity. While low density locations could also be found in settled regions in the east, such locations were not so isolated from urban centers and were unlikely to be resource-rich.

For each Census year beginning in 1790, we calculate county-level population density per square mile. For intercensal years, we interpolate county-level population density by assuming a constant annual population growth rate that matches the decadal growth rate. We maintain consistent units of observation over time by harmonizing all data to the 2010 boundaries (see Appendix L).[4]

Using annual county-level population densities, we locate the frontier line by drawing contour lines that divide counties with population densities above and below two people per square mile. Figure 1 plots the resulting lines for 1790, 1820, 1850, and 1890, and full details on the underlying GIS procedure can be found in Appendix A. In order to closely approximate historical notions of the frontier described above, we discard all line segments less than 500 km, as well as isolated pockets of low density counties within the main area of settled territory (i.e., to the east of the main frontier line).[5] Figure 2 shows the evolution of the resulting, main frontier lines in red for 1790–1890.

A second major frontier emerged on the West Coast, starting in California, in the mid-19th century. Spurred by the Gold Rush, this was a large, discontinuous leap in the advance of east-to-west expansion. Compared to frontier locations in the heartland, the West Coast frontier had a different type of isolation. It was much farther away from Eastern cities, but proximity to the ocean reduced transportation costs, facilitating flows of goods, people, and ideas. We leave this secondary frontier out of the baseline analysis but show in Section 4.4 that, in fact, frontier experience has similar long-run effects in the West.

2.2 Total Frontier Experience

The westward movement of the frontier was fast at times, slow at others. Thus, some locations spent little time in frontier conditions, while others remained on the frontier for decades. This variation in the duration of frontier exposure is key for our investigation of long-run persistence.

To measure the duration of frontier experience for each location, we calculate the number of years spent within the frontier belt from 1790 to 1890. For each year, there is a dummy variable that takes value one if a county is on the frontier as defined by proximity to the frontier line and low density. Then, total frontier experience (TFE) for each county is the sum of indicators of frontier status from 1790 to 1890.

We set 1890 as the endpoint for measuring TFE following the *Progress of the Nation* report and Turner. While many places remained sparsely populated long after 1890, the effective isolation of the frontier did not persist with the same intensity. Transcontinental railroads were built in the early 1880s, which was also when the Indian wars came to an end. Federal irrigation efforts started soon after. For robustness, we consider a longer time frame for the measurement of TFE, changing the endpoint to 1950.[6]

[4]Appendix Figure A.2 shows that harmonization has little effect on the location of the frontier lines relative to an approach based on contemporaneous historical county boundaries.

[5]Our results are qualitatively similar when discarding isolated pockets of high density settlement to the west of the main frontier line. The 500 km cutoff discards some contour lines but retains other, large unconnected lines off of the main east-to-west frontier line, e.g., the ones spanning Maine in 1820 and Michigan in 1850. Results are robust to adopting other cutoffs or having no cutoff at all (see Appendix D).

[6]This also helps ensure that counties entering the frontier just prior to 1890 can admit a complete frontier history rather than being censored as in the baseline ending in 1890. This censoring only affects 26 out of a total 2,036 counties in the baseline sample, but it nevertheless could be a potential source of bias if counties settled closer to 1890 have less individualism and

Our analysis excludes counties to the east of the 1790 east-to-west main frontier line for which we do not observe total frontier experience given the available data. To be clear, we do not claim that counties close to the East Coast were never on the frontier. We simply cannot measure their frontier experience.

Figure 3 shows the spatial distribution of TFE for the counties in our main analysis. Total frontier experience ranges from 0 to 63 years with a mean of 18.2 years and a standard deviation of 11.2. TFE exhibits considerable variation both across and within states as well as bands of latitude and longitude more generally. Moreover, the variation in TFE goes well beyond variation in contemporary population density, as seen in the maps in Appendix K.1 and shown empirically in Section 4.4. For instance, Cass County, Illinois has TFE of 10 years and Johnson County, Illinois has TFE of 32 years, but the two counties have nearly identical population density today (see Appendix I for this case study).

3 The Distinctive Features of the Frontier

This section documents the unique demographic features of the frontier and its higher levels of individualism. Historians and sociologists have devoted considerable effort to analyzing the demographics of frontier locations (e.g., Eblen, 1965). However, these studies usually focus on a specific place at a particular time, making it difficult to establish empirical regularities. In contrast, we characterize the demographics of the frontier using data from all censuses from 1790 to 1890. Moreover, we provide the first evidence of the frontier's distinctive individualism.

We focus on a set of demographic characteristics associated with the frontier in historical accounts. These include sex ratios, age distributions, foreign-born population shares, and literacy rates, all of which we draw from historical Census data in Ruggles et al. (2019). With the exception of immigrant shares, we calculate all variables over the white population as this maintains consistency across time periods and ensures that results are not driven by racial composition.[7]

We infer individualism from children's names in two ways. First, following work by social psychologists, we consider infrequent names as indicative of individualism. The prevalence of such names has been shown to correlate strongly with other proxies for individualism.[8] Second, following other social science research, we consider patronymic and matronymic names, passed from father to son and mother to daughter respectively, as evidence of non-individualistic behavior.[9] This measure complements infrequent names and does not depend on the social reference group or spelling variation. Using full-count Census data beginning in 1850 (when names first become available), we measure for each county the share of children age 0–10 with infrequent names. We define as infrequent those names outside the top 10 within one's Census division (of which there are nine), but results are robust to considering the top 100 and to expanding or narrowing the reference group from the country to the state or county. Appendix

mechanically lower TFE. The robustness checks in Table 6 rule out this concern.

[7] Slaves were not individually enumerated during the antebellum period, though their counts were recorded. The majority of Native Americans were not individually enumerated nor systematically counted throughout the frontier era.

[8] Varnum and Kitayama (2011) show a positive cross-country correlation between infrequent names and Hofstede's widely used index of individualism. Beck-Knudsen (2019) shows that the names-based measure is strongly correlated with Hofstede's index as well as with the use of first- and second-person singular pronouns across 44 countries (and across regions within five countries). In Japan, Ogihara et al. (2015) shows a strong time-series correlation between the share of common name pronunciations and an index of individualism similar to the one proposed by Vandello and Cohen (1999).

[9] We focus on parental forebears as we only observe a small subset of children with co-resident grandparents. Brown et al. (2014) show that the prevalence of patronymic/matronymic names correlates strongly with collectivism across U.S. states.

L provides a list of common names for selected years (e.g., John and Sarah) as well as a random sample of infrequent names (e.g., Luke and Lucinda).[10] We define the share of non-patronymic/-matronymic names over all children, but results are robust when restricting to first-born boys/girls.

We take several additional steps to exclude variation in names unrelated to individualism. First, we aim to remove variation associated with foreign names, which may be more common on the frontier due to the greater prevalence of immigrants. To do this, we restrict all measures to white children with native-born parents. Moreover, we implement a matching exercise to account for remaining biases towards infrequent names in locations with large immigrant populations (where native-born adults may choose infrequent names used by immigrants). Additionally, we aim to exclude variation created by spelling errors (or quirks), which may be systematically higher on the frontier, by first adjusting names using a phonetic algorithm and then determining whether that name group is infrequent. Our results are robust and offer novel empirical support for historical narratives about individualism on the frontier.

3.1 Demographics and Individualism on the Frontier: Basic Patterns

We begin by documenting the basic differences in demographics and individualism on the frontier. We estimate the following equation using all available Census data from 1790 to 1890:

$$x_{cdt} = \alpha + \beta \text{ frontier}_{ct} + \theta_d + \theta_t + \varepsilon_{cdt}, \tag{1}$$

where x_{cdt} is one of the population traits of interest in county c in Census division d at time t, frontier$_{ct}$ is frontier status, and θ_d and θ_t are Census division and year fixed effects, respectively. Panel (a) of Table 1 reports estimates of β, the frontier differential, for each of six x outcomes.

Across columns, we find that frontier populations tend to have significantly more males, prime-age adults, and foreign-born. Frontier counties have 0.19 additional males for every female relative to non-frontier counties where the average sex ratio is 1.09. The share of prime-age adults (15–49 years old) in the population is 2.6 percentage points (p.p.) higher than in non-frontier counties, for which that share is around 46 percent. These patterns suggest that the often hostile conditions of frontier life may have led to the selective migration of young men, a common theme in historical accounts. Additionally, frontier counties have 6.3 p.p. higher foreign-born population shares than the average non-frontier county where 7 percent of residents are immigrants. Finally, in column (4), literacy rates are not significantly different on the frontier. While this runs counter to the "safety valve" theory of of selective low-skilled migration to the frontier (see Ferrie, 1997; Turner, 1893), literacy may simply be a noisy measure of skill.

Columns 5 and 6 show that individualism is more pervasive in frontier counties, as reflected in the share of children age 0–10 with infrequent names and non-patronymic/-matronymic names, respectively. In frontier counties, 2.2 p.p. more children have infrequent names relative to the average of 63 percent in non-frontier counties. This result is robust to adjusting reported names for their phonetic sound using the Philips (1990) metaphone algorithm (see columns 3–4 of Appendix Table B.1).[11] More-

[10] We choose the top 10 cutoff as a baseline following the social psychology literature (Varnum and Kitayama, 2011). With this measure, the majority of children have infrequent names (e.g., 57 percent of boys and 60 percent of girls in 1850). Hence, an "infrequent name" may not be a very unusual name but simply one that is not very common. In any case, the measures for different cutoffs are highly correlated, and results are qualitatively similar across all of them (see Appendix B).

[11] To get a sense of what this algorithm does, consider the common name of John. This metaphone adjustment groups the

over, 0.9 p.p. more children have names different from their parents relative to the mean of 95 percent in non-frontier counties. Both columns 5 and 6 are robust to accounting for the differential presence of immigrants on the frontier using a nearest-neighbor matching exercise that compares frontier and non-frontier counties with nearly identical foreign-born population shares in the given Census year (see even-numbered columns in Appendix Table B.1). Overall, these results provide strong evidence of more prevalent individualism on the frontier.

We further clarify the frontier differential by distinguishing the two attributes of frontier locations: (i) proximity to the frontier line and (ii) low population density. Panel (b) of Table 1 estimates

$$x_{cdt} = \alpha + \beta_1 \text{ near frontier line}_{ct} + \beta_2 \text{ low population density}_{ct} + \theta_d + \theta_t + \varepsilon_{cdt}, \tag{2}$$

where *near frontier line*$_{ct}$ is an indicator for having a centroid within 100 km of the frontier line at time t, and *low population density*$_{ct}$ is an indicator for population density below six people per square mile. The results suggest that both remoteness and sparsity contribute to the distinctive demographics and individualism of frontier counties.[12]

When we look at variation over time, as counties from frontier conditions to more established settlement, the distinctive features of frontier counties become less and less pronounced. We document this process in Appendix Figure H.1, which generalizes equation (1) by allowing β to vary with time relative to exiting the frontier. As we show in Section 4, however, the duration of exposure to the frontier has persistent implications for culture.

3.2 The Frontier Is Qualitatively Different: Semiparametric Evidence

Figure 4 provides a stark illustration of the structural change in demographics and individualism at low levels of population density. Each graph shows the local linear regression function and 95 percent confidence interval around $g(\cdot)$ in the following semiparametric regression:

$$x_{cdt} = \alpha + g(\text{population density}_{ct}) + \theta_d + \theta_t + \varepsilon_{cdt}, \tag{3}$$

where $g(\cdot)$ is a nonlinear function recovered using the partially linear (Robinson, 1988) estimator.

In graph (a), the sex ratio displays levels around 1.6 in the most sparsely populated counties and declines sharply as population density rises to 3–4 people/mi^2. The slope of $g(\cdot)$ then abruptly flattens out as the sex ratio stabilizes at around 1.05–1.1 males for every female. In graph (b), the prime-age adult share declines sharply as we move towards density levels of 2–3 people/mi^2 and then levels off. Graphs (c) and (d) show similar downward-sloping albeit less sharply nonlinear shapes. However, graphs (e) and (f) show stark nonlinear shapes for infrequent and non-patronymic/-matronymic children's names, respectively. Appendix Figure H.2 provides evidence of similar patterns, though less stark nonlinearities, for proximity to the frontier line instead of density.

Overall, the results in Figure 4 point to a structural break in demographics and individualism at

following variants on "John"—some misspellings and others nicknames—into a single metaphone "JN": Jon, Jno, Johhn, Johnnie, Johnie, Johny, Johnny, Jonnie, Johney, Jone, Johne, and Jonny, among others.

[12] Column 4 shows that the null for illiteracy in Panel (a) is due to offsetting positive effects of low density and negative effects of proximity. This pattern does not arise for other outcomes and suggests scope for further work on the safety valve hypothesis.

levels of population density consistent with the seemingly arbitrary cutoffs stipulated in the 1890 Census report. Using the Chow (1960) test, we can easily reject the null hypothesis of a constant effect of population density above and below 6 people/mi^2 (the upper bound of frontier settlement according to Porter et al., 1890), or above and below any cutoff in the 2–6 range. We can also be agnostic about the relevant cutoff, using the Zivot and Andrews (2002) test to identify unknown structural break points in each decade. In 1850, for example, we find a break in the sex ratio at 2.7 people/mi^2, the prime-age adult share at 2.0, and non-patronymic/-matronymic names at 3.1.

Finally, we also show two important facts about institutional life on the frontier in Appendix H.3. First, we demonstrate more limited government in the form of both (i) local taxation and public spending per capita as well as (ii) state and federal (i.e., non-local) investments in postal services, railroads, and canals. Second, while non-locally-provided public goods vary smoothly with population density, local government scale exhibits a sharp structural break around the same low levels of population density (2–6 people/mi^2) seen in Figure 4. Together, these patterns suggest limited government presence on the frontier historically.

4 Long-Run Effects of Frontier Experience on Culture

In this section, we examine the long-run effects of frontier experience on culture and discuss their implications for modern political economy debates. We present our empirical framework, discuss key data sources, and then move to our main cross-county results.

Our analysis is motivated by theories of cultural persistence. While the high levels of individualism on the frontier historically could have dissipated, it is also possible that frontier experience shaped the long-run evolution of local culture. The duration of exposure to frontier conditions determined the scope for rugged individualism to flourish through a set of mechanisms we analyze in Section 5. In the presence of multiple equilibria and path dependence, these early stages of cultural formation would represent a critical juncture, and frontier experience could have a lasting legacy.

4.1 Estimating Equation

We relate historical frontier exposure to modern measures of individualism and preferences. Our main county-level, cross-sectional estimating equation is given by:

$$y_c = \alpha + \beta \text{ total frontier experience}_c + \mathbf{x}_c'\boldsymbol{\gamma} + \theta_{s(c)} + \varepsilon_c, \tag{4}$$

where y_c is some long-run outcome capturing cultural traits in county c (e.g., individualism or preferences for redistribution). Total frontier experience (TFE) is the amount of time *in decades* a given county remained on the frontier according to our baseline definition in Section 2.2. Our baseline sample, seen in Figure 3, is restricted to those counties for which the 1790–1890 period contains the whole extent of frontier experience as discussed in Section 2.2.[13] In baseline specifications, $\theta_{s(c)}$ is a state fixed effect, and the vector $\mathbf{x}_c$ comprises predetermined or fixed county-level covariates including latitude, longitude,

[13] All but 40 of the 2,036 counties in our baseline have TFE greater than zero, and results are robust to excluding these 40 counties (see Appendix Table D.1).

county area, average rainfall and temperature, elevation, potential agricultural yield, and distance to rivers, lakes, and the coast. The coefficient β therefore identifies a local effect of TFE after accounting for geoclimatic factors that may correlate with both TFE and modern culture. Following the approach suggested by Bester et al. (2011), standard errors in all specifications are clustered on 60-square-mile grid cells that completely cover counties in our sample.[14]

We measure culture and policy outcomes today using several data sources, including three nationally representative surveys: the Cooperative Congressional Election Study (CCES), the General Social Survey (GSS), and the American National Election Study (ANES). These surveys are staples in the social science literature, often asking different questions about similar underlying preferences. See Appendix L for details, including a discussion of geographic coverage.

The main threat to causal identification of β lies in omitted variables correlated with both contemporary culture and TFE. We take several steps to address this concern. First, we rule out confounding effects of population density today by estimating β for matched pairs of counties (within the same state) with nearly the same population density. Second, in Section 4.4, we add further controls to $\mathbf{x}_c$ aimed at removing variation in culture due to factors raised in prior work. Third, we use the Oster (2019) approach to show that unobservables are unlikely to drive our results. Fourth, in Section 4.5, we pursue an instrumental variables (IV) strategy that exploits variation in the speed of the frontier's westward movement induced by changes in national immigration flows over time. Together, these complementary checks help ensure that our estimates capture the effects of TFE rather than some omitted variable.

4.2 Total Frontier Experience and Persistent Individualism

We begin by documenting a long-run association between TFE and contemporary individualism. Nearly five decades after the closing of the frontier, individualistic children's names are more pervasive in counties with greater TFE. In Table 2, we report the effect of TFE on the share of children age 0–10 in 1940 with infrequent (Panel (a)) and non-patronymic/-matronymic names (Panel (b)). The data come from the full count 1940 Census and capture naming choices multiple generations after counties exited the frontier.[15] Our baseline measure of infrequent names considers those outside the top 10 within the county's Census division. We normalize these variables so that standard deviation effect sizes can be read directly from the coefficients.

In Panel (a), the baseline specification with geoclimatic controls in column 2 of Table 2 suggests that each additional decade of TFE is associated with a 0.14 standard deviation higher share of infrequent names. Comparing counties across the interquartile range of TFE (11 vs. 24 years) implies 1 percentage point more children with infrequent names. We find similar effect sizes in Panel (b) when looking at non-patronymic/-matronymic names. In both panels, relative to column 1, the additional controls leave the coefficient unchanged despite the relatively large increase in the R^2. This pattern is consistent with limited selection-on-unobservables according to the parameter δ reported in the table; Oster (2019) suggests that $|\delta| > 1$ implies limited scope for unobservables to explain the results.

[14] As detailed in Appendix C, inference is robust to several alternative approaches to adjusting for spatial correlation, including the Conley (1999) spatial HAC estimator with bandwidths from 100 to 1000 kilometers.

[15] Ideally, we could carry these results through to the contemporary period, but, unfortunately, the 1940 Census is the latest round that provides information on names. Although the Social Security Administration releases baby name counts by state, it does not do so at the county level as required for our empirical strategy.

Importantly, the greater prevalence of individualistic names in high-TFE counties is not due to differences in contemporary population density or the prevalence of foreign-born. Columns 3 and 4 of Table 2 demonstrate this robustness using a matching exercise. For each county c within state s, we find the county c' with the most similar population density (column 3) or foreign-born share (column 4).[16] We then create an indicator for this county pair (c, c'). Finally, we add these 1,018 fixed effects to our baseline specification from column 2. Even in this very demanding specification, the estimated effects of TFE remain statistically and economically significant, and the δ parameter remains well above 1. Together, these two columns suggest that the effects of TFE on individualism in the mid-20th century are not due to differences in density or immigrant populations that may have persisted from the frontier era.

Furthermore, the results in Table 2 are robust to many alternative signals of individualism inherent in children's names. Appendix Table B.3 shows that the effects of TFE hold for measures of infrequent names (i) defined at the national (column 3), state (column 5), or county (column 6) level, (ii) based on the top 100 rather than top 10 (column 7), and (iii) based on a metaphone-adjusted sound of the name rather than the spelling recorded by the enumerator (columns 8–12). Column 2 of Appendix Table B.3 shows that the non-patronymic/-matronymic results hold when restricting to first-born sons and daughters. Panels (b) and (c) of this table show further that these alternative specifications of individualistic names also exhibit significant effects of TFE with nearest-neighbor matching on, respectively, population density and foreign-born share.

Appendix Table B.4 goes even further by estimating individual- rather than county-level regressions and including fixed effects for children's age, birth order and gender. The effect sizes are similar to our preferred county-level regressions, but with the individual specification, we can also control for family surname with nearly 400,000 fixed effects. This recovers the effects of TFE within the set of families with a given surname, e.g. comparing the Smiths living in high- versus low-TFE counties. This powerful test leaves the baseline results unchanged (Panel (d)) even when coupled with the population density matching exercise (Panel (e)).

Together with the findings in Section 3, these results suggest that individualistic names were not only more pervasive in frontier areas historically but also more prevalent in the long run in areas with greater TFE. Indeed, the effect of TFE on infrequent name choices can be seen in the early 1900s with little change thereafter (see Appendix Table B.5). This points to the persistence of the early frontier culture of individualism long after frontier conditions abated.

In Appendix Table B.2, we further validate the link between TFE and contemporary individualism using a measure from the ANES data in 1990. In particular, we show that greater TFE is associated with respondents identifying more strongly with self-reliant as opposed to cooperative behaviors. We turn now to identify the closely related link between TFE and opposition to government intervention.

4.3 Total Frontier Experience and Opposition to Redistribution and Regulation

This section identifies a long-run effect of total frontier experience on contemporary political preferences. First, greater TFE is associated with opposition to redistribution, support for limited government, and lower local tax rates. Second, these differences in preferences translate into stronger Republican Party

[16] That is, we order the given covariate from lowest to highest within each state and then create pairs starting with the first and second, third and fourth, etc.

support today. Finally, we identify a link between TFE and opposition to government regulations on issues that were salient in frontier culture historically. We view all of these outcomes as closely connected measures capturing the same underlying opposition to government intervention. In all cases, we report estimates of equation (4) controlling for the geoclimatic characteristics used in column 2 of Table 2 as well as individual demographics (age, age squared, gender, and race dummies) and survey-wave fixed effects where relevant. We continue finding supportive results from (i) Oster (2019) tests for selection-on-unobservables and (ii) the population-density matching exercise described above.

Redistribution and Limited Government. Table 3 shows that greater TFE is associated with stronger opposition to income redistribution today. In column 1, we use ANES data from 1992 and 1996, which asks respondents whether they would like to see "federal spending on poor people be increased, decreased (or cut entirely) or kept about the same." Around nine percent of individuals would like to see such redistributive spending decreased. Each additional decade of TFE is associated with one additional p.p. increase in support of cuts. Column 2 provides complementary evidence, showing that each decade of TFE is associated with 0.7 p.p. higher support for cutting state spending on welfare as reported in the CCES. Following Alesina and La Ferrara (2005), column 3 uses a measure from the GSS indicating the intensity of preferences for redistribution on a scale from 1 to 7 (with 1 being that the government should not be engaged in redistribution and 7 being that the government should reduce income differences through redistribution). Each additional decade of TFE is associated with around 0.02 standard deviations lower support for redistribution. These effect sizes are akin to a 5–10 year age gap in preferences among respondents (with older respondents more in favor of welfare spending cuts).

Turning to other measures, columns 4 and 5 show that residents of areas with greater TFE exhibit stronger fiscal conservatism. Column 4 uses a CCES question on whether individuals would prefer to cut domestic spending or to raise taxes to balance the federal budget. Column 5 uses an index based on the principal components of a set of questions from the GSS on whether the government spends too much on an array of public goods and social transfers. In both cases, we find that individuals are significantly more opposed to high levels of government spending in areas with greater TFE.

Column 6 of Table 3 shows that these reported preferences line up with actual policy differences. In particular, each decade of TFE is associated with around 3.3 percent lower reported property tax rates. This measure is from the American Community Survey in 2010 and ranges from 0.1 to 2.9 percent across counties. Given that much of the variation in tax rates lies across rather than within states, this is not a small effect. It equals roughly the within-state difference between counties that are 10 percent more versus less aligned with the Republican Party, a policy outcome we consider next.

In Column 7 of Table 3, we estimate the effect of TFE on support for the Republican Party between 2000 and 2016. While people vote Republican for many reasons, one recurring party theme in recent times is that the state should not be too heavily relied upon and hence government should be small (Gentzkow et al., 2019). The results indicate that each decade of TFE is associated with around a 2 p.p. greater Republican vote share relative to the mean of 60 percent over the five elections from 2000 through 2016.[17] For perspective, the 2 p.p. effect is roughly the difference in population-weighted, average

[17] This effect size is in line with individual-level regressions using degree of stated support for the Republican Party in the CCES. Using the CCES 2007, 2012, and 2014 survey rounds, we construct an indicator equal to one if the respondent identifies as a "strong Republican" on a seven point scale ranging from "strong Democrat" to "strong Republican" with around 17 percent

county-level vote shares in Iowa (48.4 percent) and Wisconsin (46.3 percent) over these five elections. It also remains sizable and significant (1.7*** p.p.) when implementing nearest-neighbor matching on population density in 2000 (see column 4 of Table 4 and further discussion in Section 4.4).

Voting and Partisan Issues. We provide here several additional results that elucidate the connection between frontier culture, voting, and partisanship. First, we examine earlier elections when the liberal–conservative divide did not map so cleanly onto a partisan divide. Appendix Figure E.1 shows that the effects of TFE on Republican vote shares are close to zero for most of the 20th century and become significantly positive and increasing in the 2000s. This break in the effect of TFE on voting coincides with a growing divide between Democrats and Republicans, which has been linked to a change in Republican strategy in the mid-1990s when leaders launched the "Contract with America" platform for political change, emphasizing tax cuts, balanced budgets, and welfare reform (see Gentzkow et al., 2019).[18] After 2000, the effects of TFE display a sharp uptick, with significant increases from each election to the next.[19]

The significant and growing influence of TFE on voting patterns may reflect both supply- and demand-side drivers of political change. Time patterns in Congressional speech point to an increased influence of frontier culture in this period of polarization. In particular, using data compiled by Gentzkow et al. (2019) we show in Appendix Table E.1 that from the mid-1990s onward Republican legislators from congressional districts with greater TFE are more likely to discuss topics associated with the "Contract with America"—big government, taxation, budget. This may be due to an increase in the likelihood of such candidates winning elections or to a change in speech among candidates that would have won otherwise. While we do not distinguish between these two explanations, the results point to a shift towards issues relevant to frontier culture among elected Republican representatives in high-TFE regions, consistent with strong voter support for such positions as seen in Table 3.

During this period of growing partisanship, the Republican party has increasingly opposed not only tax redistribution but also government intervention more broadly. We further explore the role of TFE by examining the link with voter preferences on salient partisan issues that resonate with anti-statist principles of rugged individualism. Using the CCES, we measure opposition to (1) the Affordable Care Act (ACA) and (2) increases in the minimum wage, which are both connected to beliefs in self-reliance. Opposition to (3) the ban on assault rifles is connected to the rights to bear arms and to self-defense, and opposition to (4) Environmental Protection Agency (EPA) regulations on pollution can be connected to notions of manifest destiny and opposition to interference with the pursuit of self-interest. The results in Appendix Table E.2 show that places with greater TFE display significantly stronger opposition to each of these government regulations. Moreover, these results are robust to controlling for the strength of

of individual–years reporting the latter. The estimates imply that an additional decade of TFE is associated with around 4.5 percent greater intensity of strong Republican support.

[18] This timing also coincides with a growing cultural divide—proxied by responses to GSS questions—between those identifying as Republican versus Democrat (Desmet and Wacziarg, 2018). While the GSS goes back to the early 1970s, county-level identifiers only become available in 1993, so it is not possible to assess time-varying effects of TFE prior to the 1990s. The CCES data only begins in 2006 while the ANES only has county-level identifiers pre-2000.

[19] Taking a long difference from 2000 to 2016, the average county in our sample exhibits a 9 p.p. shift towards Republican candidates, and each decade of TFE is associated with an additional 1.6 p.p. increase. Alternatively, an interquartile shift in TFE implies an additional 2.2 p.p. Republican Party shift. As a benchmark, Autor et al. (2016) find that an interquartile shift in exposure to import competition from China induces a 1.7 p.p. Republican shift over the same period. Using the original data from Autor et al. (2013), a single regression with both measures puts the TFE effect at around one-quarter as large as the effect of the China shock, with both effects statistically and economically significant.

individual-level Republican party identification (see Appendix K.3).

Finally, Appendix Figure E.1 shows that despite limited effects of TFE on most elections before 2000, there were four notable exceptions, each with clear links to frontier culture. The first was the 1928 election when Republican Herbert Hoover, who coined the notion of "rugged individualism", performed relatively better in high-TFE counties. The second was in 1944 when Republican Thomas Dewey challenged Franklin Delano Roosevelt (FDR) campaigning against the inefficiencies and excesses of FDR's New Deal programs. The third was in 1972 when Republican Richard Nixon was challenged by Democrat George McGovern, an anomalously progressive liberal candidate who campaigned for a time on giving every American citizen $1,000 per year (akin to a universal basic income). The fourth was in 1976, when incumbent Republican Gerald Ford fared *worse* in high-TFE counties against Democrat Jimmy Carter, who came from a farming family in Georgia descended from the original settlers of Virginia.

Summary of Results. Overall, the findings in Table 3 and Appendix E paint a rich picture of the cultural and political legacy of historical frontier exposure. It is plausible that the early settlers left a lasting imprint on frontier locations and that the degree of that imprint increased with duration of exposure. As a summary takeaway, the Kling et al. (2007) mean effect on individualism (infrequent name share), conservative political preferences (Republican vote share) and policy (property tax rate) suggests that each decade of TFE is associated with roughly 0.15 standard deviations more frontier culture today. Moreover, in connecting these outcomes to define a culture of "rugged individualism," it is important that individualistic names are strongly associated with greater Republican vote shares and lower property tax rates today.[20] In other words, the effects in Table 2 are identified off of a similar set of counties as in Table 3, pointing to the close connection between individualism and opposition to redistribution.

4.4 Further Analysis and Robustness Checks

Several results point to the robustness of our key findings and interpretation. In Table 4, we probe a leading potential confounder—contemporary population density—with a number of tests adding to the results in column 3 of Table 2. In Table 5, we account for a host other potential confounders including mineral resource abundance, historical access to railroads, population diversity and slavery. In Table 6, we expand our sample to include the secondary frontier on the West Coast, we measure TFE through 1950, and we show results separately by major Census region.[21] Tables 5-6 focus on core outcomes: individualistic names, a simple mean index of preferences over all CCES outcomes in Tables 3 and E.2, property taxes, and the Republican vote share. In Table 7, we analyze racial differences to help sharpen our interpretation of the long-run effects of TFE. We conclude by exploring placebo outcomes.

Disentangling Population Density. Contemporary population density is an important potential confounder. Spatial differences in population density can be very persistent, and places with low TFE may display different cultural and political attributes simply because they are sparsely populated today. For example, as is well-known, there is a large rural–urban divide in Republican vote shares.

[20] Conditional on state fixed effects and our baseline geoclimatic controls, a one standard deviation increase in infrequent name shares in 1940 is associated with 0.42 greater (0.24 lower) standard deviations of Republican vote shares (property tax rates), and these estimates are significant at the 1% level.

[21] Appendix D shows robustness to a battery of alternative ways of measuring TFE by varying the density and proximity cutoffs as well as the treatment of small pockets of dense (sparse) settlement beyond (inside) the main frontier line.

We take several steps in Table 4 to disentangle the effects of frontier settlement history from those of present-day population density. Column 1 reports the baseline. Column 2 controls for population density measured contemporaneously with the outcome variable. Column 3 implements a flexible specification including dummies for each decile of population density within state. Column 4 implements the matching specification that includes fixed effects for county pairs with nearly the same population density within state. This specification—detailed in Section 4.2 with reference to column 3 of Table 2—is very demanding and leaves limited variation to identify our coefficient of interest. Yet, TFE has a statistically and economically significant effect on individualistic names and Republican vote shares. The effects on mean government preferences in CCES and county-level property taxes are no longer significant, which is perhaps not surprising given that these measures exhibit less variation within state. Overall, though, the results in columns 2–4 suggest that modern preferences were shaped by the history of frontier settlement through channels other than simply persistently low population density. While modern density might be a "bad control", it is reassuring that the results are robust to controlling for this potential confounder in various ways.

The remaining columns of Table 4 further clarify that the long-run effects of TFE are driven by the history of frontier settlement rather than the history and persistence of low or high density. Columns 5 and 6 show that TFE has similar effects in urban and rural areas, splitting the sample into counties above and below the 90th percentile of urban population shares, respectively. Finally, column 7 separates out the history of low density—the number of decades with density below 6 people/mi^2—from the measure of total frontier experience. Recall that this was one of the defining features of frontier locations, proximity to the frontier line being the other. The coefficient on TFE remains significant, indicating that both dimensions of the history of frontier experience are important. In sum, the effects of TFE do not simply reflect the correlation of present-day and historical low population density.

Additional Controls. While our baseline regressions include state fixed effects and an array of geoclimatic controls, there are of course other factors that may be correlated with TFE and contemporary culture. We probe this concern by including a number of additional controls in Table 5.[22] Columns 2–5 add geoclimatic features whose importance has been emphasized in previous work: ruggedness (Nunn and Puga, 2012), rainfall risk (Ager and Ciccone, 2017; Davis, 2016), distance to portage sites (Bleakley and Lin, 2012), and distance to historical mining sites (Couttenier and Sangnier, 2015). Column 6 adds distance to the nearest Indian battle site. Conflict with Native Americans probably increased TFE and plausibly affected preferences. Column 7 adds the prevalence of slavery (in 1860, just before the Civil War), another potential confounder. Column 8 adds the sex ratio, which was systematically higher in the frontier and could by itself be a force shaping long-run cultural outcomes (see Grosjean and Khattar, forthcoming, on Australia). Columns 9–11 add additional demographic variables: the share of migrants in the population, the share of Scottish and Irish migrants (which are associated with higher levels of violence, as shown by Grosjean, 2014), and a measure of birthplace diversity. We measure all of these in 1890, the end of the frontier era. Column 12 controls for the number of years that each location was connected to the railroad network (until 1890), which is likely to reduce TFE and may also affect attitudes toward government intervention. Column 13 adds the employment share in manufacturing (in

[22]See Appendix Table K.1 for the full elaboration of coefficients.

1890), a basic measure of economic development. Column 14 includes all the additional controls simultaneously. Overall, the estimated effects of TFE remain significant and relatively stable despite the substantial added explanatory power of the additional controls.

Adding West Coast, Extended Time Frame, Regional Heterogeneity. Table 6 provides further evidence that the effects of TFE cut across other well-established cultural divides in the U.S. We begin by adding West Coast frontier counties to our sample. These 105 counties were settled starting in the mid-19th century and were located to the west of the major frontier line on the West Coast in 1890 (the year in which the Census declared the frontier closed). As shown in column 1, for all key outcomes, the estimated effects of TFE remain effectively unchanged.

Then, we split the sample by Census region and show that the effects of TFE hold separately in the Midwest (column 2), the South (column 3), and the West (column 4).[23] The coefficient estimates are generally smaller and noisier in the West, which can be explained in part by the small sample size (152 counties). In subsequent columns 6–8, we extend the frontier time period through 1950, incorporating in our sample counties that experienced frontier conditions beyond 1890. Here, the effects of TFE are economically and statistically significant across all regions.

At first glance, the stability across regions may seem puzzling insomuch as the West region on average exhibits more collectivism and less opposition to redistribution. However, our empirical strategy isolates the effects of TFE within-state. That is, counties within California with greater TFE exhibit more prevalent individualism and opposition to redistribution compared to counties within California with lower TFE. If anything, the similar results across regions is reassuring and suggests that our findings capture a specific cultural legacy of settlement history that cuts across known divides in the U.S.

Finally, note in columns 5–8 that the extended 1790–1950 sample delivers estimated effects of TFE that are somewhat smaller in magnitude than the baseline 1790–1890 sample. This could be due to the changing nature of frontier life relative to the early 1800s. By the early 20th century, transcontinental railroads and improved communications meant that frontier locations were effectively less isolated than they were historically. According to Lang et al. (1995), "the modern-day [post-1890] frontier is not the nineteenth-century one. It is smaller, more law-abiding and regulated, less isolated, less rugged, and less dangerous." Moreover, these authors note that "the frontier has not for generations been the dream of those who seek a fortune or a new life." Together, these factors suggest more limited scope for the mechanisms generating and amplifying frontier culture during early stages of settlement, i.e. the selection and place effects of frontier exposure identified in Section 5.

Racial Differences. To shed further light on the link between historical frontier experience and modern preferences, we show that there are stark differences in the long-run effects of TFE by race. We saw in Table 6 that TFE has large effects on Republican vote shares in the South (Census region), but in Table 5, controlling for the slave share in the 1860 county population reduced the average long-run effect for the full sample. Table 7 offers some insight into these results. We find precise null effects of TFE for African American respondents across the six measures of opposition to redistribution and regulation in

[23]There are four Census regions: Northeast, South, Midwest, and West. We do not consider the Northeast as there are too few counties in this region (66 east of the 1790 frontier line). Note that our baseline sample includes 47 counties in Colorado, New Mexico, and Wyoming in the West region (see Figure 3) but not on the West Coast frontier.

the CCES. Moreover, while non-whites (i.e., blacks and others) generally exhibit weaker opposition to government intervention, it is only the black population for whom we find null effects.

These results support our interpretation of the origins and persistence of frontier culture. Most of today's black population in the U.S. can trace their familial roots to slavery. Given the extreme barriers to geographic and socioeconomic mobility faced by slaves and their postbellum descendants, the mechanisms linking frontier experience to modern outcomes (e.g., selective migration, upward mobility through effort) would have been largely irrelevant to blacks living in high-TFE regions.[24]

These racial differences in the effects of TFE are connected to the possibility that racial resentment by whites may explain some of our findings on opposition to redistribution and shifts in voting patterns across the United States. Indeed, part of that resentment may be linked to beliefs—accurate or not—about the role of effort versus luck in generating income (e.g., pertaining to views of affirmative action and welfare programs). Using measures from the CCES, we find a significant association between this type of racial resentment and TFE, but it does not survive controlling for contemporary population density, thus pointing to an urban–rural divide rather than a high- versus low-TFE divide.[25] This stands in contrast to the robustness of the association of TFE with our key outcomes of interest.

Placebo Outcomes. Frontier culture is a bundle of many traits, but there are policy preferences with which it does not have a clear connection. For illustration, we consider preferences over a few foreign policy issues as placebo outcomes: support for U.S. military intervention abroad in the case of genocide or civil war (35 percent in the CCES), opposition to the Iran sanctions regime (20 percent), and opposition to the U.S.-Korea Free Trade Agreement (45 percent). Estimating our baseline specification, we find relatively precise null effects of TFE on these three measures (-0.004, -0.003, and 0.003, respectively).

4.5 Instrumental Variable Strategy

The battery of robustness checks notwithstanding, it is of course impossible to control for all plausible correlates of culture that might also have shaped TFE. However, it is possible to construct an instrumental variable (IV) that isolates variation in TFE that is orthogonal to unobservable location-specific fundamentals of a given county. Our IV is based on historical shocks to the settlement process driven by immigration inflows to the U.S. These national population shocks create variation in the duration of frontier exposure across space and time. While unrelated to geoclimatic features of each location, these shocks have implications for the exclusion restriction, as we discuss below.

[24]The results in Table 7 are driven largely by the South. When splitting the sample into the South and non-South Census regions, we find more muted and in some cases no differences in the effects of TFE between black and non-black respondents. This may be due in part to the selective migration of blacks out of the South and into frontier areas in the late 1800s. While still subject to different barriers to upward mobility than whites, such self-selected black migrants were arguably more exposed to the influence of frontier conditions than those remaining in the postbellum South. See Billington and Hardaway (1998) for a rich exploration of African Americans on the frontier.

[25]The 2010, 2012, and 2014 rounds of the CCES make two statements about racial resentment and ask respondents their degree of agreement on a scale from strongly agree to strongly disagree: (i) "The Irish, Italians, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors." (64 percent somewhat or strongly agree), and (ii) "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." (51 percent somewhat or strongly disagree). While TFE exhibits a significant positive association with both measures in our baseline regression (0.010** and 0.012***, respectively), a simple control for 2010 population density, as in column 2 of Table 4, renders the estimates null and insignificant (0.001 and 0.004, respectively).

Immigrants contributed to westward expansion by exerting population pressure on the eastern seaboard and by going west themselves. Appendix F documents the connection between immigrant inflows and westward expansion. For a given location, TFE partly reflects the speed of westward movement at the national level during the relevant time frame. To construct the instrument, we determine the first year in which each county is within 110 km of the frontier line. At this time, the county's local conditions do not affect the contemporaneous process of westward expansion, but the moving frontier is getting close. We then consider the average annual immigrant inflow in the next 30 years.[26]

An important identifying assumption is that the intensity of immigrant flows to the U.S. is unaffected by the conditions of any given frontier county. This would not hold if, for example, Europeans' migration decisions in a given period were influenced by knowledge of frontier locations (e.g., their levels of land productivity). To address this concern, we isolate push factors unrelated to conditions on the frontier. Following Nunn et al. (2017), we predict migrant outflows from Europe based on climate shocks, and use these predictions to construct an alternative instrument. Appendix F provides full details.

Table 8 presents IV estimates for the same five primary outcomes as in Tables 4–5. In Panel (a), we find large, significant effects of TFE that are slightly larger but statistically indistinguishable from the OLS estimates. Panel (b) shows similar results when using predicted rather than actual migrant flows in the IV construction. Overall, the IV exercises help clarify the identifying variation in TFE. The first stage results in Appendix Table F.1 show that there are various geoclimatic predictors of TFE, but national immigration inflows, which are unrelated to local conditions of any given county, also account for a sizable amount of variation in TFE. Coupled with the robustness checks in the previous section, the similarity of the IV and OLS results in Table 8 suggests that our findings are not driven by local conditions determining both TFE and outcomes of interest today.

While this IV strategy addresses some concerns about omitted variables, the exclusion restriction may not hold. In particular, the immigration flows underlying the IV affect both the scale and composition of migrants to the frontier.[27] During periods of greater immigration (lower predicted TFE) frontier settlers may include relatively more foreigners and non-individualistic native-born. For example, in periods with many immigrants arriving to the east coast, a large number of native-born Americans flowed westward, many of whom could have been non-individualistic types. By a similar logic, large immigration inflows out of Europe (induced by weather shocks) may lead to a greater stock of non-individualistic, foreign-born arriving on the frontier. These population flows would directly lower TFE and reduce the prevalence of individualism. While these types of selective migration could invalidate the IV, neither seems pervasive during the frontier era as we show in Appendix F.

5 The Roots of Frontier Culture

This section explores how "rugged individualism" took root on the American frontier. Using historical Census data, we show that this culture arose through not only selective migration but also effects of exposure to frontier conditions. In other words, more individualistic people settled the frontier historically, and the frontier induced greater individualism among settlers over time.

[26]Nearly 85 percent of counties exit the frontier within that time frame. Results are similar for other windows.

[27]In a standard Roy-Borjas model of migration, the size and composition of migration flows are in general jointly determined and not independent (see, e.g., Grogger and Hanson, 2011).

In Section 5.1, we demonstrate the presence of selective migration. Put simply, the frontier attracted and retained individualistic migrants.

In Section 5.2, we identify causal effects of exposure to the frontier using two alternative strategies based on movers. In approach (i), we use an event-study design that isolates within-family growth in individualistic names on the frontier and shows no evidence of prior trends before moving to the frontier. In approach (ii), we use variation in age at move among the children of frontier migrants to isolate childhood exposure effects on individualism in adulthood. Both approaches exploit variation in years of frontier exposure, similar to our analysis of the long-run effects of frontier experience in the previous section. Our findings establish that frontier life led to growing individualism among settlers, an interesting case of cultural change in response to incentives in a new environment.

We argue that both forces underlying the origins of frontier culture—selective migration and cultural change—may be explained by an advantage of individualism on the frontier. Section 5.3 discusses potential explanations for this advantage and shows empirically that individualists fared relatively better economically in frontier locations.[28]

While identifying both selective migration to and cultural change on the frontier, we do not attempt to structurally quantify their relative importance (though we do offer a heuristic assessment at the end of Section 5.2). It seems plausible that the two forces would have reinforced each other historically by increasing the prevalence of individualists, thus reducing the chances of successful cooperation with others and amplifying the returns to individualism. This feedback loop between selection and "treatment" during early stages of settlement helped to shape the persistent culture of rugged individualism that we identify over the long-run in Section 4.[29]

Our analysis in this section requires information on migration flows, which is not directly available from Census data. We construct data on migration in two ways. First, we use information on state of birth of children to infer migration patterns of their parents as in Collins and Zimran (2018). Second, we track individuals over time by linking across Census rounds using an algorithm developed by Feigenbaum (2016) and detailed in Appendix L.[30]

5.1 Selective Migration

We show here that selective migration to and from the frontier contributed to its greater prevalence of individualism historically. Our analysis centers on the names given to children prior to moving. Across forty years of frontier settlement in the 1800s, we find that households moving to the frontier

[28] To the extent that more successful individuals also had more children, differential fertility may also have increased the prevalence of individualism on the frontier. Appendix K.4 examines a competing, disease-based explanation for the origins of individualism rooted in biology and known as the parasite-stress theory of values (Fincher and Thornhill, 2012). Using data on disease and illness in the 1880 Census, we do not find evidence in support of this explanation for the frontier differential in individualism.

[29] Appendix H.4 argues that frontier conditions may have also honed opposition to redistribution through the favorable prospects of upward mobility and a large perceived importance of effort in income generation. These factors, in turn, likely interacted with the process of cultural change towards individualism. For example, the greater the advantage of individualism on the frontier, the more favorable the mobility prospects. And if this led to lower tax redistribution, it may have also reinforced selective migration of individualists.

[30] In both cases, we have samples that might seem relatively small, but it is important to note that (i) the frontier comprised a relatively small share of the entire U.S. population at any given time in the 1800s, and (ii) the restriction to frontier migrants with children born prior to moving further reduces the sample.

have children with more individualistic names than households that remain behind in settled areas. The opposite holds when looking at movers from the frontier to settled areas.

Using complete-count Census data from 1850–80, we estimate the time at which a household moves to (or from) the frontier based on the current county of residence and, crucially, differences in the reported birth state of children. Consider, for example a household living in frontier county c in Iowa in 1850 whose first child was born in Virginia (a non-frontier state) in 1842, and their second child was born in Iowa in 1848. An unbiased estimate of time-at-move dates this household's arrival on the frontier to 1845. If this household did not have a second child, we would date their time-at-move to 1846. An analogous procedure can be used to identify movers from frontier to settled areas.

Of course, this approach misses moves from settled to frontier counties (and vice versa) within the same state. In Appendix J, we present complementary results that circumvent this difficulty by using a smaller, linked-sample of households where we can identify origin and destination counties in 1870 and 1880. The advantage of the approach here is that we use the full population living in counties between the 1790 and 1890 frontier lines.

Table 9 compares the prevalence of infrequent names among children whose parents move them to the frontier relative to children that remain in settled counties. The estimating equation is given by:

$$\text{infrequent name}_{ict} = \alpha + \beta\, frontier\ migrant_{ict} + \mathbf{FE} + \varepsilon_{ict}, \tag{5}$$

where the binary dependent variable equals one if child i residing in county c in Census year t has a name that falls outside the top 10 nationally in that decade. We consider all children age 0–10 with native-born parents in keeping with the restrictions earlier in the paper. The *frontier migrant* indicator equals one if c is on the frontier in t and i was born in a state with no frontier counties at his/her time of birth. This indicator equals zero for all children of households living in settled, non-frontier counties. Standard errors are clustered by county. The $\mathbf{FE}$ vector includes fixed effects for birth year×gender, birth order, and, in even-numbered columns, child birth state.

Columns 1–2 of Table 9 show that households that move to the frontier have children with more individualistic names than those remaining behind in settled areas. In column 1, the estimate of β is around 3.2 p.p. relative to mean of 65 percent for stayers in settled areas. This estimate is relatively unchanged when including child birth state FE in column 2, which ensures comparisons with children that are most similar to those had the family not migrated to the frontier.

Columns 3–4 of Table 9 repeat this exercise in reverse, documenting selective emigration of non-individualistic families from the frontier. The specification is the same as in (5) except now we construct a binary indicator for *settled migrant*, which equals one if c is not on the frontier in t and i was born in a state with at least one frontier county at his/her time of birth. This indicator equals zero for all children of households living in frontier counties. The estimate in column 4 of -2.9 p.p. relative to mean of 63 percent points to a roughly equal and opposite selection of non-individualistic types out of the frontier.

Overall, the results in Table 9 suggest that selective migration contributed to the differential prevalence of individualism on the frontier historically. At the same time, the differential infrequent names seen in column 5 of Table 1 could be explained both by selection and treatment. Although individualistic parents were more likely to move to the frontier, this does not preclude subsequent growth in individualism among those movers after sustained exposure to the frontier. We turn now to identify this type of cultural change in response to varying degrees of frontier life.

5.2 Frontier Exposure and Cultural Change

This section provides evidence consistent with frontier life changing people's culture. The frontier not only attracted individualistic settlers but also made frontier settlers more individualistic. We develop two strategies to identify such a causal effect of frontier exposure. Both exploit variation in the length of exposure to frontier conditions, one in adulthood and the other in childhood. The intuition is that longer exposure implies greater scope for the frontier environment to affect cultural traits. This is the same notion underlying the long-run effects of total frontier experience on culture at the county level. Here, we identify short-run effects at the individual level.

First, we use an *event-study* approach that exploits within-household variation in naming patterns to show that parents give their children increasingly individualistic names after arrival to the frontier and that this did not simply reflect a pre-trend in individualism. Second, we use an *age-at-move* approach that exploits cross-sibling variation in the time at which their parents chose to move the family to the frontier. Tracking siblings 30 years later, we find that people with greater childhood exposure to the frontier give their children more individualistic names. Importantly, both approaches account for household-specific, time-invariant individualism. These two complementary strategies point to causal effects of frontier exposure under a plausible set of identifying assumptions that we discuss below.

(i) Event Study: Adulthood Exposure. Our first strategy identifies changes in how parents name children born after versus before moving to the frontier. Specifically, we estimate the following equation that relates the name given to child i, born in year $\tilde{t}+j$, to the year $\tilde{t}$ in which his/her household h moved to frontier county c at some time prior to Census year t:

$$\text{infrequent name}_{iht} = \alpha + \sum_{j=-20}^{20} \beta_j 1(born\ in\ \tilde{t}+j)_{ih} + \theta_h + \mathbf{x}_i'\boldsymbol{\eta} + \varepsilon_{iht}. \qquad (6)$$

The household fixed effects, θ_h, absorb all time-invariant characteristics that affect h's choice to migrate to the frontier and its individualism. The $\mathbf{x}_i$ is a vector of child i characteristics including gender, child order, and some control for birth cohort trends. We pool across Census years 1850–80 and consider all kids ages 0–20 in 1850 and ages 0–10 in 1860, 1870 and 1880 to avoid double counting. The dependent variable here, and in the age-at-move approach below, is again based on the top 10 gender- and decade-specific names for white children with native-born parents. Standard errors are clustered by county.

Our main interest in equation (6) lies in the β_j coefficients, which identify differential individualism across siblings' names with respect to the year $\tilde{t}$ at which h moved to the frontier. The estimates are normalized with respect to children named in the year before arrival on the frontier, such that β_5, for example, identifies how much more likely it is to observe an infrequent name for a child born to family h five years after arrival on the frontier relative to their child born one year before leaving a settled area. The controls $\mathbf{x}_i$ help rule out general trends in infrequent names across time and birth order, thereby isolating within-household variation that is most plausibly related to changes in frontier exposure.[31]

[31] This specification is similar to the one in Abramitzky et al. (forthcoming) who relate time spent in the U.S. to the Americanization of names given to native-born children by foreign-born mothers. Whereas their study estimates separate equations

We also estimate an equation with continuous measures of birth years relative to move:

$$\text{infrequent name}_{iht} = \alpha + \beta_{pre}(years\ until\ move)_{ih} + \beta_{post}(years\ after\ move)_{ih} + \theta_h + \mathbf{x}_i'\boldsymbol{\eta} + \varepsilon_{iht}. \quad (7)$$

This specification identifies pre- and post-move trends but is less flexible than (6).

To estimate equations (6) and (7), we require households with at least two children and at least one of them born before the household moved to the frontier. Our sample consists of 57,097 children living in 16,901 households. Consider, for example, a household on the Iowa frontier in 1850 with four children: John born in 1840, Mary in 1843, Lisa in 1847, and Ruben in 1850. We see John and Mary are born in Virginia and Lisa and Ruben in Iowa. Hence, we impute $\tilde{t} = 1845$ and $j = -5$ for John, -2 for Mary, +2 for Lisa and +5 for Ruben. Households like this provide key identifying variation.

The key identifying assumption is one of parallel trends in children's names. In other words, the trends in individualistic children's names in household h would not have changed had the household not moved to the frontier. While this counterfactual is of course unobservable, the lack of pre-trends in Figure 5 and precise zero on β_{pre} in Table 10 are compelling. Households do not appear to be moving to the frontier as a result of a growing trend in individualism. This goes against the concern that parents had already started becoming more individualistic prior to moving and hence for reasons unrelated to frontier exposure. Although frontier migrants are self-selected on pre-existing level differences in individualism compared to those remaining in settled areas (see Section 5.1), those same migrants are not self-selected on pre-existing growth in individualism.

Turning to the main post-move patterns in Figure 5 and Table 10, we find a significant increase in individualistic children's names as migrant families reside on the frontier for longer periods of time. Figure 5(a) reveals a stark trend break in individualistic names within households after moving to the frontier. A child born one decade after their parents moved to the frontier is nearly 8 p.p. more likely to have an infrequent name than their sibling born one year prior to moving. Assuming linearity, column 1 of Table 10 suggests that each additional year of exposure to the frontier increases the likelihood of giving their next child an infrequent name by 0.7 p.p.

These estimates suggest that frontier conditions increased parents' own individualism *or* increased their preferences over their children's future individualism. In either case, the prevalence of individualism increased over time within families. Further results below support a causal interpretation.

Robustness. These baseline results are robust to accounting for a general time trend in individualistic names in five alternative ways. We control for five-yearly and three-yearly birth cohort FE in columns 2 and 3 of Table 10, respectively. We account for the general increase in individualistic names among later-born siblings by controlling for child birth order on its own and in combination with five-yearly birth cohort FE in columns 4 and 5, respectively. Corresponding graphical results for each specification can be found in Appendix Figure G.1. Finally, we control for pre-move-state trends in infrequent children's names in Figure 5(b) and column 6 of Table 10. This controls for the given family's origin-state-specific trends in individualistic names had they not migrated (to the frontier).[32] Across these robustness checks, we continue to find both a lack of pre-trends and a significant increase in individualistic names as parents spend more time on the frontier.

for children born pre- and post-move to the U.S., we combine the two in an event-study design centered on the time of move.

[32]These are gender- and birth-year-specific means based on the state of birth of the last child born before the family moved.

While the lack of pre-trends is reassuring, there remains a source of endogeneity that could be important but seems inconsistent with our findings. Suppose a household experienced an unobservable shock that simultaneously led them to move to the frontier and increased their future individualism irrespective of frontier exposure. For example, they suddenly reap large returns to prior investments with little help from neighbors and local government, inducing them to embrace individualism and also enabling them to move to the frontier in search of opportunities. Like any event-study design, the relevance of such shocks is impossible to rule out, but it seems implausible given our findings. In particular, shocks like these would lead to jumps in individualism right around the time of moving to the frontier, while our results indicate growing individualism with each additional year of frontier exposure.

There are two concerns about the post-arrival trends in individualism that hinge on sample selection. Households that experience greater returns to individualism early after arrival may be more likely to have more children and also to survive longer on the frontier. With differential fertility, we would see more children born to individualistic households in the later years post-arrival in Figure 5. Differential outmigration (or death) further implies that we are less likely to see households for whom individualism did not increase after arrival to the frontier. We now turn to an alternative approach to identifying exposure effects that is not affected by these sample selectivity concerns.

(ii) Age-at-Move: Childhood Exposure. Our second strategy to identify the causal effect of frontier exposure exploits variation in age-at-move to the frontier among siblings. We follow these siblings 30 years later to examine differences in individualism expressed in adulthood. In particular, we create a dataset linking boys aged 0 to 20 in the 1850 Census to the 1880 Census by which time they were 30 to 50 year old household heads with children of their own. With this linked sample of nearly 42,000 individuals spanning 30 years, we ask whether being exposed to frontier conditions from an earlier age makes fathers more likely to give individualistic names to their children.[33] This approach has the advantage of relying on variation in frontier exposure that is not due to the migrants' own choices but rather to their parents' choices.[34]

We estimate the following for child i in the 1880 Census with father f from household h in 1850:

$$\text{infrequent name}_{ifh} = \alpha + \sum_{j=1}^{17} \beta_j 1(f\text{'s age-at-move to frontier}_h = j) + \mathbf{x}'_{if}\boldsymbol{\eta} + \theta_h + \varepsilon_{ifh}. \tag{8}$$

As a baseline, we restrict to children whose fathers moved to the frontier with their parents as children. The key regressors are indicators for those ages $j = 1, \ldots, 17$, though we also consider a continuous age-at-move specification. The $\mathbf{x}$ vector includes controls for child i gender and birth order as well as father f birth order fixed effects. The latter helps to rule out possible differences in individualism across brothers that might be unrelated to age-at-move to the frontier. We also consider controls for state of residence fixed effects in 1880, though most fathers reside in the same state as their brother(s). Standard errors are two-way clustered on 1850 household and 1880 county.

[33] Appendix L provides full details on the linking procedure. Like other studies based on historical linked records, we focus entirely on men as women changed their names upon marriage making it impossible to credibly match them across censuses. The linking generates a sample of 41,975 fathers with 146,845 children in 1880.

[34] This approach is similar in spirit to Chetty and Hendren (2018) who study variation in childhood exposure to neighborhoods of varying quality. Their setting is different in that neighborhood quality displays continuous variation, while we define exposure to frontier conditions at any given point in time as a dummy variable.

Given the 1850 household fixed effects, θ_h, the β_j identify differences in the likelihood of individualistic names across cousins in 1880 due to the migration decisions of their grandparents prior to 1850. We normalize age 1 to zero so that each β_j identifies how much less likely we are to observe an infrequent name for cousin i whose father f moved to the frontier at age j compared to cousin i' whose father f' moved to the frontier at age 1.[35] Under certain identifying assumptions specified below, we can interpret differences in β as arising from a place effect of j fewer years of exposure to the frontier as a child.

By isolating differences in frontier exposure among siblings due to migration decisions of their parents, this approach overcomes concerns about selective migration as well as the type of sample selectivity that might affect the event-study design. The identifying assumption in this age-at-move, cross-sibling design is that the potential individualism of children is orthogonal to the timing of the family's move. This would be violated if there was selection of frontier movers based on pre-trends or unobservable shocks. However, both of these seem implausible, as discussed above with respect to the event-study. Subsequent growth in individualism created by parents' exposure to frontier conditions would not be a source of bias, but rather a channel for the frontier's treatment effect on children

The core results in Figure 6 and Table 11 point to significant effects of frontier exposure that are in line with the earlier, event-study results. In Figure 6(a), for example, we see that the likelihood of being given an individualistic name is 10 p.p. higher for children whose fathers moved to the frontier at age 1 compared to their cousins whose father was 10 years old when the family moved to the frontier. Assuming that these age-at-move effects are linear, which is not unreasonable given the patterns in Figure 6, the estimate in column 1 of Table 11 implies that with each additional year of frontier experience as a child, one is 0.7 p.p. more likely as an adult to give their own children individualistic names. This estimate is strikingly similar to the effect of frontier exposure in adulthood seen in Table 10.

Robustness. These results survive several additional identification checks. Column 2 includes contemporary state fixed effects, allowing for the possibility that the brothers in 1850 might reside in different locations in 1880 for reasons unobservable to us but perhaps confounded with their individualism. The results are effectively unchanged from the baseline. Columns 3–5 account for age differences across cousins in 1880 using increasingly stringent fixed effects for decade (column 3), five-yearly (column 4), and three-yearly (column 5) birth cohort fixed effects. These are in addition to the baseline control for child birth order and help rule out trends in individualistic names that might be confounders of older versus younger age-at-move of the fathers. Like the event-study results in Table 10, these added controls reduce precision but nevertheless leaves us with quantitatively significant effect sizes that are statistically indistinguishable from the baseline.

We report two further robustness checks in Appendix G.2. First, these findings hold across alternative approaches to addressing measurement error in the linking procedure, including a reweighting procedure that accounts for differential linking success based on the infrequency of the father's name. Second, they are robust to including children whose fathers were born on the frontier (and treating them as though they moved at age zero).

[35]To fix ideas, consider brothers John (age 10) and Paul (6) born in Virginia who we observe on the Iowa frontier in the 1850 Census. Using the procedure above based on children's birth states, we infer that the parents moved the boys to the frontier in 1847 when Paul was 3 and John was 7. Regardless of the precise moving date, Paul could ultimately have as many as four more years of childhood frontier exposure than John. Equation (8) allows us to identify whether Paul's children observed in the 1880 Census have more individualistic names than John's children.

Summary: Selection vs. Exposure Together, these two distinct identification strategies yield evidence consistent with a treatment effect of frontier conditions on the prevalence of individualism. While frontier migrants self-select on prior (levels of) individualism, the results above suggest a causal amplification of this cultural trait after arrival. Strategy (i) identified this amplification within a single generation of parents giving birth to children before and after moving to the frontier. Strategy (ii) identified it using variation across generations, linking individualistic children's names to the childhood frontier experience of their fathers resulting from the migration decisions of their grandparents.

With these mutually consistent estimates, we can assess the relative magnitude of selection and exposure effects. In particular, we ask how many years of frontier exposure it takes to double the differential levels of individualism that self-selected migrants bring with them to the frontier. Comparing estimates of selection from Table 9 with those of exposure effects from Table 10, we find that it takes around 4–8 years depending on which specification one uses. At the upper end, the estimate in column 5 of Table 10 suggests that an additional 8 years of frontier exposure increases the likelihood of giving one's child an individualistic name by 3.2 p.p., which is exactly the differential among frontier migrants compared to those remaining in settled areas as seen in column 1 of Table 9.

5.3 Returns to Individualism

The opportunities and threats on the frontier may have rewarded individualism. Because people on the frontier primarily had to rely on themselves for protection and material progress, the independent, self-reliant types were likely to fare better (Kitayama et al., 2010).[36] Moreover, frontier settlers faced novel agroclimatic conditions, and there was little local knowledge about how best to approach the harsh and unfamiliar setting (see Shannon, 1977). Adherence to old traditions and norms was less suited to the environment than non-conformism and innovation, two traits associated with individualism.[37]

This section provides descriptive evidence consistent with an advantage of individualism on the frontier. We do this by estimating the relationship between father i's economic status in county c in Census year t, y_{ict}, and predetermined infrequent names within the household according to the following difference-in-difference specification:

$$\begin{aligned} y_{ict} \quad = \quad & \alpha + \beta \text{ own infrequent name}_{ic} + \eta(\text{own infrequent name}_{ic} \times \text{frontier}_{ct}) \qquad (9) \\ & + \delta \text{ children infrequent names}_{ic} + \zeta(\text{children infrequent names}_{ic} \times \text{frontier}_{ct}) + \theta_{ct} + \varepsilon_{ict}, \end{aligned}$$

where β captures the hedonic return to the father's own infrequent name outside the frontier and η the differential return on the frontier. At the same time‘, δ captures the association of average infrequent name choices for children born during the decade prior to t and the father's economic well-being outside the frontier, and ζ the frontier differential. We restrict attention to white, native-born fathers with at least

[36] Critics of Turner emphasize the importance of cooperation on the frontier (e.g., Boatright, 1941), but his supporters have argued that cooperation was not inconsistent with individualism. For instance, according to Billington (1974), the frontiersman "spoke for individualism ... even though he was equally willing to find haven in cooperation when danger threatened or need decreed." While returns to cooperation may have been high at times, maintaining reciprocity would have been difficult in frontier settings with such high population mobility, as noted in the literature on social capital (see Munshi, 2014).

[37] The connection between innovation and individualism is discussed at length in Gorodnichenko and Roland (2012). In characterizing the traits of frontier populations, Turner (1893) himself mentions individualism along with the "coarseness and strength combined with acuteness and inquisitiveness" and the "practical, inventive turn of mind, quick to find expedients."

one child and define infrequent names as those outside the top 10 nationally. The county×year fixed effects, θ_{ct}, account for all differences in outcomes common across individuals within the same county. Standard errors are clustered by county.

We pool data across Census rounds 1850–80 and measure y_{ict} using the occupational score (*occscore*), a widely-used proxy for economic status in the historical literature. This index ranges from 0 to 100 and captures the income returns associated with occupations in the 1950 Census. We use the *occscore* proposed by Minnesota Population Center (2017) for 1850 and 1880 and construct the scores directly for 1860 and 1870 using a crosswalk of occupational strings to codes for available years.

Table 12 suggests differential returns to individualistic behavior on the frontier. Focusing on the full specification in column 3, fathers in non-frontier counties that give their children individualistic names exhibit higher occupational scores than those that give their children more common names, and this differential is more than one-third larger on the frontier. The estimate of η around 0.3 is economically meaningful, capturing around one-third the mean difference between the *occscore* for a farmer and a blacksmith. Meanwhile, although fathers with own infrequent names perform better in non-frontier counties, there is no significant differential on the frontier. These results are even stronger when restricting the analysis to fathers that are not in farming occupations (column 4). The estimate of $\eta = 0.56$ is around one-half the mean difference between the *occscore* for a blacksmith and a carpenter.

These suggestive results complement other evidence pointing to differential returns in Table 9. Columns 3 and 4 of that table identify selective outmigration of non-individualists from frontier counties. Ultimately, an advantage of individualism may explain why the frontier attracted and retained individualistic types, and also why it induced a shift toward individualism among frontier settlers.

6 Discussion

This paper provides new evidence on how frontier settlement shaped culture across the United States. Historically, frontier locations exhibited starkly different demographics and a greater prevalence of individualism as reflected in name choices for children. Today, counties with longer historical frontier experience exhibit stronger opposition to government intervention in the form of redistribution, taxation and various regulations. These same counties have a greater prevalence of individualistic names in the mid-20th century. We provide evidence on the historical roots of this culture of rugged individualism, identifying important contributions of both selective migration as well as causal effects of exposure to frontier conditions. Greater returns to individualism and expectations of upward mobility likely fueled these two forces, which together laid the foundation for persistent long-run cultural differences across the country.

We provide some of the first systematic evidence on a prominent theme in American history and lend credence to some elements of Frederick Jackson Turner's famous thesis. Turner's work has attracted immense attention as well as vast criticism. His narratives contain departures from the historical record, overblown statements, and mythological elements. They paint an idealized portrait of the frontiersman and leave women and minorities out of the picture. The term "free land" appears repeatedly when, in fact, land was violently taken from Native Americans and, in many areas, westward expansion was more about "conquest" than "settlement" (Limerick, 1988). Our research provides empirical support for some important elements of the Frontier thesis, but it is not a general assessment of Turner's work nor an endorsement of its ideological overtones.

Our findings have suggestive implications for the sharp contrast between the U.S. and Europe in terms of redistribution preferences and policies, a recurring topic in the political economy literature. According to Turner (1893), "the advance of the frontier ... meant a steady movement away from the influence of Europe," as "moving westward, the frontier became more and more American." As settlers of European origin shed their former culture and embraced rugged individualism across the U.S., America as a whole became more and more different from Europe. The frontier roots of opposition to redistribution in the United States may explain why these preferences remain stable despite rising inequality.

In closing, we note that frontier settlement may have had different effects in other countries. For instance, Argentina and Russia also underwent massive territorial expansion in their early history, but were ruled by elites that built very different institutions. In their analysis of how historical frontiers affected later democratic quality across countries in the Americas, García-Jimeno and Robinson (2011) show that the positive effects of frontiers depend on the quality of initial institutions. The national institutions of the United States, which favored relatively high levels of geographic mobility, access to land, and security of property rights, may have been preconditions for the mechanisms we emphasize. Our method for locating and tracking the frontier historically should prove useful in other attempts to understand the legacy of frontier settlement.

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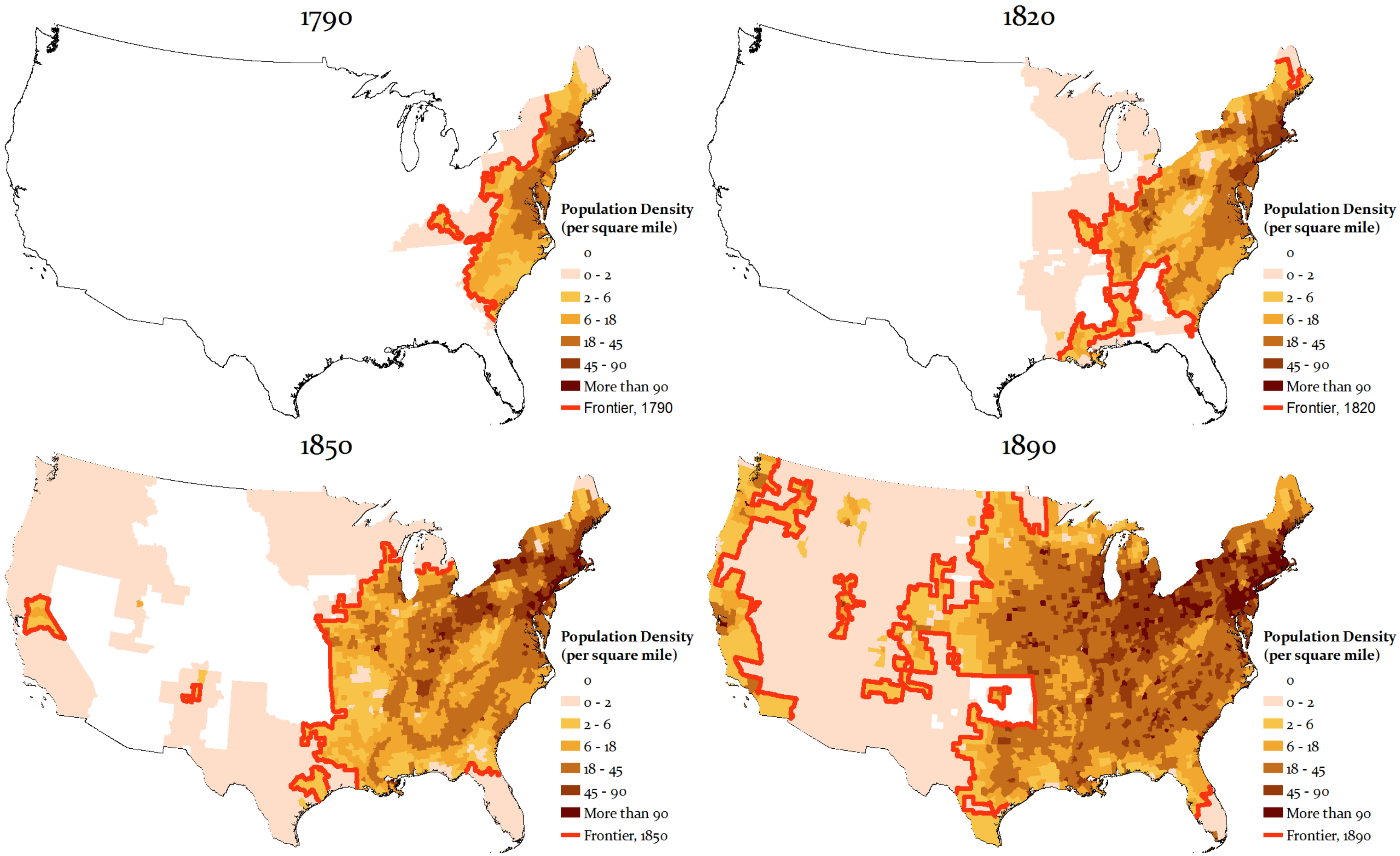
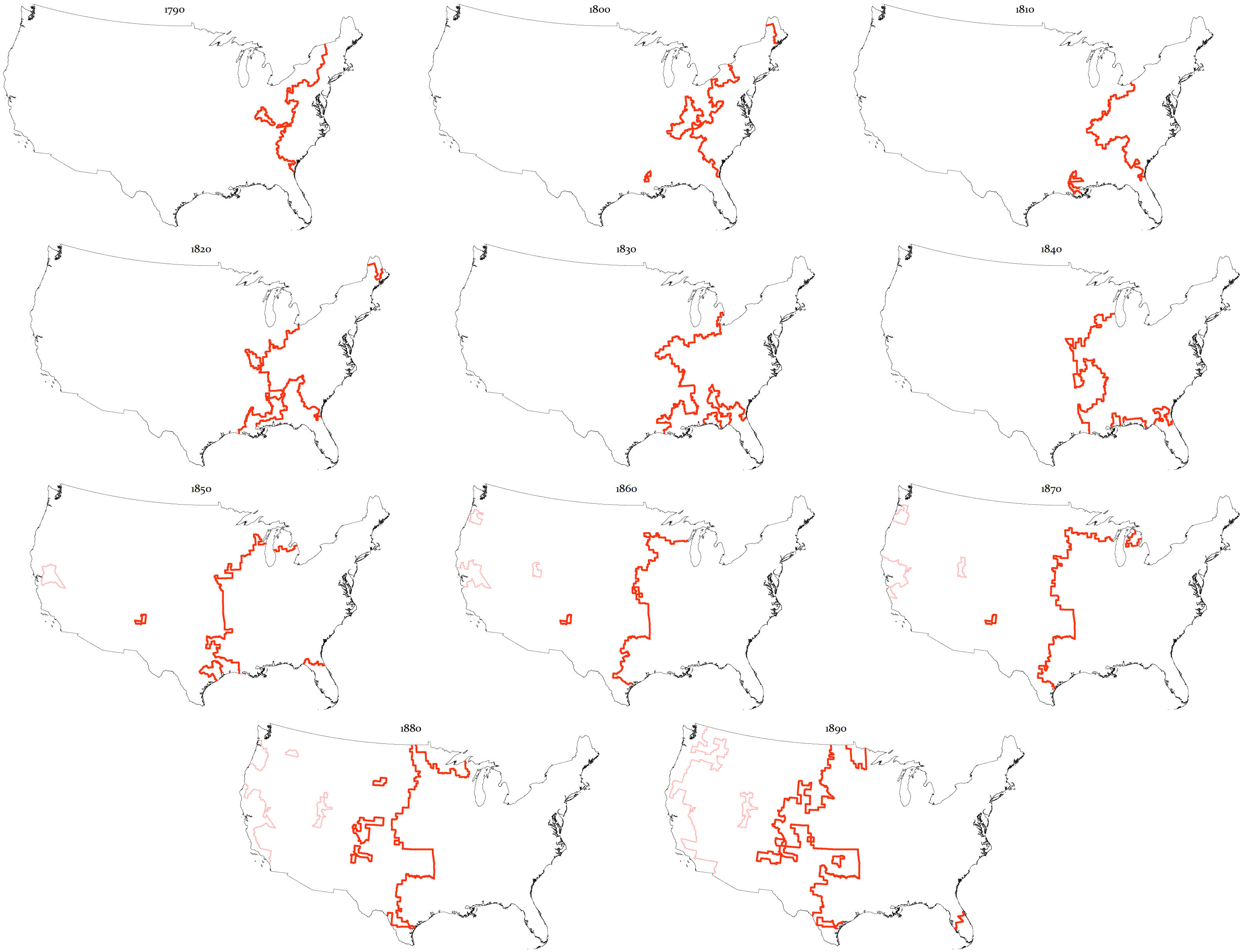
Figures

Figure 1: Population Density and the Frontier for Selected Years



Notes: Based on county-level data from National Historical Geographic Information System: Version 11.0. Population is allocated across years and counties based on the harmonization procedure described in Appendix L. The red frontier line is based on the algorithm described in Section 2.1 and Appendix A. The population density figures exclude most Native Americans because they were not counted by the Census throughout the frontier era.

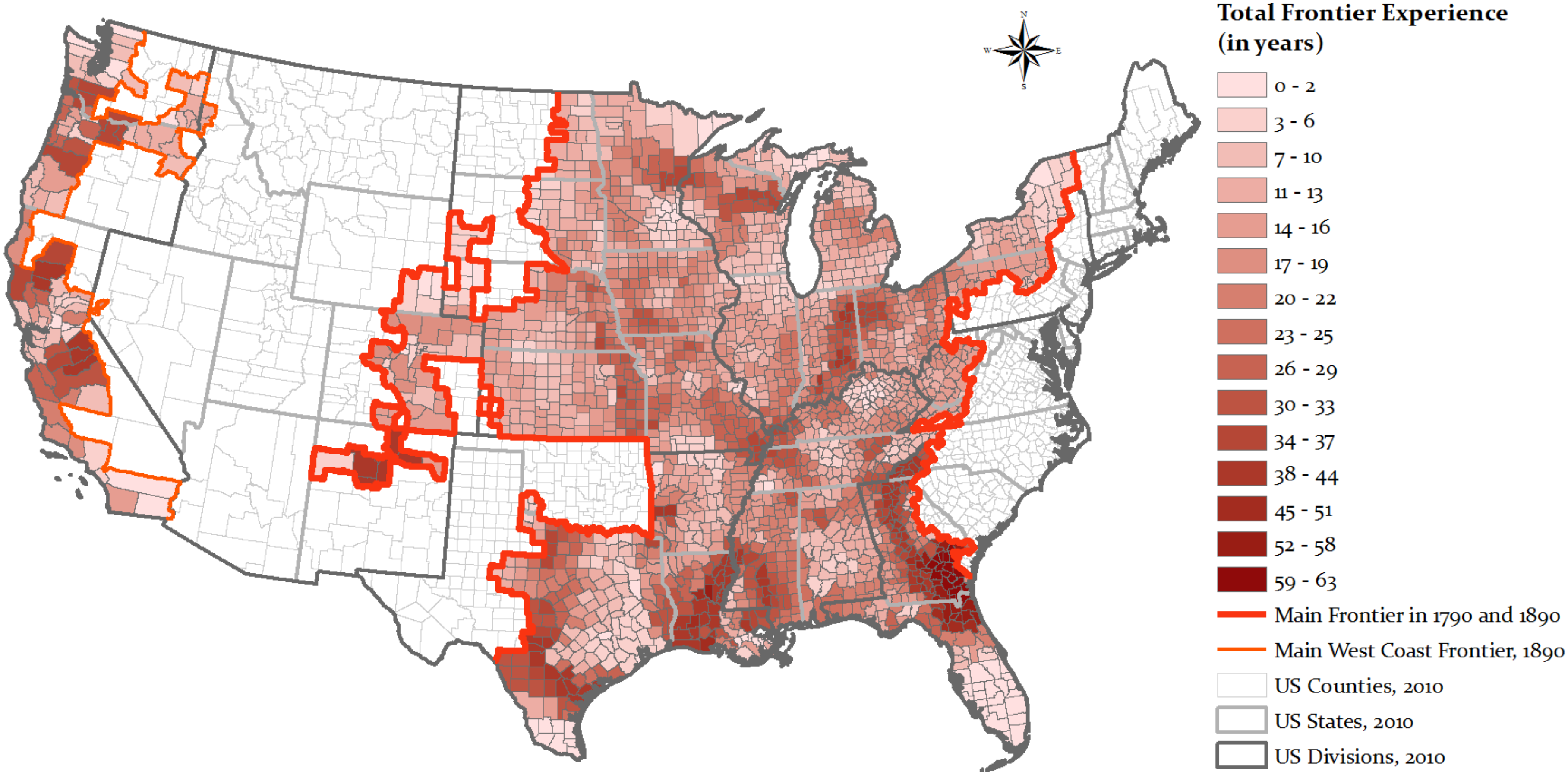
Figure 2: The Evolution of the Frontier, 1790 to 1890



Notes: Based on county-level data from National Historical Geographic Information System: Version 11.0. The frontier lines demarcate the contour of counties with population density below and above 2 people per square mile. The dark red lines correspond to the main frontier lines emerging form east-to-west expansions (our baseline analysis). The light red lines correspond to the frontiers resulting from west-to-east expansions from the West Coast, which we examine for robustness. In both cases, we exclude smaller "island frontiers" in the interior and contour line segments less than 500 km. Full details on the frontier line algorithm can be found in Appendix A.

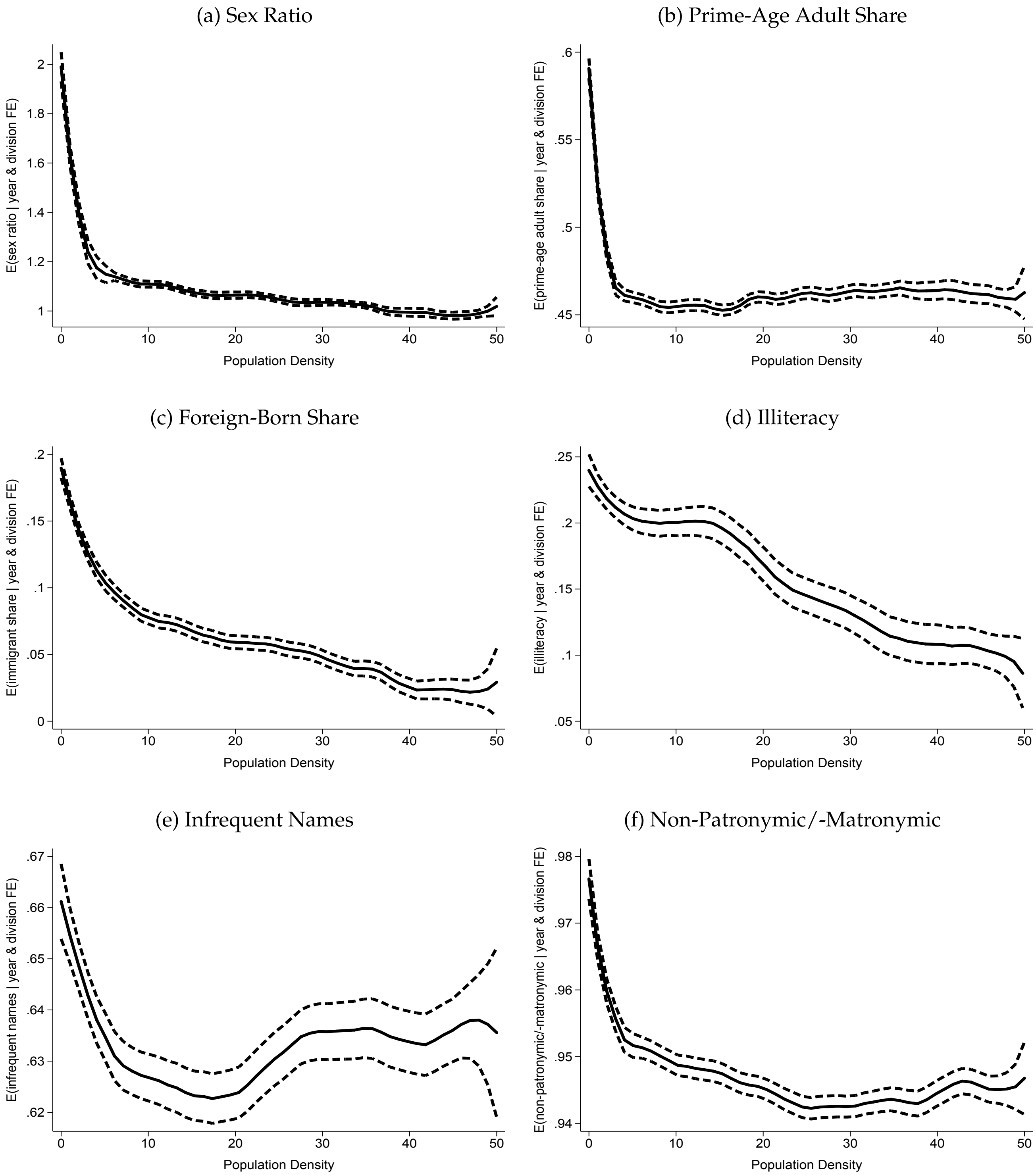
Figure 3: Total Frontier Experience, 1790 to 1890

(baseline sample lies between 1790 and 1890 main frontier lines, see Section 2.2 and notes below)



Notes: Based on county-level data from National Historical Geographic Information System: Version 11.0. Total frontier experience is the total number of years the county was within 100 km of the frontier line and its population density was below 6 people per square mile, between 1790–1890. The white areas to the east of the 1790 main frontier line are counties for which we do not know frontier history given the lack of Population Census data before 1790. The white areas to the west are beyond the 1890 frontier line and hence not included in our baseline sample, which is restricted to the frontier era as defined by Porter et al. (1890) in the Census *Progress of the Nation* report. We include many of those counties to the west when extending the frontier era through 1950 for robustness.

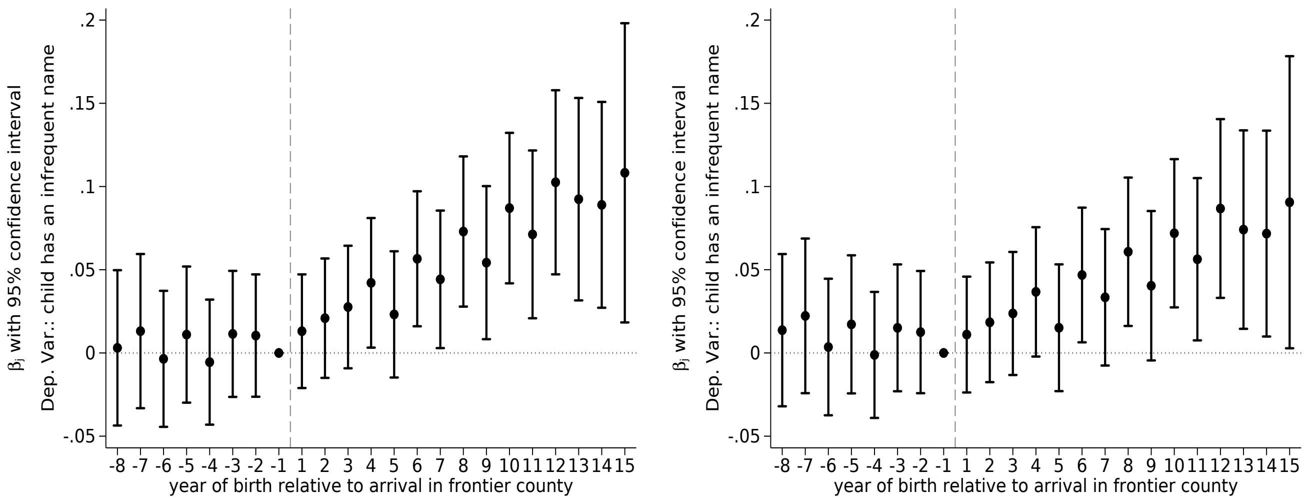
Figure 4: Demographics and Individualism by Population Density, 1790 to 1890



Notes: These figures plot semiparametric estimates of equation (3) relating population density to demographic characteristics prominent in historical accounts of the frontier (a-d) and proxies for individualism (e-f). We estimate these curves $g(\cdot)$ based on the Robinson (1988) partially linear approach, pooling across all available years 1790–1890 for each county c. The specification includes Census division and year fixed effects, which are partialled out before estimating these shapes, and are based on an Epanechnikov kernel and rule-of-thumb bandwidth. The dashed lines are 95 percent confidence intervals. The estimates are recovered over all counties, but the figure zooms in on those with less than 50 people/mi^2 for presentational purposes. (a) *Sex Ratio* for whites is the ratio of the number of white males over white females. (b) *Prime-Age Adult Share* is the fraction of whites aged 15–49 over the total number of whites. (c) *Foreign-Born Share* is the ratio of foreign-born persons over total population. (d) *Illiteracy* is the illiteracy rate for whites aged 20 or older. (e) *Infrequent Names* is the share of children with names outside of the top 10 most popular names in their Census division with the sample restricted to children aged 0–10 with native-born parents. (f) *Non-Patronymic/-Matronymic* is the share of boys/girls with names that are distinct from their fathers/mothers, again restricted to children 0–10 with native-born parents.

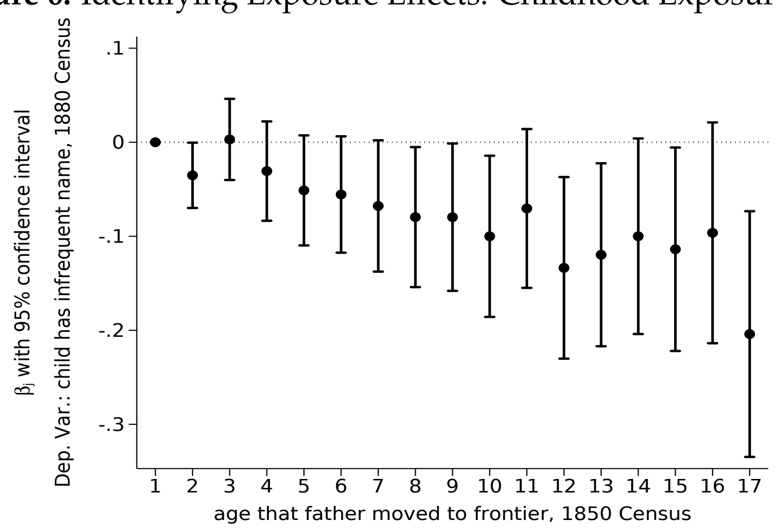
Figure 5: Identifying Exposure Effects: Adulthood Exposure (I)

(a) Baseline: Birth Decade FE (b) Pre-Move State Name Trends



Notes: This figure isolates within-household, cross-child variation in parental exposure to the frontier. Each graph reports estimates of β_j and 95% confidence intervals in equation (6) for $j = -8, \ldots, 15$ (with other j included but suppressed for presentational purposes). Each β_j can be interpreted as the differential likelihood of an infrequent name being given to a child born j years before/after their parents moved to the frontier, relative to the child born one year prior to moving. The sample includes 57,097 children born to 16,901 families headed by white, native-born parents that moved with at least one child to a frontier county as we observe them in the Census records in 1850, 1860, 1870 or 1880. All estimates control for household fixed effects and child gender. Graph (a) additionally includes child birth decade FE, and (b) includes controls for the mean gender-specific infrequent name share in each child birth year in the state from which each family migrated from before arriving on the frontier. Standard errors are clustered by contemporaneous county.

Figure 6: Identifying Exposure Effects: Childhood Exposure (II)



Notes: This figure reports estimates of β_j and 95% confidence intervals in equation (8). Each β_j can be interpreted as the differential likelihood of an infrequent name being given to a child whose father's family moved to the frontier at age j compared to a child born to that father's younger brother who was 1 when the family moved to the frontier. The sample consists of 81,823 children age 0–20 in the 1880 Census with fathers hailing from 17,778 families observed in the 1850 Census and where at least two brothers (one brother) were born before the family moved to the frontier. We link the fathers from 1850 to 1880 using a procedure detailed in Appendix L. There are 16,776 children with fathers that moved at age 1, 8,463 at age 2, ..., 3,164 at age 10, ..., and 487 at age 17. These estimates control for 1850 family fixed effects, father birth order, child gender, child birth order, and an indicator for duplicate matches in the linking process.

Tables

Table 1: Demographics and Individualism on the Frontier

Dependent Variable:	Male/Female Ratio (1)	Prime-Age Adult Share (2)	Foreign-Born Share (3)	Illiterate Share (4)	Infrequent Child Names (5)	Non-Patronymic -Matronymic (6)
	Panel (a): Baseline Frontier Definition: Low Density and Proximity to Frontier Line					
frontier county	0.190***	0.026***	0.063***	-0.007	0.022***	0.009***
	(0.021)	(0.004)	(0.008)	(0.011)	(0.007)	(0.002)
Mean Dep. Var. in Non-Frontier Counties	1.09	0.46	0.07	0.18	0.63	0.95
Number of County-Years	11,594	5,508	11,062	2,779	6,907	6,881
R^2	0.09	0.20	0.33	0.17	0.32	0.03
	Panel (b): Distinguishing Low Density and Proximity to Frontier Line					
near frontier line	0.103***	0.022***	0.058***	-0.058***	0.022***	0.002
	(0.012)	(0.003)	(0.009)	(0.013)	(0.007)	(0.002)
low population density	0.127***	0.005	0.033***	0.055***	0.004	0.008***
	(0.014)	(0.003)	(0.008)	(0.011)	(0.006)	(0.002)
Mean Dep. Var. in Non-Frontier Counties	1.09	0.46	0.07	0.18	0.63	0.95
Number of County-Years	11,594	5,508	11,062	2,779	6,907	6,881
R^2	0.10	0.19	0.35	0.19	0.32	0.04

Notes: This table reports OLS estimates of equations (1) and (2) in Panels A and B, respectively. The dependent variables and sample are the same as in Figures 4 a–f. The sample size varies across columns depending on availability in the given Census round. All variables, except foreign-born share, are defined over the white population. Infrequent names capture the share of boys and girls, respectively, with names outside of the top 10 most popular names in their Census division, and non-patronymic/-matronymic is the share of boys/girls with names that are distinct from their fathers/mothers. In both cases, the means are restricted to white children aged 0–10 with native-born parents. *Low population density* equals one if the county has density less than 6 people per square mile, and *near frontier line* equals one if the county is within 100 km of the frontier line in the given year. The sample excludes counties to the east of the 1790 frontier line and west of the main 1890 frontier line in keeping with our baseline long-run sample restrictions. All regressions include year and Census division FE. Standard errors are clustered using the grid cell approach of Bester et al. (2011) as described in Section 4.1. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 2: Total Frontier Experience and 20th Century Individualism

	(1)	(2)	(3)	(4)
	Dependent Variable:			
	Panel (a): Infrequent Names (standardized share)			
total frontier experience	0.140***	0.144***	0.098***	0.088***
	(0.024)	(0.021)	(0.025)	(0.019)
Oster δ for $\beta = 0$		-13.06	2.73	2.07
Number of Counties	2,036	2,036	2,036	2,036
R^2	0.55	0.61	0.85	0.87
	Panel (b): Non-Patronym/Matronym Names (standardized share)			
total frontier experience	0.214***	0.228***	0.184***	0.125***
	(0.031)	(0.030)	(0.029)	(0.029)
Oster δ for $\beta = 0$		-6.57	12.42	2.83
Number of Counties	2,036	2,036	2,036	2,036
R^2	0.32	0.36	0.80	0.80
State Fixed Effects	✓	✓	✓	✓
Geographic/Agroclimatic Controls		✓	✓	✓
Within-State Nearest Neighbor Matching:				
on Population Density (1,018 FE)			✓	
on Foreign-Born Share (1,018 FE)				✓

Notes: This table reports estimates of equation (4) for our two proxies for individualism in the 20th century: panel (a) infrequent names, and panel (b) non-patronymic/-matronymic names as defined in Table 1. Both are defined over white children age 0–10 with native-born parents in the 1940 Census. In the average county, 76.6 percent of children have infrequent names and 92.4 percent have non-patronymic/-matronymic names with standard deviations of 5.5 and 3.3 percentage points, respectively. Total frontier experience is expressed in decades. The dependent variables are standardized so that the coefficient indicates the standard deviation effect of each additional decade of frontier exposure historically. This baseline sample is based only on counties inside the 1790–1890 east-to-west frontier. Alternative definitions of the dependent variables names are considered in Appendix Table B.3. Column 1 simply includes state fixed effects, and column 2 adds the following controls: county area; county centroid latitude and longitude; distance to oceans, lakes and rivers from county centroid; mean county temperature and rainfall; elevation; and average potential agricultural yield. Column 3 includes fixed effects within-state for pairs of counties that have the most similar population density in 1940. Column 4 includes fixed effects for within-state pairs of counties that have the most similar foreign-born population shares in 1940. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. Columns 3 and 4 additionally cluster (two-way) on the county-pair. Alternative approaches to inference can be found in Appendix Table C.1. The Oster (2019) tests in columns 2–4 are each with reference to the baseline specification in column 1 with only state fixed effects.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 3: Total Frontier Experience and Opposition to Government Intervention and Redistribution

Dependent Variable:	Prefers Cut Public Spending on Poor	Prefers Cut Public Spending on Welfare	Believes Gov't Should Redistribute	Prefers Reduce Debt by Spending Cuts	Index of Preferences for Spending Cuts	County Property Tax Rate, 2010	Republican Presidential Vote Shr., 2000–16
Scale:	binary	binary	standardized	binary	standardized	[0, 100]	[0, 100]
Data Source:	ANES	CCES	GSS	CCES	GSS	ACS	Leip
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
total frontier experience	0.010***	0.007**	-0.022*	0.014***	0.028**	-0.034***	2.055***
	(0.004)	(0.003)	(0.012)	(0.002)	(0.011)	(0.007)	(0.349)
Oster δ for $\beta = 0$	16.01	3.10	97.95	6.05	1.79	-27.45	13.01
Mean of Dependent Variable	0.09	0.40	0.00	0.41	-0.00	1.02	60.0
Number of Individuals	2,322	53,472	9,085	111,853	5,739	–	–
Number of Counties	95	1,863	255	1,963	253	2,029	2,036
R^2	0.04	0.04	0.06	0.04	0.07	0.82	0.33
Survey Wave Fixed Effects	✓	✓	✓	✓	✓	–	–
Individual Demographic Controls	✓	✓	✓	✓	✓	–	–
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates of equation (4) for several measures capturing preferences for redistribution and state spending as well as actual property tax rates and the Republican vote share. Total frontier experience is expressed in decades. Full details on the outcomes can be found in Appendix L. We use all available survey rounds with the given outcome, and in all cases, we restrict to those counties in our baseline sample as described in the notes to Table 2. All columns are based on the specification in column 2 of Table 2 with additional individual-level controls for age, age squared, gender, and race in columns 1–5. The ANES measure in column 1 equals one if the respondent prefers that federal government spending on poor people be cut. The CCES measure in column 2 equals one if the respondent would prefer to cut public spending on welfare programs. The GSS measure in column 3 is a standardized measure of intensity of support on a 7 point scale of the statement that the government should reduce income differences in society through redistribution. The CCES question in column 4 equals one if the household would prefer that the state budget be balanced through spending cuts rather than tax increases. The GSS measure in column 5 is a standardized first principal component analysis (PCA) index based on a series of questions about whether the government spends too much on different public goods and transfer programs. The measure of county-level property tax rates in column 6 is estimated from American Community Survey data from 2010. Column 7 captures the mean county-level Republican vote share in the last five presidential elections with data from the Leip Atlas. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. The Oster (2019) tests are with reference to a baseline specification that only includes state fixed effects.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 4: Robustness I: Population Density and Urbanization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Population Density, 1940/2010		✓					
Population Density Decile Within-State, 1940/2010			✓				
Population Density Neighbor Matching Within-State, 1940/2010				✓			
Sample Restriction	None	None	None	None	$> 90th$	$\leq 90th$	None
					percentile urban pop. share, 1940/2010		
	Panel (a): Infrequent Children's Name Share in 1940 (standardized)						
total frontier experience	0.144***	0.133***	0.090***	0.098***	0.144***	0.103***	0.084***
	(0.021)	(0.021)	(0.020)	(0.023)	(0.034)	(0.022)	(0.025)
total low density experience							0.093***
							(0.019)
Oster δ for $\beta = 0$	-13.06	6.78	1.15	2.73	-48.36	0.80	0.54
Number of Counties	2,036	2,036	2,021	2,036	242	1,794	2,036
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R^2	0.61	0.62	0.68	0.85	0.84	0.60	0.62
	Panel (b): Non-Patronymic/-Matronymic Share in 1940 (standardized)						
total frontier experience	0.228***	0.208***	0.157***	0.184***	0.195***	0.198***	0.130***
	(0.030)	(0.029)	(0.027)	(0.027)	(0.063)	(0.031)	(0.031)
total low density experience							0.153***
							(0.021)
Oster δ for $\beta = 0$	-6.57	24.22	3.74	12.42	14.47	5.15	0.97
Number of Counties	2,036	2,036	2,021	2,036	242	1,794	2,036
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R^2	0.36	0.40	0.53	0.80	0.55	0.36	0.39
	Panel (c): Mean Government Preferences Outcomes (CCES), 2006–16						
total frontier experience	0.014***	0.008***	0.009***	0.005	0.013***	0.004	0.012***
	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
total low density experience							0.005**
							(0.002)
Number of Individuals	112,759	112,759	111,704	112,759	68,436	44,323	112,759
Mean of Dependent Variable	0.41	0.41	0.41	0.41	0.38	0.46	0.41
R^2	0.05	0.05	0.05	0.06	0.04	0.04	0.05
	Panel (d): County Property Tax Rate in 2010						
total frontier experience	-0.034***	-0.020***	-0.010*	-0.001	-0.022*	-0.014***	-0.028***
	(0.007)	(0.006)	(0.005)	(0.005)	(0.012)	(0.005)	(0.008)
total low density experience							-0.008
							(0.005)
Oster δ for $\beta = 0$	-207.01	0.25	0.12	0.02	0.67	0.16	0.42
Number of Counties	2,029	2,029	2,014	2,020	223	1,806	2,029
Mean of Dependent Variable	1.02	1.02	1.02	1.02	1.34	0.98	1.02
R^2	0.82	0.85	0.87	0.95	0.90	0.86	0.82
	Panel (e): Republican Vote Share, Average 2000–16						
total frontier experience	2.055***	1.532***	1.535***	1.655***	1.280	1.489***	1.255***
	(0.349)	(0.346)	(0.357)	(0.356)	(0.886)	(0.347)	(0.404)
total low density experience							1.256***
							(0.290)
Oster δ for $\beta = 0$	-8.68	3.91	3.44	12.54	-0.13	3.00	1.24
Number of Counties	2,036	2,036	2,021	2,034	223	1,813	2,036
Mean of Dependent Variable	60.04	60.04	60.11	60.04	49.29	61.36	60.04
R^2	0.33	0.40	0.38	0.73	0.27	0.38	0.35
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓	✓	✓

Notes: This table disentangles the effects of TFE from the effects of historical and contemporary population density. Those in panel (a), (b), (d) and (e) are from prior tables, with the baseline estimates reproduced in column 1. The outcome in panel (c) is the mean of the six binary indicators from the CCES survey from Tables 3 and E.2. Column 2 and control for contemporaneous population density (i.e., 1940 in panels (a) and (b), 2006 in panel (c), 2000 in panel (d), and 2010 in panel (e)). Column 3 includes indicators for the decile of within-state population density. Column 4 implements the nearest-neighbor matching specification from column 3 of Table 2. Columns 5 and 6 split the sample into counties above and below the 90th percentile of contemporaneous urban population shares. Column 7 controls for the total number of years that the country had population density less than 6 people/mi^2 from 1790–1890. This is one of the aspects of total frontier experience, the other being the total number of years that the county was within 100 km of the frontier line during that period. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. Column 4 additionally clusters (two-way) on the county-pair. The Oster (2019) tests are with reference to a baseline specification that only includes state fixed effects.

Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table 5: Robustness II: Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Baseline controlling for. . .														
ruggedness		✓												✓
rainfall risk			✓											✓
distance to nearest portage site				✓										✓
distance to nearest mine					✓									✓
distance to nearest Indian battle						✓								✓
slave population share, 1860							✓							✓
sex ratio, 1890								✓						✓
immigrant share, 1890									✓					✓
Scottish and Irish immigrant share, 1890										✓				✓
birthplace diversity, 1890											✓			✓
years connected to railroad by 1890												✓		✓
manufacturing employment share, 1890													✓	✓
	Panel (a): Infrequent Name Share in 1940 (standardized)													
total frontier experience	0.144***	0.146***	0.141***	0.146***	0.144***	0.147***	0.108***	0.144***	0.139***	0.111***	0.132***	0.097***	0.153***	0.092***
	(0.021)	(0.020)	(0.021)	(0.020)	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)	(0.019)	(0.020)	(0.022)	(0.020)	(0.019)
Oster δ for $\beta = 0$	-13.06	-10.81	-53.62	-8.12	-13.69	-6.57	1.33	-12.62	28.05	2.20	5.27	1.25	-6.61	1.75
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R^2	0.61	0.62	0.61	0.61	0.61	0.61	0.62	0.61	0.61	0.67	0.61	0.66	0.66	0.76
	Panel (b): Non-Patronymic/-Matronymic Name Share in 1940 (standardized)													
total frontier experience	0.228***	0.229***	0.229***	0.228***	0.228***	0.235***	0.170***	0.228***	0.195***	0.183***	0.187***	0.161***	0.241***	0.112***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)	(0.031)	(0.030)	(0.028)	(0.026)	(0.027)	(0.026)	(0.029)	(0.022)
Oster δ for $\beta = 0$	-6.57	-6.79	-6.22	-6.69	-6.57	-4.65	2.85	-7.44	8.16	6.52	6.80	3.31	-9.53	1.85
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R^2	0.36	0.36	0.36	0.36	0.36	0.36	0.40	0.37	0.42	0.47	0.45	0.47	0.46	0.65
	Panel (c): Mean Government Preferences Outcomes (CCES), 2006–16													
total frontier experience	0.014***	0.013***	0.014***	0.014***	0.014***	0.013***	0.014***	0.014***	0.011***	0.011***	0.010***	0.012***	0.011***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Oster δ for $\beta = 0$	6.45	6.20	7.07	6.82	6.41	5.50	7.30	6.59	3.67	3.32	3.28	4.22	3.70	2.12
Number of Counties	112,759	112,759	112,759	112,759	112,759	112,759	111,808	112,759	112,759	112,759	112,759	112,759	112,759	111,808
Mean of Dependent Variable	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41
R^2	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
	Panel (d): County Property Tax Rate in 2010													
total frontier experience	-0.034***	-0.034***	-0.034***	-0.033***	-0.034***	-0.033***	-0.033***	-0.034***	-0.028***	-0.026***	-0.027***	-0.023***	-0.036***	-0.023***
	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Oster δ for $\beta = 0$	-207.01	-8.82	-3.58	2.35	-365.67	7.11	6.74	-643.51	0.54	0.48	0.48	0.28	-2.17	0.42
Number of Counties	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029
Mean of Dependent Variable	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02
R^2	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.83	0.83	0.83	0.83	0.83	0.85
	Panel (e): Republican Vote Share, Average 2000–16													
total frontier experience	2.055***	2.050***	2.115***	2.095***	2.055***	2.172***	1.399***	2.060***	1.715***	1.717***	1.689***	1.640***	2.137***	0.931***
	(0.349)	(0.349)	(0.338)	(0.344)	(0.350)	(0.351)	(0.361)	(0.347)	(0.328)	(0.340)	(0.327)	(0.361)	(0.350)	(0.316)
Oster δ for $\beta = 0$	-8.68	-9.07	-6.50	-6.83	-8.68	-5.15	2.34	-9.05	7.29	7.35	6.69	4.51	-7.88	1.35
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
Mean of Dependent Variable	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04
R^2	0.33	0.33	0.34	0.33	0.33	0.34	0.38	0.34	0.38	0.38	0.39	0.36	0.36	0.49
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table augments the baseline specification, reproduced in column 1, with additional controls. The variables are defined in Section 4.4 and at the end of Appendix L, but we note here that the measure in column 5 is based on the known mining sites pre-1890. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. The Oster (2019) tests are with reference to a baseline specification with no controls. Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table 6: Robustness III: Adding West Coast, Extended Time Frame, Regional Heterogeneity

Frontier Time Frame:	Baseline (1790–1890)				Extended (1790–1950)			
Regional Sample Restriction:	Baseline + West Coast	Only Midwest	Only South	Only West	Extended Sample	Only Midwest	Only South	Only West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel (a): Infrequent Name Share in 1940 (standardized)							
total frontier experience	0.140***	0.252***	0.164***	0.109	0.087***	0.123***	0.116***	0.064***
	(0.020)	(0.043)	(0.030)	(0.071)	(0.013)	(0.033)	(0.026)	(0.019)
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Panel (b): Non-Patronymic/-Matronymic in 1940 (standardized)							
total frontier experience	0.219***	0.303***	0.210***	0.148*	0.125***	0.168***	0.153***	0.090***
	(0.028)	(0.054)	(0.034)	(0.075)	(0.018)	(0.048)	(0.029)	(0.020)
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Panel (c): Mean Government Preferences Outcomes (CCES), 2006–16							
total frontier experience	0.015***	0.019***	0.010***	0.026**	0.014***	0.017***	0.011***	0.013***
	(0.003)	(0.004)	(0.003)	(0.012)	(0.002)	(0.004)	(0.003)	(0.004)
Number of Individuals	140,715	49,218	52,285	32,319	158,403	49,479	55,462	46,569
Mean of Dependent Variable	0.40	0.40	0.43	0.36	0.41	0.40	0.43	0.38
	Panel (d): County Property Tax Rate in 2010							
total frontier experience	-0.031***	-0.051***	-0.027***	-0.006	-0.025***	-0.042***	-0.031***	-0.009**
	(0.006)	(0.014)	(0.007)	(0.013)	(0.004)	(0.012)	(0.006)	(0.004)
Mean of Dependent Variable	1.01	1.24	0.75	0.76	0.98	1.23	0.78	0.72
	Panel (e): Average Republican Vote Share over 2000-2016							
total frontier experience	2.070***	1.882***	2.458***	1.459	1.302***	1.515***	1.429***	1.197***
	(0.332)	(0.414)	(0.396)	(0.890)	(0.256)	(0.350)	(0.422)	(0.274)
Mean of Dependent Variable	59.43	59.15	61.78	48.81	60.49	59.43	63.18	56.10
Number of Counties	2,141	987	936	152	2,500	1,038	1,074	322
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Focusing on five key outcomes across panels (a)–(e), this table extends our baseline sample of counties and examines region-by-region sample splits. Column 1 adds 105 counties along the secondary West Coast frontier (see Figure 3). Column 2 restricts to counties in the Midwest Census region, column 3 restricts to the South region, and column 4 restricts to the West, which includes the 105 counties added in column 1 plus 47 others in states in the West region but falling inside the 1890 main east-to-west frontier line. Column 5 expands the column 1 sample to include counties beyond the (main and secondary) 1890 frontier lines but inside the eventual frontier line realized by 1950. Columns 6–8 then proceed with the same region-by-region sample splits. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 7: Racial Differences in the Long-Run Effects of Frontier Experience

Dependent Variable:	Prefers Cut Public Spending on Welfare (1)	Prefers Reduce Debt by Spending Cuts (2)	Opposes Affordable Care Act (3)	Opposes Increasing Minimum Wage (4)	Opposes Banning Assault Rifles (5)	Opposes Regulation of CO_2 Emissions (6)
total frontier experience × white	0.009***	0.016***	0.027***	0.025***	0.018***	0.018***
	(0.003)	(0.002)	(0.004)	(0.008)	(0.004)	(0.004)
total frontier experience × black	-0.008	-0.002	-0.000	0.005	-0.002	-0.008
	(0.006)	(0.005)	(0.007)	(0.014)	(0.008)	(0.007)
total frontier experience × other	0.010	0.015**	0.014	0.009	0.019**	0.029***
	(0.007)	(0.007)	(0.009)	(0.019)	(0.008)	(0.008)
white	0.044***	0.065***	0.047***	-0.064**	0.006	0.046***
	(0.012)	(0.011)	(0.015)	(0.032)	(0.012)	(0.015)
black	-0.177***	-0.065***	-0.215***	-0.285***	-0.148***	-0.067***
	(0.014)	(0.011)	(0.021)	(0.038)	(0.021)	(0.025)
Number of Individuals	53,472	111,853	29,446	5,134	29,404	29,215
Number of Counties	1,863	1,963	1,728	1,066	1,723	1,718
TFE(black)=TFE(white), p-value	0.010	0.000	0.000	0.125	0.022	0.000
Mean of Dependent Variable, Whites	0.43	0.44	0.58	0.32	0.39	0.35
Share White Respondents	0.79	0.77	0.76	0.86	0.76	0.76
Share Black Respondents	0.11	0.13	0.11	0.09	0.11	0.11
Share Other Respondents	0.10	0.11	0.13	0.05	0.13	0.13
State Fixed Effects	✓	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓	✓

Notes: This table allows the effects of TFE to vary by (self-identified) race of respondents for the six CCES outcomes used in Tables 3 and E.2. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 8: Instrumental Variables (IV) Estimates for Summary Outcomes

Dependent Variable:	Infreq. Name standardized 1940 (1)	Non-Patr./-Matr. standardized 1940 (2)	Mean Gov. Prefs. CCES 2006–16 (3)	County Property Tax Rate 2010 (4)	Republican Vote Share Avg. 2000–16 (5)
	Panel (a): IV = Log Average Actual National Migration Inflows Over 30 Years				
total frontier experience	0.196***	0.202***	0.011***	-0.045***	3.407***
	(0.038)	(0.058)	(0.004)	(0.014)	(0.585)
Number of Observations	2,036	2,036	112,759	2,029	2,036
Mean of Dependent Variable	0.00	-0.00	0.41	1.02	60.04
First Stage F Statistic	193.64	193.64	40.30	194.13	193.64
	Panel (b): IV = Log Average Predicted National Migration Inflows Over 30 Years				
total frontier experience	0.226***	0.215***	0.008*	-0.049***	3.177***
	(0.044)	(0.058)	(0.004)	(0.014)	(0.624)
Number of Observations	2,036	2,036	112,759	2,029	2,036
Mean of Dependent Variable	0.00	-0.00	0.41	1.02	60.04
First Stage F Statistic	195.84	195.84	44.53	196.31	195.84
State Fixed Effects	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓

Notes: This table reports instrumental variables estimates of equation (4) based on the instruments described in Section 4.5. We again report results for the four summary outcomes examined in prior tables, and total frontier experience is measured in decades. Panel (a) reports the IV estimates for the baseline sample and specification using the log of the average national annual actual migration inflows over the 30 years since the frontier is within 110km from the county centroid. Panel (b) reports the estimates using the IV constructed based on annual migration inflows to the US predicted by weather shocks in Europe. The details on the construction of both instrumental variables are presented in the Appendix Section F. The first-stage F statistics are cluster-robust, and standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 9: Selective Migration and Individualism on the Frontier

	Dep. Var.: Child Has an Infrequent Name (Children Named Prior to Moving)			
	(1)	(2)	(3)	(4)
omitted reference group:	stayers in settled counties		stayers in frontier counties	
household migrated from settled to frontier	0.032***	0.028***		
	(0.006)	(0.006)		
household migrated from frontier to settled			-0.049***	-0.029***
			(0.011)	(0.006)
Observations	8,734,740	8,734,740	370,999	370,999
Mean of Dep. Var., Stayers	0.65	0.65	0.64	0.64
R^2	0.02	0.03	0.04	0.07
Birth State Fixed Effects		✓		✓

Notes: This table estimates equation (5) in columns 1–2 and an analogous specification for movers from frontier areas to settled areas in columns 3–4. All columns include fixed effects for birth year×gender as well as birth order. The sample pools across Censuses from 1850–1880 and restricts to white children age 0–10 with native-born parents. The dependent variable is an indicator for whether the child has a non-top-10 name in the Census division and decade in which s/he was born. In columns 1–2, the sample includes all children living in non-frontier counties as well as children who were born in non-frontier counties and are currently living in frontier counties as a result of a family move. In columns 3–4, the sample includes all children currently living in frontier counties as well as all children who were born in frontier areas and are currently living in non-frontier counties as a result of a family move away from the frontier. The non-movers (i.e., stayers) are the omitted group to which the estimate differential refers, with the dependent variable means at the bottom of the table computed over these stayers. These mover households and children are identified using variation across reported child birth states and current county of residence (see Section 5.1 for details). Standard errors are clustered by county.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 10: Identifying Exposure Effects: Adulthood Exposure (I)

	Dep. Var.: Child Has an Infrequent Name					
	(1)	(2)	(3)	(4)	(5)	(6)
year of birth relative to move, pre-move	0.002	0.001	0.003	0.001	-0.001	0.001
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)
year of birth relative to move, post-move	0.007***	0.005***	0.007**	0.005***	0.004**	0.005
	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)
pre-move = post-move, p-value	[0.005]***	[0.018]**	[0.023]**	[0.009]***	[0.007]***	[0.032]**
Observations	57,097	57,097	57,097	57,097	57,097	57,097
Number of Families	16,901	16,901	16,901	16,901	16,901	16,901
Mean of Dependent Variable	0.65	0.65	0.65	0.65	0.65	0.65
R^2	0.36	0.36	0.36	0.36	0.36	0.36
Household Fixed Effect	✓	✓	✓	✓	✓	✓
Birth Year Fixed Effect	decade	5-yearly	3-yearly		3-yearly	
Child Birth Order				✓	✓	
Pre-Move Birth State Yearly Name Trend						✓

Notes: This table reports estimates of equation (7), which estimates a continuous version of the event study specifications in Figure 5. That is, the *year of birth relative to move, pre-move* measures the number of years until the household moves to the frontier, and *year of birth relative to move, post-move* measures years since arrival to the frontier. We also report the p-value for equality across the two. See the notes to Figure 5 for further details on the sample and specification. Standard errors are clustered by county.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 11: Identifying Exposure Effects: Childhood Exposure (II)

	Dep. Var.: Child Has an Infrequent Name				
	(1)	(2)	(3)	(4)	(5)
age-at-move to frontier	-0.008**	-0.008**	-0.007**	-0.006*	-0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	81,823	81,823	81,823	81,823	81,823
Number of Families	17,778	17,778	17,778	17,778	17,778
Mean of Dependent Variable	0.69	0.69	0.69	0.69	0.69
R^2	0.26	0.27	0.26	0.27	0.27
Extended Family (1850 Household) FE	✓	✓	✓	✓	✓
State of Residence FE in 1880	–	✓	–	–	–
Child Birth Cohort FE	–	–	decade	5-yearly	3-yearly

Notes: This table reports the continuous analogue to the age-at-move-specific estimates in Figure 6. All estimates control for 1850 family fixed effects, father birth order, child gender, child birth order, and an indicator for duplicate matches in the linking process. Column 1 is the specification used in Figures 6. Column 2 here additionally includes 1880 state fixed effects to allow for the possibility that brothers from 1850 may live in different locations today. Columns 3–5 control increasingly flexibly for child birth cohort. See the notes to that figure for details on the the sample and specifications. Standard errors are two-way clustered by 1850 family and 1880 county.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 12: Returns to Individualism on the Frontier, 1850–1880

	Dep. Var.: Father's Occupation Score			
	(1)	(2)	(3)	(4)
infrequent children's names, mean	0.834***		0.820***	1.453***
	(0.033)		(0.032)	(0.053)
frontier x infrequent children's names, mean	0.306***		0.305***	0.563***
	(0.065)		(0.064)	(0.156)
father has infrequent name		0.210***	0.174***	0.466***
		(0.016)	(0.015)	(0.027)
frontier x father has infrequent name		0.014	-0.003	-0.085
		(0.041)	(0.040)	(0.106)
Observations	5,667,473	5,667,473	5,667,473	1,991,540
Mean Dep. Var.	18.1	18.1	18.1	26.1
R^2	0.07	0.07	0.07	0.10
County×Year Fixed Effects	✓	✓	✓	✓
Excluding Farmers				✓

Notes: This table reports estimates of equation (9). The sample includes all white native-born men with a non-missing occupational score and at least one child age 0–10 in the 1850–1880 Censuses. The dependent variable, occupational score, range from 0 to 100 and are provided by the Sobek et al. (2017) for 1850 and 1880. We construct the scores directly for 1860 and 1870 using a crosswalk of occupational descriptions to codes for available years. Column 4 omits all fathers with an occupational description that includes the string "farm". The infrequent children's names are computed over all of the children age 0–10. The infrequent father's name is defined with respect to all other native-born white men born in the same decade as the father. The frontier indicator equals one if the county is within 100 km of the frontier. Standard errors are clustered by county.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Appendix

The **Online Appendix** (for publication) includes Sections A–G. The **Supplementary Material Appendix** (not for publication) includes Sections H–L.

Contents

Main Appendix

A Mapping the Frontier

This section provides a step-by-step description of how we construct the frontier lines for each year between 1790–1890.

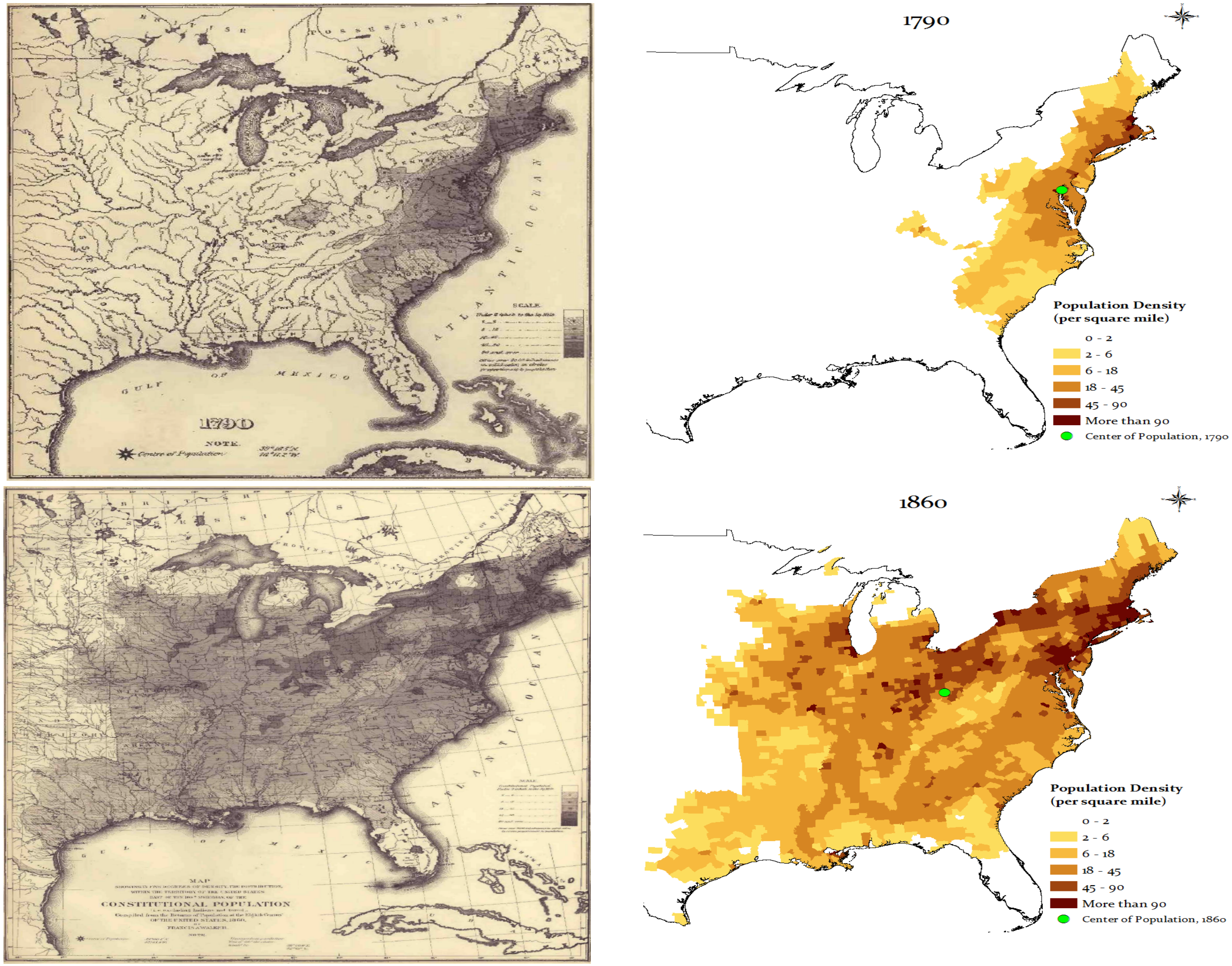
1. Calculate county level population density per square mile for each year in 1790–1890 using the 2010 county boundaries. First, we harmonize the county-level population data from each year to the 2010 county boundaries using the procedure discussed in Section 2. For intercensal years, we interpolate county-level population by assuming a constant annual population growth rate that matches the decadal growth rate (replacing initial zeros with 0.01 to avoid infinite growth rates). Then, using the 2010 county boundaries shapefile, we calculate the county-level population density as the ratio of population over county area in square miles.

2. Draw a contour line at population density equal to 2 people per square mile for each year. We use ArcGIS and the 2010 county boundaries. First, for each year, we convert the polygon containing the county level population density data into a raster file using ***PolygonToRaster*** tool and set population density for the given year as the ***"value field"*** for the conversion. Then, using the ***ContourList*** tool, select the raster file created in the preceding step as an input and set the ***"contour value"*** to "2" to create contour lines at population density equal to 2. The resulting lines delineate the counties that have a population density below 2 people per square mile from those counties that have a population density above 2.

3. Clean the contour lines to retain only the significant frontier lines. With the purpose of capturing historical notions of the frontier as "margins of civilization," we discard all contour line segments less than 500 km and also discard isolated pockets of relatively sparse populations within the main area of settled territory. These isolated pockets are the "inner islands" formed by counties with population density below 2 people per square mile surrounded by counties with population density above 2 people per square mile. A second set of frontier lines emerge in the West Coast in mid-19th century. This process of settlement was marked by the Gold Rush and different historical forces than the main east-to-west expansion, so for our baseline analysis we focus on the territory spanned by east-to-west expansion. We do this by keeping only those frontier lines that are east of the westernmost east-to-west frontier line in 1890. In the robustness analysis, we add the West Coast to our baseline sample.

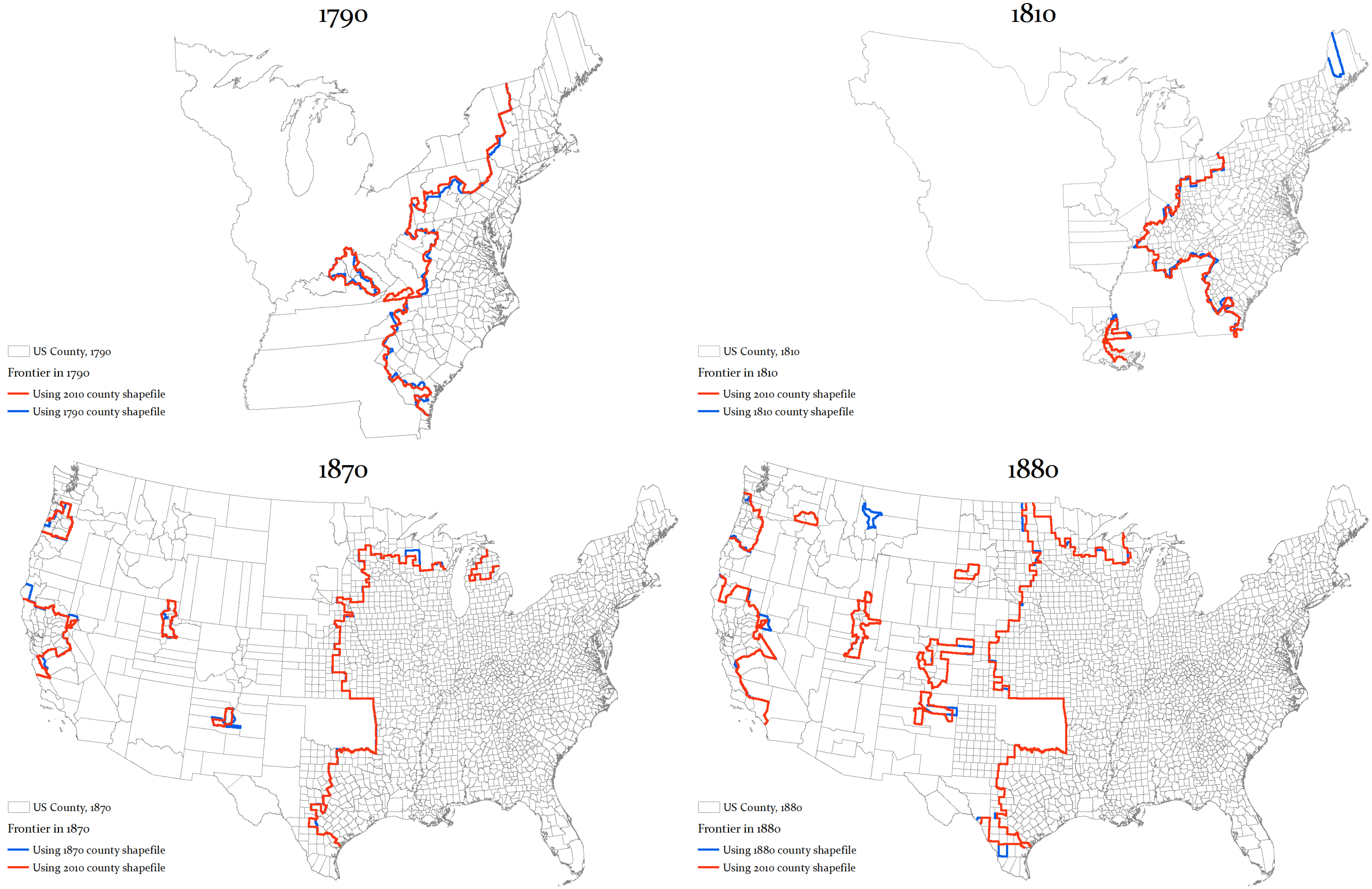
We select line segments based on length and location (e.g., X centroid of the line midpoint) in ArcGIS using the ***SelectLayerByAttribute*** tool, and apply ***CopyFeatures*** to keep only the selected lines. In the detailed robustness checks in Section 4.4, we also consider various alternatives to the frontier definition such as changing the line cutoffs, restricting to single westernmost frontier line, including the "inner island" lines, and considering the frontier lines that emerge from the West Coast.

Figure A.1: Population Density Maps from the 1890 Census Report and Our Maps (1790 and 1860)



Notes: This figure compares the maps of population density in 1790 and 1860 from the noteworthy *Progress of the Nation* Census report (on the left) with the maps we constructed for 2010 county boundaries using the harmonization procedure described in Section 2 (on the right).

Figure A.2: Frontier Lines Using Contemporaneous vs 2010 County Boundaries for selected years



Notes: Based on county level Population Census data from 1790-1880 and NHGIS county shapefiles. The figures provide the county boundaries for selected years and the frontier lines for the corresponding years drawn using the contemporaneous county boundaries as well as the 2010 county boundary. The frontier lines delineate the counties that had population density of two persons or higher. The frontier lines in blue are drawn using the contemporaneous county boundaries whereas the frontier lines in red are drawn using the 2010 county boundaries (after the data harmonization discussed in Section 2.1).

B Further Robustness Checks on Individualism

B.1 Robustness of the Historical Frontier Differential in Individualistic Names

Table B.1: Robustness Check on Historical Names Measures in Table 1

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw Reported Name		Metaphone-Adjusted Name		Non-Patronymic/-Matronymic	
	Baseline OLS	Foreign-Born NN-Matching	Baseline OLS	Foreign-Born NN-Matching	Baseline OLS	Foreign-Born NN-Matching
frontier county	0.022***	0.014**	0.018***	0.011**	0.009***	0.012***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.002)	(0.002)
Number of County-Years	6,907	6,905	6,907	6,905	6,881	6,879
Mean Dep. Var. in Non-Frontier Counties	0.631	0.631	0.602	0.602	0.946	0.946

Notes: Columns 1 and 5 report the baseline estimates from Table 1. Column 3 reports estimates for an alternative measure of infrequent names based on first adjusting the name reported in the Census to account for its phonetic sound with a metaphone algorithm. The remaining columns 2, 4, and 6 report, for these same outcomes, nearest-neighbor matching estimates of the frontier differential in individualistic names. We match on the county with the most similar foreign-born population share in the given Census year. These estimates are based on the single nearest-neighbor. Standard errors in odd columns are clustered using the grid cell approach of Bester et al. (2011) as described in Section 4.1 and in even columns are bias-adjusted and robust following best practice in the matching literature.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

B.2 Alternative Survey-Based Proxy for Contemporary Individualism

Beyond infrequent names, we draw upon a well-suited measure from the ANES data to provide further evidence of the link between TFE and high levels of individualism. Specifically, we use the 1990 ANES round in which respondents were asked whether (1) "it is more important to be a cooperative person who works well with others", or (2) "it is more important to be a self-reliant person able to take care of oneself." While this question was designed explicitly for studies of American individualism (see Markus, 2001), unfortunately, it was only asked in a single round.

Table B.2 below provides evidence that self-reliant preferences are stronger today in counties with longer exposure to the frontier historically. Around 55 percent of individuals respond in support of the cooperative answer. However, across different specifications, each decade of additional TFE is associated with around 2–6 percentage points lower support for cooperation over self-reliance. While the results with the full set of controls are noisy, we nevertheless view these findings as at least suggestive of long-standing claims about the rugged individualism pervasive on the frontier. In linking to results elsewhere in the paper, it is worth noting that individuals that identify as Republican in the ANES data are around 15–20 percent more likely to believe that it is better to be a self-reliant than a cooperative person.

Given the small number of counties, we retain this outcome in the appendix rather than in the main tables. Nevertheless, it is reassuring that the results align with our findings for other outcomes with more systematic coverage.

Table B.2: Total Frontier Experience and Contemporary Cooperation vs. Self-Reliance

	(1)	(2)	(3)	(4)
total frontier experience	-0.019*	-0.025**	-0.041***	-0.026**
	(0.009)	(0.009)	(0.014)	(0.012)
Oster δ for $\beta = 0$	-2.77	-2.61	-15.37	-249.36
Number of Individuals	567	567	567	567
Number of Counties	48	48	48	48
Mean of Dependent Variable	0.549	0.549	0.549	0.549
R^2	0.01	0.02	0.02	0.03
Individual Demographic Controls	✓	✓	✓	✓
Division Fixed Effects		✓	✓	✓
State Fixed Effects			✓	
Geographic/Agroclimatic Controls				✓

Notes: This table reports estimates for a dependent variable based on a proxy for individualism in the 1990 round of ANES, covering 567 individuals in 48 counties across 17 states in our sample. The measure asks individuals whether (1) "it is more important to be a cooperative person who works well with others", or (2) "it is more important to be a self-reliant person able to take care of oneself." The dependent variable equals one if they answer (1). We report the same set of specifications in columns 1–4 as in Table 2 to demonstrate the statistically and economically significant effect sizes despite the coverage limitations. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. The Oster (2019) tests are with reference to a baseline specification with no controls. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

B.3 Robustness of the Long-Run Effects of TFE on Individualistic Names

We present below several tables with results discussed in the paper. Appendix Tables B.3 and B.4 demonstrate robustness of the infrequent names measure to alternative specifications of the reference group (national, division, state, county), reported name versus phonetic sound (metaphone), and top 10 versus top 100 in terms of defining infrequency. These tables also report a version of the non-patronymic/-matronymic measure that is based solely on first-born children of each gender.

Appendix Table B.3 reports county-level results. Panel (a) reports estimates of the specification in column 2 of Table 2 for the different outcomes listed at the top of each column. Panels (b) and (c) report analogous estimates for columns 3 and 4 of Table 2 which include fixed effects for county pairs with, respectively, the most similar population density and foreign-born population shares in 1940.

Appendix Table B.4 reports individual-level results rather than county-level mean outcomes. Panels (a)–(c) are as in Table B.3 with added fixed effects for child age, birth order, and gender. Panel (d) augments the panel (a) specification with nearly 400,000 fixed effects for family surnames. Panel (e) adds those surname fixed effects to the panel (b) specification.

Finally, Appendix Table B.5 shows that the baseline results for individualistic names look similar in each decade before 1940 but after the closing of the frontier.

Table B.3: TFE and Alternative Measures of Individualistic Names, County-Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Non-Patronymic/Matronymic		Raw Reported Name					Metaphone-Adjusted Name				
	All	1st Born	Top 10 National	Top 10 Division	Top 10 State	Top 10 County	Top 100 Division	Top 10 National	Top 10 Division	Top 10 State	Top 10 County	Top 100 Division
Panel (a): Baseline Specification												
total frontier experience	0.228***	0.245***	0.145***	0.144***	0.149***	0.163***	0.142***	0.153***	0.144***	0.153***	0.162***	0.134***
	(0.030)	(0.029)	(0.021)	(0.021)	(0.022)	(0.021)	(0.020)	(0.022)	(0.021)	(0.024)	(0.024)	(0.022)
Oster δ for $\beta = 0$	-6.57	-5.23	-7.92	-13.06	180.41	12.35	-27.72	-8.73	-17.14	163.21	20.63	-28.91
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
R^2	0.36	0.30	0.59	0.61	0.57	0.56	0.71	0.56	0.58	0.50	0.50	0.70
Panel (b): Nearest-Neighbor Matching on Population Density in 1940												
total frontier experience	0.184***	0.188***	0.102***	0.098***	0.098***	0.134***	0.099***	0.105***	0.091***	0.097***	0.121***	0.092***
	(0.029)	(0.029)	(0.025)	(0.025)	(0.027)	(0.025)	(0.024)	(0.024)	(0.025)	(0.028)	(0.027)	(0.026)
Oster δ for $\beta = 0$	12.42	9.64	3.14	2.73	2.59	5.16	2.08	3.11	2.36	2.97	4.33	2.26
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
R^2	0.80	0.79	0.84	0.85	0.83	0.83	0.88	0.84	0.85	0.81	0.80	0.87
Panel (c): Nearest-Neighbor Matching on Foreign-Born Share in 1940												
total frontier experience	0.125***	0.142***	0.093***	0.088***	0.099***	0.115***	0.107***	0.101***	0.093***	0.110***	0.125***	0.107***
	(0.029)	(0.030)	(0.020)	(0.019)	(0.020)	(0.021)	(0.017)	(0.022)	(0.020)	(0.023)	(0.023)	(0.017)
Oster δ for $\beta = 0$	2.83	3.54	2.41	2.07	2.86	2.85	3.06	2.75	2.57	4.47	4.99	4.64
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
R^2	0.80	0.79	0.86	0.87	0.86	0.84	0.90	0.85	0.86	0.82	0.82	0.90

Notes: This table reports estimates in panel (a) based on the column 2 specification in Table 2, in panel (b) based on the column 3 specification in Table 2, and in panel (c) based on the column 4 specification in Table 2. The outcomes from Table 2 are in columns 1 and 4. The other columns of Table are based on alternative specifications of the dependent variable as listed at the top of the table. All other specification details are as in Table 2. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table B.4: TFE and Alternative Measures of Individualistic Names, Individual-Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Non-Patronymic/Matronymic		Raw Reported Name					Metaphone-Adjusted Name				
	All	1st Born	Top 10 National	Top 10 Division	Top 10 State	Top 10 County	Top 100 Division	Top 10 National	Top 10 Division	Top 10 State	Top 10 County	Top 100 Division
Panel (a): FE: Age, State, Birth Order, Gender												
total frontier experience	0.011***	0.017***	0.014***	0.013***	0.012***	0.011***	0.018***	0.014***	0.012***	0.010***	0.010***	0.013***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Number of Individuals	10,611,834	3,321,325	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567
Dep. Var. Mean	0.926	0.889	0.735	0.732	0.724	0.711	0.354	0.702	0.693	0.680	0.666	0.229
R^2	0.06	0.09	0.03	0.03	0.03	0.02	0.04	0.02	0.02	0.02	0.01	0.03
Panel (b): FE: Panel (a) + Neighbor Population Density Pair												
total frontier experience	0.004***	0.007***	0.005***	0.004***	0.004***	0.003***	0.007***	0.005***	0.003***	0.003***	0.003***	0.005***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Number of Individuals	10,611,834	3,321,325	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567
Dep. Var. Mean	0.926	0.889	0.735	0.732	0.724	0.711	0.354	0.702	0.693	0.680	0.666	0.229
R^2	0.07	0.10	0.03	0.03	0.03	0.03	0.05	0.02	0.02	0.02	0.02	0.03
Panel (c): FE: Panel (a) + Neighbor Foreign-Born Share Pair												
total frontier experience	0.003***	0.006***	0.005***	0.005***	0.005***	0.004***	0.009***	0.005***	0.005***	0.004***	0.004***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Number of Individuals	10,611,834	3,321,325	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567	11,292,567
Dep. Var. Mean	0.926	0.889	0.735	0.732	0.724	0.711	0.354	0.702	0.693	0.680	0.666	0.229
R^2	0.07	0.10	0.03	0.03	0.03	0.03	0.05	0.02	0.02	0.02	0.02	0.03
Panel (d): FE: Panel (a) + Last Name												
total frontier experience	0.010***	0.016***	0.014***	0.013***	0.012***	0.011***	0.016***	0.014***	0.012***	0.010***	0.009***	0.012***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Number of Individuals	10,408,373	3,065,430	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946
Dep. Var. Mean	0.927	0.891	0.736	0.732	0.725	0.711	0.355	0.703	0.694	0.680	0.666	0.229
R^2	0.10	0.14	0.07	0.07	0.07	0.06	0.08	0.06	0.06	0.06	0.05	0.07
Panel (e): FE: Panel (b) + Last Name												
total frontier experience	0.004***	0.006***	0.005***	0.005***	0.004***	0.004***	0.006***	0.005***	0.003***	0.003***	0.003***	0.004***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Number of Individuals	10,408,373	3,065,430	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946	11,070,946
Dep. Var. Mean	0.927	0.891	0.736	0.732	0.725	0.711	0.355	0.703	0.694	0.680	0.666	0.229
R^2	0.10	0.15	0.07	0.07	0.07	0.07	0.09	0.06	0.06	0.06	0.06	0.07

Notes: This table reports analogous individual-child-level regressions of Table B.3. This allows for the inclusion child age, gender, and birth order fixed effects. Panels (d) and (e) additionally include fixed effects for family surname. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. The sample size in column 1 is smaller than in columns 2–12 because it excludes all boys with no father and girls with no mother present in the household.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table B.5: Persistence of the Effect of TFE on Individualistic Names, 1910–1940

	Sample: White Children Aged 0-10 with Native-Born Parents							
	1910		1920		1930		1940	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel (a): Infrequent Names							
total frontier experience	0.144***	0.194***	0.151***	0.184***	0.142***	0.134***	0.169***	0.182***
	(0.020)	(0.042)	(0.021)	(0.045)	(0.023)	(0.044)	(0.023)	(0.039)
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
R^2	0.66	0.04	0.61	0.05	0.56	0.04	0.49	0.05
First Stage F Statistic		193.6		193.6		193.6		193.6
	Panel (b): Non-Patronymic/-Matronymic							
total frontier experience	0.199***	0.180***	0.196***	0.182***	0.202***	0.190***	0.228***	0.202***
	(0.038)	(0.066)	(0.033)	(0.066)	(0.031)	(0.056)	(0.030)	(0.058)
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
R^2	0.18	0.04	0.22	0.04	0.31	0.05	0.36	0.07
First Stage F Statistic		193.6		193.6		193.6		193.6

Notes: This table reports analogous OLS and IV estimates of Table 2 but for each year since 1910. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

C Robust Inference

Appendix Table C.1 demonstrates the robustness of our baseline approach to inference in county-level regressions. We consider seven alternative approaches to accounting for spatial autocorrelation in the error terms. Column 1 is our baseline approach of clustering at the level of arbitrary 60 square-mile grid cells following the procedure in Bester et al. (2011). Columns 2–7 adopt the Conley (1999) GMM-based spatial heteroskedasticity-autocorrelation (HAC) consistent procedure that allows for arbitrary correlation in unobservables across all counties within 100, 200, 300, 500, and 1000 km respectively. Column 7 uses two-way clustering on both the arbitrary grid cells in column 1 and the year in which each county entered the frontier. This procedure, based on Cameron et al. (2011), allows for possible correlated unobservable across counties subject to shocks at the same time historically that would have led their county to become relevant to frontier settlers. Finally, column 8 clusters at the state level, using a wild bootstrap procedure to account for the relative small number of states (30 in our main sample). Across all columns, we find no meaningful departures from the precisely estimated effects in our baseline approach. We therefore retain this computationally simple baseline throughout the paper.

Table C.1: Alternative Approaches to Inference

	Base	Spatial HAC: [...] km Bandwidth					Two-Way	State
		100	200	300	500	1000	Base + Yr. Entry	Wild Clust.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel (a): Infrequent Name Share in 1940							
total frontier experience	0.144***	0.144***	0.144***	0.144***	0.144***	0.144***	0.144***	0.144***
	(0.021)	(0.027)	(0.027)	(0.027)	(0.033)	(0.042)	(0.018)	[0.004]
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
	Panel (b): Non-Patronymic/-Matronymic Name Share in 1940							
total frontier experience	0.228***	0.228***	0.228***	0.228***	0.228***	0.228	0.228***	0.228***
	(0.030)	(0.041)	(0.045)	(0.048)	(0.053)	(.)	(0.042)	[0.000]
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
	Panel (c): County Property Tax Rate in 2010							
total frontier experience	-0.034***	-0.034***	-0.034***	-0.034***	-0.034***	-0.034***	-0.034***	-0.034***
	(0.007)	(0.009)	(0.009)	(0.009)	(0.012)	(0.009)	(0.010)	[0.006]
Number of Counties	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029
	Panel (d): Average Republican Vote Share over 2000–2016							
total frontier experience	2.055***	2.055***	2.055***	2.055***	2.055***	2.055***	2.055***	2.055***
	(0.349)	(0.438)	(0.469)	(0.477)	(0.404)	(0.249)	(0.376)	[0.003]
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036

Notes: This table reports alternative approaches to inference for the four main county-level outcomes used in the paper. The baseline estimates from the paper are reproduced in column 1 for reference. The estimate in column 6 of panel (b) is omitted due to computational problems with the matrix inversion procedure. The bracketed numbers in the final column correspond to p-values from a wild bootstrap procedure clustering at the state level.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

D Alternative Measures of Total Frontier Experience

Appendix Table D.1 reports baseline results excluding the 40 counties with zero TFE in the data. As noted in the paper, the findings are not sensitive to excluding this extensive margin of variation in TFE.

Our baseline measure of TFE closely followed definitions in the historical literature as discussed in Section 2. In Table D.2, we demonstrate the robustness of our results to three relevant margins of adjustment to our measure of TFE. In each case, we redefine what it means for county c to be on the frontier at time t. First, we reduce the catchment area from 100 km to 50 km in proximity to the frontier line. Second, we adjust the density restriction to include counties with > 2 people/mi^2 but still less than 6, counties with ≤ 18 people/mi^2, and then remove the population density restriction altogether. Finally, we consider defining the frontier line as including only the main, westernmost extent of all contour lines identified by the GIS algorithm. The overall message is that our particular choice of the frontier definition based on the historical record is not driving the main findings.

Table D.1: Robustness to Excluding Counties with Zero TFE

	Dependent Variable:				
	Infrequen Names	Non-Patr./ -Matr. Names	Mean CCES Preferences	Property Tax Rate	Republican Vote Shr.
	(1)	(2)	(3)	(4)	(5)
	Panel (a): Baseline				
total frontier experience	0.144***	0.228***	0.014***	-0.034***	2.055***
	(0.021)	(0.030)	(0.002)	(0.007)	(0.349)
Number of Observations	2,036	2,036	112,579	2,029	2,036
Mean of Dependent Variable	0.00	0.00	0.412	1.02	60.0
	Panel (b): Excluding Counties with Zero TFE				
total frontier experience	0.141***	0.193***	0.013***	-0.031***	1.664***
	(0.020)	(0.030)	(0.002)	(0.007)	(0.340)
Number of Observations	1,996	1,996	105,623	1,990	1,996
Mean of Dependent Variable	0.00	0.00	0.413	1.02	60.3

Notes: This table reports estimates in panel (b) of our baseline specification (reproduced in panel (a)) excluding up to 40 counties with zero TFE. The specification is otherwise
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table D.2: Robustness to Alternative Measures of TFE for Summary Outcomes

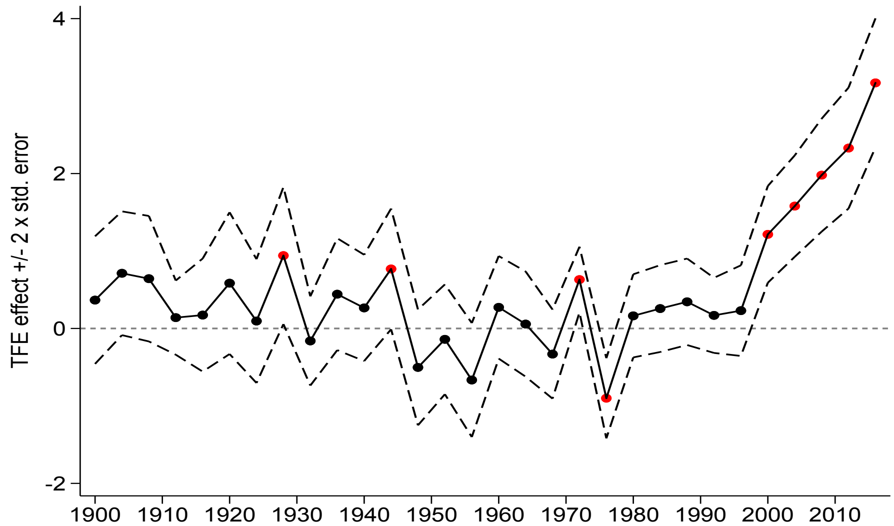
Dependent Variable:	Infreq. Name standardized 1940 (1)	Non-Patr./-Matr. standardized 1940 (2)	Mean Gov. Prefs. CCES 2006–16 (3)	County Property Tax Rate 2010 (4)	Republican Vote Share Avg. 2000–16 (5)
TFE: 100 km, $\leq 6/\text{mi}^2$, no inner or outer islands	0.144***	0.228***	0.014***	-0.034***	2.055***
	(0.021)	(0.030)	(0.002)	(0.007)	(0.349)
TFE: 50 km, $\leq 6/\text{mi}^2$, no inner or outer islands	0.146***	0.220***	0.017***	-0.035***	2.051***
	(0.021)	(0.030)	(0.003)	(0.007)	(0.358)
TFE: 100 km, $\leq 18/\text{mi}^2$, no inner island lines	0.109***	0.174***	0.011***	-0.027***	1.575***
	(0.021)	(0.029)	(0.002)	(0.007)	(0.339)
TFE: 50 km, $\leq 18/\text{mi}^2$, no inner island lines	0.096***	0.143***	0.011***	-0.025***	1.458***
	(0.020)	(0.027)	(0.003)	(0.006)	(0.351)
TFE: 100 km, 2-6/mi^2, no inner island lines	0.101***	0.195***	0.025***	-0.014*	1.877***
	(0.035)	(0.045)	(0.006)	(0.008)	(0.485)
TFE: 50 km, 2-6/mi^2, no inner island lines	0.083**	0.189***	0.026***	-0.012	1.771***
	(0.039)	(0.049)	(0.006)	(0.009)	(0.530)
TFE: 100 km, no density restriction, no inner island lines	0.038*	0.077***	0.006***	-0.011	1.001***
	(0.021)	(0.028)	(0.002)	(0.007)	(0.335)
TFE: 50 km, no density restriction, no inner island lines	0.060***	0.086***	0.006*	-0.018***	1.078***
	(0.019)	(0.027)	(0.003)	(0.006)	(0.339)
TFE: 100 km, $\leq 6/\text{mi}^2$, including inner island lines	0.164***	0.240***	0.014***	-0.032***	2.048***
	(0.019)	(0.027)	(0.002)	(0.006)	(0.320)
TFE: 50 km, $\leq 6/\text{mi}^2$, including inner island lines	0.173***	0.244***	0.017***	-0.035***	2.098***
	(0.021)	(0.028)	(0.003)	(0.007)	(0.335)
TFE: 100 km, $\leq 6/\text{mi}^2$, main single contour line	0.116***	0.211***	0.011***	-0.037***	1.872***
	(0.027)	(0.034)	(0.004)	(0.008)	(0.436)
TFE: 50 km, $\leq 6/\text{mi}^2$, main single contour line	0.109***	0.196***	0.018***	-0.043***	1.787***
	(0.029)	(0.037)	(0.004)	(0.008)	(0.460)
TFE: 50 km, $\leq 6/\text{mi}^2$, no inner or outer island lines	0.142***	0.232***	0.012***	-0.034***	2.133***
	(0.021)	(0.031)	(0.002)	(0.007)	(0.357)
TFE: 50 km, $\leq 6/\text{mi}^2$, no inner or outer island lines	0.141***	0.226***	0.016***	-0.035***	2.116***
	(0.022)	(0.031)	(0.003)	(0.007)	(0.373)
State Fixed Effects	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓	✓

Notes: This table reports estimates of equation (4) for three measures of infrequent names for white children, age 0–10 in the 1940 Census. Each cell is a different regression based on the given dependent variable in the column and the measure of total frontier experience in the given row. The frontier lines considered in the baseline are countour lines longer than 500km after removing all "inner island lines" that are east of the main frontier line. The alternative measures of frontier experience considered above vary (i) the catchment area from 100 to 50 km from the contour lines, (ii) the density restriction from ≤ 6 people/mi^2 to 2$\geq$people/mi^2≤ 6 to no restriction, (iii) including inner island lines, and (iv) including only the longest single contour line. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

E Further Results on Voting and Partisanship

We present here two sets of results discussed in Section 4.3 of the paper. First, Appendix Figure E.1 reports estimates for the effects of TFE on the Republican vote share for all elections going back to 1900.

Figure E.1: TFE and the Republican Presidential Vote Share, 1900–2016



Notes: This figure reports point estimates and +/- 2×standard error confidence bands on the effects of TFE on the Republican Presidential vote share in each election from 1900 to 2016. The red circles indicate statistical significance at the 95% level.

Second, Appendix Table E.1 reports estimates for the effects of TFE on partisan legislative speech associated with opposition to big government and redistribution. We compute measures of speech intensity based on the corpus of legislative speech and associated bigrams provided in Gentzkow et al. (2019). In particular we estimate the following specification:

$$\left(\frac{\text{bigram } b}{\text{all words}}\right)_{d(\ell)t} = \alpha + \beta\text{total frontier experience}_d + \mathbf{x}'_d\gamma + \mathbf{FE} + \varepsilon_{d(\ell)t}, \tag{E.1}$$

where the dependent variable captures the share of bigrams related to topic b in all words used by Republican legislator ℓ from congressional district d in congress year t.[1] We include fixed effects $\mathbf{FE}$ for the Census division in which d lies as well as the congress year. Standard errors are clustered at the congressional district level.

We consider three topics b that are particularly relevant to the other anti-statist outcomes we consider in the paper. These include, across panels (a) "Big Government" based on the "big govern" bigram, (b) "Taxation" based on the top four Republican-leaning bigrams on this topic with highest average partisanship across all sessions identified by Gentzkow et al. (2019) (tax increas, rais tax, tax relief, american taxpay), and (c) "Budget" based on the top four Republican-leaning bigrams on this topic with highest average partisanship across all sessions identified by Gentzkow et al. (2019) (govern spend, feder spend, intern revenu, treasuri depart). TFE is computed at the congressional district level rather than the county level as in our core specifications in the paper.

The estimates in Table E.1 suggest that TFE amplified the supply of political opposition to big government among Republican legislators, especially beginning in the mid-1990s. While partisanship around these themes and issues grew for politicians everywhere around this time, our estimates suggest that this

[1]We multiply the dependent variable by 1,000 for presentational purposes.

growth may have been differential in regions with greater TFE. While some of the differences with earlier periods are noisy (e.g., in panels (b) and (c)), this supply-side pattern is consistent with the growing demand-side differential seen in the Republican presidential vote shares in Appendix Figure E.1. These findings paint a consistent picture of TFE capturing latent cultural attitudes that can be activated around salient political themes.

As noted in the paper, these time patterns could be due to changes in the type of elected representative or to a change in the type of speech used by representatives that would have been elected otherwise. What's important here is that the patterns line up with the strong voter demand for attention to such issues seen in Table 3.

Table E.1: TFE and Opposition to Big Government in Republican Legislator Speech

	1902-30	1932-60	1962-90	1992-2016
	(1)	(2)	(3)	(4)
	Dep. Var.: Share of Legislator Speech with Bigrams Including [...]			
	Panel (a): Big Government			
total frontier experience	-0.000	-0.008	0.001	0.021**
	(0.000)	(0.015)	(0.009)	(0.010)
Number of Counties	2,100	1,510	1,447	1,638
R^2	0.01	0.02	0.04	0.06
	Panel (b): Taxation			
total frontier experience	-0.009	-0.003	0.079	0.122
	(0.013)	(0.055)	(0.105)	(0.136)
Number of Counties	2,100	1,510	1,447	1,638
R^2	0.04	0.04	0.12	0.10
	Panel (c): Budget			
total frontier experience	-0.034	0.099	0.067	0.079**
	(0.070)	(0.097)	(0.056)	(0.036)
Number of Counties	2,100	1,510	1,447	1,638
R^2	0.02	0.03	0.09	0.06

Notes: This table reports estimates of equation (E.1) across four different time periods: 1902–30 (column 1), 1932–60 (column 2), 1962–90 (column 3), and 1992–2016 (column 4). Other details on the specifications can be found in the discussion above.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table E.2: Total Frontier Experience and Preferences Over Partisan Policy Issues

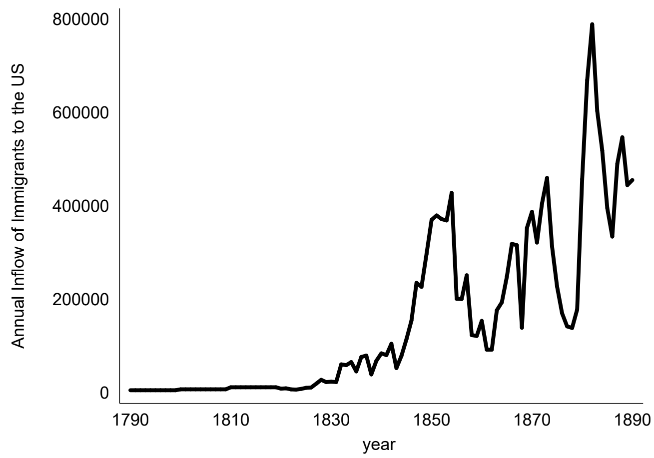
Dependent Variable:	Opposes Affordable Care Act (1)	Opposes Increasing Minimum Wage (2)	Opposes Banning Assault Rifles (3)	Opposes Regulation of CO_2 Emissions (4)
total frontier experience	0.022***	0.023***	0.015***	0.016***
	(0.004)	(0.008)	(0.004)	(0.004)
Oster δ for $\beta = 0$	2.96	3.05	2.46	2.22
Number of Individuals	29,446	5,134	29,404	29,215
Number of Counties	1,728	1,066	1,723	1,718
Mean of Dependent Variable	0.53	0.31	0.37	0.32
R^2	0.06	0.06	0.09	0.08
Survey Wave Fixed Effects	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	✓	✓

Notes: This table reports estimates of equation (4) for four measures of support for conservative issues that are particularly relevant to the frontier setting in historical accounts. The dependent variables are all binary indicators based on questions in the CCES across different years. The measure in Column 1 equals one if the individual in 2014 believes that the Affordable Care Act (ACA) should be repealed, in Column 2 equals one if the individual in 2007 opposes an increase in the minimum wage, in Column 3 equals one if the individual in 2014 opposes a ban on assault rifles, and in Column 4 equals one if the individual in 2014 opposes regulation of pollution by the Environmental Protection Agency (EPA). The set of specifications are otherwise the same as in Table 3; see the notes therein for details. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. The Oster (2019) tests are with reference to a baseline specification that only includes state fixed effects. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

F Instrumental Variables Strategy: Background and Additional Results

Figure F.1 shows immigration inflows to the U.S. over the study period.

Figure F.1: Annual Migration Inflows

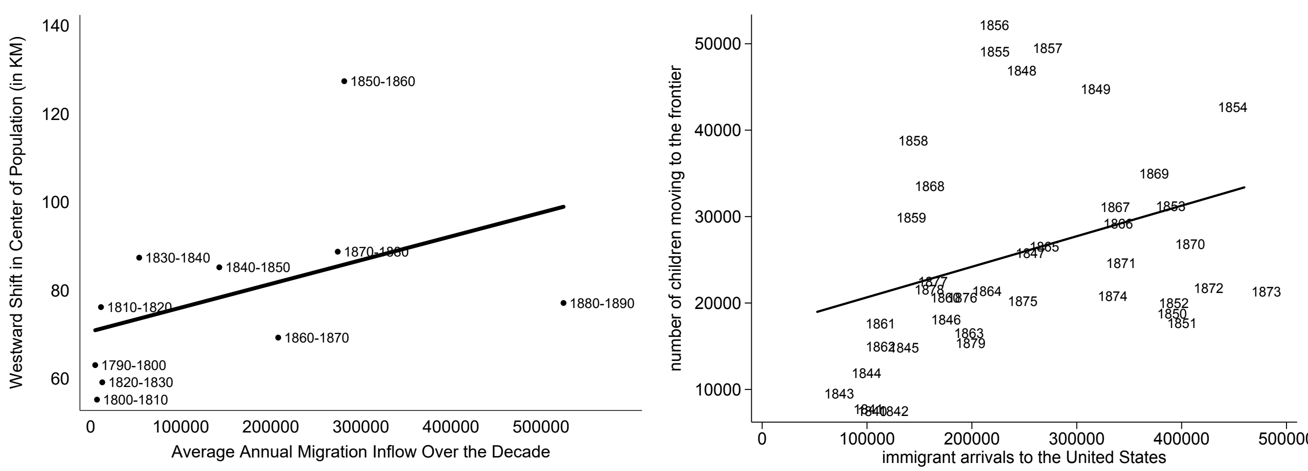


Notes: This figure plots the total number of migrants entering the United States, 1790-1890. The data for 1820–1890 is available from the Migration Policy Institute (2016), while the data for 1790-1819 is imputed from Tucker (1843).

Figure F.2(a) then shows the strong positive correlation between these inflows by decade and the speed of westward expansion, proxied by the east-to-west distance traveled by the country's population centroid (the green dot in Figure A.1(b) for 1860). Figure F.2(b) shows that the scale of native-born migration to the frontier is greater in years with more immigrants arriving to the United States. We identify migrants moving to the frontier using the same procedure based on differences in children's birthplaces as detailed in Section 5.

Figure F.2: Immigration and Westward Expansion

(a) Center-of-Population Shift (b) Migration of Native-Born Children to the Frontier



Notes: Graph (a) plots the length of the decadal westward shift of the center of population (in km) against the average annual immigrant inflow during the decade. The center of population is the point at which weights of equal magnitude corresponding to the location of each person in an imaginary flat surface representing the U.S. would balance out. Graph (b) plots the relationship between the number of immigrant arrivals to the U.S. in a given year and the number of children brought to the frontier with their parents, a sample that we use throughout Section 5.

These scatterplots help visualize the process by which immigrants arriving in the U.S. pushed the edges of settlement westward, which in turn hastened the onward march of the frontier line. In periods with low immigrant inflows, this push slowed down, leading some counties to remain part of the frontier for longer than those that just happened to be getting closer to the frontier line at a time of rapid inflows into the U.S. Table F.1 demonstrates the strong first stage in our main IV regressions from Table 8.

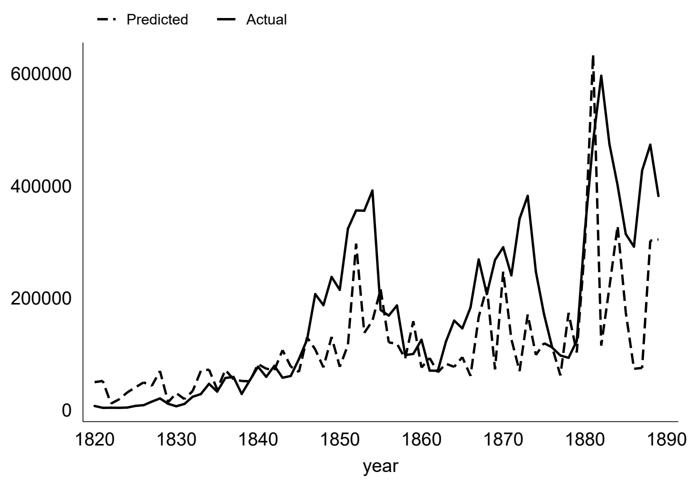
Table F.1: First Stage Results for the Instrumental Variables Estimates in Table 8

Dependent Variable (in first stage):	total frontier experience	
	(1)	(2)
Log Average Actual National Migration Inflows	-1.016***	
	(0.073)	
Log Average Predicted National Migration Inflows		-2.010***
		(0.144)
Log county area	0.234***	0.232***
	(0.072)	(0.073)
Latitude	-0.091	-0.076
	(0.079)	(0.075)
Longitude	-0.153***	-0.176***
	(0.029)	(0.030)
Mean Annual Temperature	-0.144**	-0.102
	(0.071)	(0.068)
Mean Annual Rainfall	-0.002***	-0.002***
	(0.001)	(0.001)
Mean of Median Altitude	-0.001**	-0.001**
	(0.000)	(0.000)
Distance to Coast	-0.001**	-0.001**
	(0.000)	(0.000)
Distance to Rivers	0.003***	0.004***
	(0.001)	(0.001)
Distance to Lakes	-0.000	-0.000
	(0.000)	(0.000)
Average Agricultural Suitability	2.686***	0.876
	(0.788)	(0.752)
Number of Counties	2036	2036
First Stage F Statistic	193.6	195.8

Notes: This table reports the first stage results corresponding to the baseline IV regressions presented in Table 8. Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Section 4.5 shows that the main results are hold in an instrumental variable specification exploiting time series variation in national migration inflows. To address concerns regarding the excludability of the baseline instrument due to pull factors associated with immigrant inflows, we show in Panel B of Table 8 that the IV results are qualitatively unchanged when using an instrument based on push factors unrelated to frontier conditions. For this version of the IV, we draw on the approach in Nunn, Qian and Sequeira (2017), using country-year level data on migrant inflows from 16 European countries to the US from 1820–1890 and constructing predicted migration outflows induced by weather shocks. First, using country-specific regressions, we predict the annual migrant outflows from each country to the US as a function of country-specific shocks to temperature and rainfall in the prior year (see Nunn, Qian and Sequeira, 2017, for details on these measures). Second, we aggregate across countries to obtain the total predicted migrant inflows to the US for each year. Analogous to our baseline instrument, we then construct the IV for each county in our sample by calculating the average annual predicted migrant inflow to the US over the 30 years starting from the first year in which the given county is just west of the frontier. Figure F.3 shows how the predicted inflows, which isolate push factors, compare to the actual inflows, which naturally include both push and pull. While the data on migrant inflows from Europe to the US is available only starting in 1820, we retain the full sample of counties in the IV regressions by imputing the inflows for 1790-1819 using linear extrapolation of the post-1819 predicted inflows.[1]

Figure F.3: Actual vs. Predicted Immigration Inflows from Europe to the United States



Notes: This figure compares the actual migration inflows from Europe from 1820–1890 to the predicted flows based on the total country-specific predicted outflows using the climatic shocks approach in Nunn, Qian and Sequeira (2017) as described above.

Probing Instrument Validity. In Section 4.5, we discussed an important concern about instrument validity, namely that greater immigration flows might mechanically reduce TFE and individualism. The key threat has to do with the joint determination of the scale and composition of frontier migration flows. Intuitively, with more migrants moving to the frontier due to push factors in Europe and/or the East Coast, the prevalence of individualism among those migrants might be higher than in periods with few migrants to the frontier when the push factors were weaker and the selection on individualism stronger.

Here, we explore this potential bias in the latter half of the 1800s when it is possible to separately relate scale and composition of frontier migrant flows to the national immigration shocks underlying our IV. We identify migrants moving to the frontier using the same procedure based on differences in children's birthplaces as detailed in Section 5. We measure individualism based on the names of children born prior to moving.

In certain contexts, one might expect the prevalence of individualists to be inversely related to the scale of migration. However, Appendix Figure F.4(a) suggests that this is not the case when looking at native-born migration flows to the frontier. Each point on the graph reflects a given year's number of children under the age of 20 migrating to the frontier with their parents and the mean prevalence of individualistic names among those children. The lack of correlation between scale and individualism suggests that there is not a mechanical relationship between the two in our setting.

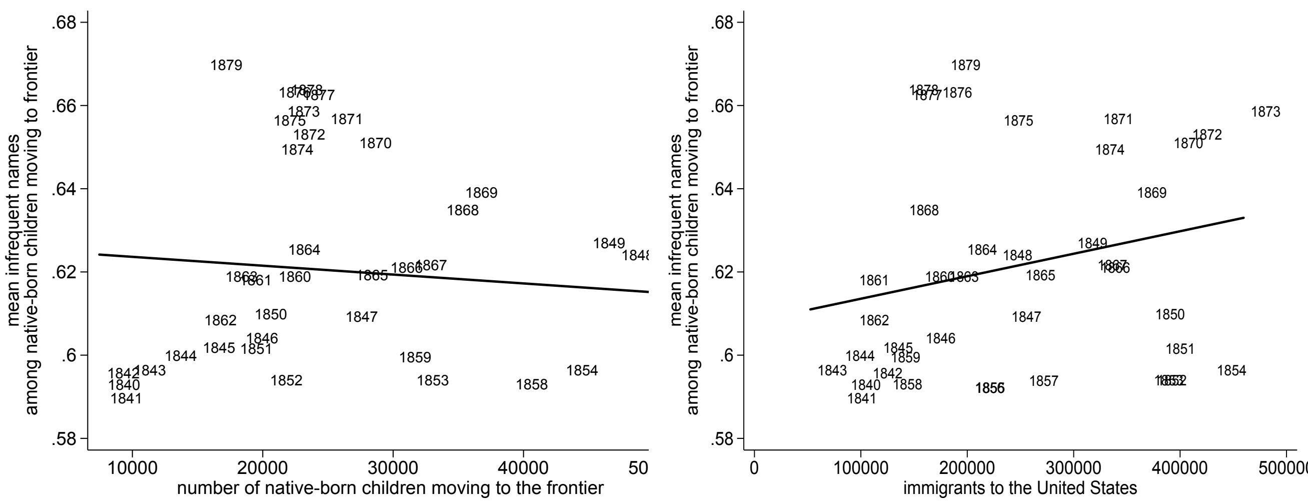
Appendix Figure F.4(b) shows that the prevalence of individualism among frontier migrants is not

[1] Restricting the sample to counties just west of the frontier after 1820—for which the IV is solely based on predicted flows without extrapolation—delivers similar results, though the estimates are noisier due to the smaller sample size.

lower in years with more immigrants arriving to the United States, but rather, weakly higher. This goes against the intuition that selective migration of individualists would be weaker in periods with greater push factors in settled areas. One explanation might be that non-individualists have stronger social networks that allow them to deal with adverse labor market shocks and ultimately remain in settled areas. Without deep social networks, individualists might be more readily pushed to move to the frontier. In any case, the observed patterns tend to alleviate a salient concern about the exclusion restriction in the IV estimation.[2]

Figure F.4: U.S. Immigrant Arrivals and Native-Born Frontier Migration Scale and Individualism

(a) Frontier Migrant Scale and Individualism (b) U.S. Immigration and Frontier Migrant Individualism



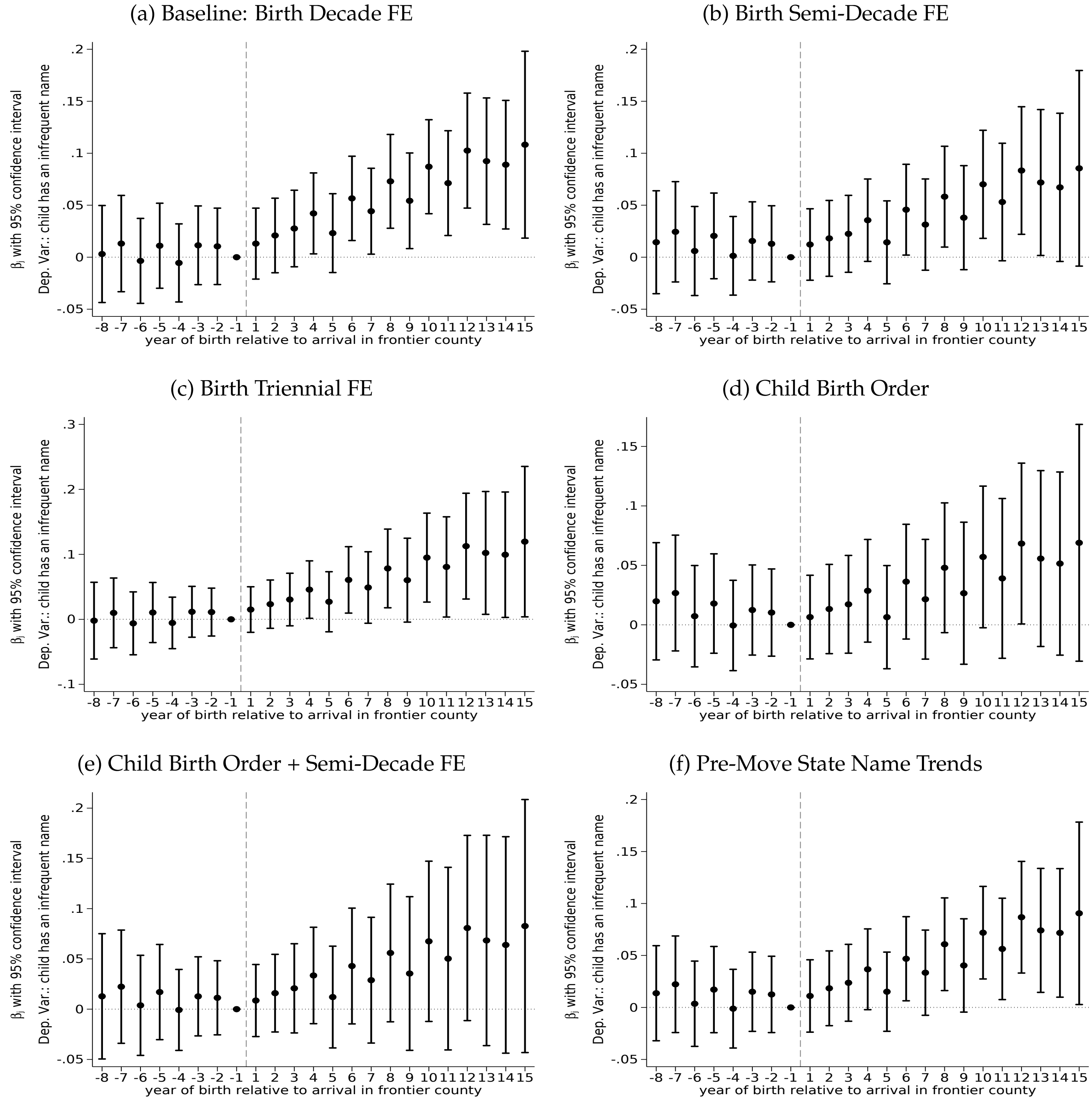
Notes: Graph (a) plots the relationship between the number of children brought to the frontier with their parents and the prevalence of individualistic names among them. Each point is a given year of migration computed based on the procedure used in Section 5 based on the 1850–80 Censuses. Graph (b) plots the relationship between the number of immigrant arrivals to the U.S. in a given year and the prevalence of individualistic children's names among frontier migrants.

[2] Note that each of the graphs in Figure F.4 looks similar when allowing for (cumulative) lags in the number of immigrants to the U.S. and/or when using the alternative, predicted immigrant flows based on weather shocks in Europe.

G Further Robustness Checks on the Exposure Effects in Section 5.2

G.1 Additional Results: Adulthood Exposure Event Study

Figure G.1: Identifying Exposure Effects: Adulthood Exposure (I)

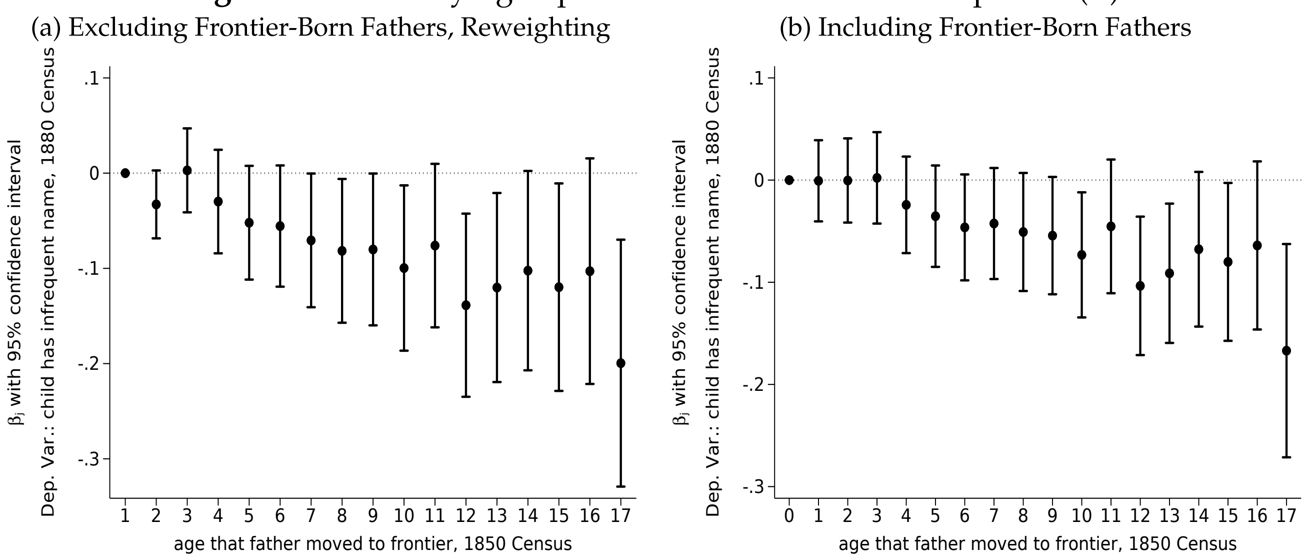


Notes: These graphs report estimates of β_j +/- 95% confidence intervals in equation (6) for $j = -8, \ldots, 15$ (with other j suppressed for presentational purposes). Each β_j can be interpreted as the differential likelihood of an infrequent name being given to a child born j years before/after their parents moved to the frontier, relative to the child born one year prior to moving. The sample includes 57,097 children born to 16,901 families headed by white, native-born parents that moved with at least one child to a frontier county as we observe them in the Census in 1850, 1860, 1870 or 1880. All estimates control for household fixed effects and child gender. Graph (a) additionally includes child birth decade FE, (b) includes 5-yearly birth cohort, (c) includes 3-yearly birth cohort FE, (d) controls for child birth order, (e) controls for child birth order, and (f) controls for the mean gender-specific infrequent name share in each child birth year in the state from which each family migrated from before arriving on the frontier. Standard errors are clustered by contemporaneous county.

G.2 Additional Results: Childhood Exposure Age-at-Move

As discussed in Section 5.2, our results are also robust to accounting for measurement error in the linking procedure. First, in Figure G.2(a) and panel (a) of Table G.1 below, we reweight the baseline sample by the odds of a successful link estimated as a flexible function of the father's age interacted with whether or not the father himself has infrequent name. Following Bailey et al. (forthcoming), we use these propensity scores to construct inverse probability weights. The results are very similar and in some specifications more precisely estimated than the baseline in Figure 6 and Table 11. This helps to rule out a sample selection bias wherein fathers with more individualistic names, and hence greater inherited individualism, might be more likely to be linked across Censuses. Second, we can further restrict our baseline sample to the children of fathers with unique matches between the 1850 and 1880 Census. This substantially cuts the sample by more than half, which leads to sizable reductions in statistical power. The resulting estimates of continuous age-at-move effects range from -0.010 (0.007) to -0.007 (0.007) across the analogous specifications 1 and 5 in Table 11. In other words, while discarding the considerable information in non-unique matches we use in the baseline (which includes a dummy indicator for such matches), we find estimates that are quantitatively similar but noisy. We cannot reject that the coefficients equal zero, but we also cannot reject that they equal the baseline estimates.

Figure G.2: Identifying Exposure Effects: Adulthood Exposure (II)



Notes: Graph (a) reports a reweighted estimate of Figure 6. We reweight each child observation by the estimated odds (inverse probability weights) that the father was successfully linked across Census rounds. These weights are estimated as a function of the interaction of father's age in 1850 and whether or not the father has infrequent name. In graph (b), the estimates are with respect to children born to fathers who were themselves born on the frontier. The sample in graph (a) (graph (b)) consists of 81,823 (146,085) children age 0–20 in the 1880 Census with fathers hailing from 17,778 (28,776) families observed in the 1850 Census and where at least two brothers (one brother) were born before the family moved to the frontier.

Moreover, the age-at-move estimates are robust to including children whose fathers were born on the frontier. We generalize equation (8) to include all brothers who were born after their parents moved to the frontier, normalizing their ages-at-move to $j = 0$. Doing so in Figure G.2(b) and panel (b) of Table G.1 suggests similar patterns, despite the substantial increase in the sample size.[1] We omit the children of fathers born on the frontier from the baseline in Figure 6 since they may only appear in the sample as a result of selective fertility among parents that found high returns to individualism after

[1]This procedure adds a large number of 1850 households for whom a first son was born prior to moving while a second (and higher-order) son was born after moving to the frontier.

arrival to the frontier. This need not introduce a source of bias per se as these fathers would also have experienced more years of frontier conditions than their older siblings. However, they do introduce a source of sample selectivity just as they did in the event study. That the results look similar with and without these additional fathers suggests that this type of sample selectivity is not a first-order concern.

Table G.1: Identifying Exposure Effects: Childhood Exposure (II)

	Dep. Var.: Child Has an Infrequent Name				
	(1)	(2)	(3)	(4)	(5)
	(a) Excluding Frontier-Born Fathers and Reweighting by Link Probability				
age-at-move to frontier	-0.009**	-0.008**	-0.007**	-0.006*	-0.006*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	81,823	81,823	81,823	81,823	81,823
Number of Families	17,778	17,778	17,778	17,778	17,778
Mean of Dependent Variable	0.68	0.68	0.68	0.68	0.68
R^2	0.27	0.27	0.27	0.27	0.27
	Panel (b): Including Frontier-Born Fathers				
age-at-move to frontier	-0.007***	-0.006***	-0.005**	-0.004*	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	146,085	146,085	146,085	146,085	146,085
Number of Families	28,776	28,776	28,776	28,776	28,776
Mean of Dependent Variable	0.69	0.69	0.69	0.69	0.69
R^2	0.25	0.25	0.25	0.25	0.25
Extended Family (1850 Household) FE	✓	✓	✓	✓	✓
State of Residence FE in 1880	–	✓	–	–	–
Child Birth Cohort FE	–	–	decade	5-yearly	3-yearly

Notes: This table reports reweighted estimate of panel (a) in Table 11. We reweight each child observation by the estimated odds (inverse probability weights) that the father was successfully linked across Census rounds. These weights are estimated as a function of the interaction of father's age in 1850 and whether or not the father has infrequent name. Standard errors are two-way clustered by 1850 family and 1880 county.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Supplementary Material Appendix

H Further Background Characterizing Frontier Life

H.1 Demographics and Individualism Along the Transition out of the Frontier

Figure H.1 reports estimates time-varying estimates of equation (1):

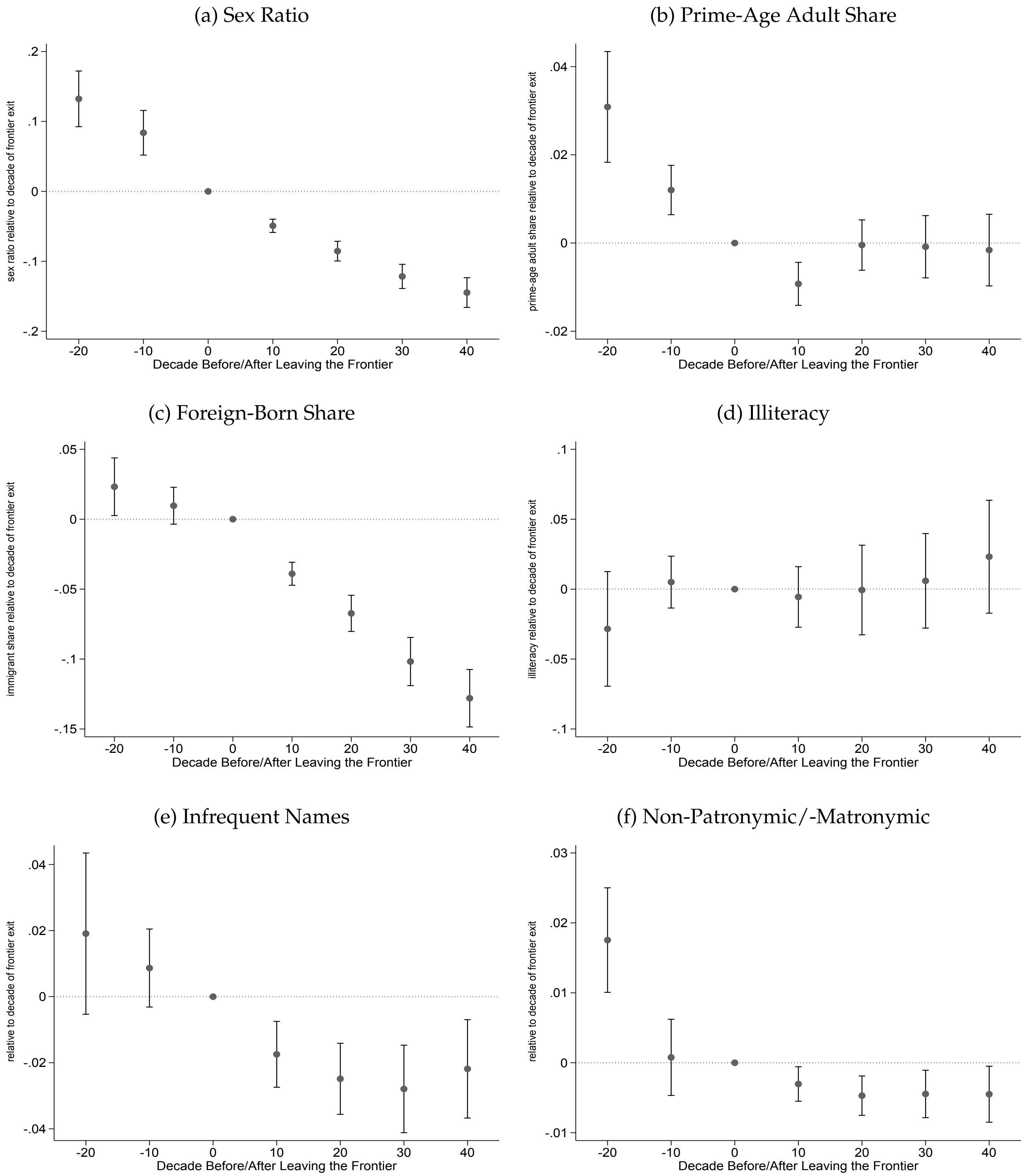
$$x_{cdt} = \alpha + \sum_{j=-20}^{40} \gamma_j \mathbf{1}(\text{years since exiting frontier = j}) + \theta_d + \theta_t + \varepsilon_{cdt}, \tag{H.1}$$

where the γ_j coefficients identify the average x for counties that have exited or will exit the frontier j years prior or in the future, respectively. We plot 95 percent confidence intervals for the γ terms, each of which are estimated with reference to the decade in which the county transitioned out of the frontier.

The estimates in Figure H.1 provide additional insight into the process of demographic and cultural change along the frontier. Panel (a) reveals an abrupt shift in the sex ratio as counties exit the frontier. On average, counties have 0.25 higher sex ratios in the two decades prior to exiting the frontier whereas those decades thereafter exhibit lower ratios that stabilize by the second decade. Panel (b) provides similar evidence of convergence towards a lower prime-age adult share as counties exit the frontier. Panel (c) shows that the foreign-born population share exhibits a steady and roughly linear decline along the frontier transition path. Results are noisier for illiteracy rates in panel (d).

Panels (e) and (f) demonstrate the declining prevalence of individualistic children's names as counties approach the decade in which they exit the frontier. Thereafter, we see naming patterns stabilize around a less individualistic equilibrium in which popular names becomes more common at the local level. Note, however, that these estimates are with reference to the year of exiting the frontier and do not address the long-run differences in individualistic names as a function of years of exposure to frontier conditions. This is precisely what the analysis in Section 4 provides.

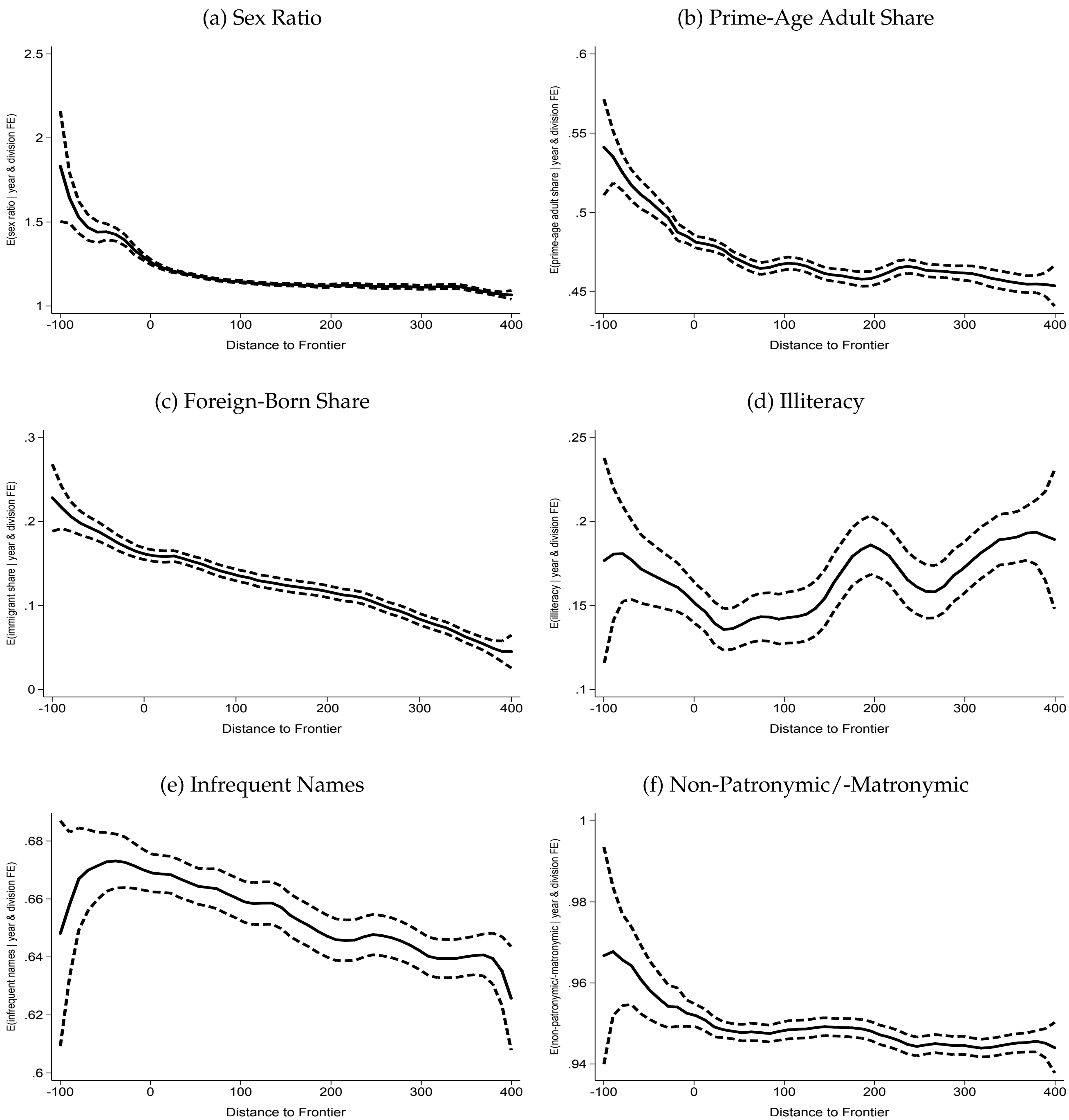
Figure H.1: Demographics and Individualism Along the Transition out of the Frontier
Estimates with Respect to Decade of Exiting the Frontier



Notes: This figure plots coefficients from the event study regressions in equation (H.1) for each of the outcomes in Table 1. The decade-specific point estimates and 95 percent confidence intervals are each with reference to the county-specific decade of exiting the frontier. All regressions include division and year fixed effects. Standard errors are clustered using the grid cell approach of Bester, Conley and Hansen (2011) as described in Section 4.1.

H.2 Demographics and Individualism by Distance to the Frontier

Figure H.2: Distribution of Demographics and Individualism by Distance to the Frontier



Notes: This figure reports the distance-based analogues to the density plots in Figure 4. Distance to the frontier, measured in kilometers, is the distance from the county's centroid to the nearest frontier line. The distance is negative if the county centroid is to the west of the nearest main frontier line. Figures (a)-(f) provide the semiparametric estimates of the corresponding dependent variables, with 95 percent confidence intervals, as a function of distance to the frontier estimated using county-level pooled data and applying a nonlinear function recovered using the partially linear Robinson (1988) estimator. The specification includes Census division and year fixed effects and are based on an Epanechnikov kernel and rule-of-thumb bandwidth.

H.3 Taxes and Public Goods on the Frontier

We present evidence here that demonstrates two important facts about the prevalence of taxation and public goods on the frontier historically.

First, *local* taxation and public spending were significantly lower in frontier counties historically. We demonstrate this for tax revenue per capita in column 1 of Appendix Table H.1 using county- and city-level tax revenue per capita, which is available for 1870 and 1880 as reported by Ruggles et al. (2019). Similarly, column 2 demonstrates lower government spending per capita in frontier than in non-frontier counties in 1870. Moreover, graphs (a) and (b) in Figure H.3 demonstrate the same sort of structural break in these outcomes at levels of population density around the cutoffs defining frontier locations historically, i.e., 2–6. Together, these results are consistent with smaller local government and weaker taxation on the frontier historically, consistent with the lack of social infrastructure described in most accounts of frontier life.

Second, *non-local* public goods were also less pervasive on the frontier, but this infrastructure did not exhibit the same sort of structural break at very low levels of population density seen for local public goods. Columns 3–5 of Appendix Table H.1 show, respectively, that frontier counties had a smaller number of post offices and more limited railroad and canal access historically. Appendix Figure H.3 shows that these non-locally-provided public goods were more pervasive in counties with greater population density. However, unlike local government efforts to raise taxes and make public investments, neither post offices nor railroads and canals exhibit structural changes at low levels of population density characterizing frontier conditions. All three outcomes vary smoothly if not roughly linearly from counties with population density ranging from just above zero to around 40. Importantly, this suggests that non-local government investments were not necessarily leading the westward expansion of the frontier as seen through the types of individuals underlying our results in Sections 3 and 5.

Table H.1: Taxes and Public Goods on the Frontier

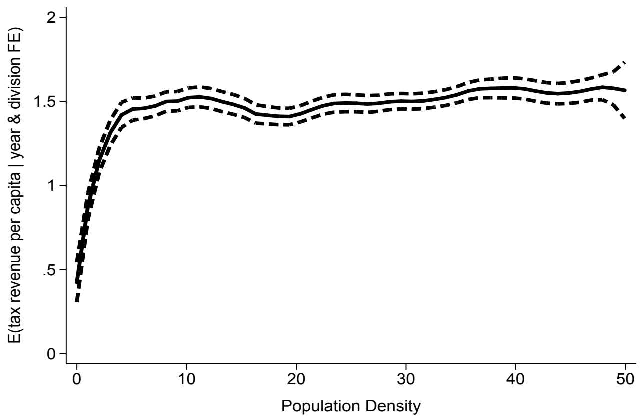
	Local Government		Non-Local Government		
Dependent Variable:	Tax Revenue Per Capita	Public Debt Per Capita	Number of Post Offices	Railroad Access	Canal Access
	(1)	(2)	(3)	(4)	(5)
frontier county	-0.565***	-4.914***	-4.708***	-0.114***	-0.010*
	(0.089)	(0.438)	(0.360)	(0.013)	(0.006)
Mean Dep. Var. in Non-Frontier Counties	1.49	8.18	10.02	0.43	0.05
Number of County-Years	3,756	1,789	9,801	13,458	14,043
R^2	0.42	0.16	0.44	0.54	0.15

Notes: This table reports estimates of the historical frontier differential in public goods using the same specification as in Table 1. Column 1 is the inverse hyperbolic sine of the sum of `taxcountyppop` and `taxcityetcppop` reported by Ruggles et al. (2019) for 1870 and 1880. Column 2 is the inverse hyperbolic sine of the sum of `pubdebtcounty` and `pubdebtcityetc`. Column 3 is the number of post offices in the given county–decade as reported in Acemoglu et al. (2016). Columns 4 and 5 are indicators equal to one if the given county–decade has railroad and canal access, respectively. See the notes below that table for details on the specification.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

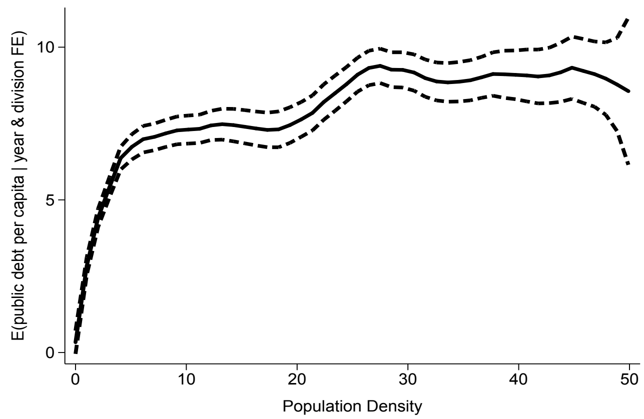
Figure H.3: Taxes and Public Goods by Population Density, 1790–1890

Local Government Taxation and Spending

(a) Tax Revenue

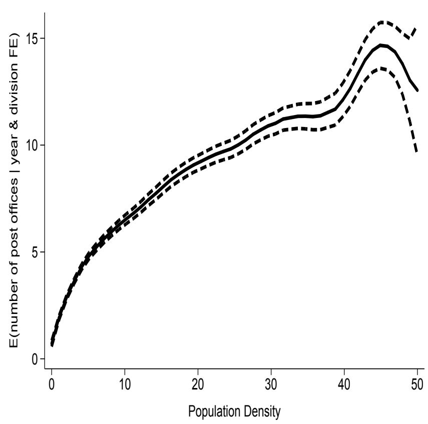


(b) Public Debt

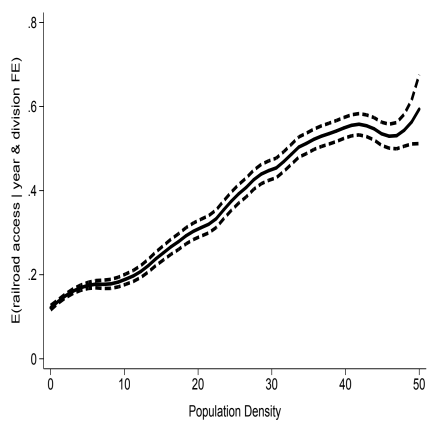


Non-Locally-Provided Public Goods

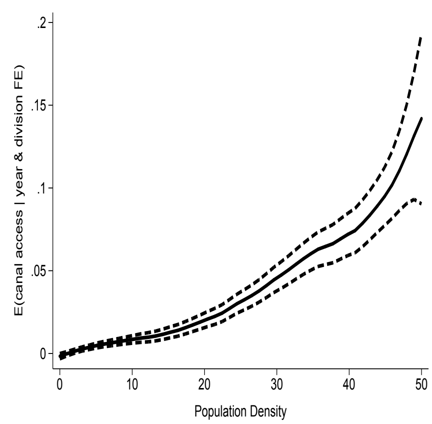
(c) Post Offices



(d) Railroad Access



(e) Canal Access



Notes: This figures reports semiparametric estimates of the relationship between population density and public goods measures from Table H.1 using the same specification as in Figure 4. See the notes below that table for details on the specification.

H.4 Low Inequality and Effort as the Road to Riches on the Frontier

This section argues that the opportunities and challenges on the frontier contributed to a culture of opposition to government intervention. The frontier's favorable prospects of upward mobility and a large perceived importance of effort in income generation may have fostered opposition to taxes, as suggested by the political economy theories of preferences for redistribution. This connection between the American frontier and theories of preferences for redistribution, hinted at by Alesina et al. (2001), echoes Billington (1974), who argued that the frontiersman "wanted not government interference with his freedom as he followed the road to riches."

In his analysis of the Turner thesis, Billington (1974) emphasizes the implications of the frontier's land abundance and "widespread property holdings." In these conditions, "a man's capacities, not his ancestry, determined his eventual place in the hierarchy, to a greater degree than in older societies." The frontiersman believed that "his own abilities would assure him a prosperous future as he exploited the natural resources about him." Access to land offered profit of opportunities, even for settlers with low initial wealth. Class distinctions were also weakened by the ubiquity of threats characterizing frontier life. As Overmeyer (1944) argues, since everyone "had to face the same hardships and dangers," the frontier was a "great leveling institution."

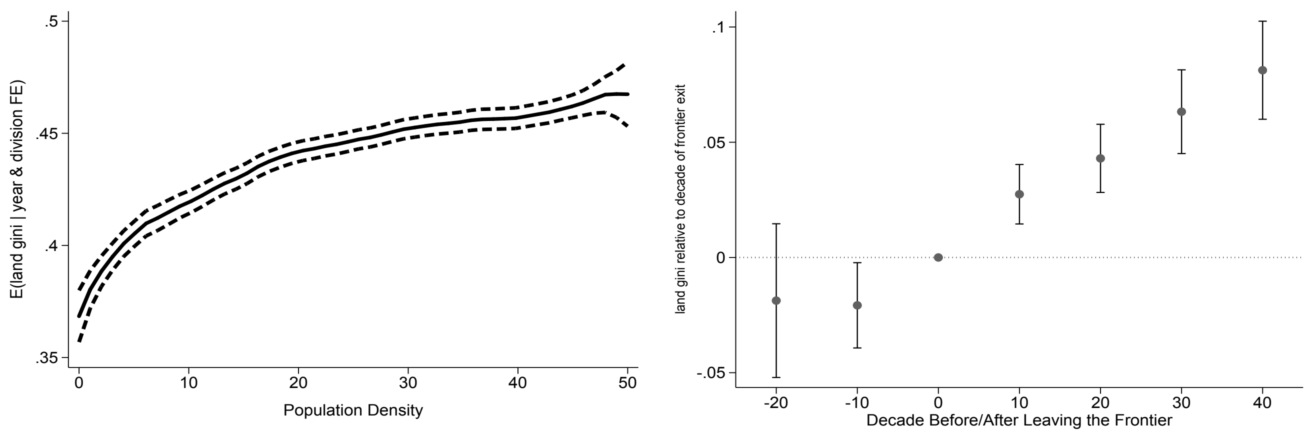
Numerous historical studies present stylized facts consistent with favorable prospects of upward mobility on the frontier and also with a large perceived importance of effort. As summarized by Stewart (2006), the frontier was "a place of economic opportunity," where settlers had low levels of initial wealth, but land-holding was widespread and rates of wealth accumulation were high, especially for early settlers and those that were able to endure.

Indeed, as shown in Appendix Figure H.4, historical Census data on landholdings is consistent with the idea that frontier locations offered a more level playing field. Land inequality, captured by the Gini coefficient, was significantly lower on the frontier, with sharp differences resembling what we documented for key demographics and individualism in Section 3. Moreover, this difference dissipated over time as counties exited the frontier and the usual forces giving rise to inequality took hold.

In sum, the stylized facts summarized above suggest a relatively limited role for inherited social class as a key determinant of income and wealth generation in the frontier economy. This implied a level playing field offering equality of opportunity, and a relatively high importance of effort as opposed to luck (of being born into a given class). Together with the selection and cultivation of individualism, these conditions plausibly contributed to the origins and persistence of frontier culture.

Figure H.4: Inequality is Lower on the Frontier

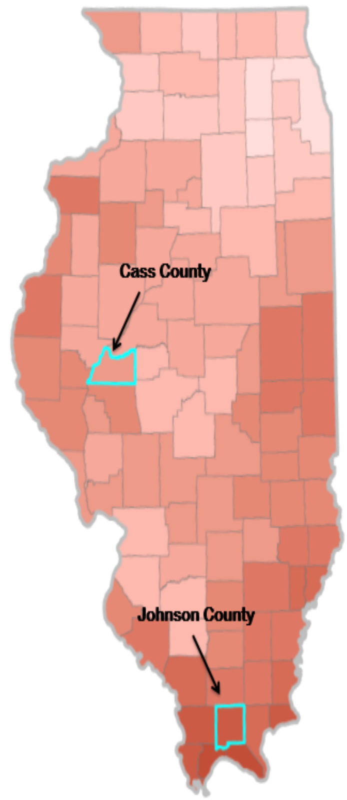
(a) Semiparametric (b) Event Study



Notes: Based on county level data from Manson et al. (2019) from 1790-1890. Land inequality is measured using the county level gini coefficient based on the number of farms in seven bins of farm size. The semiparametric specification in (a) is the same as in Figure 4, and the event study specification in (b) is the same as in Figure H.1. See the notes therein for details.

I Case Study Illustrating Long-Run Effects

To fix ideas, consider the two counties of Cass and Johnson mentioned in Section 2.2 and seen in the TFE map on the right, which is a snapshot of Illinois from Figure 3. Both are roughly equidistant from the Mississippi River and the important historical city of St. Louis. Today, the two rural counties look very similar in terms of population density: Cass has 36.3 people/mi^2, and Johnson has 36.6 people/mi^2. These two counties had very similar population density in 1890 as well. However, they differ significantly in their total frontier experience historically. Cass was on the frontier for 10 years, and Johnson for 32 years. This difference may be explained by any number of factors shaping the westward movement of the frontier through this area of the midwest in the early 1800s as seen in Figure 2. One potentially important contributor lies in our instrumental variable. Johnson entered the frontier in 1803 whereas Cass entered in 1818. While only 15 years apart, this implied a considerable difference in exposure to subsequent immigration-induced pressure on the westward expansion of the frontier over the next few decades as evidenced in Figures F.1, F.2, and especially F.3.



These historical differences in TFE translate into substantial long-run differences in the prevalence of rugged individualism in local culture. In Cass, 75 (64) percent of girls (boys) have infrequent names in 1940, Republican presidential candidates captured 55 percent of the vote in the average election since 2000, and local property tax rates are around 1.9 percent in 2010. Meanwhile, in Johnson, 78 (71) percent of girls (boys) have infrequent names in 1940, 68 percent average Republican vote shares since 2000, and 1.3 percent local property tax rates in 2010. This is striking insomuch as the two counties have such similar contemporary population density.

J Other Evidence on Selective Migration

This section provides additional evidence on selective migration and returns to individualism using a linked sample of men from 1870 to 1880. These results are discussed at greater length in an earlier working paper, but we retain them here given that they complement the more comprehensive results in Section 5 of the paper.

Compared to the results in the paper, those presented below are based on a narrower, linked sample of households from 1870 to 1880. We create this sample using full count data from the 1870 and 1880 Censuses provided by `ancestry.com` and (Sobek et al., 2017), respectively. We focus on the latest consecutive rounds available within the frontier period to ensure a large sample. We link individuals across rounds using an algorithm developed by Feigenbaum (2016). The base sample in 1880 is restricted to male household heads, native-born, aged 30–50, white, and who have at least one child aged 0–10.[1]

Appendix Table J.1 reports estimates of the frontier differential in infrequent naming patterns based on versions of the following equation for different sub-populations of movers and stayers:

$$\text{child has infrequent name}_{ic,1880} = \alpha + \beta\,\text{frontier}_{c,1880} + \mathbf{x}_{ic}'\boldsymbol{\zeta} + \varepsilon_{ic,1880}, \tag{J.1}$$

where the binary dependent variable equals one if child i residing in county c in 1880 has a name that falls outside the top 10 nationally in that decade, and the frontier indicator equals one if county c lies on the frontier according to our baseline definition. We restrict attention to white children aged 0–10 with native-born parents and cluster standard errors at the county level. The $\mathbf{x}_{ic}$ vector includes age$\times$gender and birth order fixed effects as well as indicators for whether the parents have infrequent names, but results are identical without these controls.

Column 1 of Appendix Table J.1 identifies the significance of selective migration. Children in households that migrated to the frontier between 1870 and 1880 are 4.2 p.p. more likely to have infrequent names than those remaining in non-frontier areas during that period, 71 percent of whom have infrequent names. While we do not observe whether these children were born before or after arriving on the frontier, this differential points to the self-selection of individualist types.

Column 2 captures the overall frontier differential in individualism. Children in frontier counties in 1880 are 7.5 p.p. more likely to have an infrequent name relative to children in non-frontier locations. Next, we show that the longer-term frontier residents (stayers) exhibit stronger individualism than recent arrivals from other counties. Column 3 decomposes the 7.5 p.p. differential into differences coming from early versus later frontier settlers. Early settlers in frontier counties are nearly three times more likely to give their children infrequent names than those that arrived more recently during the 1870s. Column 4 corroborates this differential, restricting the sample to those living in frontier counties in 1880. These results suggest that greater time on the frontier is associated with more individualistic naming patterns, a result that is rigorously borne out in Section 5.2 of the paper.

[1]The target year is 1870. The set of potential matches for these men are first identified based on first and last name, birth state and birth year. A random training sample is then drawn from among the potential matches and manually trained. The importance of each match feature is quantified using a probit model, and used to estimate a probability score for each link. A true match is defined as one with a sufficiently high score both in absolute and relative terms. The match rate was 25 percent, which is comparable with the rates achieved by recent studies linking records with broadly comparable data albeit different target populations (e.g., 29 percent in Abramitzky et al., 2012; 26 percent in Collins and Wanamaker, 2017; and 22 percent in Long and Ferrie, 2013). Although matching on names leaves scope for sample selection, the results look similar when reweighting using the odds of being linked across Census rounds (following Bailey et al., forthcoming). We estimate these probabilities using the same characteristics used for linking as well as an interaction of infrequent name status and frontier location in 1880. These interactions re-balance the linked sample to account for differential missing-ness along our key variables of interest.

Table J.1: Frontier Individualism and Selective Migration

	Dependent Variable: Child Has Infrequent Name in 1880			
	(1)	(2)	(3)	(4)
omitted reference group:	non-frontier resident, 1870–80	non-frontier resident, 1880	non-frontier resident, 1880	frontier immigrant, 1870–80
frontier county resident in 1880, arrived between 1870 and 1880	0.042***		0.055***	
	(0.012)		(0.011)	
frontier county resident in 1880		0.075***		
		(0.018)		
frontier county resident in 1880, arrived before 1870			0.186***	0.118***
			(0.035)	(0.026)
mean infrequent name, omitted group:	0.707	0.708	0.708	0.767
Number of Individuals	1,223,600	1,239,513	1,239,513	12,630
R^2	0.02	0.02	0.02	0.05
Gender×Age Fixed Effects	✓	✓	✓	Yes
Birth Order Fixed Effects	✓	✓	✓	✓

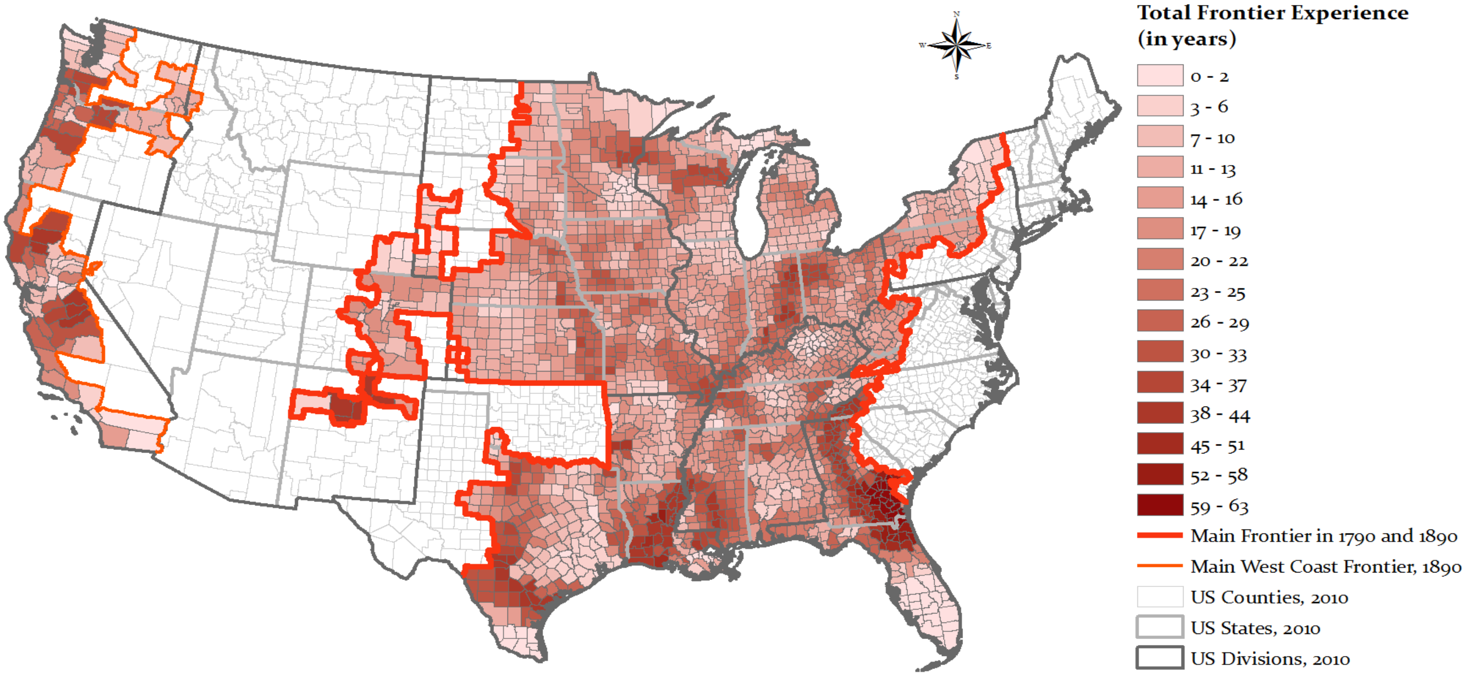
Notes: This table reports estimates of equation (J.1) based on the linked historical Census data from 1870 to 1880 for households with white, native-born fathers age 30–50 and children aged 0–10 in 1880. The dependent variable is an indicator equal to one if the child is given a name that falls outside the top 10 most popular names nationally in the 1870s. The top of each column reports the omitted reference group and the mean infrequent name share among them. We define immigrant status here based on whether the father switched counties between 1870 and 1880. Frontier counties are as defined in 1870 and 1880 based on the main east-to-west frontier line. Column 1 reports the selective migration differential between migrants from non-frontier to frontier counties and those that remained in non-frontier counties in both 1870 and 1880. Column 2 reports the overall differential in infrequent names between frontier and non-frontier counties in 1880, i.e., inclusive of stayers in frontier counties. Column 3 breaks down the overall differential into the component due to migrants between 1870 and 1880 and those that resided in the frontier county prior to 1870 (either by birth or earlier migration). Column 4 then restricts to frontier county residents, identifying the differential between recent immigrants and longer-term residents. In addition to gender×age and birth order fixed effects, all regressions control for indicators for whether the mother and father have infrequent names. Standard errors are clustered by county in 1870.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

K Additional Empirics

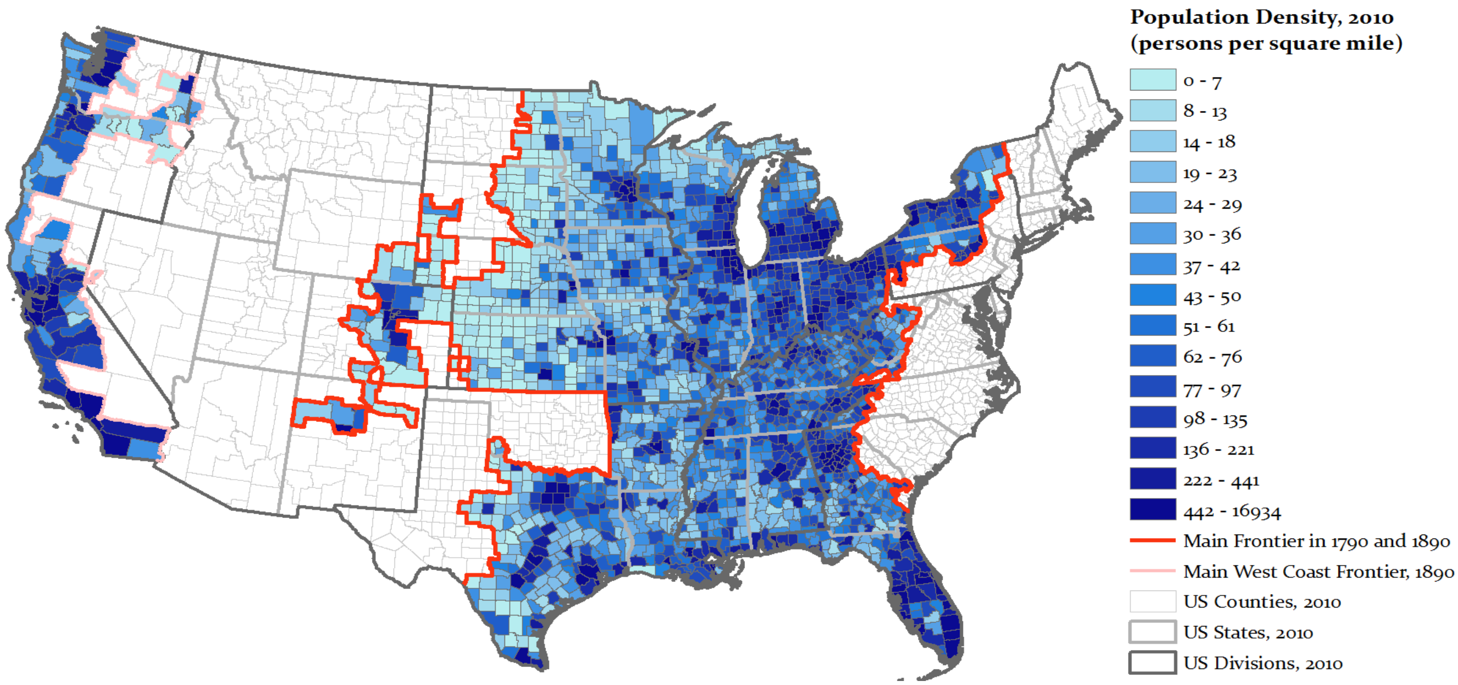
K.1 Comparing Total Frontier Experience and Current Population Density

Figure K.1: TFE is Distinct from Current Population Density

(a) Total Frontier Experience, 1790 to 1890



(b) Population Density, 2010



Notes: Panel (a) reproduces Figure 3, and (b) presents a similarly scaled map of population density in 2010 for the same counties.

K.2 Full Elaboration of Additional Controls in Table 5

Table K.1: Robustness (III): Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel (a): Infrequent Name Share in 1940 (normalized)														
total frontier experience	0.144***	0.146***	0.141***	0.146***	0.144***	0.147***	0.108***	0.144***	0.139***	0.111***	0.132***	0.097***	0.153***	0.092***
	(0.021)	(0.020)	(0.021)	(0.020)	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)	(0.019)	(0.020)	(0.022)	(0.020)	(0.019)
ruggedness		4.980***												3.426***
		(0.863)												(0.597)
rainfall risk			3.303*											0.176
			(1.878)											(1.501)
distance to nearest portage site				0.049										0.035
				(0.045)										(0.032)
distance to nearest mineral discovery site					0.098***									0.102***
					(0.026)									(0.022)
distance to nearest Indian battle						-0.042								-0.061**
						(0.037)								(0.025)
slave population share, 1860							-0.013***							-0.008***
							(0.002)							(0.001)
sex ratio, 1890								-0.397***						-0.272***
								(0.130)						(0.084)
immigrant share, 1890									-0.006					0.033***
									(0.005)					(0.009)
scottish/irish immigrant share, 1890										-0.266***				-0.192***
										(0.020)				(0.019)
birthplace diversity, 1890											-1.065***			-1.605***
											(0.273)			(0.604)
years connected to railroad by 1890												-0.021***		-0.012***
												(0.002)		(0.002)
manufacturing employment share, 1890													-0.076***	-0.054***
													(0.006)	(0.005)
Oster δ for $\beta = 0$	-13.06	-10.81	-53.62	-8.12	-13.69	-6.57	1.33	-12.62	28.05	2.20	5.27	1.25	-6.61	1.75
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
Mean of Dependent Variable	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R^2	0.61	0.62	0.61	0.61	0.61	0.61	0.62	0.61	0.61	0.67	0.61	0.66	0.66	0.76
Panel (b): Non-Patronymic/-Matronymic Name Share in 1940 (normalized)														
total frontier experience	0.228***	0.229***	0.229***	0.228***	0.228***	0.235***	0.170***	0.228***	0.195***	0.183***	0.187***	0.161***	0.241***	0.112***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)	(0.031)	(0.030)	(0.028)	(0.026)	(0.027)	(0.026)	(0.029)	(0.022)
ruggedness		2.692***												-0.102
		(0.793)												(0.498)
rainfall risk			-1.096											-4.202***
			(2.202)											(1.585)
distance to nearest portage site				-0.006										0.018
				(0.069)										(0.041)
distance to nearest mineral discovery site					0.002									0.006
					(0.034)									(0.025)
distance to nearest Indian battle						-0.086*								-0.066**
						(0.050)								(0.034)
slave population share, 1860							-0.021***							-0.019***
							(0.003)							(0.002)
sex ratio, 1890								-0.546***						-0.298***
								(0.174)						(0.068)
immigrant share, 1890									-0.041***					0.021***
									(0.005)					(0.007)
scottish/irish immigrant share, 1890										-0.361***				-0.165***
										(0.029)				(0.021)
birthplace diversity, 1890											-3.674***			-3.302***
											(0.291)			(0.559)
years connected to railroad by 1890												-0.030***		-0.019***
												(0.002)		(0.002)
manufacturing employment share, 1890													-0.108***	-0.066***
													(0.008)	(0.006)
Oster δ for $\beta = 0$	-6.57	-6.79	-6.22	-6.69	-6.57	-4.65	2.85	-7.44	8.16	6.52	6.80	3.31	-9.53	1.85
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
Mean of Dependent Variable	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
R^2	0.36	0.36	0.36	0.36	0.36	0.36	0.40	0.37	0.42	0.47	0.45	0.47	0.46	0.65
Panel (c): Mean Government Preferences Outcomes (CCES), 2006–16														
total frontier experience	0.014***	0.013***	0.014***	0.014***	0.014***	0.013***	0.014***	0.014***	0.011***	0.011***	0.010***	0.012***	0.011***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
ruggedness		-0.332***												-0.337***
		(0.110)												(0.129)
rainfall risk			-0.400											-0.125
			(0.297)											(0.337)
distance to nearest portage site				0.000										-0.000
				(0.000)										(0.000)
distance to nearest mineral discovery site					0.000									0.000
					(0.000)									(0.000)
distance to nearest Indian battle						0.000								0.000
						(0.000)								(0.000)
slave population share, 1860							0.000							0.000
							(0.000)							(0.000)
sex ratio, 1890								0.008						-0.008
								(0.018)						(0.019)
immigrant share, 1890									-0.002***					0.001
									(0.000)					(0.002)
scottish/irish immigrant share, 1890										-0.014***				-0.005*
										(0.002)				(0.003)
birthplace diversity, 1890											-0.152***			-0.067
											(0.025)			(0.117)
years connected to railroad by 1890												-0.001***		-0.001***
												(0.000)		(0.000)
manufacturing employment share, 1890													-0.004***	-0.002***
													(0.001)	(0.001)
Oster δ for $\beta = 0$	6.45	6.20	7.07	6.82	6.41	5.50	7.30	6.59	3.67	3.32	3.28	4.22	3.70	2.12
Number of Counties	112,759	112,759	112,759	112,759	112,759	112,759	111,808	112,759	112,759	112,759	112,759	112,759	112,759	111,808
Mean of Dependent Variable	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41
R^2	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
Panel (d): County Property Tax Rate in 2010														
total frontier experience	-0.034***	-0.034***	-0.034***	-0.033***	-0.034***	-0.033***	-0.033***	-0.034***	-0.028***	-0.026***	-0.027***	-0.023***	-0.036***	-0.023***
	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
ruggedness		-0.959***												-0.724***
		(0.219)												(0.208)
rainfall risk			0.959*											0.736
			(0.581)											(0.516)
distance to nearest portage site				0.026										0.006
				(0.019)										(0.016)
distance to nearest mineral discovery site					0.026***									0.024***
					(0.009)									(0.008)
distance to nearest Indian battle						-0.004								0.002
						(0.011)								(0.009)
slave population share, 1860							0.000							-0.001
							(0.000)							(0.001)
sex ratio, 1890								-0.010						-0.024
								(0.027)						(0.024)
immigrant share, 1890									0.007***					-0.003
									(0.001)					(0.003)
scottish/irish immigrant share, 1890										0.059***				0.029***
										(0.008)				(0.006)
birthplace diversity, 1890											0.636***			0.541**
											(0.084)			(0.244)
years connected to railroad by 1890												0.005***		0.003***
												(0.000)		(0.000)
manufacturing employment share, 1890													0.016***	0.008***
													(0.002)	(0.002)
Oster δ for $\beta = 0$	-207.01	-8.82	-3.58	2.35	-365.67	7.11	6.74	-643.51	0.54	0.48	0.48	0.28	-2.17	0.42
Number of Counties	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029	2,029
Mean of Dependent Variable	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02	1.02
R^2	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.83	0.83	0.83	0.83	0.83	0.85
Panel (e): Republican Vote Share, Average 2000–16														
total frontier experience	2.055***	2.050***	2.115***	2.095***	2.055***	2.172***	1.399***	2.060***	1.715***	1.717***	1.689***	1.640***	2.137***	0.931***
	(0.349)	(0.349)	(0.338)	(0.344)	(0.350)	(0.351)	(0.361)	(0.347)	(0.328)	(0.340)	(0.327)	(0.361)	(0.350)	(0.316)
ruggedness		-11.710												-34.668***
		(11.247)												(9.508)
rainfall risk			-77.882*											-108.600***
			(42.010)											(36.779)
distance to nearest portage site				0.930										0.966
				(0.777)										(0.623)
distance to nearest mineral discovery site					0.191									0.137
					(0.406)									(0.344)
distance to nearest Indian battle						-1.393**								-0.794*
						(0.603)								(0.471)
slave population share, 1860							-0.237***							-0.270***
							(0.035)							(0.034)
sex ratio, 1890								-5.000***						-1.948**
								(1.856)						(0.940)
immigrant share, 1890									-0.417***					-0.217
									(0.051)					(0.198)
scottish/irish immigrant share, 1890										-2.738***				-1.427***
										(0.263)				(0.243)
birthplace diversity, 1890											-32.582***			-4.970
											(3.563)			(13.286)
years connected to railroad by 1890												-0.186***		-0.089***
												(0.026)		(0.025)
manufacturing employment share, 1890													-0.689***	-0.422***
													(0.090)	(0.075)
Oster δ for $\beta = 0$	-8.68	-9.07	-6.50	-6.83	-8.68	-5.15	2.34	-9.05	7.29	7.35	6.69	4.51	-7.88	1.35
Number of Counties	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
Mean of Dependent Variable	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04	60.04
R^2	0.33	0.33	0.34	0.33	0.33	0.34	0.38	0.34	0.38	0.38	0.39	0.36	0.36	0.49
State Fixed Effects	✓	✓	Yes	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Geographic/Agroclimatic Controls	✓	✓	Yes	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table reproduces the estimates from Table 5, showing the coefficient estimates for the additional variables listed at the top of that table. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

K.3 Addressing Potential Individual-Level Confounders of Policy Preferences

Many of the policies in Tables 3 and E.2 elicit strong partisanship within the U.S. as Republicans and Democrats hew closely to the party line. However, as seen in Tables K.2, greater TFE is associated with stronger opposition to government intervention even after controlling for the strength of Republican party support reported in the CCES. Moreover, these results survive further controls for individual education and family income. Again, although these covariates are "bad controls," their inclusion helps rule out the concern that all of the observed effects are driven by prolonged frontier experience simply leading to tribal party- and class-based identity unrelated to the deep roots of frontier culture.

Table K.2: Robustness to Controls for Income, Education, and Partisan Identification

Dependent Variable:	Prefers Cutting Public Spending on Welfare		Prefers Balancing Budget By Cutting Spending		Prefers Repealing Affordable Care Act		Opposes Increasing Minimum Wage		Opposes Banning Assault Rifles		Opposes EPA Regulation of CO_2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
total frontier experience	0.006*	0.006*	0.011***	0.009***	0.022***	0.019***	0.020**	0.015**	0.016***	0.015***	0.017***	0.016***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.008)	(0.007)	(0.004)	(0.004)	(0.004)	(0.003)
identifies as very strong Republican		0.299***		0.379***		0.415***		0.457***		0.284***		0.338***
		(0.008)		(0.006)		(0.009)		(0.017)		(0.008)		(0.009)
family income > USD 50,000		0.099***		0.048***		-0.019***				-0.004		0.019***
		(0.006)		(0.005)		(0.007)				(0.005)		(0.006)
education > high school		0.007		-0.007		-0.080***		0.076***		0.015**		0.018***
		(0.006)		(0.005)		(0.006)		(0.014)		(0.007)		(0.006)
Oster δ for $\beta = 0$	2.62	2.57	5.96	4.86	7.69	5.48	10.33	4.95	12.78	10.02	11.92	10.48
Number of Counties	47,851	47,851	80,155	80,155	26,131	26,131	4,618	4,618	26,093	26,093	25,938	25,938
Mean of Dependent Variable	0.39	0.39	0.38	0.38	0.53	0.53	0.30	0.30	0.36	0.36	0.31	0.31
R^2	0.04	0.10	0.03	0.11	0.05	0.14	0.05	0.21	0.09	0.13	0.08	0.14

Notes: This table subjects the results in Table E.2 to additional, non-predetermined controls for education, family income, and Republican Party identification as described in Appendix L. Standard errors are clustered based on the grid-cell approach of Bester, Conley and Hansen (2011) as detailed in Section 4.1. The Oster (2019) tests are with reference to a baseline specification with no controls.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

K.4 The Parasite-Stress Theory of Values

The parasite-stress theory of values due to Thornhill and Fincher (2014) argues that the prevalence of infectious diseases leads to higher levels of in-group assortative sociality, which they associate with collectivism, as an adaptive response that minimizes contagion. In the context of our study, this theory might suggest that frontier individualism resulted from the low prevalence of infectious diseases on the frontier. However, this potential mechanism does not arise in historical narratives. Nor do we find evidence of differential disease prevalence or morbidity on the frontier. As seen in Table K.3 below, the prevalence of pathogens—associated with tuberculosis, malaria, and typhoid, among other diseases considered in Gorodnichenko and Roland (2016)—does not exhibit any differential intensity on the frontier. We can measure the incidence of these specific infectious diseases as well as a broad array of other illnesses for the first time in the 1880 Population Census. Adopting specifications similar to Table 1, we find little evidence that individuals living on the frontier had differential (infectious) disease or illness. If the parasite-stress mechanism were salient, we would find that frontier locations exhibit significantly less prevalence of infectious diseases. While the relatively precise zeros in the table may be specific to 1880, this provides suggestive evidence that the parasite-stress channel is not a first-order factor in explaining the differential individualism on the frontier.

Table K.3: No Differential Infectious Diseases or Sickness on the Frontier

Dependent Variable:	Share of Pop. with Infectious Disease		Share of Pop. with Any Illness	
	(1)	(2)	(3)	(4)
on the frontier	0.0001		0.0009	
	(0.0003)		(0.0013)	
near frontier line		-0.0001		0.0011
		(0.0001)		(0.0007)
low population density		0.0000		0.0001
		(0.0002)		(0.0010)
Mean Dependent Variable	0.001	0.001	0.009	0.009
Number of County-Years	1,780	1,780	1,780	1,780
R^2	0.05	0.05	0.08	0.08

Notes: This table reports estimates of the relationship between frontier definitions and the share of the county with any of the infectious diseases considered in Gorodnichenko and Roland (2016) (columns 1–2) and any illness (column 3–4). The infectious diseases of interest include tuberculosis, malaria, and typhus. The specification is otherwise similar to that in Table 1, with Census division FE and standard errors clustered using the grid-cell approach of Bester, Conley and Hansen (2011). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

L Data Sources and Construction

Harmonization to 2010 Boundaries

We harmonize all historical Census data to the 2010 boundaries using an approach suggested in Hornbeck (2010). First, we intersect the county shapefiles from each of the decadal census years with the 2010 county shapefile and calculate the area of each intersection. When the 2010 county falls in one or more counties of the earlier shapefile, each piece of the 2010 county is assigned a value equal to the share of the area of the piece in the earlier county multiplied by the total value of the data for the earlier county. Then, the data for each county in 2010 is the sum of all the pieces falling within its area. This harmonization procedure would be exact if all the data from the various years are evenly distributed across county areas.

Demographic Variables and Individualism

Population density. Population/area. Digitized U.S. Census data on population for every decade in 1790–2010, from Manson et al. (2019). The data on area is calculated using the 2010 county shapefiles from NHGIS (Ruggles et al., 2019) using GIS software. The county-level population data along with other pre-2010 data are harmonized to the 2010 county boundaries and the data for intercensal years is imputed using the procedure detailed in Section A. The population density figures include slaves in the antebellum period but exclude most Native Americans throughout the frontier era as they were not counted by the Census.

Sex Ratio. Whites males/white females. The data is available for every decade in 1790-1860, 1880 and 1890. Data source: (Ruggles et al., 2019).

Prime Age Adult Share. Whites aged 15–49/all whites. The data used is consistently available for every decade in 1830-1860. Data source: (Ruggles et al., 2019).

Illiteracy. Illiterate whites aged above 20/whites aged over 20. The variable is available consistently for 1840 and 1850. Data source: (Ruggles et al., 2019).

Immigrant Share. Foreign born/population. The variable used is available for every decade in 1820-1890 (excluding 1840). Data source: (Ruggles et al., 2019).

Out of State Born Share. Out-of-state born/population. The variable is consistently available for every decade in 1850-1880. Data source: (Ruggles et al., 2019).

Land inequality. Gini index using distribution of farm sizes, based on county level data on the number of farms of sizes 0–10, 10–19, 20–49, 50–99, 100–499, 500–1000, and above 1000 acres. Available for every decade in 1860-1890. (Ruggles et al., 2019).

Infrequent Children Names. White Children Aged 0–10 with Non-Top 10 First Names in Division/White Children Aged 0-10. We also construct similar variables further restricting to children aged 0–10 with native parents, and native grandparents. In addition, for the same sample, we construct additional variables by calculating the popularity of names at the national level instead of the Census division. We use the following procedure to generate the name shares: start by restricting the sample as desired (e.g. white children aged 0-10 with native parents), then calculate the number of children in the county for each given name, then using that value identify the top 10 given names within the census division (or nationally), and then accordingly count the number of children in that county with the identified top 10 names in their corresponding census division. The variables restricting to white children aged 0-10 is available for every decade in 1850–1940 (excluding 1890), with further native-parent restriction for 1850 and 1880-1940 (excluding 1890), and with grandparent restriction for 1880–1940 (excluding 1890).

To give some examples, in 1850 the top 10 boy names nationally in descending order of popularity were John, William, James, George, Charles, Henry, Thomas, Joseph, Samuel and David. Meanwhile, a random sample of less common names (outside the top 25) includes ones like Alfred, Nathan, Patrick, Reuben, Herbert, Matthew, Thaddeus and Luke. For girls, the top 10 include Mary, Sarah, Elizabeth, Martha, Margaret, Nancy, Ann, Susan, Jane, and Catherine while less common names (outside the top 25) include ones like Rachel, Susannah, Nina, Olive, Charlotte, Lucinda, and Roxanna. By 1880, the rankings shifted only slightly for boys with Samuel falling outside the top 10 and Harry entering. For girls, the changes were a bit more dramatic with the new top 10 list being Mary, Sarah, Emma, Ida, Minnie, Anna, Annie, Martha, Cora, and Alice. Data source: The NAPP full count census data for 1850 and the *Ancestry* data collected by NBER for 1860–1940.

Economic Status. We measure economic status using the occupational score (*occscore*) provided by the North Atlantic Population Project: Complete Count Microdata. Both measures range from 0 to 100, and capture the income returns associated with specific occupations in the 1950 Census while the *sei* measure additionally captures notions of prestige as well as educational attainment. Data source: (Ruggles et al., 2019).

Public Goods, Local Taxation and Spending

Local Tax Revenue Per Capita. County taxes and other local taxes (town, city, etc) divided by county population. Data Source: Manson et al. (2019).

Local Public Debt Per Capita. County-level public debt and other local debt (town, city, etc) divided by county population. Data Source: Manson et al. (2019).

Infrastructure. Rail Access and Canal Access are indicator variables for whether the county is intersected by a railroad line or a canal, respectively. Data source: Atack et al. (2010).

Access to Irrigation Dam is an indicator on whether the county has an irrigation dam within its boundaries. We also generate similar variables separately by ownership of the dams (federal, state, local and private). Data source: Washington (2018).

Survey-Based Cultural Outcomes

Some of our key measures of contemporary preferences for government policy are based on data from multiple rounds of three widely used, nationally representative surveys: the Cooperative Congressional Election Study (CCES), the General Social Survey (GSS), and the American National Election Study (ANES). These surveys are staples in the social science literature on political preferences and social norms. For instance, Acharya, Blackwell and Sen (2016) uses CCES and ANES in a related methodological setting, and Alesina and Giuliano (2011) conducts a thorough investigation of the determinants of preferences for redistribution using the GSS. The CCES is a web-based survey conducted every two years, the ANES is an in-person survey conducted annually since 1948, and the GSS is an in-person survey conducted annually since 1972. All three are repeated cross-sections.

One advantage of working with three surveys is that we can cross-validate the findings across surveys that ask different questions about similar underlying preferences. For example, the CCES asks respondents if and how respondents would like state-level welfare spending to change whereas the ANES asks respondents if and how federal spending on the poor should change. The CCES also includes a set of questions on policy issues such as gun ownership that are particularly relevant to some of the mechanisms driving the persistence of frontier culture. For all measures, we link county-level identifiers in the underlying data to the 2010 county boundaries.

Despite their rich level of detail, these surveys have one important limitation for our purposes, namely the limited geographic scope. The three surveys are nationally representative, but their coverage differs. While the CCES has broad spatial coverage, the GSS and ANES do not (see Appendix

Figures L.1). Despite its broader coverage, the CCES has the potential disadvantage that it captures an internet-savvy sample that may not be reflective of the underlying population in the way that an in-person survey generally would. This is particularly disadvantageous given our focus on county-level variation in TFE across a swathe of the United States outside of major coastal population centers.

Prefers Cutting Public Spending On Poor. The Prefers Cutting Public Spending On Poor is an indicator variable based on the following survey question: "*Should federal spending be increased, decreased, or kept about the same on poor people?*" The variable takes a value of 1 if the respondent answered "*decreased*" and 0 otherwise, and it is available for 1992 and 1996. Data source: The American National Election Studies Cumulative Data (2012). The ANES is a large, nationally-representative survey of the American electorate in the United States taken during the presidential and midterm election years. See Appendix Figure L.1(a) for the map of the maximum survey coverage in the final sample of ANES data merged with the frontier related data.

Prefers Cut Public Spending on Welfare. This is an indicator variable based on the following survey question: "*State legislatures must make choices when making spending decisions on important state programs. Would you like your legislature to increase or decrease spending on Welfare? 1. Greatly Increase 2. Slightly Increase 3. Maintain 4. Slightly Decrease 5. Greatly Decrease.*" Prefers Cut Public Spending on Welfare takes a value of 1 if the respondent answered "*Slightly Decrease*" or "*Greatly Decrease*" and 0 otherwise. The data is available in the 2014 and 2016 waves. Data source: Cooperative Congressional Election Study (Ansolabehere and Schaffner, 2017) Common Content surveys. The CCES was formed in 2006, through the cooperation of several academic institutions, to study how congressional elections, representation and voters' behavior and experiences vary with political geography and social context using very large scale national surveys. The 2014 and 2016 CCES surveys were conducted over the Internet by YouGov using a matched random sample methodology. The Common Content portion of the survey, which contains our variables of interest, surveyed 56,200 adults in 2014 and 64,600 adults in 2016. See Appendix Figure L.1(b) for the map of the maximum survey coverage in the final sample of GSS data merged with frontier-related data.

Believes Government Should Redistribute. Based on the following survey question: "*Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Here is a card with a scale from 1 to 7.*" We have recoded the variable so that it is increasing in preference for redistribution, where a score of 1 means that the government should not concern itself with reducing income differences and a score of 7 means the government ought to reduce the income differences between rich and poor. The Believes Government Should Redistribute is a normalized version of the above variable, and it is available in our sample for 1993 and all even years between 1994-2016. Data source: The General Social Survey (Smith, Marsden, Hout and Kim, 2015). The GSS is a repeated cross-sectional survey of a nationally representative sample of non-institutionalized adults who speak either English or Spanish. The surveys has been conducted since 1972, almost every year between 1972-1993 and biennial since 1994. While the sample size for the annual surveys was 1500, since 1994 the GSS administers the surveys to two samples in even-numbered years, each with a target sample size of 1500. The surveys provide detailed questionnaires on issues such as national spending priorities, intergroup relations, and confidence in institutions. See Appendix Figure L.1(c) for the map of the maximum survey coverage in the final sample of CCES merged with frontier related data.

Prefers Reducing Debt by Cutting Spending. The variable is based on the CCES survey question: *"The federal budget deficit is approximately [$ year specific amount] this year. If the Congress were to balance the budget it would have to consider cutting defense spending, cutting domestic spending (such as Medicare and Social Security), or raising taxes to cover the deficit. Please rank the options below from what would you most prefer that Congress do to what you would least prefer they do: Cut Defense Spending; Cut Domestic Spending;*

Raise Taxes.". While this question varies slightly from year to year, the underlying theme is the same. The Prefers Reducing Debt by Cutting Spending variable takes a value of 1 if the respondent chose "Cut Domestic Spending" as a first priority. The data is available for 2006-2014 (excluding 2013). Data source: Ansolabehere and Schaffner (2017).

Index of Preferences for Spending Cuts. The index is the principal component of nine dummy variables that take the value of 1 if the respondents answers "too much" to the following questions: "*We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. First (READ ITEM A) . . . are we spending too much, too little, or about the right amount on (ITEM)?*". The items considered are improving and protecting the environment, improving healthcare, solving big city problems, halting increasing crimes, dealing with drug addictions, improving the education system, improving conditions for blacks, military spending, foreign aid, welfare, and roads. The variable is available in our sample for 1993 and all even years between 1994-2016. Data source: Smith et al. (2015).

Opposes Affordable Care Act. Based on the CCES survey question: *"The Affordable Health Care Act was passed into law in 2010. It does the following: Requires all Americans to obtain health insurance, Prevents insurance companies from denying coverage for pre-existing condition, Allows people to keep current health insurance and care provider, and Sets up national health insurance option for those without coverage, but allows states the option to implement their own insurance system. Would you have voted for the Affordable Care Act if you were in Congress in 2010?"* The Prefers Repealing Affordable Care Act variable takes a value of 1 if the respondent answers "Yes" and 0 if the answer is "No". The data is available for 2014. Data source: Ansolabehere and Schaffner (2017).

Opposes Increasing Minimum Wage. Based on the survey question:*"As you may know, the federal minimum wage is currently $5.15 an hour. Do you favor or oppose raising the minimum wage to $7.25 an hour over the next two years, or not?"*. The variable Opposes Increasing Minimum Wage takes a value of 1 if the respondent choses "oppose" and 0 otherwise. Available in 2007. Data source: Ansolabehere and Schaffner (2017).

Opposes Banning Assault Rifles. Based on the CCES survey question: *"On the issue of gun regulation, are you for or against for each of the following proposal? proposal: banning assault rifles"*. Opposes Banning Assault Rifles takes value 1 if the respondent is against banning assault rifles and 0 otherwise. Available for 2014. Data source: Ansolabehere and Schaffner (2017).

Opposes EPA Regulations of CO_2 Emissions. Based on the CCES survey question *"Do you support or oppose each of the following proposals? proposal: Environmental Protection Agency regulating Carbon Dioxide emissions."* The Opposes EPA Regulations of CO_2 Emissions takes one if the respondent supports the proposal and 0 the respondent opposes. Available for 2014. Data source: Ansolabehere and Schaffner (2017).

Cooperation vs. Self-Reliance. Based on the survey question: *"I am going to ask you to choose which of two statements I read comes closer to your own opinion. You might agree to some extent with both, but we want to know which one is closer to your views: ONE, it is more important to be a cooperative person who works well with others; or TWO, it is more important to be a self-reliant person able to take care of oneself"*. The Cooperation vs. Self-Reliance variable takes a value of 1 if the respondent chooses "cooperative" and 0 otherwise. Available in 1990. Data source: The American National Election Studies.

Identifies As A Strong Republican. An indicator variable that takes 1 if the respondent identifies as a "Strong Republican." Available for 2007, 2012, 2014 and 2016. Data source: (Ansolabehere and Schaffner, 2017).

Other Long-run Outcomes

County Property Tax Rate. The average effective property tax rates per $100 of value, calculated at the county level as the ratio of the average real estate tax over the average house value. Data source: The data is obtained from the National Association of Home Builders, which calculated the average effective property tax rates based on the 2010-2014 American Community Survey (ACS) data from the Census Bureau.

Republican Vote Share in Presidential Elections. Votes for a GOP candidate/total votes, at the county level. For simplicity, we only consider the five presidential elections since 2000. Data source: Dave Leip's Atlas of U.S. Presidential Elections (2017).

Geographic and Agroclimatic Controls

Land productivity measures. Average of attainable yields for alfalfa, barley, buckwheat, cane sugar, carrot, cabbage, cotton, ax, maize, oats, onion, pasture grasses, pasture legumes, potato, pulses, rice, rye, sorghum, sweet potato, tobacco, tomato, and wheat. We normalize each product's values dividing it by the maximum value for that product in the sample. Measures of attainable yields were constructed by the FAO's Global Agro-Ecological Zones project v3.0 (IIASA/FAO, 2012) using climatic data, including precipitation, temperature, wind speed, sunshine hours and relative humidity (based on which they determine thermal and moisture regimes), together with crop-specific measures of cycle length (i.e. days from sowing to harvest), thermal suitability, water requirements, and growth and development parameters (harvest index, maximum leaf area index, maximum rate of photosynthesis, etc). Combining these data, the GAEZ model determines the maximum attainable yield (measured in tons per hectare per year) for each crop in each grid cell of 0.083×0.083 degrees. We use FAO's measures of agroclimatic yields (based solely on climate, not on soil conditions) for intermediate levels of inputs/technology and rain-fed conditions.

Area. The log of surface area in square miles, calculated using the 2010 county shapefiles from NHGIS using GIS software. Data source: Ruggles et al. (2019).

Temperature. County-level mean annual temperature measured in Celsius degrees. Data source: IIASA/FAO (2012).

Rainfall. County-level average annual precipitation measured in mm. Data source: IIASA/FAO (2012).

Elevation. County-level average terrain elevation in km. Data source: IIASA/FAO (2012).

Latitude. Absolute latitudinal distance from the equator in decimal degrees, calculated from the centroid of each county using GIS software and county shapefiles from NHGIS. Data source: Ruggles et al. (2019).

Longitude. Absolute longitudinal distance from the Greenwich Meridian in decimal degrees, calculated from the centroid of each county using GIS software and county shapefiles from NHGIS. Data source: Ruggles et al. (2019).

Distance to the coastline, rivers, and lakes. Minimum distance to a point in the coastline, rivers, and lakes in km, calculated from the centroid of each county using GIS software and county shapefiles from NHGIS. Data source: Ruggles et al. (2019).

Additional Variables

Annual Migration Inflow. Total number of migrants entering the United States every year. The data for 1820–1890 is available from the Migration Policy Institute (2016), which tabulates data from the Office of Immigration Statistics, while the data for 1790–1819 is imputed from Tucker (1843). To construct the instrumental variable based on annual migration inflows predicted by weather shocks in Europe, we use the annual migration inflows to the U.S. from Belgium, Denmark, England, France, Germany, Greece, Ireland, Italy, Norway, Poland, Portugal, Russia, Scotland, Spain, Sweden, Wales from 1820–1890. Data source: Willcox (1929).

Years Connected to Railroad by 1890. The number of years since the county is first intersected by railroad to 1890. Data source: Atack et al. (2010).

Birthplace diversity, 1890. We take $1 - \sum_o (birthplace_{oc}/population_c)^2$, which is simply 1 minus the Herfindahl concentration index for origin o birthplace diversity in county c in 1890. Birthplaces include US or a given country or country grouping abroad. Data source: Ruggles et al. (2019)

Ruggedness. County-level average Terrain Ruggedness Index computed using 30-arc grid data on terrain variability. Data source: Nunn and Puga (2012).

Distance to nearest portage site. Minimum distance from county centroid to the nearest portage site, which is defined as the location where a river basin intersects the fall line. Data source: Bleakley and Lin (2012).

Manufacturing Employment Share. County-level percent of employment in manufacturing industries in 1890. Data source: Ruggles et al. (2019).

Distance to Nearest Mine. Minimum distance from county centroid to a site where there was a mineral discovery before 1890. The data is from the Mineral Resources Data System (MRDS) edited by the US Geological Survey. Data source: McFaul et al. (2000).

Distance to nearest Indian battle sites. Minimum distance from county centroid to major Indian battle sites. The battles sites are digitized using a map from McFaul et al. (2000).

Immigrant share, 1890. County-level percent of foreign born population in 1890. Data source: Ruggles et al. (2019).

Scottish and Irish immigrant share, 1890. County-level percent of population born in Scotland or Ireland in 1890. Data source: Ruggles et al. (2019)

Slave population share, 1860. County-level percent of slave population in 1860. Data source: Ruggles et al. (2019).

Rainfall Risk. Following Ager and Ciccone (2017), county level rainfall risk is constructed as the variance of the annual average log monthly rainfall from 1895-2000. Data source: Oregon State University (2018).

Linked Sample 1850–1880

This section provides a step-by-step description of how we construct the 1850 to 1880 linked sample and how we select the final sample used for the analyses in Section 5.2. The linking procedure closely follows the approach detailed in Feigenbaum (2016).

Constructing the linked sample

1. Select the sample to be linked from the 1850 full count census: First, using the full count 1850 census, we select all white males aged 0-20 in 1850. As is common in the literature, we restrict the linking sample to only males because last names are key in the linking process and the task of linking women across censuses is much more difficult given the frequency with which women change their last names in marriage. We keep the information on each individual's first name, last name, age and birth country. Call this dataset X1.

2. Identify all targets that could be potential matches from the 1880 census: The linking procedure requires that for each individual in 1850, the target person in 1880 has the same gender, birth country and within

+/- 3 years of the predicted age from the 1850 census. In addition, to limit the set of possible links, we impose an additional restriction that the potential link has a first name Jaro-Winkler distance of less than .2, and a last name Jaro-Winkler distance of less than .2. Call this dataset X2.

3. Create all possible matching pairs between X1 and X2. For each male aged 0-20 in 1850, we create a dataset that contains all the possible matching pairs with the candidates we identified in Step 2. Call this dataset X1X2. Hence, X1X2 contains all the possible match pairs for the 5,329,750 white men we extracted from the 1850 census.

4. Create a training dataset based on manual linking of a random subset of all potential pairs: For a random sample of 10,000 individuals in X1, we extract all the possible matches from the X1X2 dataset. The paired data contains information on first name, last name and age of the individuals both in 1850 and 1880. After discarding matches with very small link probabilities based on first and last name similarity, we are left with a random sample of potential matching pairs for 8,557 white boys from 1850 matched with all potential targets in 1880.

Then, experienced research assistants examined the randomly sampled dataset and decided which pairs formed true matches by comparing the similarities in first name, last name and age for each possible link. If there are no good matches for a given unique record in X1, then all the links were marked as non-match. The manual linking procedure generates a training dataset which contains a random subset of X1X2 in which we have determined that a record is either a match or not a match. The research assistants were able to locate 42.7% (n=3,654) of our randomly selected 8,557 white boys from 1850 in the 1880 census.

5: Construct the training algorithm: The algorithmic approach aims to approximate the best efforts of the researcher assistants in the manual linking procedure to assess matches. In particular, using the sample of manually linked matches from Step 4, we train an algorithm that predicts the likelihood of a match between all potential match pairs from 1850 and 1880 in the X1X2 sample.

The algorithm considers several variables that are constructed to capture similarities based on first name, last name and age similarity which aim to describe the features of the potential matches. The algorithm, which is a probit model, provides us with weights that we can put on the aforementioned variables when deciding on which links to consider as matches.

6: Apply the algorithm generated using the training dataset to the full X1X2 data: Using the same set of variables and the algorithm produced in Step 5, we predict the probability that each pair of matches in X1X2 is a true match. We then select which pair of matches are true matches based on an optimal cutoff that minimizes the likelihood of making false negative and false positive errors. See Feigenbaum (2016) for the details.

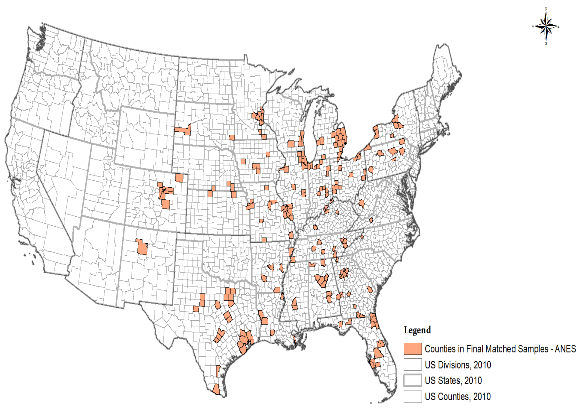
The matching process allows for multiple individuals in 1850 to be matched with a single individual in 1880. When we allow for multiple individuals from 1850 to match to a single individual in 1880, the algorithm has a match rate of 47.9% (n=2,552,950). When we restrict that only one individual in 1850 can match to an individual in 1880, the algorithm has a match rate of 33.04% (n=1,761,408). These compare favorably to other recent efforts in the literature (see footnote 1 in Appendix J).

Selection of Sample Used in Section 5.2.

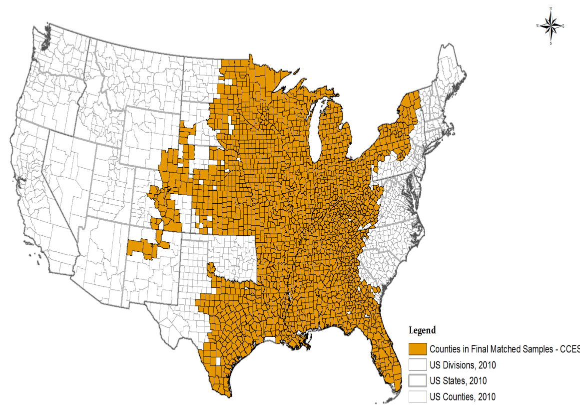
7: Identify the individuals in the linked sample who are fathers in 1880: We take our sample of linked white men that we traced from 1850 to 1880, and we keep those that are household heads with children in 1880. We then bring all the relevant data for our linked men both from the 1850 and 1880 census and match it to their household members in 1880. Then, we keep the data on all their children, both boys and girls, who are under the age of 20 when we observe them in 1880. Hence, the final sample used in the analysis contains the 1880 record of the children who are descendants of the men we were able to link between 1850 and 1880.

Figure L.1: Data Availability For Main Survey Data Sources

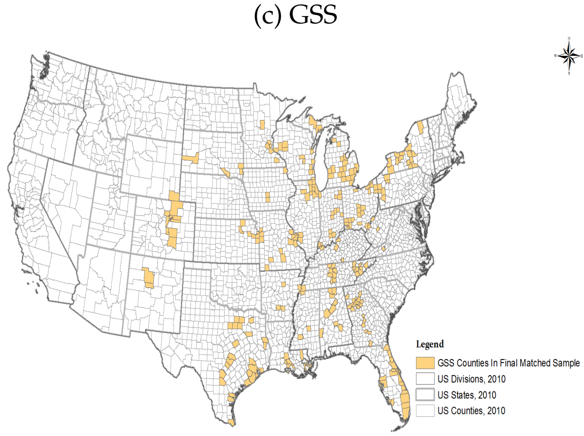
(a) ANES



(b) CCES



(c) GSS



Notes: Figures (a), (b), and (c) provide the geographical distribution of the maximum number of counties available in our baseline sample matched with the ANES, CCES, and GSS data, respectively. Additional counties are included when incorporating the West Coast sample or extending the historical frontier window to 1950 (see Section 4.4). Due to varying data availability across rounds, not all the counties in the above map are included in every regression using the corresponding survey data.

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