Do Relief Programs Compensate for Affected Populations? Evidence from the Great Depression and the New Deal*

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Abstract

This paper explores the short- and long-run effects of the Great Depression and the New Deal on the well-being of the population, measured by longevity. We use a novel dataset that allows us to track a large number of individuals alive in 1930 until their deaths and match it to information on the severity of the economic crisis and the extent of transfers provided by the New Deal at the county level. First, we document the dynamic effects of the Great Depression (GD) on survival rates and longevity and we show that individuals, in particular young men, living in the most several affected locations lived substantially shorter as a result of the GD. Second, we assess whether the New Deal compensated individuals for the negative effects of the GD. To identify the causal effects of the New Deal programs, we leverage variation across counties in New Deal spending that was politically motivated. More specifically, we use an instrumental variable strategy based on voting culture. Intuitively our approach compares outcomes of individuals in counties that were equally affected by the GD but who received more money as a result of politicians' desire to be re-elected. We find that the New Deal increased longevity and more than offset the negative effects of the recession. In the absence of the New Deal, on average individuals would have lived 6 months less. The benefits of the New Deal were larger for men, and for those aged 15-25 in 1930.

The Great Depression (hereafter GD) was the deepest and longest downturn in modern US history. It started in the fall of 1929, peaked in severity in 1933 and lasted roughly 10 years, until the beginning of WWII in 1939. During that period, a third of the banks failed, prices went down by 27%, and unemployment rates reached 25% on average, with substantially larger effects among youth (BLS).[1] The GD had massive consequences for the population: it increased poverty, homelessness, and malnutrition for many groups (Kiser and Stix 1933, Jacobs 1933). In response to the large negative shock, in 1933 President Roosevelt implemented a set of programs known as the New Deal (hereafter

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[1] https://www.bls.gov

ND), with the purpose of restoring economic growth and to provide relief to the most affected segments of the population.

This paper studies the short- and long-term effects of the Great Depression and the New Deal on longevity. First, we document whether the Great Depression affected longevity, and to understand when the effects became visible, we investigate how the GD affected survival to various ages. Since it is extremely difficult to find exogenous sources of variation to predict the severity of the GD, our analysis of these effects is descriptive. Second, we study whether New Deal relief compensated individuals for the negative effects of the GD, by asking whether individuals living in counties that received larger amount of funds lived longer as a result. To identify causal effects of the New Deal relief, we implement an instrument variable approach that leverages an important source of exogeneity in relief funds distribution: political incentives.

To estimate the impact of the GD and ND on longevity we use a novel dataset which follows white native born individuals alive in 1930 until their deaths. We use the 1930 full-count US Census as a baseline and link it to death dates using information available on family trees from FamilySearch. Since we observe individuals' residence in 1930, we can also match them to county-level data on the severity of the Great Depression and to information on spending on New Deal programs. We focus on relief programs that provided unconditional cash transfers or relief through work—these programs were most directly intended to provide relief and thus, more likely to affect health outcomes.[2] Finally we can also match individuals to the 1940 census to investigate potential mechanisms. These data have many advantages. Because we can track individuals from 1930 until the present, we can compare the short- and long-run effects of the GD and the ND on survival. The data is also very large and includes a large fraction of women allowing for detailed heterogeneity analysis.

We estimate causal effects of the New Deal relief by employing an instrumental variable approach since geographic allocation of New Deal relief was not random. The main purpose of New Deal relief was to alleviate the negative effects of the recession, hence, the federal government targeted the states and counties the hardest hit by the crisis (Fishback et al. 2003, 2007). Thus, individuals in these areas would have likely fared worse even in the absence of the relief, which negatively biases estimates of

[2] The programs included in our analysis are the Works Progress Administration (WPA), The Federal Emergency Relief Administration (FERA), Social Security Administration Public Assistance (SSAPA), Civil Works Administration grants (CWA), and Public Work Grants.

the relief. For the same reason, estimates of the GD that do not account for the ND are also biased and likely underestimate the impact of the GD, since most affected areas received more relief. To address this concern, we leverage variation in spending that was driven by political considerations.

Previous literature has documented that political incentives influenced the distribution of funds: in addition to targeting affected areas, the government favored areas that could help them ensure reelection (Wright 1974; Wallis 1998; Fleck 2001). We use an instrumental variable (IV) approach based on these political incentives to predict where the relief was allocated, while we control for the severity of the crisis. To select the IV, we make use of an IV Lasso approach. We build on prior literature and collect all variables previously used (Wright 1974, Fleck 2001, Fishback et al. 2005, Fishback et al. 2006, Fishback et al. 2007). These variables, together with their higher terms and interactions are considered as potential instruments. We then select the best instruments (and set of controls) using a parsimonious IV Lasso approach following Chernozhukov et al. (2015). The instrument selected, which we term "voting culture exploitability", is a function combining the 1932 presidential election voters' turnout, and the 1928 congressional election voters' turnout. This voting culture exploitability variable takes larger values in areas where relief funds would be most likely to increase votes. Conceptually, the IV estimates compare individuals in counties that suffered equally from the GD, but who received different amounts of relief because of political considerations.

Our findings suggest that although the Great Depression was bad for the health of the population, the New Deal relief more than compensated for its negative effects. The GD reduced survival rates in the short- and long-run, but the effects on survival only become substantial after individuals reach age 50 and max out around age 70. Thus, short-term estimates of the effects of the GD substantially underestimate its negative consequences. Moreover, failure to account for the ND and its endogeneity also substantially biases estimates of the effects of the crises. We also find that on average the ND extended longevity and affected positively survival rates both in the short- and long-run. More specifically, the IV estimates show that a one standard deviation increase of relief per capita ($140) extended longevity by 6 months.[3] In addition, the predicted net effect of the Depression GD and relief is an average extension in longevity of one month.

[3] $140 is equivalent to 24% of the average annual in the 1940 Census. $140 in 1967 are equivalent to approximately $2000 in 2020. The relief is not in annual terms, it is the total amount of funds from 1933 to 1939.

Previous literature (e.g. Margo 1991) has shown that the effects of the GD were not equally distributed among the population. It affected especially blue-collar and unskilled workers. And because of the hit to manufacturing and construction, men were affected particularly hard. As in other recessions, youth also suffered larger losses in employment.[4] To investigate this we re-estimate our model separately by gender and age. Consistent with this evidence we find that men were the only ones affected by the Depression, and they were also the main beneficiaries of the New Deal. On average, a standard deviation increase in the relief extended their longevity by 1 year. We only find significant effects of the ND for teenage women in 1930.

We also find that young adults suffered the largest effects of the GD and obtained the greatest benefits from the ND. There are two main reasons for this finding. First, men between the ages of 16 and 21 years old had large unemployment rates and as result were more likely to receive relief.[5] Second, because relief programs were most often provided through employment, these programs could have improved their labor opportunities in the future, explaining part of the extension in longevity (Schwandt and von Wachter 2020). In fact, recent research shows that young men participating in the CCC program (an employment program targeted towards young men) increased lifetime incomes and longevity (Aizer et al. 2020).

Interestingly the effects of the GD and the ND are not larger among those born during the GD or who were children at the time. This evidence is also consistent with observation at the time that children's heights, and disability rates later in life were unaffected by the GD (Cutler et al. 2007). This might be explained by the fact that the GD might have in fact benefitted some families so long as the main bread earner kept their job since the price of housing and food fell. Also, a lot of people moved back to rural settings and farms for the first time in history and this could have positive health effects, as there was less disease exposure in rural areas (Spengler1936, Boyd 2002).

To understand how the ND helped affected individuals we investigate mechanisms using information from the 1940 census. Two main mechanisms help explain the benefits of the ND: increases in income and years of education. We find that a standard deviation increases in New Deal relief, resulted in a 3% increase in income for thus who were teenagers in 1930. We also find increases in years of

[4] Black populations living in urban areas and working in services were also very affected. This paper does not study effects on them, see details on the data collection.
[5] Individuals aged 15 to 19 had unemployment rates of 60% in 1934 in the state of Pennsylvania (Margo 1991).

education for teenagers and young adults. We don't find effects on employment on labor force participation, consistent with Modrek et al. (2022).

This paper contributes mainly to three different strands of the literature. First, it studies the relationship between recessions and health outcomes, specifically on mortality and longevity. In this area, studies investigating developed countries in contemporary times show that, in the short run, recessions improve health outcomes and lower mortality rates (Ruhm 2000, Ruhm and Black 2002, Deheija and Lleras-Muney 2004, Ruhm 2005, Miller et al. 2009, Stevens et al. 2015, Strumpf et al. 2017, Tapia Granados and Ionides 2017).[6] However, in the medium to longer run this pro-cyclicality does not seem to hold. Several studies document negative long-term effects of recessions on life expectancy, and disability, as well as on lifetime income (Coile et al. 2014, Thomasson and Fishback 2014, Cutler et al. 2016, Schwandt and von Wachter 2020, Duque and Schmitz 2020). On the other hand, studies in developing countries tend to find the recessions increase mortality, which many authors believe is due to the absence of well-developed safety net programs (Doerr and Hofmann 2022).

A few studies have investigated the effects of the GD on health and mortality. Using aggregate data, the literature finds that the GD resulted in short-term declines in mortality, despite the fact that during this time in the US there were very few safety-net programs available to the population (Tapia Granados and Diez 2009; Stuckler et al. 2012). Our findings differ from this literature. One reason is that we use of individual data which allows us to tract the individuals even if they move. Arthi et al. (2022) demonstrate that in settings where individuals move in response to economic shocks, aggregate mortality rates for a given region will fall artificially because those who might die in badly affected areas die elsewhere. Another reason is that our data might not include all affected populations—it is possible that the individuals that are not in our study (immigrants and non-whites) benefited from the GD.

Our study contributes to this literature by comparing the short- and long-term effects of a recession using individual-level deaths, for the same economic shock, the Great Depression, and the same population. Our findings are in line with those of Cutler et al. (2016), and Schwandt and von Wachter

[6] The literature has documented several reasons for these surprising results: health improves in the short run because during recessions there is a reduction in alcohol use and smoking (Ruhm 2000; Ruhm and Black, 2002; Ruhm 2005; Krüger and Sevensson, 2008;). Additionally, during recessions individuals have more time to care for their dependent children and elderly family members (Dehejia and Lleras Muney 2004; Aguiar et al. 2013). Finally, the quality of healthcare appears to increase during recessions due to the greater availability of health care workers (Stevens et al. 2015).

(2020) who also document long term losses in life expectancy associated with recessions that manifest later in life. However, we improve upon these studies because we are able to consider control for the effect of anti-recessionary programs. Indeed, we find that failure to do this underestimates the impacts of recessions. This provides yet another reason why our study finds more negative effects of the recessions than previous studies which only considered the effects of the GD.

Second, we contribute to the literature studying the effects of the New Deal. There are many studies exploring the effects of the New Deal on various outcomes as consumption, mobility, crime, employment and the spending multiplier among others (Wallis and Benjamin 1981, Balkan 1998, Fleck 1999, Cole and Ohanian 2004, Fishback et al. 2005b, Fishack et al. 2005, Fishback et al. 2007, Fishback et al. 2010, Fishback and Stoian 2010, Neumann and Taylor 2013, Fishback and Kachanovskaya 2015, Arthi 2018, Liu and Fishback 2019). However, few of them explore the effects of the programs on health (Fishback et al. 2007, Modrek et al. 2022). Fishback et al. 2007 found that the ND decreased infant-mortality while Modrek et al. 2022 did not find any effects on mortality in the long-term.

There are two studies which are the closest to us. Aizer et al (2020) who investigate the lifetime benefits of participating in the CCC among young men in Colorado and New Mexico. Using individual data, they also find that the CCC (a ND program) extended longevity. Our study covers individuals of both genders and of all ages across the nation, estimates the effects of the GD and includes all ND relief programs. Our findings are consistent with theirs. Second, Modrek et al (2022) study the long-term effect of the New Deal work-relief programs on longer term child outcomes using a sample of individuals born in Wisconsin between 1937 and 1940. Like in our study, they find increases in income and education, however they do not find effects on survival. Our paper extends the analysis to all the mainland US and cohorts alive in 1930 and uses and IV approach to address potential biases.

Finally, our research also relates to the literature exploring the effects of social programs and programs to compensate shocks on health outcomes (Aizer et al. 2016; Barhan and Rowberry 2013; Hoynes et al. 2012; Barr et al. 2022; Guarin et al. 2022). Our findings are consistent with most of this literature. For example, Aizer at al. (2016) find extensions in longevity when studying the long-term effects of the US Mother Pensions program that took place in the 1920s. Guarin et al. (2022) find positive effects on health outcomes when investigating economic compensation to victims of the Colombian violent conflict.

The paper is organized as follows. Section 2 provides background on the New Deal relief and allocation of the funds. Section 3 describes the different datasets used. Section 4 explains the identification strategy. Section 5 presents the effects of the Great Depression. Section 6 studies the causal effects of the New Deal. Section 7 discusses the potential mechanisms. Section 8 presents some robustness checks; and section 9 concludes.

2. Background: The Great Depression and the New Deal

The Great Depression was the deepest and longest economic decline in modern history. To offset its negative effects, the federal government created the Neal Deal, a set of policies to promote economic growth and help the most affected citizens. This section describes the background of the Great Depression, the New Deal and the geographic allocation of public funds.

2.1. The Great Depression (1929-1941)

The Great Depression is usually defined as the period that started in October 1929, with the stock market crash and lasted until 1941. This period was characterized by four years of large economic declines (1929-1933) and eight years of slow recovery. In the United States, real GDP dropped around 30%, prices went down by 27%, unemployment rose to 25%, about one-third of the workers were employed only part-time, and one-third of the banks failed (Chandler 1970, Romer 2003, Richardson 2007, US Bureau of Labor Statistics).

The negative effects on the economy led to massive consequences for the well-being of the population: increases in poverty, homelessness, hunger and malnutrition, and lack of medical care (Kiser and Stix 1933, Jacobs 1933, Chandler 1970, Poppendieck 1997, Kusmer 2002). Moreover, the context of crisis and job loss resulted in negative psychological impacts on a great share of the population (Zivin et al. 2011). The Dust Bowl, a period of drought and dust storms, also took place in the same period. The damage to the American ecology led to an agricultural depression, intensifying the impact on hunger and malnutrition (Phillips 1999). However, the GD did not affect everybody equally. Young people, the elderly and non-white individuals faced the largest levels of unemployment. Some sectors, like construction, iron and steel, durable goods and automobiles, manufacturing, and real estate, were more affected than others (Chandler 1970, Margo 1991).

The economic effects of the GD also varied across the country. Figure 1 shows the county variation of an index for the severity of the crisis from 1929-1933 (more details on how this index is constructed are provided below). Some areas in the South and Southwest were more affected, in the states of New Mexico, Mississippi, Arkansas, and Oklahoma. In the West, some of the most affected states include Arizona, Utah, and Washington. The East Coast and the Northeast regions were less affected. The difference in industrial composition across regions is one of the reasons for the geographic variation in the severity of the crisis as manufacturing of durable goods and construction fared the worst (Rosenbloom and Sundstrom 1999). Our analysis exploits this county-level variation to identify the effects of the GD on longevity.

2.2. The New Deal and its geographic allocation

In 1933, President Roosevelt approved a vast set of programs for relief and recovery, commonly known as the New Deal.[7] The New Deal included programs for public assistance, public works, housing, and loans. Some of these programs are precursors of modern welfare programs. Yet, most of the New Deal programs offered relief through employment.

We focus on the relief programs, which accounted for 63% of the New Deal non-repayable grants, and Public Works Grants, that accounted for 24% (Fishback et al. 2003). These programs operated through direct work contracts and public assistance. They targeted the most affected individuals, providing access to satisfy their basic needs as food, housing, and healthcare. Hence, they are the most likely to have direct effects on health outcomes. We analyze these programs together because the distribution of funds is highly spatially correlated and thus it is hard to separately identify the effects of any single program.[8]

Our analysis includes the following programs: the Federal Emergency Relief Administration (FERA), which involved direct and employment relief payments; the Social Security Administration Public Assistance (SSAPA), which provided public assistance payments, especially for children, single mothers, and people with disabilities; the Works Progress Administration (WPA), which provided work relief with hours and wage limits; and the Civil Works Administration grants (CWA), which created jobs for millions of people that were unemployed (Schwartz 1976, Fishback et al., 2003). The

[7] New Deal grants between 1933-1939 sum a total of 16 billion dollars (in 1967$).

[8] For example, the correlation between CWA and WPA is 0.94. Appendix Table 13 shows the geographic correlation across all New Deal programs.

Public Works Grants program focused on the construction of highways and public buildings, which were highly labor-intensive projects. In fact, during this period, the federal government became the largest employer in the nation, as these programs employed millions of citizens. The programs we concentrate on account for 87% of the non-repayable spending. Although we exclude some programs as a robustness check, we investigate if our results are sensitive to what programs we include.[9]

The geographic allocation of the funds was not random, and it resulted in geographic variation both at the county and state level.[10] Figure 2 shows the spatial distribution of New Deal funds in absolute and per capita terms, respectively. By comparing it with Figure 1—which shows the spatial distribution of the severity of the crisis—we notice that the government targeted areas with more pronounced economic downturns. Indeed Figure 3 shows that relief spending and economic severity are highly correlated across counties.

Yet, the most affected regions did not always get the largest amounts of money. Previous research shows that in addition to targeting more affected areas, other factors also affected the allocation of the funds. For example, Southern states received less money (Fishback et al. 2007) because politicians argued that living costs there were lower (Couch and Shughart II 1998). States in the West received more funds because they had more federal land, where more public work and infrastructure could be done (Wallis 1998, Fleck 2001). Bureaucrat hurdles also affected where some programs received more funding.[11]

Finally, more funds were sent to areas as a function of political considerations, which we use as an exogenous determinant of the geographic allocation of funds. An extensive literature documents that political incentives partly determined where funds were disbursed. Wright (1974) finds that voters' turnout was an important determinant of funds distribution. Anderson and Tollison (1991) find that indicators of relative political influence are strongly correlated to spending patterns. More recently, Fleck (2001) shows that the fraction of loyal and swing voters across counties affected the allocation of the New Deal spending, as predicted by a model of political choice. The underlying mechanism in

[9] The programs not included are: Agricultural Adjustment Administration (AAA) which accounts for 12.1% of grants; Farm Security Administration (FSA), 0.6%; and US Housing Authority (USHA), 0.8%. We also exclude loans from the analysis. See Appendix Table 14 for the robustness checks.
[10] The Federal government distributed the funds across the states, and then the states distributed the funds across counties and municipalities.
[11] For some programs, the state governor had to sign a statement justifying the need and providing diverse information, and other programs had matching requirements by the state, and this could result in richer states getting more funds.

the model is that the government uses the relief to try to ensure reelection. Fishback et al. (2005) and Fishback et al. (2007) find that different electoral variables such as voters' turnout in different elections, the fraction of votes for democrats, and the variance in democrat's vote over time, are strongly correlated with the New Deal spending per capita. In summary, it is well established by previous research that political variables predict the allocation of the New Deal relief.[12] We consider all these variables as potential instruments for ND funds.

3. Data

To study the long-term effects of the Great Depression and the New Deal relief on longevity, we match individual-level data from the 1930 and 1940 US Census with family tree data from FamilySearch, county-level data on New Deal spending, county-level data on the severity of the crisis, and election results.

3.1. Individual-level data

3.1.1. US Census

We use the 1930 Census and the 1940 Census in this analysis. The baseline sample is the 1930 Census. It provides the county of residence of all individuals living in the US at the very beginning of the GD and three years prior to the New Deal. We use the full count, which includes 120 million individuals. The 1930 census also includes various predetermined characteristics of individuals, such as age, gender, race, nationality, and marital status.

We link the 1930 Census to the 1940 Census using the Census Tree linking set developed by Price et al. (2021). The 1940 Census includes information on intermediate outcomes like income, education, employment, number of children, and marital status.[13] By matching both censuses we also know whether a person moved between 1930 and 1940. We use these variables to understand the mechanisms behind the effects of the New Deal relief on longevity.

3.1.2. FamilySearch – Family Trees

To compute individual longevity, we match the 1930 census to family tree data from FamilySearch. FamilySearch hosts both the world's largest single family tree platform and an archive of historical

[12] We discuss the instrument and the necessary assumptions in Section 4.
[13] As a robustness check, we also link the two census years using the MLP Linking Method (Helgertz et al. 2022).

records that contains information on billions of deceased individuals. Instead of making their own personal family trees, FamilySearch's users connect their genealogies to the public, wiki-style Family Tree by creating profiles for their deceased ancestors, attaching historical records to those profiles, and linking those profiles to the profiles of those ancestors' relatives.[14] The sources users can attach to these profiles include various types of death records, including death certificates, obituaries, gravestones, funeral home records, and Social Security records. Appendix Figure 1 shows an example of a Family Tree from the point of view of a regular user.[15] While anyone can access individual records on Family Search's website, the large-scale compilation of the dataset used in this paper was done in partnership with the Record Linking Lab at Brigham Young University (BYU). Using this dataset we are able to link 30% of the population in the 1930 Census, which is comparable or higher to that achieved in other historical studies. [16] Appendix B explains the linking process from the 1930 Census to FamilySearch deaths in detail.

The resulting dataset has two main advantages. First, our data includes almost 50% of women. Because women tend to change their last name after marriage, historical studies using the census, social security data, and other sources of data do not usual include them because they cannot be linked. As a result, the study of women has been very absent in the economic history literature (Abramitzky et al. 2014, Feigenbaum 2016, Bailey et al. 2017, Bailey et al. 2020, Abramitzky et al 2021). Because the family trees include information on parents' names, we observe women's maiden and married last names so we can link them at the same rate as men.

Second, FamilySearch death data includes deaths since 1930 up to the present day. This allows us to study and compare both short- and long-run effects on longevity. For comparison, a commonly used source of death and birth dates is the Death Master Files (DMF) records which only include information on birth and death dates for men who died between 1975-2005. We also use Death Master Files (DMF) records to compute longevity as a robustness check. Some additional problems appear when matching

[14] A machine algorithm uses the user-made links to records, to suggest record links to profiles, increasing the number of profiles linked to death records.
[15] www.familysearch.org
[16] The Life-M Project links by hand between 28.7-31.1% of a subsample of individuals from birth certificate to deaths in the states of Ohio and North Carolina. For the full sample they link individuals to deaths at a rate of 17.8-23.6% (Bailey et al. 2022). Abramitzky et al. (2014) link 16% of native men from the 1900 Census to the 1910 and 1920 Census. Abramitzky et al. (2012) link 29% of men from the 1865 Norwegian Census to either the 1900 Norwegian or US Censuses.

these records since this data has only been linked to the 1940 census (and not to the 1930 census, our base data).[17]

Our dataset has some limitations: the sources of death data might be of uneven quality; not all the counties are equally represented due to the matching process; and not everybody is equally likely to have a tree profile. For these reasons and others, there may be some selection problems in our sample; we discuss these issues below.

3.2. County-level data

3.2.1 New Deal Relief Data

We use data on New Deal spending by program at the county level published in 1940 by the Statistical Section of the Office of Government. It reports all Federal spending on New Deal programs from March 1933 to June 1939.[18] The data includes information on the loans and grants given to different agencies, such as the Federal Works Agency, the Federal Security agency, the department of Agriculture, and the Federal Housing Administration. To our knowledge, this is the only source of New Deal spending by county. Unfortunately, the data is not broken down by year.

Using data at the county level is important for two main reasons. First, the New Deal programs entailed multiple layers of political administration. Therefore, the final success of each program was determined as much by what happened within states as by what happened across states (Fishback et al 2003). Second, to evaluate the effects of the relief on longevity, it is important to measure the relief received by individuals, and the most disaggregated data available is at the county level.[19]

More than 16 billion dollars were distributed from March 1933 to June 1939 on different non-repayable New Deal funds. From those, 14.1 billion (87%) were allocated to the relief programs of interest here. On average, each county received, for the whole duration of the New Deal (1933-1939), $261.54 per

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[17] The linkage was done by the Censoc project. https://censoc.berkeley.edu

[18] These reports were digitized by Fishback, Horrace and Kantor (2005). Fishback, Price, and Kantor, Shawn. New Deal Studies. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2018-11-18. https://doi.org/10.3886/E101199V1

[19] In the 1940 Census, there is an individual measure of relief participation, however, most participants would be missed as most of the New Deal relief programs ended in 1939. Only 1% of the population answers to be working on relief in the 1940 Census. Modrek et al. (2022) use this data to create a county-level index to New Deal exposure. Individual participation in these programs is available in the National Archives but the records for these have not been digitized. To our knowledge, the only individual level records of participants that have been digitized were digitized by Aizer et al (2020) for men participating in the CCC in Colorado and New Mexico.

capita in 1967$, with a standard deviation of 287.48. In 2020$, this would be an average of $4869.76 per capita.[20] The average relief from 1933 to 1939 represented a 31.6% of the average annual income in 1939.[21] Mohave in Arizona was the county with the highest per capita funds, with more than $9000 per capita, and Armstrong in South Dakota with the lowest, receiving less than $0.28 per capita.[22]

3.2.2. Severity of the Economic Crisis (1929-1933)

To assess the severity of the crisis, we create an index using economic variables from different data sources. This allows us to get a single estimate of the effects of the depression on mortality and longevity, and to compare counties that differed on the relief spending but had the same crisis severity.

The index is the standardized and adjusted sum of the following variables, transformed such that larger values correspond to greater severity of the crisis: the 1930 unemployment rates (from the full-count US Census), the change in retail sales from 1929 to 1933 and from 1929-1935 (from Fishback et al. 2005), the change in farm value (from the Agricultural Census), and the change in income per capita (from 1929 to 1933 from the US Bureau of Economic Analysis). Some of these variables are based on estimates and might not be exact, this might cause some measurement error.[23]

3.2.3. US Election results 1920-1932

We use information from the election results from 1920 to1932 to understand how political incentives affected the distribution of ND funds. The political variables come from data available in "United States Historical Election Results, 1824–1968" (ICPSR 1). They include information on how many votes each party got for different elections. The variables used include the voter's turnout in presidential and congressional elections, the average and standard deviation of the turnout from 1920-1932, the fraction of votes for the democrats and republicans, the average and standard deviation of the fraction of votes for democrats and republicans, the number and fraction of loyal and swing voters, the number of house representatives and their tenures, and the closeness of the elections.[24] In section 4 we explain how we use these political variables for our identification strategy.

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[20] These are the total amounts of relief per capita for the full 1933-1939 period, annually it would be equivalent to $695.68 in 2020 $.

[21] The average income in 1939 was $442.14 ($1062.41 in 1967$). This data comes from the 1940 full-count US Census, and it is top coded at $5001. If we divide the amount of relief by the seven years, it represents a 4.5% of the average income.

[22] In our sample we drop 2 counties with extremely large values of New Deal relief per capita. They represent less than 1% of our original sample.

[23] We investigate if our results are sensitive to the construction of the index as a robustness check. We also re-estimated our results including all the variables instead of the index. See appendix table 15.

[24] Voter's turnout, votes for democrats and republicans are included for all election years since 1920 to 1932.

3.3. Estimation Sample and Summary Statistics

Table 1 shows summary statistics of individuals in the full-count 1930 US census (column 1) and in our FamilySearch linked sample (column 2). Less than 1% of our linked sample is non-white, and only around 3% are foreign born. Since these populations are under-represented in our data, we restrict our analysis sample to white, US born individuals.[25] Columns 3 and 4 of Table 1 present the same summary statistics as columns 1 and 2, but for our analytic sample. The statistics in column 4 are weighted at the cohort and county level to be representative of the white, US born 1930 population.

There are 89,677,282 white native-born individuals in full-count 1930 US Census. We link 26,508,899 individuals to their death dates, 29.6% of the 1930 census sample. This matching rate is comparable or higher to that achieved in other historical studies. [26]

Table 1 shows that once we restrict our sample and weight it, our analytic sample is broadly representative of the 1930 population we target. The average New Deal relief per capita across the entire sample is $270, slightly more than the county level average provided in section 3.2.1. The average age of the individuals in our sample in 1930 is 27. Although women are slightly under-represented (we link 31.95% of the men, and 27.2% of the women), about half of our sample are women, which is significantly higher than other studies that use linked historical records (Craig et al. 2019, Abramitzky et al. 2021). Our linked sample is a bit less urban than the full-count census, and individuals in our sample are more likely to be married. This likely happens because of the construction of the Family Tree: married people are more likely to be on the tree because they are more likely to have had descendants who could later add them to the tree.

3.4. Matching and Sample Selection

Not all counties are equality represented in our sample. Match rates at the county level are presented in Appendix Figure 2, and they range from 5% to 77%. The larger match rates are in Utah and Idaho, where FamilySearch's modern users are overrepresented. The lowest match rates are in New Mexico

[25] Other studies that use FamilySearch data also face this issue and take the same approach (Lleras-Muney et al. 2022).
[26] The Life-M Project links by hand between 35.8-37.8% of men and 21.5-24.4% of women from birth certificate to deaths for a subsample of individuals in the states of Ohio and North Carolina. For the full sample they link individuals to deaths at a rate of 22.9-27.8% for men and 12.7-19.3% for women (Bailey et al. 2022). Abramitzky et al. (2014) link 16% of native men from the 1900 Census to the 1910 and 1920 Census. Abramitzky et al. (2012) link 29% of men from the 1865 Norwegian Census to either the 1900 Norwegian or US Censuses. Craig et al. 2019 matched 30% of married women of specific cohorts from marriage certificates in Massachusetts to the 1850, 1880 and 1900 US Census.

and the south of Texas. To account for this problem, we weight our dataset at the cohort and county level, and as already discussed and shown in Table 1, using these weights we obtain a sample that is mostly representative of the white, US born 1930 US population.

Nevertheless our final linked sample suffers from sample selection in some dimensions for various reasons. First, we are more likely to observe ancestors of people who are interested in their genealogy. Second, our linked sample has a smaller fraction of people who were relatively young in 1930 compared to the full-count census. This is shown in Appendix Figure 3. This could be due in part because individuals that are still alive do not have deaths on the tree. Finally, FamilySearch's users tend to only introduce information regarding their own ancestors. People who died young are less likely to be known by their family members, so they are less likely to appear in our sample. Compared to Vital Statistics deaths for the 1929 Cohort, our sample misses a large amount of infant and young deaths (Appendix Figure 4). To account for this selection, we condition our sample to survival to age 20 in the robustness checks.

To account for other types of selection, we study who has missing longevity information, and whether individuals who miss this information differ from the general population. Table 2 presents the estimates of the effects of different individual characteristics to an indicator whether the individual has a death record. Some individuals have higher probabilities to be linked to their deaths than others. Linked individuals have larger families, have higher socioeconomic status, and live in areas where the recession was less severe recession and that received less relief. So our analytic sample is a positively selected sample of individuals who would be expected to live longer than average. As mentioned above, to solve some of these issues, we weigh the population at the county-cohort level, and we control for factors that affect the probability of being linked when doing our analysis.[27]

3.5 Who received relief

To understand who was most likely to receive ND relief, we use the full-count 1940 census which included a question asking if the individuals were at work on, or assigned to, public emergency project work or local work relief. The main limitation of this source of information is that there were many fewer people on relief by 1939 than there had been in previous years.

[27] We investigate if our results are robust to weighting using the predicted probability of being linked in Appendix Table 16, following the work by Bailey et al. (2020).

We find that only 2% of individuals were working on relief, and only 8% of the households had a member receiving relief. Appendix Table 1 presents the results from a regression of the indicator of receiving relief in the 1940 census for both single individuals and households on individual characteristics. People in households receiving the relief are less likely to be white, married, have children, own a house, and live in urban areas. They are more likely to have larger family sizes and to be native born.

These patterns can be partly explained by age differences. Appendix Figure 5 shows the age distributions of individuals who worked on relief in 1940, compared to the ones who did not. A large fraction are young individuals between 18 and 22 years old who are less likely to be married or to have kids. In fact, most individuals working on relief were young adults, possibly entering the labor market. Moreover, the individuals receiving relief were poorer as Appendix Figure 6 shows, they had lower family wages. However, these differences do not seem to be statistically significant (Appendix Table 1).

4. Empirical approach

To obtain causal effects of the New Deal relief and the Great Depression on longevity, we would like to estimate the following Accelerated Failure Time (AFT) model of duration[28]:

$$Log(Age\ \ at\ \ Death)_{ict} = \beta_0 \ + \ \beta_1 \ Log(Relief\ \ Spending)_c \ + \delta \ Crisis\ Severity_c$$
$$+ \alpha_1 X_i + \ \alpha_2 X_c \ + \gamma_t \ + \ \gamma_s \ + \ u_i \qquad (1)$$

Where *ict* stands for an individual *i* living in county *c* and born in the year *t*. *Xi* are the individual covariates from the 1930 census: age, urban, married, schooled, employed, in the labor force, occupation score, family size and number of children. *Xc* are county controls selected using lasso: severity index, % black, % rural farm, farms per capita, % farms area, % farms 50-99, % farms 500-999. *γt* are cohort fixed effects, and *γs* are state of birth fixed effects.[29]

[28] This is one of the main two models used to study durations, and it assumes that the covariates have proportional effects on the duration. Alternatively, one could use a proportional hazard model. Since we do not have time-varying covariates, it is not clear this alternative presents any advantages, but it would present large computational difficulties as the data would have to be transformed into a panel of individual * year observations.

[29] In appendix table 11, we present the results for the analysis on longevity using levels instead of logs.

To estimate and compare the short- and long-run effects of the Great Depression and the New Deal we estimate a survival model instead, where we will estimate the following regression for each 10-year age cohort separately:

$$1(Survived\ to\ m)_{ict} = \beta_0 + \beta_1\ Relief\ \ Spending_c + \delta\ Crisis\ \ Severity_c + \alpha_1 X_i + + \alpha_2 X_c + \gamma_t + \gamma_s + u_i \quad (2)$$

Where m is a year from 1930 to 2020. Since we estimate this for a given cohort (e.g. those born between 1915 and 1925 , who were between 6 and 15 years old in 1930), surviving to a given year is equivalent to surviving to a given age.[30] So, 1(Survived to m) =1 if the person died after the year m, and 1(Survived to m) =0 if the person died the year m or before. *ict* stands for an individual *i* living in county *c* and born in the year *t.* The covariates are the same as in equation 1. In both specifications the standard errors are clustered at the county level.

Even accounting for county severity, there are counties that received different amounts of relief. To address this we include the set of county controls described above that are both predictors of the relief and longevity. We do not observe who receive relief at the individual level, only at the county level. However, we know, that some individuals were more likely to receive relief that others depending on demographic characteristics. For this reason, we include the pre-determined individual covariates from the 1930 census, defined above.

The coefficient delta estimates the effect of the recession on outcomes in relative terms. Since the index has been normalized the coefficient measures the impact of an increase in one standard deviation in the index on outcomes. The coefficient β_1 estimates the effect of one more dollar in ND relief on outcomes. For a causal interpretation of β_1 and δ_c we further require that the New Deal relief spending and the crisis severity be orthogonal to other determinants of the longevity that are not controlled for in the model. We do not have access to an instrument for severity. Thus, the analysis of these effects will be descriptive.

[30] We grouped the youngest cohorts in an interval of 5 instead of 10 years, because under 5 mortality tends to differ from mortality in older ages.

However, we attempt to obtain causal estimates of the effects of the ND. Naive OLS estimates of the effects of the New Deal relief on longevity from equation (1) might be biased for several reasons. First, there might be omitted variables related to the crisis severity. Even though we control for the severity of the GD, this severity might be poorly measured. For example, there might be some relevant variables that we can't observe, such as the change in personal income or individual wages, which we cannot include in our computation of the severity index. Second, there can be different sources of measurement error related to both the New Deal relief spending and the crisis severity, leading to attenuation bias. The data available about the New Deal spending provides information on the funds from the federal government to the counties, but, for example, there could be missing transfers if there are independent funded programs at the municipal or at the individual level. Finally, there could also be some error from assuming people suffers the recession and get relief in the county they are in 1930. We will separate movers from non-movers in our robustness.[31]

4.1. Identification Strategy using IV-LASSO

To assess the long-term effects of the New Deal relief and address the issues described above, we use an instrumental variable approach based on political variables from 1920-1932. The ideal instrument predicts where the funds are allocated (relevance assumption), but it is otherwise uncorrelated with predictors of longevity, conditional on the severity of the crises (exclusion restriction assumption).

Our Instrumental Variable (IV) approach is based on the political incentives that influenced the geographic allocation of the New Deal relief funds. The political models in the literature agree that the main variables affecting the relief were the voter's turnout, the support to democrats, how tie the elections were, the number of loyal and swing voters and congressional influence among others (Anderson and Tollison 1991, Wright 1974, Fleck 1999, Fishback et al. 2005, Fishback et al. 2006). However, it is hard to select which political variables affected the New Deal relief the most and how. Many of these variables could matter, and their interactions could also matter. There are 25 potential instruments previously used in the literature. If we allow for interactions, second order terms then the set of potential instruments include more than 1000 variables.

We use a sparse model that identifies and uses optimal and parsimonious controls to select our instruments among this set of potential instruments. We use least absolute shrinkage and selection

[31] See appendix table 7.

operator (LASSO) for instrumental variables to select the best predictors or relief (Belloni et al., 2012; Belloni et al., 2014; Chernozhukov et al., 2015). This machine learning methodology results in the selection of optimal instruments and a sparse set of controls, given the assumption of approximate sparsity. This assumption assumes that the conditional expectation of the endogenous variables given the instruments can be approximated well by a parsimonious yet unknown set of variables and imposes a restriction that only some of the variables have non-zero coefficients.[32]

Thus, we select the instruments and controls by estimating:

$$\beta = arg\,min \sum_{i=1}^{n} \quad (y_i - \sum_{j=1}^{n} \quad x_{i,j})^2 + \lambda \sum_{j=1}^{p} \quad |b_i|\gamma_j \tag{3}$$

where \lambda is the "penalty level" and \gamma_j are the "penalty loadings." The penalty loadings are estimated from the data to ensure the equivalence of coefficient estimates to a rescaling of x_ij and to address heteroskedasticity, clustering, and non-normality in model errors. Similarly, standard errors are clustered at the county level to address within-county correlation.

The algorithm for the IV-LASSO methodology does the following: First, it estimates a Lasso regression with the New Deal relief as a dependent variable, including all the potential instruments (Z) and potential controls (X). From this first regression a group of instruments and controls are obtained. Second, it estimates a Lasso regression with the outcome variable, longevity, and all the control variables (X) (but not the instruments) as regressors. From this second regression, we get a second set of controls. Third, it estimates a Lasso regression where the New Deal relief spending is the dependent variable and all the controls (X) are the regressors. Finally, we estimate a 2SLS regression using the selected instruments in step 1, and the controls selected in steps 2 and 3, to get the post-lasso IV estimator.[33] When using the Lasso algorithm, we partial out cohort fixed effects and state of birth fixed

[32] The potential set of county controls includes total population, population for different age intervals, population density, % of black, % of foreign born, % of schooled in different age intervals, % of urban and rural population, % of people in urban and rural farms, % of people not in farms in rural areas, illiteracy rates, manufacturing establishments pc., % of wage earners in manufacturing, average manufacturing wages, manufacturing product value, manufacturing added value, manufacturing added value pc., % of gainful workers, % out of work, % layoff, whole establishments pc., whole average wages, % stocks, retail stores pc., % of retail employment, retail sales pc., retail stocks pc., average retail payroll, value of crops pc., number of farms, farms pc., area, area of farms, % farms' area, average farm size, area for crop, area for pasture, % farms of different sizes, and farmland value pc.

[33] All the county controls defined at the beginning of this section and the crisis severity are selected by our IV Lasso approach.

effects – in other words we always include these controls.[34] The Post-Lasso estimator refits the regression via two-stage least squares to alleviate Lasso's shrinkage bias.[35]

After this process, the lasso algorithm selects one instrument that we label "voting culture exploitability", and the sparse set of controls defined at the beginning of section 4. The voting culture exploitability instrument is the interaction of the standard deviation of the 1932 presidential election voters' turnout with the standard deviation of the 1928 Congressional election voters' turnout.[36] By the nature of the standard deviation of the voter turnout, our instrument will take values from 0 to 0.0625 (as each standard deviation takes values from 0 to 0.25). The instrument takes larger values when the county has a medium level of voters' turnout, and low values in areas with very low and very high turnouts.

This instrument reflects the voting culture exploitability in different areas: how easy it is to obtain additional votes in a given location given their voting behavior. Places with very low turnouts do not have voting culture, so obtaining and extra vote in these locations may be very expensive: even if the incumbent spends money in those areas, it will be hard to induce additional people to vote. Places with very high turnouts have a robust voting culture. As a result, there are fewer people left to be convinced. Places with medium-level turnout, have some voting culture so it might be possible to induce people to vote. Because there are also more people that could potential vote obtaining more votes there is likely cheaper. Thus, it is efficient to allocate funds in places with medium-level turnouts.

The key identification assumptions are that the IV is relevant and that the exclusion restriction holds. We discuss each assumption now. Voting culture exploitability is strongly correlated with the New Deal relief spending per capita, as shown in the bin scatter plot in Figure 4. Appendix Table 2 shows that the instrument is strongly predictive of ND relief both at the county level, and at the individual level. The F-Statistic has values of 108, 41.99 and 29.79 in the different specifications, well above the recommended cutoffs.[37] Figure 5 documents that there is substantial cross-county variation in the

[34] We partial out the fixed effects because they are important in our model form a theoretical point of view. We want to compare individuals born in the same year and same state, as both will affect the age of death.
[35] We use the ivlasso package to compute these estimators (Ahrens et al., 2020).
[36] Standard deviation is defined as turnout*(1-turnout).
[37] The highest F-stat corresponds to the county-level specification. The others correspond to the individual-level specifications without and with controls respectively. Appendix Figure 7 shows the distribution of the voters' importance instrument. The instrument is concentrated between the values 0.04 and 0.06, with some counties with values between 0 and 0.2. Counties with lower values have very low or very large voters' turnout.

instrument. The South is the region with lowest values, as voters' turnout was very low in the area. Interestingly this area also received the lowest relief.

We also gather some empirical evidence to support the exclusion restriction assumption. For the exclusion restriction to hold, we need the instruments to affect longevity only through the New Deal relief funds, conditional on the severity of the crises and on the other controls. A possible way to get this evidence, is to test the correlation between health variables before the New Deal and the instrument. Thus, we explore if the county level mortality rates from 1920-1928, are correlated with our instrument. Appendix Figure 8 shows the voting culture exploitability is not correlated with the mortality rates before the New Deal. This provides evidence that the selected instrument is valid.

5. The Dynamic Effects of the Great Depression

In this section we descriptively analyze the short- and long-run effects on longevity and survival.

We start by analyzing effects on longevity. Table 3 presents in column 4 the OLS estimates of the effects of the severity of the crisis on longevity including controls, but without accounting for New Deal relief. In columns 5 to 7, we control for the New Deal relief. Individuals who lived in places with a more severe depression lived shorter. When we do not account for the relief generosity, one standard deviation increase in the severity of the Great Depression is associated with a decrease in longevity of 0.12%, which on average represents a decline of about 1 month. When we control for the New Deal relief, the estimate is 60% larger in magnitude, and the average decrease in longevity is 1.6 months.[38] Because more funds went to places with larger recession, when we do not account for the relief, the Great Depression coefficient is biased and smaller, consistent with the idea that it captures some of the positive effects of the New Deal. But since the ND was not randomly allocated these OLS estimates are still biased.

In Table 4 we present the post IV-Lasso estimates, where we use an instrumental variable for the New Deal relief. The coefficient for the severity index almost doubles compared to the OLS coefficient. A one standard deviation increase in the severity index decreases longevity by 2.8 months on average. We are interested in exploring how these effects differ by gender, as. Most relief recipients were men.[39]

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[38] The coefficients are not statistically different when we analyze them by gender in Appendix Table 3.

[39] See Appendix Table 1.

The point estimate for men more than doubles, leading to a reduction in longevity of 3.5 months. For women, the IV coefficients are small and statistically insignificant.[40]

The effects of the depression could differ by age, as some groups could me more sensitive to the economic shock than others. Appendix Table 4 shows the effects of the GD on longevity by birth cohorts, where a cohort is defined as a 10-birth year group.[41] We find that individuals aged 10-20 have the largest effects, and they experience decreases in lifespan of 2.6 months, for a one standard deviation increase in the crisis. Followed by individuals aged 0-10, who experienced decreases in longevity of 2 months on average.

We want to understand when the longevity declines occur. To do this, we study the effects of severity on survival to each year from 1930 to 2020 separately by birth cohorts. As survival rates depend on the age of the individuals, we study the effects on survival separately by cohorts.[42] Figure 6 presents the OLS and IV estimates for the cohorts aged 16 to 25 in 1930, which are the most affected cohorts. For this group of cohorts, we can see that negative effects appear right after the start of the Great Depression and become significant after 1939, when the cohort was ages 26 to 35. The magnitude of the effects increases steadily with age and peaks around age 60, 30 years after the Great Depression ended. One of the reasons for seeing delayed effects could be that there are few deaths before age 60 – indeed the survival rate to age 60, conditional on being alive at age 20 is 0.83%. The largest effect is found in 1984—when the cohorts are 74. In that year, a standard deviation increase in the crisis severity decreased survival by 2.7%. Appendix Figure 9 shows the IV estimates on survival for the other groups of cohorts.[43]

For all the cohorts we find a similar pattern, larger negative effects in the long-run compared to the short-run. This delay in effects likely occurs because health responses to economic shocks take some time to accumulate and cause individuals to die. Schwandt and von Wachter (2020) indeed document an increasing pattern of mortality effects of the 1982 recession, similar to the pattern found here. These cumulative and delayed effects are also predicted by the model of Lleras-Muney and Moreau who simulate the impact of temporary shocks among 20-year olds on cohort mortality profiles.

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[40] The severity coefficients for men and women are statistically different in the IV specification.
[41] The estimates are conditional on surviving to age 20. They are not statistically different to the unconditioned estimates.
[42] To further account for trends in longevity, these regressions also control for cohort fixed effects.
[43] For the groups 0-5 and 6-15 we condition the sample on survival to 20 years old, to address the fact that young deaths are underrepresented in our sample.

If we disaggregate the effects by gender, we observe in Appendix Figures 10 and 101that the magnitude of the effects for men is larger than the ones for women in all cases. The largest effects for men are for the 1914-1924 birth cohorts in 1994, where one standard deviation increase in the severity of the crisis is related to a decrease in the survival rates of 7%. For women this happens in 2001 for the 1914-1925 birth cohorts, where an increase in one standard deviation of the severity of the Great Depression associates with a decrease in survival rates of 6%.[44]

In summary we find that the Great Depression was bad for the well-being of the population. The effects on health appear to be larger in the long run, teenagers, children and men have larger effects. A possible reason why young men have the largest effects is that they had the largest unemployment rates during the recession, so they were one of the groups suffering the most in 1930. Also, they were ending school and entering the labor market in a recession which has long-term negative consequences on income and longevity (Schmitz and von Watcher, 2019, 2020).

6. The Causal Effects of the New Deal Relief on Longevity and Survival Rates

In this section we estimate the casual short- and long-term effects of the New Deal relief spending, using the identification strategy explained in section 4.1.

Table 3 shows the OLS estimates of the New Deal relief on Longevity. Columns 1-3 present the estimates without accounting for the crisis severity while column 5-7 do account for it. In the first column we can see that the New Deal relief is associated with negative effects on longevity. When we add county controls in column 2, to account for the fact that places who got relief were doing worse, the magnitude of the coefficient decreases almost to half. This is expected, as the New Deal relief and the Great Depression are very correlated. In column 5, when we add the crisis severity, the relief coefficient decreases in magnitude by half compared to column 1. This indicates that the OLS coefficients have some negative bias: when we don't account for severity the effects are more negative.[45]

[44] We repeat our estimation using with mortality rates instead of survival rates. The findings are very similar. However, the effects on mortality are less precise.

[45] By gender, the OLS coefficients are not statistically different (Appendix table 3).

To address the potential bias in the OLS estimates, we now show the results from the IV specifications. Recall the intuition for this identification strategy: we are comparing individuals in counties that obtained more relief because of political motivations, to individuals in counties with the same severity of the GD but in which less money was allocated for political reasons. Table 4 presents the post IV-Lasso estimates from equation (5) on longevity. The odd columns show the first stage estimates. As we noted earlier, the coefficients on the severity index are positive and statistically significant, indicating that more New Deal funds went to places where the crisis was more severe. The voting culture exploitability instrument is positive and statistically significant, so places with larger values of the instrument got more funds.[46]

The coefficient on the relief is now positive and statistically significant. Unlike the OLS, these estimates imply that the New Deal relief extended longevity. In column 2, the specification without controls, the coefficient for the New Deal relief is positive, and compared to the same OLS estimate, its magnitude more than doubled. One standard deviation increase in New Deal relief ($140), increased longevity by 9 months on average. When we include all the controls, in column 6, the coefficient is still positive and significant but slightly decreases in magnitude, indicating an average extension in longevity of 6 months.[47]

Next, we investigate if the New Deal compensated for the negative effects of the Great Depression. To do this, we estimate the predicted effects of the New Deal and of the GD severity and compute the net effect. Figure 7a presents the histograms for the predicted effects of the Great Depression and the New Deal using the post IV-Lasso specification. The predicted effects of the crisis on longevity are mainly negative, and positive for the relief. Figure 7b presents the density for the computed net effects. On average the New Deal more than offset the negative effects of the recession. On net, there is an average one-month extension in longevity.

Heterogeneity

Understanding how the effects of the New Deal relief on longevity differ across the population is key for policy evaluation and future policy design. Individuals who received relief during their working

[46] The F-stats range from 56.3 to 23.33, indicating the instrument is strong Moreover, the Anderson-Rubin test rejects the null hypothesis that the coefficient of the relief on longevity is zero in all the specifications (Lee et al. 2021).

[47] The $140 of New Deal relief are equivalent to approximately $2000 in 2020 $ for the full period 1933-1939. We could think about it as an amount of $285.7 a year for 7 years in 2020$.

age can be affected differently than children. Women and men worked in different industries and occupations and were affected differently by the GD. They also received relief at different rates. When we analyze the causal effects of the New Deal relief on longevity by gender, in Appendix Table 4, we see that the main effects come from men. Women have a positive coefficient, but it is smaller and not statistically significant. For men, a one standard deviation increase in the New Deal relief ($140), extended longevity by 11.4 months.[48]

We next analyze the causal effects of the New Deal on longevity by cohort. Figure 8 presents the post Lasso-IV estimates for the New Deal relief on longevity by cohort for the whole sample. The results are only significant for young individuals born between 1915 and 1905, who were teenagers in 1930. For them, an increase of one standard deviation of the relief ($140) caused an extension in longevity of 15.4 months (1.3 years). This is consistent with the work of Aizer et al. (2020) on Civilian Conservation Corps. For men, Appendix Figure 13 we find positive effects of the New Deal for all cohorts, but significant for most cohorts born before 1915. The point estimates are largest for those born between 1913 and 1915, for whom an increase of one standard deviation of the New Deal relief ($140) extended their longevity by 23.5 months (1.95 years). For women the magnitude of the effects is lower (Appendix Figure 14) and only statistically significant for women born between 1910-1912, who present increases in longevity of 12.6 months. For other cohorts, the effects appear to be zero.[49]

To study the dynamic effects of the New Deal relief we investigate the effects on survival. Figure 9 and Appendix Figures 15 to 18 show the dynamic effects for different groups of cohorts estimated both by OLS and IV-Lasso. We can see in the figures that the OLS estimates for all the cohorts are practically zero. However, when we look at the IV estimates, the New Deal relief has positive effects on the survival rates for all the cohorts, with larger magnitudes in the long-run. The cohorts that benefited the most are the individuals aged 16-25 and 6-15 in 1930. The effects are largest in 1988 and 1984 respectively, when the cohorts are around ages 60-80 which is again consistent with the model of cohort mortality by Lleras-Muney and Moreau (2022). For that period, a standard deviation increase

[48] In Appendix Table 11, we present these estimates using specifications in levels instead of logarithms. The results are very similar, an increase of one standard deviation in the New Deal relief per capita extended, on average, longevity by 5.7 months when we account for all the white native population, and by 11.3 months for men. For women, the effects are not statistically significant. In Appendix Table 8, we present the same results as in Table 4 but conditioning the sample to individuals surviving to 20 years old, to account for the fact that our dataset does not accurately report young deaths. The coefficients are not statistically different.

[49] As young cohorts are not that well represented in our data, especially if they died young, in Appendix Figure A23 we show the same graph conditional on surviving to age 20, the coefficients do not differ from the ones in Appendix Figure 14.

in the New Deal relief results in an increase in survival rates of 18.3% and 11% respectively. For the rest of cohorts, the effects on survival are smaller.

Appendix Figures 19 and 20 confirm that men are much more affected than women. The figures present the IV coefficients on survival by gender.[50] For men we see the largest effect for cohorts aged 16-25 in 1930. Women also get the larger effects in the cohort 16-25, although the coefficients are smaller than for men.[51]

Summarizing, we find that men and teenagers were the most affected by the New Deal relief. Two main hypothesis could explain the fact that men and teenagers are the main beneficiaries of the New Deal relief. First, they suffered more the negative effects of the crisis than women and other cohorts, so they got positive effects of being compensated by the relief. Second, they were the main recipients of the New Deal relief, a large share on men and teenagers got relief, compared to a lower share of women, as we have seen in section 3.[52] These results are consistent with other studies documenting men seem to be more sensitive to adverse shocks and disadvantaged environments than women (Autor et al. 2019; Van den Berg et al. 2016; Bertrand and Pan 2013). Moreover, teenagers might experience larger effects because they are in age of finishing school and entering the labor market, and the benefits of getting a relief job in this situation might be larger.[53]

We also investigate if there are other sources of heterogeneity. First, we want to know if the relief compensated more for the poor. We divide the sample by occupation score in 1930, our results show in Appendix Table 5 that the lowest quartile was more effected by the recession and more compensated by the New Deal relief than the upper quartile.[54] Since we do not find any effects for women, we explore if whether married women benefit from the ND relief through their spouses in Appendix Table 6. We do not find different effects. Finally, in Appendix Table 7 we compare the IV estimates for men who changed (or not) counties between 1930 to 1940.[55] Since we use their county of residence in 1930 when assigning New Deal relief and Great Depression values, if the individuals move it could be a

[50] The OLS coefficients on survival by gender are available upon request.
[51] For men, the largest coefficient is for survival to 1984, where a standard deviation increase in the New Deal relief increases survival rates by 27.6%. For women, it is in 1987, increasing survival rates by 10.2%.
[52] See Appendix Figure 5.
[53] See Appendix Figure 12.
[54] Occupation score in 1930 is an approximation for income, as the 1930 US Census did not include income information. We can’t reject the null hypothesis that the crisis severity coefficients are equal. The coefficients for New Deal relief are statistically different.
[55] About 20% of our linked sample moved from one county to another in another between 1930 and 1940.

source of measurement error. Men that did not move were more affected by the recession, and slightly more affected by the New Deal. Considering that individuals in places were the recession was more severed moved more could explain why movers have smaller point estimates.[56]

7. Mechanisms

In this section we explore the potential mechanisms to understand why and how the Great Depression and the New Deal relief affected longevity for a subsample of the population, by investigation outcomes in 1940 as a function or relief.

The Great Depression affected labor market outcomes, years of education, family composition and cross county mobility from 1930 to 1940. Table 5 presents the IV results of the Great Depression and New Deal relief on the different 1940 outcomes for our linked sample to FamilySearch deaths.[57] The GD negatively affected the 1940 labor market outcomes. In places in which the crisis was more severe, people were less likely to be employed, in the labor force, and they had lower incomes, although the effects are modest. The Great Depression also affected family composition. The GD decreased the probability of being divorced, and increased the probability of being widowed, which is expected if it increased mortality. Finally, people in places in which the recession was more severe, leaved their homes and moved to other counties more.

The New Deal relief also affected the 1940 outcomes, but the effects are less precise. On average, it improved labor market outcomes. However, these improvements are only statistically significant for labor force participation. The ND also has positive point estimates for the years of education, but nonsignificant. It also affected family structure, increasing (decreasing) the probability of being divorced (widowed).

Since we find that men were most affected by both the New Deal and the Great Depression, we study the effects on 1940 outcomes for men in Appendix Table 8. The Great Depression negatively affected the labor market outcomes and had very small effects on family structure. It affected the probability of moving counties by 11%. Although men present the largest effects on longevity, the New Deal, on

[56] The crisis coefficients for the Great Depression are statistically different for movers and non-movers. For the new deal, we can't reject the null hypothesis that they are equal.
[57] The first stage is presented in table 17, and it is strong for all the specifications. In Appendix table 18 we present the same results for the whole 1930 population linked to the 1940 census, without the need of having a FamilySearch death.

average, did not affect the 1940 outcomes for men. Since teenagers had the largest effects on longevity, we explore whether the effects on 1940 outcomes differ by cohort in Appendix Figures 21 and 22. Teenagers in 1930,, are the only group to present positive and significant effects of the ND on income. They also present positive effects on education. We don't find effects on the probability of being employed or in the labor force, although teenagers have large positive point estimates for employment. We also find that in places with more new deal, teenagers married more and stayed longer in school. [58]

8. Robustness checks

This section presents the different robustness checks considered to address issues in our data that could bias our results. We present the main results at the county level, in levels, and conditioned on surviving to 20 years old.

The New Deal spending data available to us is at the county level, as a robustness check we present the estimates on longevity at the county level.[59] The OLS estimates for the New Deal and the Great Depression on average longevity in Appendix Table 9 are smaller compared to the individual-level estimates (Table 3). But now, they are positive for both the ND and the GD in the joint and men specifications. It could be that when grouping the information at the county level the bias acts differently, and now the Great Depression is partially absorbing the positive effects of the New Deal. The county-level IV estimates in Appendix Table 10 are more similar to the individual ones. The New Deal has positive effects for the full sample, men and women, and the coefficients for the Great Depression are negative in all the specifications. However, we find larger effects in the county level specifications. The magnitude of the coefficients almost doubles, and the effects became significant also for women.[60]

When analyzing the effects on longevity, we follow an accelerating failure time model, thus instead of using as a dependent variable the age of death we use its logarithm. In Appendix Table 11, we present

[58] The joint effects for women over 32 years old on years of education are not statistically significant.
[59] In the county-level specifications, the dependent variable is the average of the logarithm of the individuals' age at death at the county level. Besides the county controls, we are also include individual covariates as county-level averages.
[60] A one standard deviation increase the New Deal relief, extended longevity by 10.9 months overall, by 22.5 months for men, and 5.8 months for women

the main results in levels, and the effects are equivalent to the ones in our preferred specification in logs.

Young deaths are underrepresented in our sample as individuals who die young are less like to be linked to their deaths. To address this issue, we estimate the main model restricting our sample to individuals who survived the age of 20 and show the estimates in Appendix Table 12. The estimates are consistent with our findings in the main specification.

9. Conclusion

We use a novel dataset linking the population alive in 1930 to their deaths, to study the short- and long-term effects of the Great Depression and New Deal relief on the well-being of the population, measured with longevity. We provide evidence that the Great Depression was bad for people's health, especially in the medium- to long-run. Also, we find that New Deal relief extended individuals' longevity, and it more than compensated for the negative consequences of the Great Depression. These findings are driven by men and teenagers, and we do not find effects for women. It is well documented that young men suffered the largest levels of unemployment during the Great Depression and were therefore among the most affected sectors, so this result is encouraging. We find that much of the effect of New Deal spending on longevity for the most affected groups likely came through increases in income and education using outcomes from the 1940 US Census. Interestingly, we find that New Deal spending had no effect on employment or labor force participation.

The literature on the effects of recessions on health traditionally finds that in the short-run mortality is contracyclical, meaning that recessions are good for health (Ruhm 2000, Ruhm and Black 2002, Deheija and Lleras-Muney 2004, Ruhm 2005, Miller et al. 2009, Stevens et al. 2015, Strumpf et al. 2017, Tapia Granados and Ionides 2017, Tapia Granados and Diez 2009; Stuckler et al. 2012). However, our findings suggest that the GD decreased longevity both in the short and long run. This is consistent with recent literature, exploring effects of recessions using individual data that allows to track population even if they moved (Arthi et al. 2022). Our negative long-term results are consistent with the literature exploring the long-term effects of recessions (Coile et al. 2014, Thomasson and Fishback 2014, Cutler et al. 2016, Schwandt and von Wachter 2020, Duque and Schmitz 2020).

Our results on the effects of the New Deal are consistent with Fishback et al. (2007) and Aizer et al. (2020) which find positive effects of the New Deal on mortality, and a specific ND program, CCC, on longevity, respectively. Like Modrek et al (2022) we find increase in income and educational outcomes. However, our findings on health outcomes differ, as they do not find effects on mortality. This could be explained by differences in the empirical strategy, as they estimate the association of work relief in 1940 and mortality later in life. Our OLS estimates are consistent with these results.

The results in this paper could have important implications when evaluating or designing public policy, as it provides evidence that both recessions and the policies designed to address them can have large effects on individuals' lives in the long run. For example, in the last two decades the US suffered two main recessions, in 2008 and 2020, during the covid pandemic. Our results could shed some light on who to target during this economic downturn, as we have seen that the most affected also benefit the most from the relief. However, when trying to generalize these findings, we need to consider that in our setting, the "social safety net" was inexistent in the United States. Nowadays, there exist several types of policies that may dampen the negative effects of a recession. In addition, our sample is positively selected towards individuals with above-average lifespans, which could lead to our results having underestimated the effects of both the Great Depression and the New Deal.

As new data become available and as existing data and record linking processes are refined, future research that builds on this study will benefit from better linking rates and the ability to examine other outcomes than lifespan. For example, when the full count 1950 US Census becomes broadly available for research use, future researchers could replicate our methods and potentially explore medium-term effects on income, employment, and so on. In addition, with the improvements in matching rates, this analysis could potentially be performed including populations we were unable to study, as minorities.

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Figures and Tables

(In the attached pdf)

Appendix A. Additional Figures

(In the attached pdf)

Appendix B. Data Appendix

Our analysis relies on linking data from several sources. We begin with the set of white, US-born people recorded in the 1930 full-count US Census (for reasons that are explained below). We link those individuals to 1) themselves in the 1940 full-count US census; and 2) their death year as recorded on FamilySearch. This appendix will describe our methods for obtaining and linking that data in order to create the datasets we used for our analysis. It will also describe match rate outcomes at several levels, including geographic breakdowns at the state and county levels, and discuss potential issues in our matching processes.

I. Linking individuals from the 1930 Census to the 1940 Census

IPUMS USA provides the high quality pre-cleaned full-count US Census datasets from which we obtain the majority of our useful variables, like a person's birth year and place of residence. Their full-count census datasets identify individuals within that census by a uniquely assigned HISTID. These HISTIDs are not consistent between census years; i.e. a person's HISTID in the 1930 census is not the same as their HISTID in the 1940 census.

FamilySearch, one of the world's largest genealogical organizations, also maintains full-count US Census datasets. In place of HISTIDs, their census datasets identify individuals by a uniquely assigned Archival Resource Key (hereafter ARK). Like HISTIDs, these ARKs are not consistent between census years. This lack of consistency across census years is useful for indexing records to large websites like FamilySearch.org, but it creates a challenge for researchers who want to compare people across multiple censuses.

We link people in our dataset across census years using the Census Tree method (Price et. al., 2021) developed in part at the BYU Record Linking Lab (hereafter RLL). However, the Census Tree links are built on ARKs, not HISTIDs, so we also have to link our HISTID-based IPUMS datasets to their corresponding ARK-based FamilySearch datasets. Examples of a HISTID and an ARK are presented below:

histid1940
00000256-F115-4E18-A124-D78C70F2C985

ark1940
VTWB-WZP

Because we need to incorporate the ARK-based Census Tree links into our HISTID-based datasets, we match individuals in the 1930 census to their 1940 selves over three linking steps:

1. Use a HIST-ARK crosswalk developed by the RLL to link the 1930 IPUMS dataset to the 1930 FamilySearch individual identifiers (HISTID1930 → ARK1930).
2. Use the Census Tree links to link the 1930 FamilySearch identifiers to the 1940 FamilySearch identifiers (ARK1930 → ARK1940).
3. Use another HIST-ARK crosswalk developed by the RLL to link the 1940 FamilySearch identifiers to the 1940 IPUMS dataset (ARK1940 → HISTID1940).

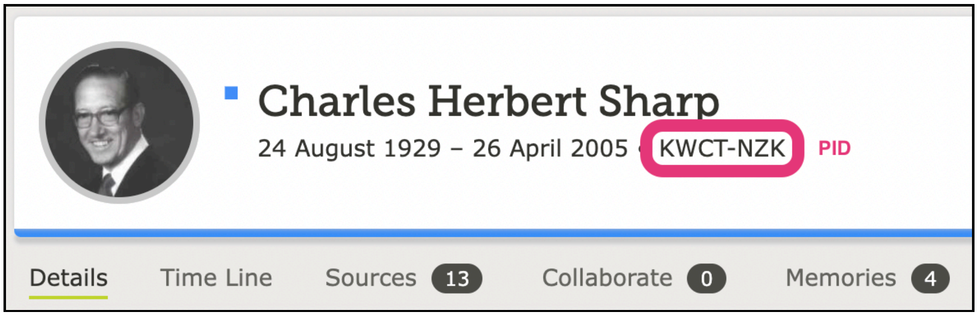
Those three steps result in a linking process that uses RLL crosswalks and the Census Tree links to go from HISTID1930 → ARK1930 → ARK1940 → HISTID1940, thereby linking our 1930 IPUMS individuals to their corresponding entries in the 1940 IPUMS dataset.

This process is not perfect; the methods by which the Census Tree links were created do lead to selection in the kinds of individuals more likely to successfully link from 1930 to 1940. In addition, there are a few counties and/or states in both the 1930 and 1940 HIST-ARK crosswalks that appear to have suffered from structural data inconsistencies during the crosswalk creation process, leading to unusually low crosswalking rates. These match rates and other related issues are described in section III of this appendix. Selection bias and other potential issues with our linking processes are discussed in section IV. Choropleth maps showing general success rates in matching at the state and county levels are also presented in that section.

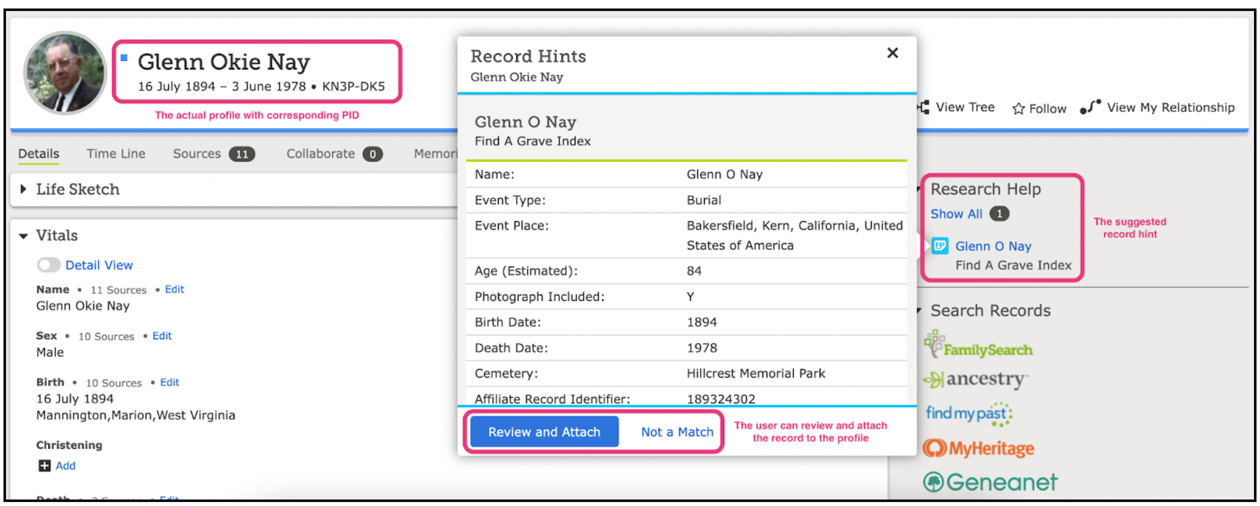
II. Linking individuals in the 1930 Census to their death information

We used the 1930 IPUMS census dataset as our base dataset for all linking. As described above, their datasets index individuals by HISTID. Because census records provide no information about a person's death, we need to link the individuals in that dataset to a different dataset that does provide death information. We use data from the public wiki-style Family Tree from FamilySearch.org as our source for that death information.

As described above, FamilySearch indexes their census records at the individual level by ARK. Those indexed records are made available to the public on FamilySearch.org, where users are encouraged to contribute to a shared Family Tree. Those profiles are created by descendants of the deceased people, and each profile is uniquely assigned a PersonID, or PID. An example profile is presented below, with its PID highlighted:



Users search FamilySearch's indexed records and attach information from matching records to a given profile's PID. FamilySearch's record matching algorithms also frequently suggest potential record matches on a given person's profile, allowing users to find and verify potential record matches with minimal effort. An example of one such record "hint" is presented below:



Importantly, the records that a user might attach to a given profile can include both death records and ARK-indexed census records, giving us an extremely reliable set of links from people's entries in census records to their death information.

We therefore have a path to link people in our 1930 IPUMS dataset to reliable death information. Doing so again involves three distinct linking steps:

1. Use a HIST-ARK crosswalk developed by the RLL to link the 1930 IPUMS dataset to the 1930 FamilySearch individual identifiers (HISTID1930 → ARK1930).
2. Use a list of ARKs that are either already attached to or likely to match with existing PIDs on the Family Tree to link the 1930 FamilySearch identifiers to those people's profiles on the Family Tree (ARK1930 → PID).
3. Use an API caller developed by the RLL to find and link death year information from the public profiles of each of the matched PIDs to that PID (PID → Death Year).

Those three steps result in a linking process that uses RLL crosswalks, API calls, and a list of attached or likely-match ARK-PID sets from FamilySearch to go from HISTID1930 → ARK1930 → PID → Death Year, thereby linking many of the individuals in our 1930 IPUMS dataset to their respective death years.

Again, this process is not perfect; FamilySearch's user base has not historically been representative of the United States as a whole, so the set of people whose death information can be linked is likely to suffer from selection. Specifically, FamilySearch's primary user base is composed of members of The Church of Jesus Christ of Latter-day Saints, who are more likely to be of white European descent than the average person in the United States. Though projects like the African-American Families Project from the RLL are improving the representativeness of the Family Tree as a whole, our dataset still reflects selection in favor of the ancestors of FamilySearch's uses. Overall linking success rates are reported in section III of this appendix. Choropleth maps showing success rates in matching at the state and county levels are presented in section IV, and the potential issues those breakdowns highlight are also discussed in that section.

III. Overall match rates

No individual step in any of our matching processes ever matched 100% of the individuals it was meant to match, but this is not unexpected. The match rates from each step of the HISTID1930 → HISTID1940 matching process and its overall match rate are presented below:

Step of HISTID1930 → HISTID1940 Process	*Matching Success Rate*
HISTID1930 → ARK1930	99.536%

ARK1930 → ARK1940	63.787%
ARK1940 → HISTID1940	95.009%
HISTID1930 → HISTID1940	**60.323%**

Likewise, the match rates from each step of the HISTID1930 → Death Year matching process and its overall match rate are presented below:

Step of HISTID1930 → Death Year Process	*Matching Success Rate*
HISTID1930 → ARK1930	99.536%
ARK1930 → PID	56.338%
PID → Death Year	50.804%
HISTID1930 → Death Year	**28.489%**

Each of the step match rates presented above is dependent on the step that precedes it; a person whose HISTID1930 does not match an ARK1930 cannot match to either an ARK1940 or a PID. This makes the key HISTID1930 → HISTID1940 and HISTID1930 → Death Year match rates equal to the product of the match rates of their steps. Luckily, the match rate for people who matched from HISTID1930 to both HISTID1940 and a death year is not a product of the two end match rates:

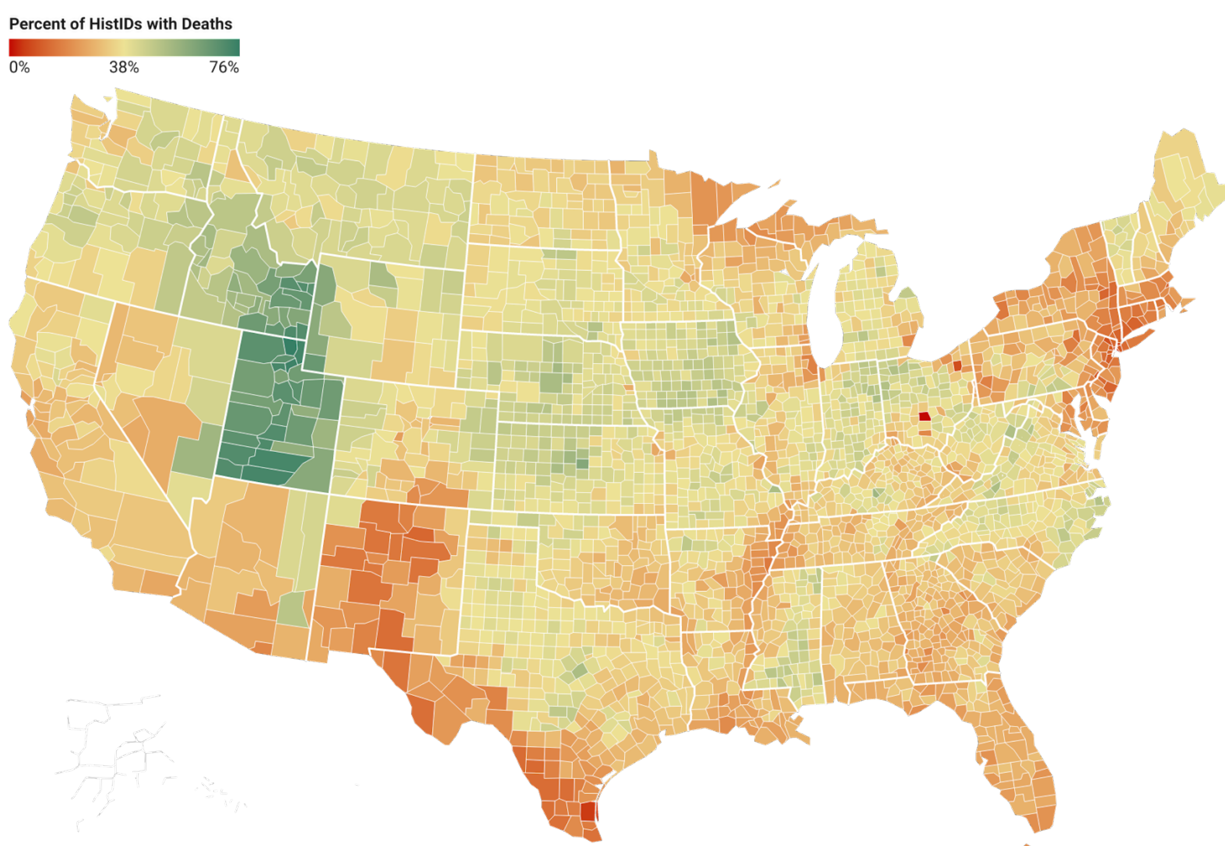
Matching Process	*Matching Success Rate*
HISTID1930 → HISTID1940	60.323%
HISTID1930 → Death Year	28.489%
HISTID1930 → HISTID1940 & Death Year	**22.659%**
Product of rows 1 & 2	**17.185%**

The fact that our HISTID1930 → HISTID1940 & Death Year match rate is higher than the product of the two individual match rates suggests that the probability that a person matches to a HISTID1940 is not independent from the probability that a person matches to a death year.

IV. Match rate breakdowns by county and birth year cohort

In our dataset, match rates of every kind vary by state, county, and birth year cohort. Some of this variation introduces interesting challenges to the interpretation of our results. We present choropleth maps of match rates by county that show possible issues in regional selection. We also show a chart of match rates by birth year cohort beginning in 1880.

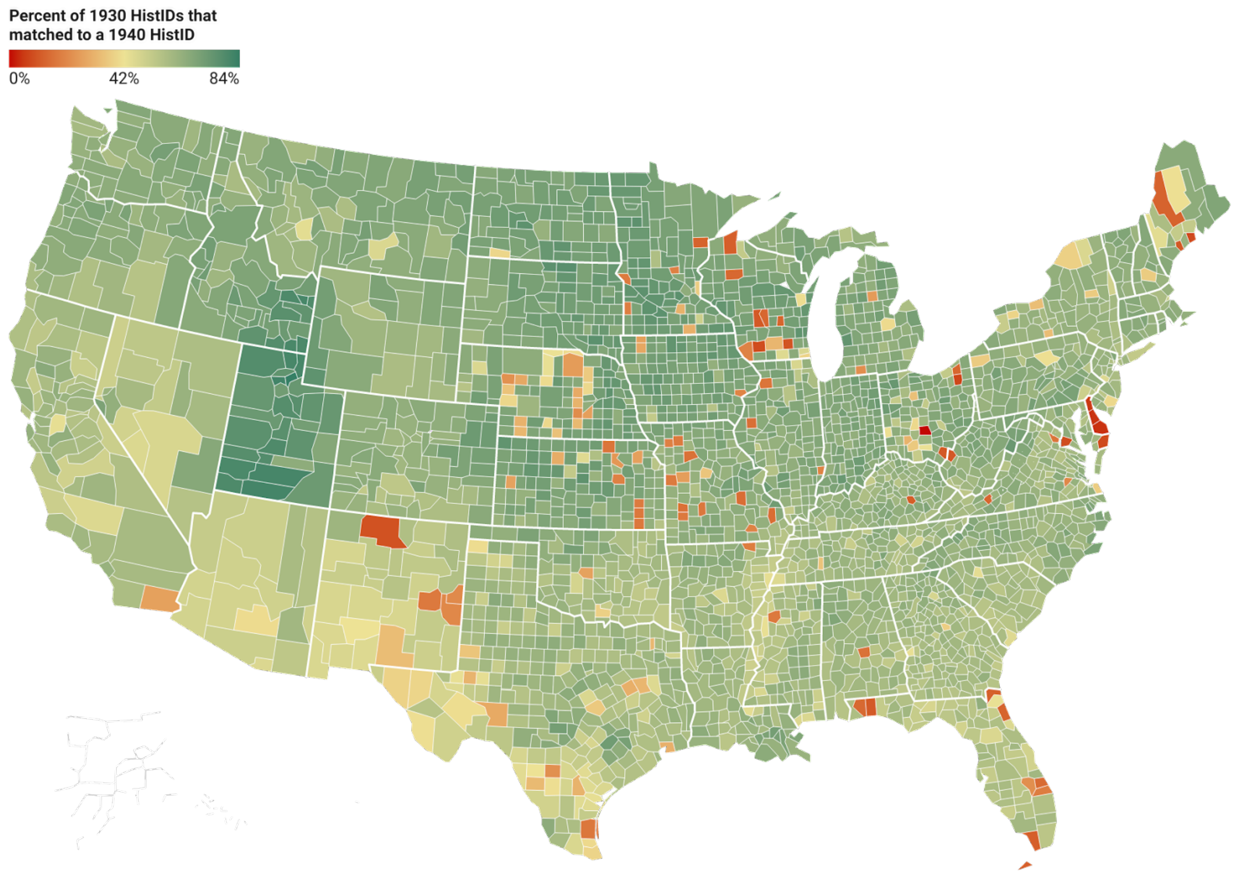
We first examine variation in match rates at the county level. Below are three choropleth maps showing match rates from HISTID1930 to death years, HISTID1930 to HISTID1940, and HISTID1930 to both death years and HISTID1940, respectively, with more detailed interactive versions available upon request. First, the map of HISTID1930 to death year:



Several trends stand out. First, counties in Utah and Idaho drastically outperform counties in other states. Because we can only link a person in the census to their death year if that year is recorded on FamilySearch, this huge green region reflects an overrepresentation of FamilySearch users' ancestors having lived and died in those counties compared to other counties in the country. Next, we have a 0% at the back end of our color key and a very dark red county in central Ohio. That is Pickaway County, OH, where the Record Linking Lab's 1930 crosswalk from HISTID to ARK has almost zero coverage. It is a clear outlier as the only county in our dataset whose 1930 HISTID-ARK match rate is below 40%, and it drastically underperforms the overall 1930 HISTID-ARK match rate of 99.5%.

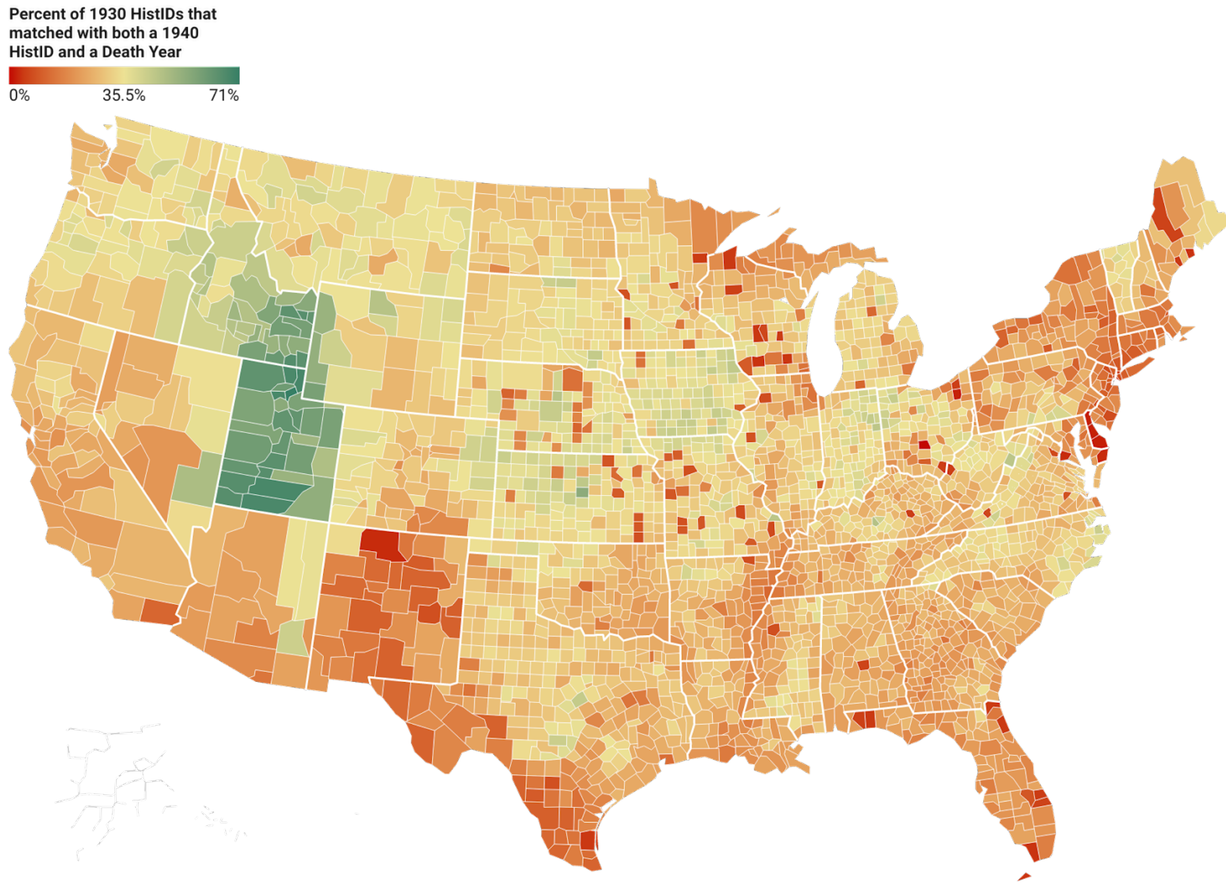
Importantly, the distribution of red counties does not signal any obviously problematic areas outside of the counties in the region around New York City, almost the entire state of New Mexico, and many counties along the U.S.-Mexico border. For the first area of issue, we reason that the very, very large population of the New York City area in 1930 made keeping, organizing, and indexing records difficult, which would make their descendants less likely to have recorded their deaths on FamilySearch. Happily, that large population provides many people to our sample even with relatively low match rates. For the other two areas of issue, we reason that the relatively sparse population of U.S.-born white people in New Mexico and southern Texas makes those areas less likely to have a large number of FamilySearch users tracing their ancestors to those areas. This would drastically lower the chance of a person in those areas having their death recorded on FamilySearch.

We next consider the map of match rates from 1930 HISTIDs to 1940 HISTIDs:



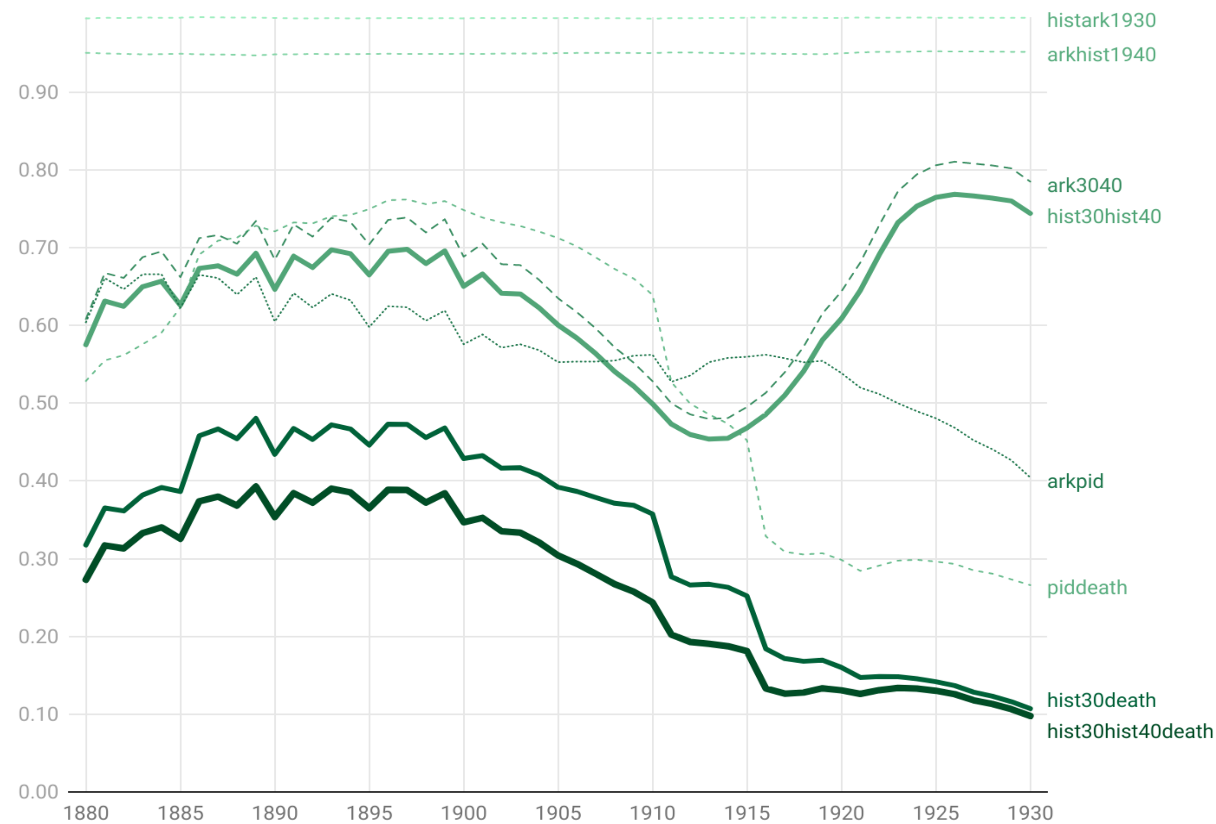
This map presents fewer immediate problems to our sample, though it is not free from areas of concern. The scattered distribution of red and orange counties on this map suggests that their lower match rates are more random than selected. Delaware's three counties are an obvious exception; for some reason, the Record Linking Lab's ARK1930 to ARK1940 crosswalk has a critical gap in coverage in that state. That gap will be repaired in the future, so a future rerun of our analysis with a more robust set of crosswalks would serve as an easy robustness check to our results.

Wrapping up our county-level examination of match rates, we consider the map of match rates for people who matched from their 1930 HISTID to both their 1940 HISTID and their death year:



This map reflects all of the issues discussed in examination of the first two county-level maps. Outside of the critical Delaware gap, the overrepresentation of Utah and Idaho, and the strange case of Pickaway County, OH, this red- and orange-majority map is probably more reflective of the difficulty of matching historical records than any kind of selection in match rates. As matching techniques and data cleaning improve in the future, we understand that our results could become outdated and look forward to revisiting and possibly revising our analysis.

To conclude, we examine a chart of match rates for every step of every matching process separated by birth year cohort:



Intermediate matching steps are denoted in dashed or dotted lines, while the three final match rates are denoted with bolder lines. This chart shows that the HISTID-ARK matching steps are extremely consistent and very robust. It also shows that our match rates to death years dip noticeably in cohorts who are more likely to still be alive. Future repetition of our analysis will likely see a PID to death year match rate for people born between 1915 and 1930 that lines up better with the over 50% match rate we see for people born between 1880 and 1910. We do not anticipate that this would increase because many of those people are still alive today (which would drastically change our results) as there are probably not very many people still alive in 2022 who were born in 1915; rather, that match rate would increase due to future contributions from FamilySearch users.