The Freezing Point of History: How Cold Weather Exposure Affects the Emergence of Future Leaders*

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Abstract

Adverse environmental conditions impair children's average health and development, yet the impact on "tail outcomes" remains underexplored. We combine U.S. temperature data since 1790, census data, and historical data on history-making political/cultural leaders and document that harsh winters during the first year of life reduce the likelihood of obtaining a postgraduate degree, earning in the top quartile, or leaving a notable mark on history. Effects on these tail outcomes are more pronounced than average outcomes. Our results imply the risk of a poverty trap, where early misfortune obstructs the rise of leaders capable of driving long-term growth and progress.

Keywords: poverty trap, early childhood development, environmental shocks, historical determinism

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1 Introduction

A large literature has established that harsh environmental conditions and adverse shocks early in life affect children's health, development, and long-run prospects. This suggests that the experiences of poverty, hardship, and deprivation—seen around the world today—hinder a society's ability to accumulate human capital and achieve the growth necessary to escape these hardships.

Yet, growth is not solely determined by the average human capital level but is also significantly influenced by the contributions of outstanding individuals (Jones and Olken, 2005). These contributions include the advancement of new technologies by inventors, the establishment of new firms by entrepreneurs, the development of new governance and legal frameworks by policymakers, and the advocacy for cultural and political reforms by leaders of social movements. We refer to these as "tail outcomes" in which individuals have transformative and lasting impacts on their society. Despite their pivotal role, a significant gap in empirical evidence persists regarding the influence of material or environmental hardships on the emergence of such figures.

In this paper, we study whether shocks early in life—the sort shown in the environmental literature to affect average infant health and development[1]—have long-run effects on the emergence of history-shaping individuals. To this end, we combine weather data back to 1790 with the Human Biographical Record (HBR), a comprehensive dataset of more than seven million notable individuals across recorded human history. We use this data to identify individuals' state and year of birth, and use variation in weather across states and years to ask whether harsh winters (which drive mortality, both today and historically) reduce the number of people born in a particular state-birth-year cohort who go on to shape history.

Despite evidence that harsh environmental conditions harm infant development, there are four reasons why they might not reduce the long-run leadership potential of a given birth cohort. First, elites are disproportionately drawn from well-off echelons of society (Nekoei and Sinn, 2021b) and thus better insulated from the environmental shocks (Basu, 2009; Chakma et al., 2023). Second, if history is more determined by aggregate structural forces than the influence of any particular individuals, as many historians contend (Butterfield, 1931; Carr, 1961), then shocks like the ones we study would not reduce the number of individuals subsequently appearing in the historical record. Third, early adversities can be pivotal, potentially *enhancing* tail outcomes. Notables ranging from Sheldon Adelson and Malcolm X have attributed their character and viewpoints to their upbringing in adversity. Finally, shocks affecting an entire cohort might increase the resources (such as teachers' time and attention) available to the most

[1]See Currie and Almond (2011); Xu et al. (2012); Graff Zivin and Shrader (2016); Isen et al. (2017).

promising individuals. Thus, it is *ex ante* ambiguous whether adverse conditions would amplify, diminish, or leave unchanged tail outcomes.

To understand the long-run effects of extreme weather, we compile data on daily average temperatures across US states since 1790. We combine this with date on: (*i*) mortality spanning 1900 to 1960, (*ii*) earnings and educational attainment for state/birth-year cohorts 1900–1960, and (*iii*) individuals born in each state-birth-year cohort from 1790–1960 who go on to make history, as measured by appearing in the Human Biographical Record (Nekoei and Sinn, 2021a).

We explore the effects of harsh winters by estimating how cold weather affects contemporaneous mortality and long-run outcomes of those born in those conditions. Our regressions include state and year fixed effects, as well as state-by-time-period specific trends (which we discuss below). These controls are sufficient to isolate exogenous shifts in extremely cold weather, as evidenced by the fact that we find no placebo effects of extreme cold on the outcomes of cohorts not yet born (i.e., "pre-shock trends" are parallel).[2] Interestingly, we also generally find zero effects on the outcomes of cohorts who were one or more years old at the time of the shock. This is consistent with evidence that the first year of life (as well as during pregnancy) is the period at which humans are most fragile and where environmental shocks are most important.

Consistent with the existing literature (Barreca et al., 2016), we find that colder weather (more days below 5°C) significantly increases mortality. Given that mortality is an extreme health outcome—and the only health outcome that is reliably measured back to 1900—we interpret this as evidence that very cold weather is an adverse health shock more broadly. That is, we do not interpret our subsequent effects as operating solely through the increased mortality caused by the extreme cold.

The cohorts born in cold years have significantly worse adult outcomes, including years of education and average earnings. Importantly, we find larger effects at the "top" of the outcome distribution: effects are larger on the probability of attaining the highest levels of education observable in our data (roughly 1-4% of the population in any given year) than are the effects on years of education. Likewise, effects are larger on the 90^{th} percentile of earnings than on median earnings. This suggests that extreme environmental conditions are especially important for stunting the advancement of the most promising members of society.[3]

[2]Although formal weather forecasts existed during much of the sample period, they were generally of low skill and for short horizons prior to the 1970s (Teague and Gallicchio, 2017), limiting the type of anticipatory reduction on temperature-related mortality observed in Shrader et al. (2023).

[3]These larger effects at the top of the distribution all refer to the outcome distribution and not the distribution of baseline advantage. It is more common in this literature to look at heterogeneity by baseline characteristics (Basu, 2009), with a common conclusion that it is the least well-off who are most affected by environmental shocks. None of our results contradict this conclusion. The fact that our effects are larger at the top than the middle of the *outcome* distribution does not contradict the possibility that they are larger at the bottom than the middle of the distribution of *background characteristics*.

We then estimate the effects on the number of individuals born in a given state and year who go on to appear in the HBR. We find that a one standard deviation increase in the number of very cold days leads to a 1% decrease in the number of elites to emerge from the cohort. Elites, of course, make history for all sorts of different reasons. We are particularly interested in the sorts of elites who shape history and institutions and are most likely to contribute to long-run economic development, and so we also focus specifically on political elites. Here, we see that a one standard deviation increase in cold days decreases the number of political elites by 2%. Interestingly, when we look at these effects over time, we see that the effects of extreme weather fell considerably during the 19^{th} and early 20^{th} centuries, ultimately disappearing by the middle of the 20^{th} century. We see this as an important sign of progress. Finally, we show that a cohort becomes *more* likely to produce elites when extreme weather affects its neighboring states or neighboring birth cohorts, although this effect is not statistically significant. This suggests there is some historical substitution wherein a dearth of potential leaders in one cohort is partially offset by leaders from other cohorts.

Our results shed new light on the long-run, dynamic effects of climate on human society. Previous work has documented that environmental shocks have a range of effects on contemporaneous health and human capital outcomes including mortality (Deschênes and Moretti, 2009; Carleton et al., 2022), productivity (Graff Zivin and Neidell, 2012), and education (Graff Zivin et al., 2018; Park et al., 2020) among other outcomes (Graff Zivin and Neidell, 2013; Graff Zivin and Shrader, 2016). These effects can be particularly severe for children in utero or those exposed at very young ages (Almond and Currie, 2011; Currie and Almond, 2011). More recent work has linked these childhood environmental shocks to later life outcomes for those directly affected (Isen et al., 2017) and their descendants (Colmer and Voorheis, 2020). Our results show that for a range of outcomes, effects at the top of the distribution are larger than those found on the average levels of those outcomes, which are more commonly studied in the literature.

Our results also contribute to the literature studying the long-run determinants of development, and especially work on "poverty traps" in which initial conditions of poverty and disadvantage perpetuate themselves in the long run. While numerous studies identify mechanisms by which poverty traps might emerge (Dasgupta and Ray 1986; Azariadis and Drazen 1990; Banerjee and Newman 1993; Arthur 1994; Lorentzen, McMillan, and Wacziarg 2008; Haushofer and Fehr 2014; see Azariadis and Stachurski 2005 and Ghatak 2015 for reviews), to our knowledge, the possibility that poverty might stifle the emergence of history-making elites is a new idea.[4] This is despite growing awareness among economists that the direct influence of

[4]Most closely related, Lorentzen et al. (2008) provide a model in which elevated mortality can reduce long-run growth, although the mechanism is through discount rates and savings behavior, and Lloyd-Ellis and Bernhardt (2000) provide a model in which poverty traps can persist if growth-generating entrepreneurs are rare, although the mechanism is through credit access. Our results unite these ideas, suggesting that elevated

extraordinary individuals matters for important aggregate outcomes and meaningfully shapes the direction of society (Jones and Olken, 2005; Bassi and Rasul, 2017; Dippel and Heblich, 2021; Bai, Jia, and Yang, 2023; Buggle and Vlachos, 2023; Assouad, 2023).

The remainder of the paper is organized as follows. In Section 2 we discuss our data, and in Section 3 we discuss our methods. Section 4 presents all of our results, and Section 5 concludes.

2 Data

2.1 Weather

Our weather data come from Berekeley Earth—a high-resolution, gridded temperature dataset (Rohde et al., 2013). The Berkeley Earth dataset is well-suited to our application because it incorporates more historical temperature observations than any other available temperature dataset. In the dataset, temperature measurements for some land areas in the US go back to the 1750s, with consistent coverage across the entire US by the mid to late 1780s. We restrict the sample to observations from 1790 to 1960. The data are on a 1 by 1 degree latitude-longitude grid. We aggregate the gridded measures to the state level following a similar practice to other studies of weather impacts in the US (e.g., Schlenker and Roberts 2009; Barreca et al. 2016; Carleton et al. 2022). First, for each grid cell, we calculate the fraction of days in the year that fall within eight temperature ranges (< 0°C, 0–5°C, 5–10°C, 10–15°C, 15–20°C, 20–25°C, 25–30°C, and > 30°C). Second, we merge each grid cell with gridded measures of population from HYDE version 3.2 (Klein Goldewijk et al., 2017). We use the nearest decadal measure of population to the year of weather being merged (e.g. weather from 1794 is merges with the population grid from 1790). Third, we calculate population-weighted averages of the grid-level measures within each state to arrive at the final state-by-year dataset used for estimation.

2.2 Mortality

Our data on mortality come from Barreca et al. (2016). They compile vital statistics data back to 1900 for their study of extreme heat and mortality, and how this relationship has changed over time. This data is an unbalanced panel. As the authors discuss, only 11 states reported mortality in 1900, but all began reporting by 1933. The data we use is all-cause, all-age mortality. Cause-specific and age-specific mortality rates are only available starting in 1960. By this time, extreme weather had little effect on mortality (Barreca et al., 2016), and these post-1960 birth cohorts are too recent for us to examine long-run effects on earnings and

mortality might reduce growth by preventing the emergence of some rare growth-generating entrepreneurs.

elite status. Thus, we do not systematically explore cold weather effects by age but assume that the types of weather shocks that increase total mortality have some effects on the health of infants (regardless of whether they increase the mortality of those infants).

We should note that our primary use of mortality data is to establish that extreme cold has meaningful effects on health. Mortality is obviously a very extreme health outcome. When we estimate the effects of cold weather on mortality, and then subsequently on education, earnings, and status as a history-making elite, we are not assuming that all of the long-run effects operate through increased mortality. Rather, we are assuming that any weather shock significant enough to affect mortality likely has broader effects on population health, and that these sorts of health shocks during a person's year of birth likely have long-run effects on that person's development.

2.3 Earnings and education

Our data on earnings and education outcomes are taken from the decennial census, accessed via the Integrated Public Use Microdata Series (IPUMS; Ruggles et al. 2023). We use four variables from the census: earnings (income from wages and salary), educational attainment, state-of-birth (which is reported in all years), and year of birth (which is inferred from age and year of interview). Throughout the paper, all of our state-level regressions refer to state-of-birth and not state-of-residence at the time of the interview. Since earnings only began being collected in 1940, we measure these outcomes at age 40, starting with the 1900 birth cohort.

For education, we measure years of education and a notion of "elite" education, intended to reflect the top of the education distribution. This measure is not consistent across years. It is based on the top one or two categories in the census' educational attainment variable, but those categories change over time. For instance, in 1940 and 1950, the highest category was "5 or more years of college" while in 2010 the highest category was "doctoral degree." In Appendix Table A1, we list the categories used during each year, but in general, our elite education variable covers 1-4% of the population.

For earnings, we measure average earnings (as well as various percentiles) by aggregating up to the state-by-birth-year cohort level, and only then taking logs of the average. We do not make top code adjustments because our identifying variation is within-cohort (i.e., all regressions include year fixed effects).

We aim to measure education and earnings outcomes at age 40. Of course, the census is only conducted every 10 years, and so most birth cohorts are not observed at exactly age 40. For instance, those born in 1923 or 1933 are only ever observed at age 37 and 47, not at age 40. Thus, for each state-by-birth-year cohort, we estimate age-40 earnings and education by interpolating across ages for the age immediately before 40 and the age immediately after 40. Since we interpolate based on only two observations, this is equivalent to a weighted average of those two observations. For example, for the 1923 or 1933 birth cohorts, we would calculate $\hat{y}_{40}$ as $y_{37} + \frac{3}{10}\left(y_{47} - y_{37}\right) = \frac{7}{10}y_{37} + \frac{3}{10}y_{47}$.

Note that we only interpolate across ages *within* birth cohorts. We never interpolate across birth cohorts. Thus, our measures for consecutive cohorts are independent from one another. This is important to keep in mind when we explore the timing of extreme weather effects across cohorts. There is no mechanical reason why outcomes of consecutive cohorts would be linked.

2.4 History-making elites

Our data on history-making elites comes from the Human Biographical Record (HBR), described in detail in Nekoei and Sinn (2021a). The HBR is a comprehensive dataset that combines information from traditional encyclopedias, such as Britannica, with the extensive structured and unstructured data available in Wikipedia and Wikidata. Wikidata is a collaborative, multilingual knowledge base that provides structured data to complement Wikipedia articles. The HBR extracts and harmonizes biographical information from these diverse sources using advanced machine learning techniques.

Data on an individual's birthplace in the US is aggregated at the state level and involves the following steps. Initially, we gather from all sources all potentially relevant locations that indicate an individual's birthplace. These locations are transformed from their original level of aggregation (coordinates, town, county...) to the US state-level. Discrepancies in the data, e.g. an individual having locations across two separate states, are settled through a majority vote mechanism.

Our methods for extracting the year of birth and occupations involve some modifications. The process of extracting the year of birth follows a similar process without the need for an aggregation template. To classify individuals as politicians we employ a BERT neural network (Devlin et al., 2019), a state-of-the-art model for natural language processing tasks, to analyze the text across all language versions. Each language and encyclopedia provides us with a single probability of the individual being a politician which we aggregate into a single measure.

The HBR also provides several measures to estimate the level of eliteness in the data. In particular, it contains the number of languages in which an individual appears on Wikipedia, the length of the article measured in number of words, and the pagerank of each individual (Brin and Page, 1998). Among these, we focus on individuals who appear in multiple languages as our primary proxy for particularly important individuals, although in the appendix we also include heterogeneous effects by pagerank.

2.5 Statehood

Our data on weather and elites includes observations from before states achieved statehood. HBR maps the place of birth to a current state independently of whether it was not a state at the time. For instance, HBR reports that the poet Sarah Dyer Hobart was born in 1845 in Wisconsin as she was born in Otsego, Wisconsin, although Wisconsin did not become a state until 1848. Berkeley Earth does the same for temperature.

We define our sample based on the weather data from Berkeley Earth. We begin including a "state" in our data as soon as weather data is available, regardless of whether that "state" was officially a state yet. This results in an unbalanced panel, as well as a slight abuse of the terminology "state." In our data, 34 states (including the District of Columbia) are included from 1790 on (despite there being only 13 states at the time), 42 from 1796 on, and 49 from 1821 on. Alaska and Hawaii are the last states to enter our sample, in 1828 and 1883, respectively.

2.6 Summary statistics

In Table 1 we present the summary statistics for our main measures. For each variable, we present the mean and standard deviation, as well as the interquartile range and the 90^{th} percentile. The standard deviations we present are raw, unconditional standard deviations across state-years in our data. However, many of these outcomes show significant trends. For instance, average years of education is increasing over time, but since our regressions incorporate year fixed effects, this over-time variation plays no role in identification. Thus, we always interpret the magnitude of our estimates based on a "residualized" standard deviation. To do this, we first adjust the variable by removing the influence of all fixed effects, trends, and controls included in our main analysis (which we will discuss below), and we then calculate the standard deviation of the residual outcome. This residualized standard deviation is always much lower.

3 Methods

3.1 Estimating equations

We use two primary specifications to estimate the effects of extreme weather on outcomes. Our first specification closely follows the existing literature and flexibly estimates effects of different weather conditions on outcomes. Specifically, we regress some outcome Y_{st} at the state (s) by year (t) level on temperature bins denoting the fraction of days in the year that

Table 1: Summary statistics

Variable	(1) Mean	(2) Standard dev. Raw	(3) Standard dev. Residual	(4) Percentiles 25	(5) Percentiles 75	(6) Percentiles 90
	Available 1790-1960					
Cold days	3.5	1.8	.15	2.1	5	5.2
Num. of elites (IHS)	4.2	1.2	.24	3.4	5.0	5.7
Num. political elites (IHS)	3.1	.9	.35	2.5	3.7	4.2
	Available 1900-1960					
Log mortality	10.7	.9	.028	10.1	11.4	11.9
Years education	11.1	2.9	.15	10.2	13.2	13.5
Elite education	.028	.014	.004	.018	.037	.048
Log earnings	8.9	1.2	.050	8.0	10.0	10.2
Log 90^{th} pctl. earnings	9.9	1.1	.056	9.1	10.9	11.1

All statistics are weighted by population. "IHS" denotes inverse hyperbolic sine (we study an alternative normalization in Appendix Table B2). Residual standard deviation is based on first residualizing the variable of state fixed effects, year fixed effects, state-by-period linear trends, and temperature bins of width 5°C for temperatures 5°C and above. Note that all of these are controls in our main specification. For state-by-period trends, we define time periods to be approximately 35 years long. See Section 3.1 for further discussion. Aside from mortality, all variables are approximately a balanced panel (8,269 observations 1790-1960, 2,989 observations 1900-1960). Mortality is available for 76% of state-years from 1900-1960. Mortality is defined as the total number of deaths (all causes, all ages). Cold days can be interpreted as the number of months with an average temperature below 5°C (see Section 3.2). See appendix for definition of elite education (which changes over time as the census changes its education questions). The number of elites refers to the number born in the given state-year (which includes zeros). Note that in calculating log earnings, we first calculate mean age-40 earnings by state-by-birth-year cohort, and we then take logs of the mean. Thus, we do not have to worry about zero earnings, since no state-by-birth-year cohorts have zero *average* earnings.

had an average temperature fall within a given range:[5]

$$Y_{s,t} = \alpha_s + \delta_t + \mu_{s,T(t)} + \gamma_{s,T(t)}t + \sum_k \beta_k TMEAN^k_{s,t} + \varepsilon_{s,t} \tag{1}$$

In equation (1), the key variables of interest are the variables $TMEAN^1$, $TMEAN^2$, ..., $TMEAN^K$, which capture the fraction of all days in the year which had an average temperature within a particular range. We use temperature ranges from less than 0°C, 0-5°C, 5-10°C, ..., 25-30°C, more than 30°C. Of course, because every day must fall into one of these ranges, they sum to one, and one range must be omitted as a normalization. We omit the 25-30°C range.[6] Thus, the coefficient on each $TMEAN^k$ variable can be interpreted as the effect of having more days in that temperature range rather than in the 25-30°C range.

We first estimate this equation with mortality occurring in year t as the outcome. Previous work by Barreca et al. (2016) has shown that mortality has a "U-shaped" relationship with temperature during our sample period—with both hot and cold temperatures leading to elevated mortality. Our first estimates allow us to replicate this finding using our sample and data. We then estimate the effect of weather on other outcomes that measure human capital and production of elites for different state-by-birth-year cohorts. In those specifications, t refers to year of birth.

The specification shown in equation (1) includes state and year fixed effects to account for time-invariant, cross-state differences as well as aggregate time trends. However, some of the outcomes we are interested in are measured over long time periods (e.g., the 171 years from 1790–1960). It seems unrealistic to expect state fixed effects to be constant over this full period, and also unrealistic to assume they could be captured by a parsimonious state-specific linear or quadratic time trend. Instead, we assume that they are linear only over a relatively short period of time. Thus, we divide our full sample into five periods of approximately 35 years: 1790–1825, 1826–1860, 1861–1895, 1896–1930, and 1931–1960. In equation (1), these time periods are represented by $T(t)$. In all of our regressions, we include state-by-period specific linear time trends $\left(\gamma_{s,T(t)}t\right)$. This assumes linearity in states' secular outcome trends only for a period of roughly 35 years at a time. In Appendix Table B3 we show similar results from using different lengths of "time periods," quadratic instead of linear trends, or using decade-by-Census-division fixed effects. All are similar.

[5] This empirical strategy has been widely used in economics papers studying the effect of temperature on a variety of outcomes (Deschênes and Moretti, 2009; Graff Zivin and Neidell, 2014; Barreca et al., 2016; Graff Zivin et al., 2018). The results are similar to those found by alternative flexible estimation strategies using splines or polynomials (Gasparrini et al., 2015; Carleton et al., 2022).

[6] 25-30°C is approximately the temperature range that maximizes agricultural output (Schlenker and Roberts, 2009). Mortality is minimized at or slightly below this range (Wilson et al., 2023).

The specification in equation (1) is flexible with respect to temperature because it estimates the effects of each potential temperature bin, relative to a temperature in the 25–30°C range. However, it only estimates cumulative effects of temperature up to one year from the time of the temperature shock.[7] This ignores the possibility of cross-cohort spillovers (e.g., the possibility that negative effects on one cohort might increase or decrease the opportunities available to previous and subsequent cohorts) or effects on slightly older children (e.g., that individuals might be affected by extreme weather when they are one year old, and not only the weather during the year of their birth). It also does not allow for the inspection of pre-trends.

For this reason, we also use a specification that is flexible in time. In doing so, we focus only on one specific temperature coefficient: $ColdDays_{s,t}$ which captures the fraction of days with an average temperature below 5°C. That is, $ColdDays$ pools the two lowest temperature bins included in equation (1). We focus on these two temperature bins because we typically find that those cold temperatures are where effects are the largest. Prior economics literature has focused on the highest temperature bins (i.e., the number of very hot days rather than the number of very cold days). Doing so is obviously relevant for understanding the consequences of climate change going forward. We focus on cold days for two reasons. First, throughout the historical period for which data is available, cold weather has consistently had a large effect on mortality in the US, a result that we corroborate and also find generally holds true for other outcomes. Second, during our sample period very cold days are more common than very hot ones (including after controlling for the fixed effects and time trends that we use). Thus, there is more identifying variation in the colder temperature bins than there is in the hotter ones.

In order to study the timing of our estimated effects of extreme cold, we use the following specification:

$$Y_{s,t} = \alpha_s + \delta_t + \mu_{s,T(t)} + \gamma_{s,T(t)}t + \sum_{\tau=-3}^{3} \theta_\tau ColdDays_{s,t-\tau} + \sum_k \beta_k TMEAN_{s,t}^k + \varepsilon_{s,t} \qquad (2)$$

where we obviously omit the lowest two temperature bins (which are subsumed into $ColdDays$) from the vector of temperature bins given by $TMEAN^k$. Here, the main coefficients of interest are the θ_τ coefficients showing how outcomes are affected by the leads and lags of $ColdDays$.

How should one think about these leads and lags? This depends on the outcome. For example, for mortality, it seems unlikely that mortality is affected by extreme weather that has not occurred yet.[8] In this case, the θ_τ coefficients should be zero for the leads ($\tau < 0$),

[7] Previous research argues that this is sufficient to capture intertemporal dynamics in mortality (sometimes called harvesting; see Deschênes and Moretti 2009).

[8] Particularly for weather that occurs sufficiently far in advance. One could imagine, for example, that temperature-driven crop losses documented in Schlenker and Roberts (2009) could affect mortality in a relatively autarkic, agrarian area.

and evidence to the contrary would suggest a violation of our identification assumptions, as we would expect, for instance, if our state-specific time trends are not correctly specified. For other outcomes, however, it may well be the case that θ_τ is non-zero for the leads. For instance, when we look at political elites, then *someone* must become governor of the state. A decrease in the number of political elites from one cohort directly affected by extreme weather might increase the elite status among younger cohorts who had not been born yet at the time of the shock.[9] Thus, depending on the outcome, the leads and lags may be informative about violations of the identification assumptions, or they may be substantive results in and of themselves.

3.2 Identifying variation

Our identifying variation comes from changes from year to year in the number of days falling into various bins of average temperature, net of year fixed effects, state fixed effects, and state-by-period linear time trends. Exactly how much identifying variation is there, and which temperature bins show the most identifying variation? To answer this, we regress all temperature bins on the fixed effects and trends, calculate the residuals, and then calculate the standard deviation of these residuals. We do this separately for each temperature bin and for our 1790–1960 and 1900–1960 samples. The results are displayed in Figure 1.

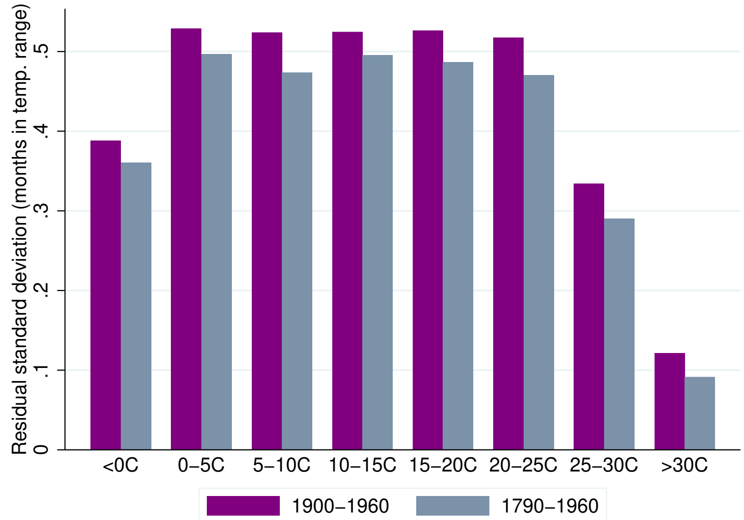
Our measure of temperature is the fraction of all days with an average temperature falling within the given range. Thus, it ranges from zero to one. Throughout the paper, we multiply this measure by 12 (the number of months in a year) so that it ranges from zero to 12, and magnitudes can be interpreted as the effect of one additional month of temperatures in a given range. For most temperature bins, the residual standard deviation is about 0.5, or roughly two weeks of temperatures. It is slightly smaller for temperatures below 0°C (one of the two bins we mainly focus on) and temperatures in the 25-30°C range (which we use as our normalization), and much smaller (only 0.1) for very hot temperatures (above 30°C).[10]

The literature on the consequences of extreme temperatures has mostly focused on extremely hot temperatures rather than extremely cold ones, partly because hot temperatures are so important for understanding climate change. For our purposes, there are four advantages of

[9] Below, we include increasingly stringent fixed effects to test for evidence on spillovers across similar states or adjacent birth cohorts. Consistent with spillovers, we find that the estimated effects of cold weather become larger when compared to spatially or temporally adjacent cohorts.

[10] In the summary statistics in Table 1 above, we reported a residual standard deviation for cold days that was much smaller than what is seen in Figure 1. This is because all of our regressions control for the other temperature bins, and therefore the ultimate identifying variation in cold days that we use is variation that has been residualized of the fixed effects and trends *and the other temperature bins*, while what we show in Figure 1 is residualized only of the fixed effects and trends. Unsurprisingly, the distribution of other temperatures are very useful for predicting cold weather in the state-year, and so the residual variation that identifies our main effects in the regressions (which control for those temperature bins) is substantially less.

Figure 1: Identifying variation by temperature bin



Notes: Figure displays the standard deviation (across state-years) of our temperature variables after residualizing them on the fixed effects we include in our main specification (year fixed effects and state-by-35-year-period specific levels and linear trends). Temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 so as to be interpreted as the number of months in the range.

studying extreme cold rather than extreme heat. First, historically, cold weather has been much more common than hot weather, although extremely hot days have become much more common since 1960. In our data, there are 36 times as many days below 0°C as there are above 30°C. While some of these are stable and predictable across years, and thus not useful for identification, the residual standard deviation of cold days is still four times that of hot days. Second, cold days are more likely to be a package of cold temperature and other adverse weather events like snow or ice, which exacerbates their health effects.[11] Third, because cold temperatures are meteorologically less predictable than hot days, they are more difficult to forecast, making it more difficult for individuals to prepare for and adapt to (Shrader et al., 2023). Finally, the effects of extreme cold have been far more stable over time (see Barreca et al. (2016) for evidence on how the effects of hot days have changed over time).

All of these are statistical advantages of studying the consequences of extreme cold rather than extreme heat. These statistical advantages are particularly important for us because we are interested in studying a rare event (becoming a history-making elite) over a very long period. We should note that the primary contribution of our paper is not to document whether one

[11]If our primary interest were in the effect of cold temperatures specifically, or some other specific weather characteristic, then we would need to control for these non-temperature variables. However, we are instead interested in the effects of extreme cold broadly, including its various non-temperature manifestations like precipitation.

particular type of extreme weather matters more for health than another, but to document how extreme weather events already known to be important for health are also important for rare events and tail outcomes that are important for long-run growth and economic development.

Given that we focus on cold days, it is important to understand where the identifying variation comes from. To answer this question, we again regress cold days on our fixed effects and trends, calculate the residual number of cold days, and then calculate the standard deviation of that residual at the state-level. The results are displayed in the maps in Appendix Figure B1. The geographic distribution of our identifying variation is fairly similar for both time periods and is dispersed across the country. We are not identified by only one state or region. However, it should be noted that neither the deep south (where cold days are very rare) nor the northeast (where cold days are very common) contribute much identifying variation.

4 Results

4.1 Effects on mortality

We begin by estimating the effects of extreme weather on mortality in order to establish which sets of weather events have sizable health effects. Our claim is not that weather's adverse effects on earnings, education, and elite production operate entirely through increased mortality. Rather, our view is that the effects of weather on health more broadly likely mirror its effects on mortality, since that is one extreme indicator of health.

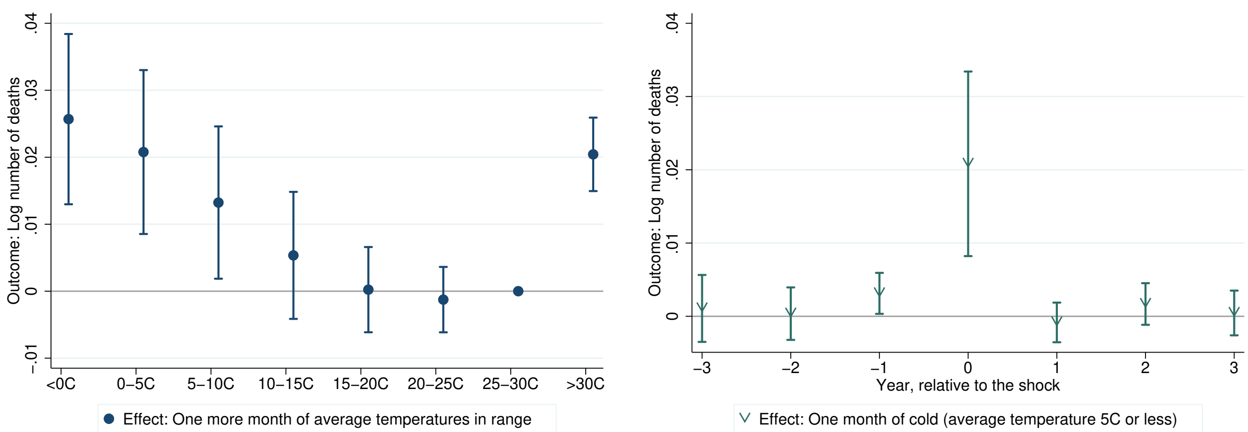
For most of our outcomes of interest, we must focus on birth cohorts before 1960 so that we have time to observe adult (age 40) earnings and education, and to leave enough time for people to begin to establish their impact on history. Thus, our main mortality estimates also focus on the 1900-1960 period (since 1900 is the earliest year for which we have mortality data). However, we should note that our mortality estimates for the longer period (1900-2004) are much smaller because of the flattening of the temperature-mortality since 1960 shown by Barreca et al. (2016).

In Figure 2, we present the effects of extreme weather on mortality. In Panel (a), we show effects that are flexible by temperature bin. All magnitudes can be interpreted as the effect of one additional month of days with average temperatures in the given bin, rather than average temperatures in the 25-30°C bin. We find that an additional month of weather with average temperatures below 5°C weather increases mortality by 2-3%.[12] The effects decline such that any temperatures between 10–30°C have roughly equal implications for mortality, but temperatures above also substantially increase mortality. As noted above, however, there

[12]As we show in Figure 1, one month of cold weather is roughly 2 standard deviations of the residuals.

is far more identifying variation in cold days than hot days, and the mortality effects of cold days are slightly larger than those of hot days.

Figure 2: Extreme weather effects on mortality



(a) By temperature bin

(b) Effects on other years' mortality

Notes: Dependent variable is log mortality in the given state during the year (almost balanced panel, 1900–1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Panel (a): See equation (1) for estimating equation. Panel (b): See equation (2) for estimating equation.

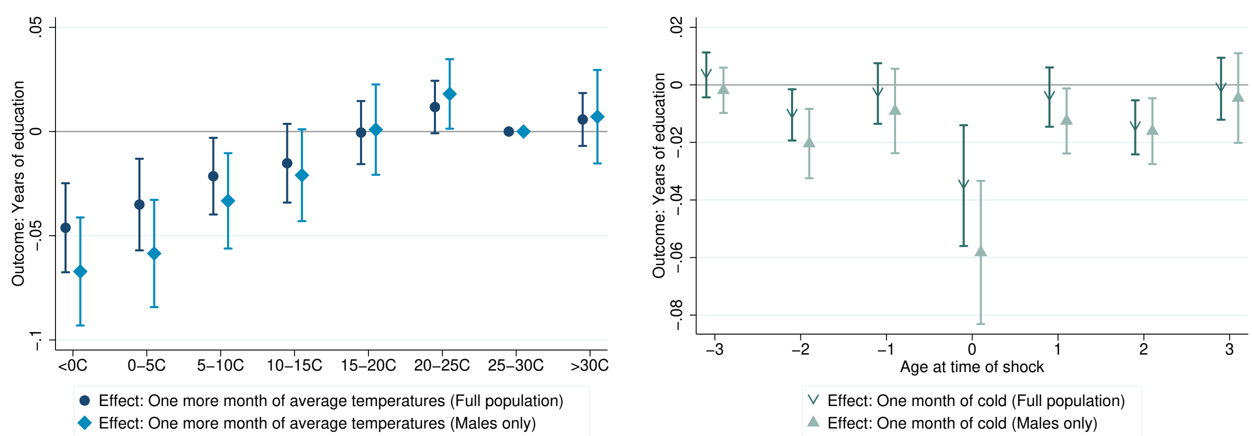
Panel (a) is flexible with respect to temperature, but only estimates contemporaneous effects on mortality during the year of the extreme weather shock. In Panel (b), we focus specifically on cold days (average temperature below 5°C), but present leads and lags of this extreme temperature, which allow us to trace the effects over time. We generally find that mortality is only correlated with contemporaneous extreme weather, and shows little correlation with extreme weather from earlier or subsequent years. This suggests that our identification strategy does isolate exogenous variation in extreme weather phenomena across years which is separate from broader health-related trends that affect longevity.

Note that this is not a trivial identification test. Mortality and weather have both changed dramatically over time, and differently in different states, making spurious correlations very plausible. To illustrate this, in Appendix Figure B2 we replicate Panel (b) of Figure 2 but exclude the state-by-period specific trends. This shows dramatic pre-trends and strong correlations between mortality and both future and past extreme weather events. Clearly these results instill suspicion, and so we consider it important to account for heterogeneous (across states) secular trends. Conditional on including these trends, however, we have found that our results are quite stable with respect to different definitions of a time period, different polynomials, or different ways of defining regional trends and shocks (see Appendix Table B3).

4.2 Effects on education

Having replicated results showing that extreme weather does affect health, we now turn to its consequences for the adult educational attainment of the children born during the cold weather year. In Figure 3, we find that an additional month of very cold weather reduces years of education by roughly 0.04 years. These effects are somewhat larger for men than women.

Figure 3: Extreme weather effects on years of education



(a) By temperature bin

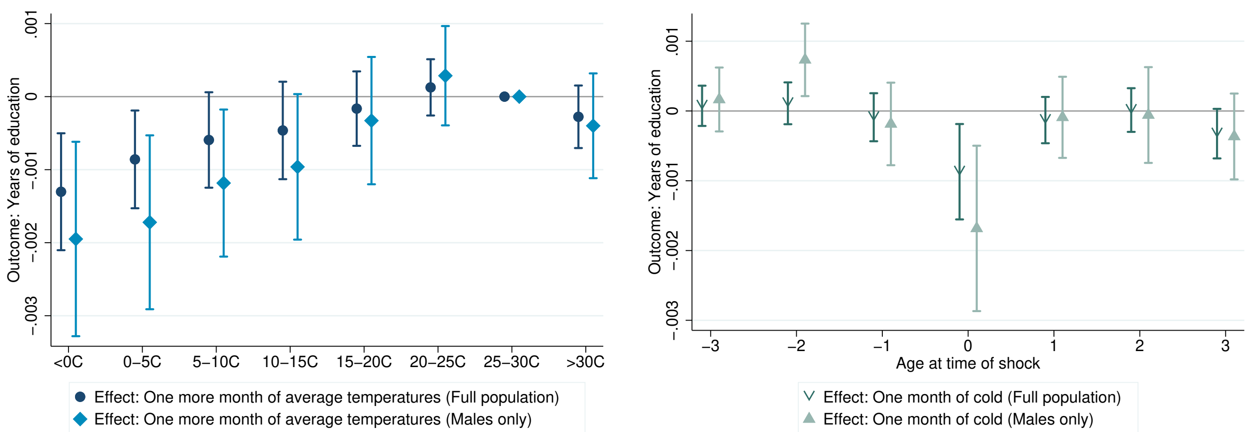
(b) By age at time of shock

Notes: Dependent variable is average years of education at age 40 for individuals born in the given state-of-birth birth-year cohort (balanced panel, 1900–1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Panel (a): See equation (1) for estimating equation. Panel (b): See equation (2) for estimating equation.

However, society may particularly rely on those with very high levels of education, who make up a large share of those in fields related to medicine, research and innovation, the determination of law and policy, etc. For this reason, we also focus on the fraction of the cohort attaining an "elite" education (roughly 1–4% of the population). In Figure 4, we also find a significant decline caused by extremely cold weather. Here, the effects are roughly twice as large for men as for the full population, as might be expected given the exclusion of women from many professional and doctoral programs during these birth cohorts.

How do the magnitudes of these effects compare with one another? A one standard deviation increase in cold days implies a decrease in years of education that is roughly equal to 4.7% of

Figure 4: Extreme weather effects on attaining an "elite" education



(a) By temperature bin

(b) By age at time of shock

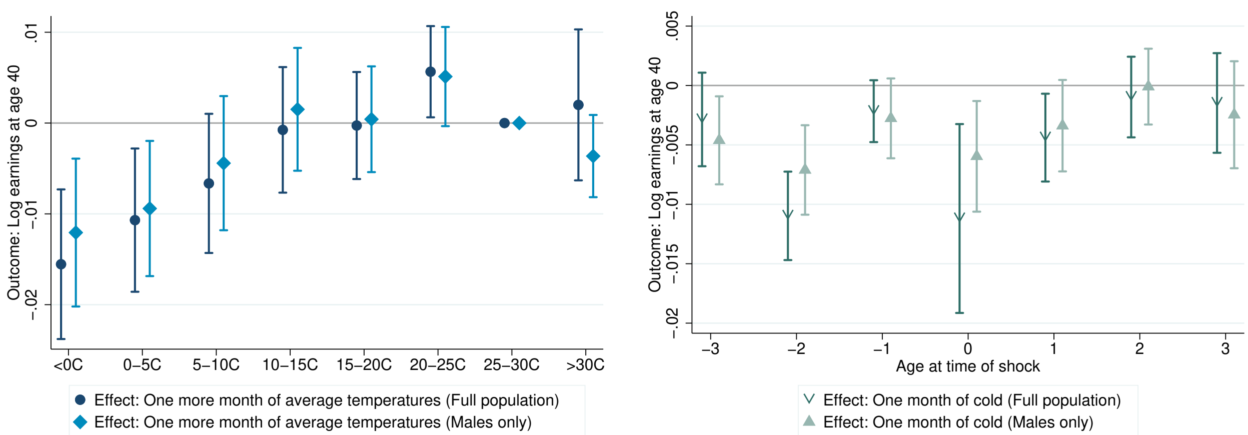
Notes: Dependent variable is share of individuals holding "elite" education at age 40, roughly 1–4% of the population (see Table A1 for definition) for individuals born in the given state-of-birth birth-year cohort (balanced panel, 1900-1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Panel (a): See equation (1) for estimating equation. Panel (b): See equation (2) for estimating equation.

a residualized standard deviation. The same increase in cold days, however, implies a decrease in elite education that is about 7.6% of a standard deviation. Thus, the effects of extreme weather appear to be larger for the attainment of elite education than for overall educational attainment, although the difference between these estimates is not statistically significant.

4.3 Effects on earnings

Do these effects also translate into earnings effects? In Figure 5 we find that they do. One month of cold weather implies a roughly 1% decrease in average earnings.[13] Unlike for education, however, we find that the effects are somewhat smaller for men than for women.

Figure 5: Extreme weather effects on earnings



(a) By temperature bin

(b) By age at time of shock

Notes: Dependent variable is log of average age 40 earnings for individuals born in the given state-of-birth birth-year cohort (balanced panel, 1900-1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Panel (a): See equation (1) for estimating equation. Panel (b): See equation (2) for estimating equation.

It is worth mentioning one concerning result shown in Figure 5. This is the only instance in which we find large effects on an outcome that one would reasonably consider a placebo. Specifically, we estimate that a birth cohort's adult earnings are significantly reduced by cold occurring two years before they were born (though not one or three before being born). This

[13]For a limited set of years (birth cohorts 1920-1940), the census reports quarter of birth, and we can estimate separate effects by quarter of birth. While none of these estimates is significantly different from zero or from one another, we do find that the largest effects are found among those born in October-December. This is reassuring, as these are the cohorts for whom the cold weather was most concentrated during their infancy, when children are most vulnerable to environmental conditions (see Appendix Table B1).

"effect" is nearly as large as the effect we estimate for those born during the cold year (which we consider to be the directly exposed cohort). While it is possible that the extreme cold weather might have some spillover effects onto younger cohorts (for instance, by reducing the success of their older siblings), we find it implausible that these spillover effects would be as large as the direct effects on the cohort born during the cold year. This suggests some degree of caution in interpreting our results, but at the same time, we are reassured that none of our other outcomes show meaningful violations of this cross-cohort placebo test, and that our results are robust to alternative reasonable specifications.

Table 2 shows that, like our estimated effects on educational attainment, the estimated earnings effects are also concentrated at the top of the distribution. Specifically, we estimate the effects of extreme cold on the log of mean earnings and of the 50^{th}, 75^{th}, and 90^{th} percentiles of the state-by-birth-year cohort earnings. We find effects on the 90^{th} percentile that are 10 times as large as the (non-significant) effects on median earnings.

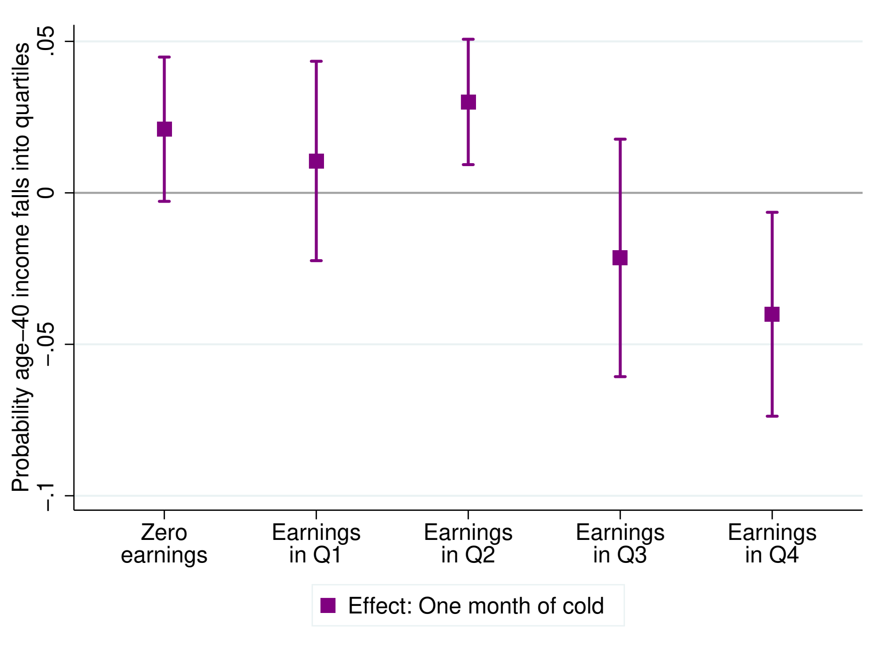
Table 2: Extreme weather effects on earnings percentiles

	(1)	(2)	(3)	(4)
DV: Log of	Mean earnings	Median	75^{th} pctl.	90^{th} pctl.
ColdDays	-0.0110**	-0.0011	-0.0097*	-0.0115**
	(0.0048)	(0.0094)	(0.0049)	(0.0047)
R^2	0.998	0.989	0.998	0.997
N	2988	2983	2985	2985

* $p < .10$, ** $p < .05$, *** $p < .01$. Dependent variable is log of of the average (1), median (2), 75^{th} percentile (3), or 90^{th} percentile (4) of age 40 earnings for individuals born in the given state-of-birth birth-year cohort (1900-1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.

Similarly, in Figure 6, we estimate the effects on the probability of having zero earnings, the probability of having positive earnings in the first quartile of the national cohort-specific earnings distribution, etc. We find that extreme cold modestly (but not significantly) increases the probability of zero earnings, significantly increases the probability of earnings in the $25^{th} - 50^{th}$ percentiles of the national earnings distribution, and significantly decreases the probability of earnings in the top quartile of the national distribution.

Figure 6: Extreme weather earnings effects across the distribution



Notes: Dependent variable is the share of the state-of-birth birth-year cohort with zero earnings at age 40 or positive earnings in each quartile (balanced panel, 1900-1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.

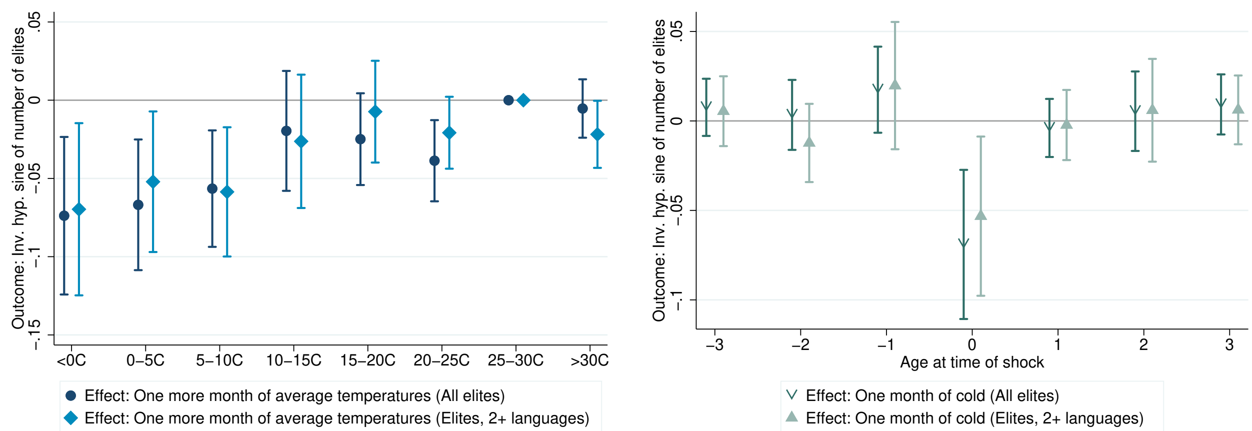
In summary, then, we find that the largest of the adverse effects of extreme weather are concentrated at the top of the earnings distribution (i.e., effects on the 90^{th} percentile and the probability of falling into the top quartile of earnings).

4.4 Effects on history-making elites

Given our evidence that extreme weather during one's birth year reduces educational attainment and earnings (with particularly large effects at the top of the outcome distribution), it is natural to wonder whether it affects the probability that one leaves a significant mark on society. Here, we estimate effects on the number of people from the state-by-birth-year cohorts who go on to to make history, as proxied by their appearance in the HBR. It is worth repeating that this is a different sample from the previous regressions because we can measure outcomes for birth cohorts 1790-1960, rather than only 1900-1960 for the census-based outcomes.

In Figure 7, we find that extreme weather significantly reduces the number of history-making elites arising from a birth cohort. One month of extreme cold reduces the number of elites by about 7%.[14] We find similar effects on the number of elites whose Wikipedia page has been translated into other languages. We see this as a proxy for having been particularly influential, since those with a more limited influence are less likely to warrant translation.[15] Throughout the paper, we always consider these multi-language elites separately. We generally find very similar point estimates, which are sometimes slightly smaller (as in Figure 7) and sometimes slightly larger. This shows that the effects of extreme weather that we document are not driven simply by the disappearance of a handful of marginal or unimportant historical figures.

Figure 7: Extreme weather effects on number of elites



(a) By temperature bin

(b) By age at time of shock

Notes: Dependent variable is inverse hyperbolic sine of number of history-making elites who were born in the given state-of-birth birth-year cohort (balanced panel, 1790-1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Panel (a): See equation (1) for estimating equation. Panel (b): See equation (2) for estimating equation.

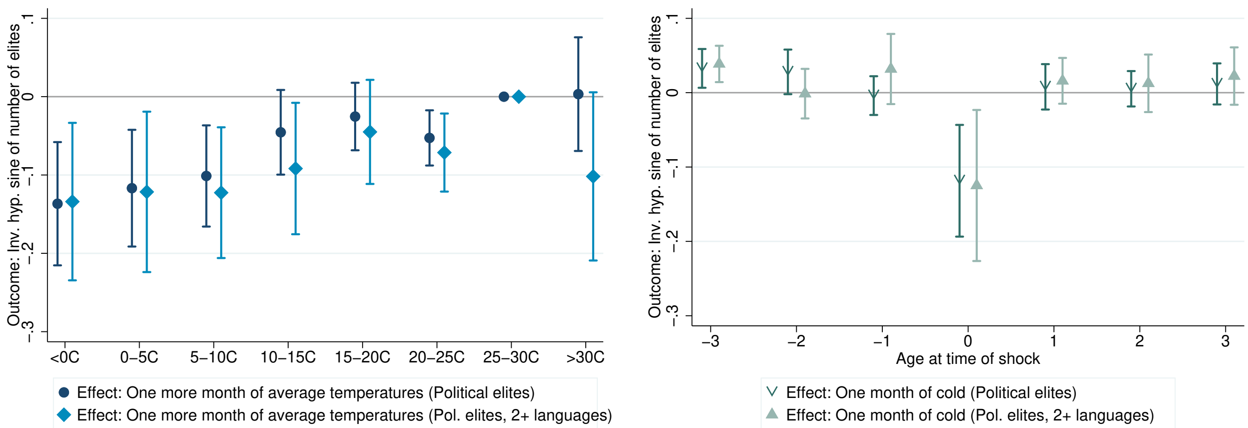
The above results included all elites, including artists and writers. For the purposes of shaping institutions, however, it is plausibly more important to consider only political elites (those who influenced government or policy). These results are shown in Figure 8. Here, we

[14] Our main specification focuses on the inverse hyperbolic sine of the number of elites so that coefficients can be interpreted as a percent change. In Appendix Table B2 we present an alternative normalization in which coefficients can be interpreted as the change in the share of the year's elites which come from the cold-affected state.

[15] As an alternative measure of how influential elites are, we consider their page rank (Nekoei and Sinn, 2021a). These results are in Appendix Table B4 and show a very similar pattern.

find somewhat larger effects, with a month of cold weather reducing the expected number of political elites by about 12%.

Figure 8: Extreme weather effects on number of political elites



(a) By temperature bin

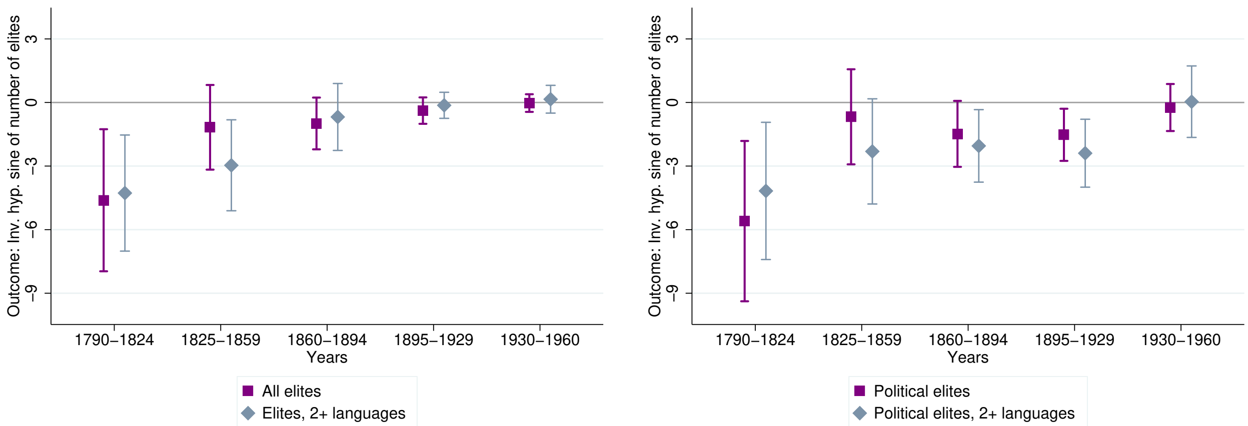
(b) By age at time of shock

Notes: Dependent variable is inverse hyperbolic sine of number of political elites who were born in the given state-of-birth birth-year cohort (balanced panel, 1790-1960). All estimates are based on specifications that control for year fixed effects, and state-by-35-year-period fixed effects and linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Panel (a): See equation (1) for estimating equation. Panel (b): See equation (2) for estimating equation.

Are these effects driven by a decline in elites who were likely most susceptible to extreme weather events? We present two types of evidence that they are. First, as noted in the introduction, many elites are drawn from elite families (Nekoei and Sinn, 2021b), who are likely particularly well-insulated from extreme weather shocks (Basu, 2009). Thus, the effects might be larger in places and times where "social mobility" is higher, in the sense that elites are more likely to come from non-elite families. We test this by calculating the share of elites in each state-by-time-period whose father also is included in the HBR, and dividing states into terciles of social mobility within each time period (roughly 35 years at a time from 1790–1825 to 1930–1960). Appendix Figure B3 shows that the effects are larger and more precisely estimated in the places where elites are less likely to be drawn from elite families, although the difference across terciles is not statistically significant.

Second, the morality effects of environmental shocks have declined dramatically over time, perhaps due to technological improvements (Barreca et al., 2016), economic development (Fukushima, 2021), improved forecasting and adaptation (Shrader et al., 2023), or other forms of progress. It is plausible this trend stretches back to the 18^{th} century, and so it is interesting how the effects of extreme weather have changed over time. In Figure 9, we estimate effects separately for each time period. The effects of cold weather on elites have fallen dramatically over time, ultimately vanishing by the 1930–1960 period. This is an encouraging result, suggesting that the consequences of severe weather are not permanent, but can be ameliorated through growth and progress.

Figure 9: Cold weather effects on elites over time



(a) All elites

(b) Political elites

Notes: Dependent variable is inverse hyperbolic sine of number of elites who were born in the given state-of-birth birth-year cohort (balanced panel, 1790-1960). All estimates are based on specifications that control for year fixed effects, state fixed effects, and state-by-time-period specific linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.

Finally, we ask what happens when extreme weather leads to a reduction in the number of history-making elites from one particular state-birth-year cohort. One possibility is that the hole left in history is filled by people from other state-birth-year cohorts. To investigate this, we test for two types of spillovers.

First, we test for spillovers within the same birth cohort, but across other nearby states that are culturally and historically similar. To do this, we use the nine census divisions developed in 1910 by the Census Bureau to create groups of states that "are roughly similar in terms of historical development, population characteristics, economy, and the like" (Census, 1994, 6-1). Census divisions are collections of contiguous states that accord well with intuitions about states' culture and history. We add division-by-year fixed effects to our main specification. This means that a state-year cohort experiencing an extremely cold winter is being compared to the same birth cohort in other nearby and similar states. If those born in the same year in those other states were *more* likely to become elites because of the dearth of elites caused by the cold winter, then our estimated effects of extreme cold would become larger.

This approach tests for within-cohort, across-state spillovers. On the other hand, spillovers might instead come from within-state, across-cohorts. That is, the would-be elites pushed out of elite status by the extreme cold during their first year of life might be replaced by marginally older or younger cohorts from the same state. This might be particularly likely for political elites, where one's elite status as a governor or member of Congress is conditioned on the state on lives in. To test for this, we define a function $\Lambda(t) \equiv \lfloor t/5 \rfloor \times 5$ that maps year to five year periods. So, for instance, $\Lambda(t) = 1910$ for $t \in \{1910, 1911, 1912, 1913, 1914\}$. This groups consecutive years into five-year periods. We then include state-by-five-year-period fixed effects in our main specification. As we argued above, if the decline in elites induced by a harsh winter leads to increased elite production from earlier and later cohorts, then the inclusion of these fixed effects should increase the estimated effects of cold weather.

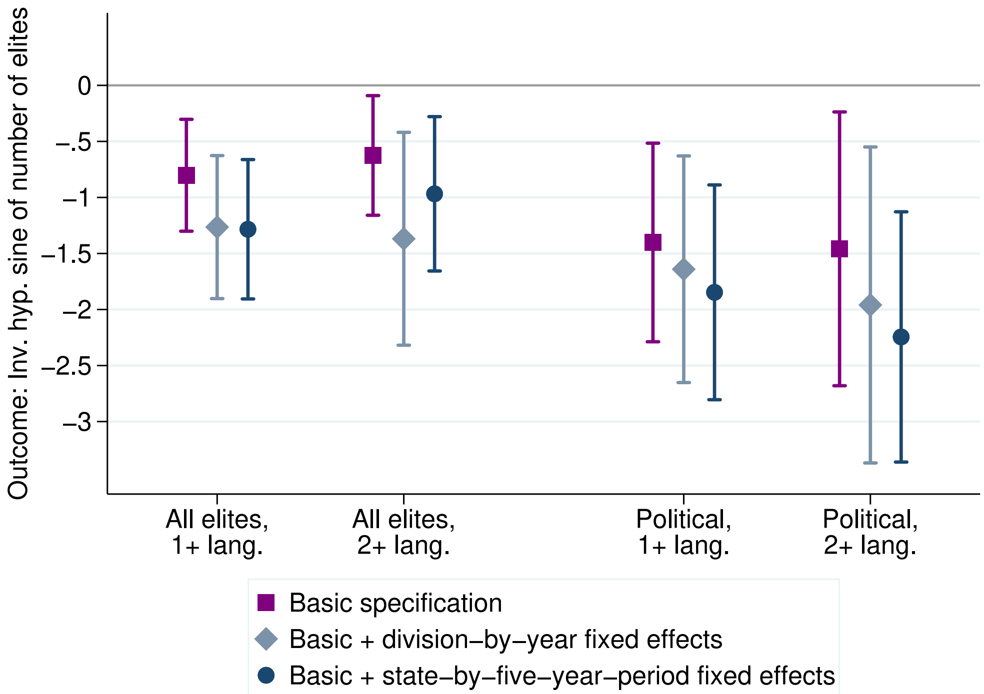
Figure 10 presents the results. The purple squares represents our baseline estimates, on all elites and political elites, separately by whether the elites' page has been translated into other languages. The gray diamonds then present the estimated effects of cold weather after adding division-by-year fixed effects to test for within-cohort, across-state spillovers. The blue circles instead add state-by-five-year-period fixed effects to test for within-state, across-cohort spillovers.

Although none of these estimates are statistically significantly different from one another, we do find that the inclusion of fixed effects increases the size of the estimated effects of cold weather, consistent with spillovers. Interestingly, comparing the two types of spillovers, we find that for all elites, within-cohort across-state spillovers appear to be modestly more important. However, for political elites, who often directly represent their state, it is the within-state, across-cohort spillovers that appear to be more important.

Reassuringly, when we run the same regressions using years of education and average earnings as outcomes—where we do not find spillovers plausible because it is unlikely that success in one state-birth-year cohort would crowd out success for another—we find that the estimated effects become smaller and less precise, rather than larger and sometimes more precise. We interpret this as evidence that this approach does effectively test for meaningful spillovers driven by "elite substitution."

Overall, these results suggest that extreme weather events that affect mortality (as we show above) as well as infant health and development (as the literature has shown) have significant effects on education, earnings, and individuals' emergence as leaders in their societies. These effects tend to be larger for more positively selected "tail outcomes" than for average outcomes. Looking at the harsh environmental and development challenges faced by billions around the

Figure 10: Evidence of substitution in elite production



Notes: Dependent variable is inverse hyperbolic sine of number of elites who were born in the given state-of-birth birth-year cohort (balanced panel, 1790-1960). All estimates are based on specifications that control for year fixed effects, state fixed effects, and state-by-time-period specific linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Relative to our main specification – see equation (1) for estimating equation – these specifications also include either division-by-year fixed effects or state-by-five-year-period fixed effects (i.e., fixed effects for $s - \Lambda(t)$ pair where $\Lambda(t) \equiv \lfloor t/5 \rfloor \times 5$.

world today (and likely to grow with climate change and the resulting increase in extreme weather shocks), this is a concerning result.

At the same time, our results do not show an immutable pattern of fate. Growth and progress can reduce the effects of extreme weather, and when there are some states and cohorts that are less affected, then history has a way of partially offsetting the negative results by drawing influential people from those states and cohorts instead.

5 Conclusions

This paper uses long-run panel data on weather, health, earnings, education, and elite members of society to provide novel evidence on the role of extreme weather in shaping economic outcomes. We find that cohorts born during years with especially severe winters have significantly worse adult outcomes in terms of years of education and average earnings, with particularly large effects at the top of the distributions for both of these outcomes. Extreme weather also reduces the emergence of history-making elites. This suggests that extreme environmental conditions play an especially strong role in blunting outcomes for the most promising members of society. This provides complementary evidence to widely documented inequities terms of larger effects of environmental conditions on the less well off members of society as measured by baseline characteristics.

The results also indicate that conditions have improved over time in the US. Future work could explore the mechanism underlying this change. Understanding the mechanism could be particularly valuable if other places around the world are currently less adapted to extreme weather events—and are therefore still experiencing negative effects on consequential economic outcomes—or if larger environmental shocks due to climate change outpace society's adaptive capacity.

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Online Appendix

A Data

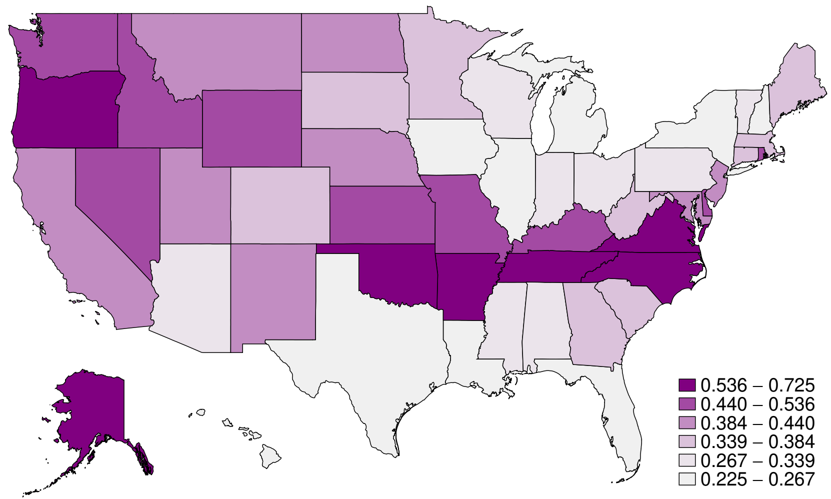
Table A1: Definitions of elite education

Census years	Definitions of elite education (and percent of respondents)	Next highest education level (and percent of respondents)
1940-1950	5+ years of college (1.2%)	4 years of college (2.6%)
1960-1970	6+ years of college (3%)	5+ years of college (2%)
1980	7 years of college (1.6%) 8+ years of college (2.4%)	6 years of college (3%)
1990-2010	Professional degree (2.1%) Doctoral degree (1%)	Master's degree (6.8%)

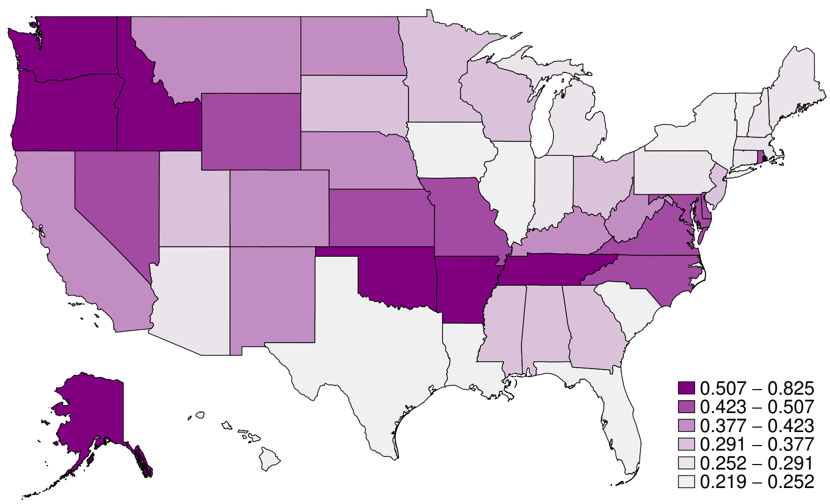
The table shows the definition of elite education over the sample period.

B Additional results

Figure B1: Geographic distribution of identifying variation in cold days



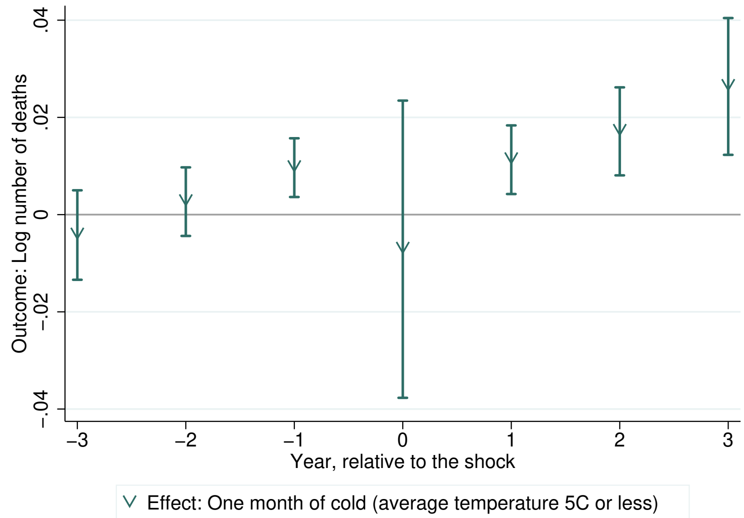
(a) 1900-1960



(b) 1790-1960

Notes: Figure displays the standard deviation (across years) of our "cold days" temperature variable after residualizing it of the fixed effects we include in our main specification (year fixed effects and state-by-35-year-period specific levels and linear trends). "Cold days" are defined as the fraction of days in the year with an average temperature below 5°C, multiplied by 12 so as to be interpreted as the number of months in the range.

Figure B2: Mortality "effects" without state-by-period specific trends



Notes: Dependent variable is log mortality in the given state during the year (almost balanced panel, 1900-1960). Unlike the figure in the main text (Figure 2), specification for this figure *does not* control for state-by-35-year-period specific levels or trends; it only controls for state and year fixed effects. Estimates are still based on specifications that are population weighted and in which standard errors are clustered at the state level. Figure plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature.

Table B1: Extreme weather effects on earnings by birth quarter

	(1)	(2)	(3)	(4)
DV: Log of	By quarter of birth			
mean earnings	Jan.-Mar.	Apr.-Jun.	Jul.-Sep.	Oct.-Dec.
ColdDays	-0.0027	-0.0049	0.0012	-0.0185
	(0.0109)	(0.0107)	(0.0106)	(0.0120)
R^2	0.987	0.989	0.989	0.988
N	931	931	931	931

* $p < .10$, ** $p < .05$, *** $p < .01$. The dependent variable is the log of the average earnings at age 40 for individuals born in the given state-of-birth birth-year cohort during the given quarter (1920-1940). All estimates are based on specifications that control for year fixed effects, state fixed effects, and state-specific linear trends; are population weighted. Standard errors are clustered at the state level. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.

Table B2: Alternative normalization of number of elites

	(1)	(2)	(3)	(4)
DV	All elites		Political elites	
	Any lang.	2+ lang.	Any lang.	2+ lang.
Panel A: Inverse hyperbolic sine of elites (main spec.)				
ColdDays	-0.802**	-0.625*	-1.402**	-1.459*
	(0.304)	(0.325)	(0.540)	(0.745)
R^2	0.960	0.941	0.848	0.763
N	8267	8267	8267	8267
Panel B: State's share of year's elites				
ColdDays	-0.049**	-0.048**	-0.056*	-0.060
	(0.021)	(0.022)	(0.032)	(0.036)
R^2	0.972	0.958	0.914	0.859
N	8267	8267	8267	8267

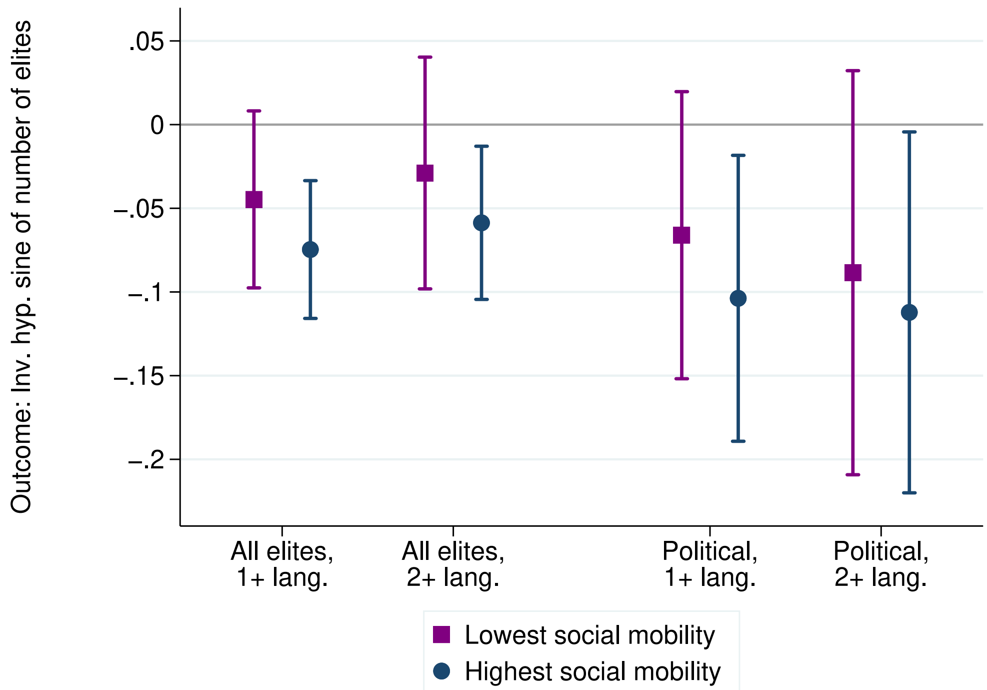
* $p < .10$, ** $p < .05$, *** $p < .01$. The dependent variable is the number of elites from the state-birth-year cohort. In Panel A, we take the inverse hyperbolic sine (as in our main specification). In Panel B, we divide the number of elites from the state-birth-year cohort by the number of elites from the birth-year cohort across all states. Thus, the dependent variable is the state's share of all elites from this birth-year cohort. All estimates are based on specifications that control for year fixed effects, state fixed effects, and state-specific linear trends; are population weighted; and in which standard errors are clustered at the state level. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.

Table B3: Alternative state-by-period specific trends

	(1)	(2)	(3)	(4)
DV	All elites		Political elites	
	Any lang.	2+ lang.	Any lang.	2+ lang.
Panel A: State-by-35-year-period linear trends (main)				
ColdDays	-0.802**	-0.625*	-1.402**	-1.459*
	(0.304)	(0.325)	(0.540)	(0.745)
R^2	0.960	0.941	0.848	0.763
N	8267	8267	8267	8267
Panel B: State-by-20-year-period linear trends				
ColdDays	-0.795***	-0.701**	-1.373**	-1.536**
	(0.294)	(0.338)	(0.521)	(0.727)
R^2	0.963	0.946	0.857	0.775
N	8265	8265	8265	8265
Panel C: State-by-45-year-period linear trends				
ColdDays	-0.687**	-0.553*	-1.374***	-1.605**
	(0.286)	(0.312)	(0.493)	(0.726)
R^2	0.958	0.939	0.844	0.756
N	8267	8267	8267	8267
Panel D: State-by-35-year-period quadratic trends				
ColdDays	-0.901***	-0.677*	-1.528***	-1.577**
	(0.311)	(0.352)	(0.546)	(0.744)
R^2	0.962	0.944	0.853	0.771
N	8267	8267	8267	8267
Panel E: Division-by-decade fixed effects				
ColdDays	-1.812**	-1.748**	-1.912**	-2.222**
	(0.770)	(0.802)	(0.736)	(0.856)
R^2	0.925	0.903	0.803	0.719
N	8267	8267	8267	8267

* $p < .10$, ** $p < .05$, *** $p < .01$. The dependent variable is the inverse hyperbolic sine of the number of elites from the state-birth-year cohort. All estimates control for state-by-time-period specific trends, as well as state and year fixed effects. In Panel A, time periods are approximately 35 years and trends are linear (as in our main specification). In Panel B, time periods are approximately 20 years and trends are linear. In Panel C, time periods are approximately 45 years and trends are linear. In Panel D, time periods are approximately 35 years and trends are quadratic. In Panel E, instead of state-by-time-period trends, we include census-division-by-decade fixed effects. All regressions are population weighted and cluster standard errors at the state level. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.

Figure B3: Heterogeneous effects by social mobility



Notes: Dependent variable is inverse hyperbolic sine of number of elites who were born in the given state-of-birth birth-year cohort (balanced panel, 1790-1960). All estimates are based on specifications that control for year fixed effects, state fixed effects, and state-by-time-period specific linear trends; are population weighted; and in which standard errors are clustered at the state level. Figures plot estimates and 90% confidence intervals. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. Estimates are based on heterogeneous effects by within-time-period terciles of social mobility. To measure social mobility at the time-period-by-state level, we calculate the share of elites (political elites, resp.) who's father also is in the HBR page. We then divide states into terciles within each time-period, and estimate $Y_{s,t} = \alpha_s + \delta_t + \mu_{s,T(t)} + \gamma_{s,T(t)}t + \theta_L\big(ColdDays_{s,t} \times LowSocMob_{s,T(t)}\big) + \theta_I\big(ColdDays_{s,t} \times IntermedSocMob_{s,T(t)}\big) + \theta_H\big(ColdDays_{s,t} \times HighSocMob_{s,T(t)}\big) + \sum_k \beta_k TMEAN^k_{s,t} + \varepsilon_{s,t}$. The figure plots the $\hat{\theta}_L$ and $\hat{\theta}_H$ coefficients corresponding to the effects of cold weather in the lowest and highest, respectively, terciles.

Table B4: Heterogeneity by pagerank

	(1)	(2)	(3)	(4)	(5)	(6)
	All elites			Political elites		
DV	All	Below median	Above median	All	Below median	Above median
ColdDays	-0.067**	-0.076**	-0.058	-0.117**	-0.146**	-0.102
	(0.025)	(0.036)	(0.035)	(0.045)	(0.059)	(0.072)
R^2	0.960	0.930	0.931	0.848	0.785	0.743
N	8267	8267	8267	8267	8267	8267

* $p < .10$, ** $p < .05$, *** $p < .01$. The dependent variable is the inverse hyperbolic sine of the number of elites from the state-birth-year cohort, separately for those who have above or below median pagerank. Pagerank (Brin and Page, 1998) is a network measure that evaluates the importance of an individual within the Wikipedia hyperlink network based on the quantity and quality of links to an article. We measure the pagerank by combining hyperlinks across all languages. All estimates are based on specifications that control for year fixed effects, state fixed effects, and state-specific linear trends; are population weighted; and in which standard errors are clustered at the state level. All temperature variables reflect the fraction of days in the year with an average daily temperature in a given range, multiplied by 12 to be interpretable as the effects of one month of days with such a temperature. See equation (1) for estimating equation.