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Understanding the Determinants of Health and Well-being: Exploring Longevity, Economic Shocks, and Educational Disparities

A dissertation submitted in partial satisfaction of the
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in Economics

by

Ariadna Jou Fuya

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ABSTRACT OF THE DISSERTATION

Understanding the Determinants of Health and Well-being: Exploring Longevity, Economic Shocks, and Educational Disparities

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Professor Adriana Lleras-Muney, Chair

This dissertation comprises three essays at the intersection of public finance, labor economics, health, and economic history. In Chapter 1, co-authored with Tommy Morgan, we explore the impact of the Great Depression and the New Deal on longevity. Our analysis reveals that individuals, particularly young men in severely affected areas, experienced significant reductions in lifespan. Furthermore, we find that New Deal relief increased life expectancy of men by approximately one year. Chapter 2, co-authored with Núria Mas and Carles Vergara-Alert, investigates the causal effects of changes in housing wealth on health outcomes and the drug crisis in the US, revealing positive implications of housing wealth shocks for self-reported health and mental well-being, emphasizing the importance of housing-related policies in addressing the opioid crisis. Chapter 3, co-authored with Tomás Guanziroli, studies the factors behind the flattening of the college premium in Brazil, focusing on changes in the average quality of college graduates and their impact on wages. Our analysis reveals that the increased supply of college-educated workers originates from newer, lower-ranked universities with lower wage premiums. However, when considering a specific set of universities, we observe the college premium is still increasing, signaling a decline in degree quality and its association with lower average wages.

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To my beloved mother Imma and sister Carla, and to my dear father Gaspar, who remains in my heart and memory. Their permanent love and presence have shaped my journey in immeasurable ways.

To my soon-to-be husband Tomás, whose unconditional support has been a constant source of strength.

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Introduction

This dissertation consists of three essays in the interaction of labor economics, health and economic history.

Chapter 1, co-authored with Tommy Morgan, explores the short- and long-run effects of the Great Depression and the New Deal on the well-being of the US population, measured by longevity. We constructed 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 county level. First, we document the dynamic effects of the Great Depression on survival rates and longevity and show that individuals—in particular, young men—living in the most severely affected locations lived substantially shorter lives as a result of the Great Depression. Second, we assess whether the New Deal compensated individuals for the negative effects of the Depression. To identify the causal effects of New Deal programs, we leverage variation across counties in New Deal spending that was politically motivated. More specifically, we use an instrumental variable strategy that allows us to compare the outcomes of individuals in counties that were equally affected by the Great Depression but who received more money as a result of politicians' desire to be reelected. We find that the New Deal increased longevity and more than offset the negative effects of the Depression. 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.

In chapter 2, co-authored with Núria Mas and Carles Vergara-Alert, We use household-level data to study the causal effects of exogenous changes in housing wealth on health and the drug crisis in the US attributed to "deaths of despair". We find that a one standard deviation positive shock in housing wealth increases the probability of an improvement in self-reported health (mental health) by 1.0 (1.10) percentage points, decreases the change in drug-related mortality rate by 4.3%, and has no effect on alcohol- or suicide-related deaths. The opposite effect also holds, such that a negative shock on wealth increases the probability of a decline in health. We also find that the impact of housing wealth on health varies across socioeconomic groups and is more pronounced in MSAs in which housing supply is more inelastic, which explains the differential effect of economic cycles across geographical areas. Our results suggest that housing-related policies could have important implications for general health outcomes as well as for the opioid crisis.

In Chapter 3, co-authored with Tomás Guanziroli, we investigate whether the flattening in the college premium in Brazil is due to changes in the average quality of college graduates. Recent studies show that the college premium has flattened in the last two decades in many Latin American countries. At the same time, there was a great expansion in the number of college graduates and college institutions in the region. This paper shows that the college premium in Brazil has not flattened, instead it is increasing. We do this by matching novel data with the names of around one million college graduates from 42 schools and 20 different cohorts with the Brazilian employer-employee matched dataset. First, the increase in the supply of college workers came from newer, lower ranked, and lower wage-premium universities. Second, the college premium has increased for workers from our constant sample of universities. Combining these two facts, we infer that there are more workers with a college degree but lower quality degrees, reflecting lower average wages. The findings in this paper are relevant for any study that uses college premium proxies in countries, or periods, with increasing access to lower-quality colleges. More precisely, we are concerned that the market equilibrium effects of college access have been overestimated by not accounting for these changes in quality.

Chapter 1

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

1. Introduction

Economists have long been concerned about the effects of recessions on well-being and health. Yet empirical studies disagree whether these effects are positive or negative (Ruhm, 2000; Ruhm, 2005; Arthi et al., 2022). These opposite findings can be partially attributed to the use of different settings and data. For example, recessions appear to be more damaging in poor countries (Doerr and Hofmann, 2022) and over the long run (Schwandt and Von Wachter, 2020). More importantly, previous studies have ignored a crucial aspect: Government responses to recessions might also affect health. Ignoring government responses might lead researchers to underestimate the negative effects of recessions.

We study the short- and long-term effects of the Great Depression and its governmental response—the New Deal—on longevity. The Great Depression was the deepest and longest downturn in modern US history, and since it occurred in the 1930s, only now has enough time passed to analyze its long-term effects.[1] The New Deal featured the first major social welfare programs and the first countercyclical unemployment programs in the US.

We first document whether the Great Depression affected longevity and survival to various ages. Second, we present causal evidence that New Deal relief compensated individuals for the negative effects of the Great Depression. We obtain causal estimates of the impact of New Deal relief on longevity by analyzing whether individuals living in counties that received larger amount of funds lived longer as a result. To identify the causal effects of New Deal relief, we use an instrumental variable approach that leverages an important source of exogeneity in relief funds distribution: political incentives.

We estimate the impact of the Great Depression and New Deal on longevity by creating a novel dataset that 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 the genealogical site 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 offer many advantages. Because we can track individuals from 1930 until the present, we can compare the short- and long-run effects of the Great Depression and the New Deal on survival. The resulting dataset is very large (27 million observations) and includes a large fraction of women, which allows for detailed heterogeneity analysis.

We estimate causal effects of New Deal relief by employing an instrumental variable

[1] It difficult to find exogenous sources of variation to predict the severity of the Depression. Therefore, our analysis of these effects is descriptive.

[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.

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; Fishback et al., 2007). Thus, individuals in these areas would have likely fared worse even in the absence of the relief, which negatively biases estimates of the relief. For the same reason, estimates of the Great Depression that do not account for the New Deal are also biased and likely underestimate the impact of the Great Depression, since the most affected areas received more relief.

We leverage variation in spending that was driven by political considerations to create our instrumental variable. 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 ensure their 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 controlling for the severity of the crisis. The novelty of our IV approach relative to prior studies of the New Deal is our use of an IV-LASSO approach. We collect all variables identified as political predictors of New Deal spending (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 that combines voter turnout for the 1932 presidential election and the 1928 congressional election. This voting culture exploitability variable takes larger values in areas in which relief funds would be most likely to increase votes.

Our findings suggest that although the Great Depression was bad for the health of the population, New Deal relief more than compensated for its negative effects. First, we find that the Great Depression reduced survival rates in the short and long run, but the effects on survival only become substantial after individuals reach age 50 and decline after age 70. Thus, short-term estimates of the effects of the Great Depression substantially underestimate its negative consequences. Moreover, failure to account for the New Deal and its endogeneity also substantially biases estimates of the effects of the crisis. Second, we find that on average, the New Deal extended longevity and positively affected survival rates in both the short and long run. Our IV estimates show that a one-standard-deviation increase in relief per capita ($140) extended longevity by 6 months.[3] In addition, the predicted net effect of the Depression and relief is an average extension in longevity of 1 month.

We find that primarily men were hurt by the Great Depression and that they also were the main beneficiaries of the New Deal. The Great Depression disproportionately affected blue-collar and unskilled workers, particularly those in manufacturing and construction (Margo, 1991; Wallis, 1989; Chandler, 1970). As in other recessions, youth also suffered larger losses in employment.[4] When we re-estimate our model separately by gender and age, we find that a one-standard-deviation increase in relief extended men's longevity by 1 year. Among women, we find statistically and quantitatively significant effects of the New Deal only for those in their teenage years in 1930.

We also find that young adults suffered the largest longevity declines from the Great Depression and obtained the greatest benefits from the New Deal for two main reasons. First, men between the ages of 16 and 21 years 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; this could explain part of the extension in longevity (Schwandt and Von Wachter,

[3] $140 is equivalent to 24% of the average annual income in the 1940 Census. $140 in 1967 is equivalent to approximately $2,000 in 2020. The relief is not in annual terms; it is the total amount of funds from 1933 to 1939.

[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 data collection.

[5] Individuals aged 15 to 19 had unemployment rates of 60% in 1934 in the State of Pennsylvania (Margo, 1991).

2020). In fact, recent research shows that young men participating in the CCC program (a New Deal employment program that targeted young men) increased their lifetime incomes and longevity (Aizer et al., 2020).

Interestingly, the effects of the Great Depression and the New Deal are not larger among those born during the Great Depression or who were children at the time. This evidence is consistent with observations at the time that children's height, and disability rates later in life were unaffected by the Great Depression (Cutler et al., 2007). The Depression might have in fact benefited some families provided the main bread earner remained employed, since the price of housing and food fell. There may also have been some benefits from the move back to rural areas and farms—the first reversal of urbanization in US history—because there was less disease exposure in rural areas (Spengler, 1936; Boyd, 2002).

We identify two main mechanisms behind the beneficial effects of the New Deal on longevity: increases in income and years of education. We linked our sample to 1940 Census schedules and find that a standard-deviation increase in New Deal relief resulted in a 3% increase in income for those who were teenagers in 1930. We also find increases in years of education for teenagers and young adults, but don't find effects on employment on labor force participation, consistent with Modrek et al. (2022).

This paper mainly contributes to three strands of the literature. First, it studies the relationship between recessions and health outcomes, specifically mortality and longevity. In this area, studies on 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; Dehejia and Lleras-Muney, 2004; Ruhm, 2005; Miller and Urdinola, 2010; Stevens et al., 2015; Strumpf et al., 2017; Tapia Granados and Ionides, 2017).[6] However, in

[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 Svensson, 2010). Also, 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).

the medium to longer run this procyclicality 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 et al., 2020). On the other hand, studies in developing countries tend to find that 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 Great Depression on health and mortality. Using aggregate data, the literature finds that the Great Depression 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 Roux, 2009; Stuckler et al., 2012). Our findings differ from this literature. One reason is that we use individual data, which allow us to track individuals even if they move. Arthi et al. (2022) demonstrate that in settings in which 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 individuals who are not in our study (immigrants and non-whites) benefited from the Great Depression.

Our study expands on the literature of the effects of recessions on health outcomes 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. We also improve on previous studies by accounting for the effects of anti-recessionary programs, which could be a reason why we find more negative effects of the recession than previous studies that only considered the effects of the Great Depression.

We also contribute to the literature on the effects of the New Deal. Many studies examine the effects of the New Deal on various outcomes (Wallis and Benjamin, 1981; Balkan, 1998; Fleck, 1999; Cole and Ohanian (2004); Fishback et al. (2005); Fishback et al., 2007;

Neumann et al., 2010; Stoian and Fishback, 2010; Taylor and Neumann, 2013; Fishback and Kachanovskaya, 2015; Arthi, 2018; Liu and Fishback, 2019). However, few explore the effects of the programs on health (Fishback et al., 2007; Modrek et al., 2022). Fishback et al. (2007) find that the New Deal decreased infant mortality, and Aizer et al. (2020) show that the CCC extended the longevity of young men in Colorado and New Mexico. Modrek et al. (2022) did not find any effects; however, they follow individuals until 2011, many of whom could still be alive. We extend the analysis to all of the mainland US and cohorts alive in 1930, use an IV approach to address potential biases, and follow individuals until 2020, which is critical for the longevity analysis.

Finally, our research also relates to the literature on the effects of social programs and programs to compensate for negative shocks on health outcomes (Aizer et al., 2016; Barham and Rowberry, 2013; Hoynes et al., 2016; Guarín et al., 2022). Our findings are consistent with most of this literature. For example, Aizer et al. (2016) find extensions in longevity when studying the long-term effects of the US mothers' pensions program in the 1920s. Guarín et al. (2022) find positive effects on health outcomes when investigating economic compensation for victims of the Colombian armed conflict.

The chapter is organized as follows. Section 2 provides background on New Deal relief and allocation of the funds. Section 3 describes the 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 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 New Deal, which was 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 with the stock market crash in October 1929 and lasted until 1941. This period was characterized by 4 years of large economic declines (1929-1933) and 8 years of slow recovery. In the United States, real GDP dropped by around 30%, prices went down by 27%, unemployment rose to 25%, about one-third of workers were employed only part-time, and one-third of all banks failed (Chandler, 1970; Romer, 2003; Richardson, 2007; US Bureau of Labor Statistics).

The negative effects on the economy had 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, occurred during the same period. Damage to the American ecology led to an agricultural depression, intensifying the impact on hunger and malnutrition (Phillips, 1999). However, the Great Depression did not affect everybody equally. Young people, the elderly, and non-white individuals faced the largest levels of unemployment. Some sectors, such as 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 Great Depression also varied across the country. Figure 1.1 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: New Mexico, Mississippi, Arkansas, and Oklahoma. In the West, some of the most affected states were Arizona, Utah, and Washington. The east coast and Northeast were less affected. The difference in industrial composition across regions is one reason for the geographic variation in the severity of the crisis, since 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 Great Depression 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 which were precursors of modern welfare programs. Yet most New Deal programs offered relief through employment.

We focus on relief programs, which accounted for 63% of New Deal non-repayable grants, and public works grants, which accounted for 24% (Fishback et al., 2003). These programs operated through direct work contracts and public assistance. They targeted the most affected individuals and provided assistance to satisfy basic needs such as food, housing, and health care. 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 Civil Works Administration grants (CWA), which created jobs for millions of people who were unemployed (Schwartz, 1976; Fishback et al., 2003). The Public Works Grants program focused

[7] New Deal grants between 1933 and 1939 totalled $16 billion (in 1967$).

[8] For example, the correlation between CWA and WPA is 0.94.

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

The geographic allocation of funds was not random, and resulted in geographic variation at both county and state level.[10] Figure 1.2 shows the spatial distribution of New Deal funds in absolute and per capita terms. By comparing it with Figure 1.1—which shows the spatial distribution of the severity of the crisis—we find that the government targeted areas with more pronounced economic downturns. Indeed, Figure 1.3 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 funds. For example, southern states received less money (Fishback et al., 2007) because politicians argued that living costs there were lower (Couch and Shughart, 1998). States in the West received more funds because they had more federal land, where more public works and infrastructure could be undertaken (Wallis, 1998; Fleck, 2001). Bureaucratic 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.

[9]Programs not included are the 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.

[10]The federal government distributed funds across states, and states distributed funds across counties and municipalities.

[11]For some programs, the state's governor had to sign a statement justifying the need for relief and provide diverse information. Other programs had funding requirements the state had to match, and this could result in richer states' receiving more funds.

Wright (1974) finds that voter 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 New Deal spending, as predicted by a model of political choice. The underlying mechanism in 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 voter turnout in different elections, the fraction of votes for Democrats, and the variance in Democrats' votes over time, are strongly correlated with New Deal spending per capita. In summary, it is well established by previous research that political variables predict the allocation of New Deal relief.[12] We consider all these variables as potential instruments for New Deal funds.

3. Data

To study the long-term effects of the Great Depression and New Deal relief on longevity, we match individual-level data from the 1930 and 1940 US Censuses 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

US Census

We use the 1930 and 1940 Censuses in this analysis. The baseline sample is the 1930 Census, which provides the county of residence of all individuals living in the US at the very beginning of the Great Depression and 3 years prior to the New Deal. We use the full count, which includes 120 million individuals. The 1930 Census also includes various predetermined

[12] We discuss the instrument and necessary assumptions in Section 4.

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, such as 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 New Deal relief on longevity.

FamilySearch—The Family Tree

To compute individual longevity, we match the 1930 census with family tree data from FamilySearch. FamilySearch hosts both the world's largest single family tree platform and an archive of historical records that contains information on billions of deceased individuals. Instead of creating 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 them 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 A.1 shows an example view of the Family Tree from the point of view of a regular user.[15] Whereas anyone can access individual records on Family Search's website, the large-scale compilation of the dataset used in this paper is maintained by 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 to their death data, which is comparable to or higher than that achieved in other historical studies.[16] Appendix

[13] As a robustness check, we also link the two Census years using the MLP linking method (Helgertz et al., 2022).

[14] A machine algorithm uses the user-made links to suggest potential record links to other 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

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% women. Because women tend to change their last name after marriage, they are more difficult to link across years and not usually included in similar historical studies using Census or Social Security data. As a result, the study of women has been notably scant 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 Tree includes information on parents' names, we observe women's maiden and married last names so that we can link them at nearly the same rate as men.

Second, the FamilySearch death data includes deaths from 1930 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), which only includes information on birth and death dates for men who died between 1975 and 2005. As a robustness check, we also use the DMF records to compute longevity. Some additional problems appear when matching these records, since these data have only been linked to the 1940 Census (And not to the 1930 Census, which is our base data).[17]

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

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.

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

3.2 *County-level data*

New Deal Relief Data

We use data on New Deal spending by program at 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 include information on 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 are not broken down by year.

Using data at county level is important for two main reasons. First, 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 are at county level.[19]

More than $16 billion were distributed from March 1933 to June 1939 in different non-repayable New Deal funds. Of 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), $261.54 per capita in 1967$, with a standard deviation of 287.48. In 2020$, this

[18] These reports were digitized by Fishback et al. (2005). 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, since most of New Deal relief programs ended in 1939. Only 1% of the population reports working on relief in the 1940 Census. Modrek et al. (2022) use this data to create a county-level index of New Deal exposure. Individual participation in these programs is available in the National Archives, but the records 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.

would be an average of $4,869.76 per capita.[20] Average relief from 1933 to 1939 represented 31.6% of average annual income in 1939.[21] Mohave Counyt, Arizona, was the county with the highest per capita funds—more than $9,000 per capita—and Armstrong County, South Dakota, with the lowest, receiving less than $0.28 per capita.[22]

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 obtain a single estimate of the effects of the Depression on mortality and longevity and to compare counties that differed on 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: 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, which might cause some measurement error.

US Election results 1920-1932

We use information on election results from 1920 to 1932 to understand how political incentives affected the distribution of New Deal funds. The political variables come from data available in the "United States Historical Election Results, 1824–1968" (ICPSR 1), which

[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 ($1,062.41 in 1967$). This data come from the 1940 full-count US Census, and it is top coded at $5,001. If we divide the amount of relief by 7 years, it represents 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.

reports how many votes each party got for different elections. The variables used include voter turnout in presidential and congressional elections, averages and standard deviations the turnout from 1920 to 1932, fractions of votes for Democrats and Republicans, averages and standard deviations of the fractions of votes for Democrats and Republicans, numbers and fractions of loyal and swing voters, numbers of representatives and their tenures, and closeness of the elections.[23] In Section 4, we explain how we use these political variables in our identification strategy.

3.3 Estimation Sample and Summary Statistics

Table 1.1 shows summary statistics of individuals in the full-count 1930 US Census (column 1) and 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 underrepresented in our data, we restrict our analytic sample to white, US-born individuals.[24] Columns 3 and 4 of Table 1.1 present the same summary statistics as columns 1 and 2, but for our analytic sample. Statistics in column 4 are weighted at cohort and county level to be representative of the white, US-born 1930 population.

There are 89,677,282 white, native-born individuals the 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 to or higher then that achieved in other historical studies.[25]

Table 1.1 shows that once we restrict our sample and weight it, our analytic sample is

[23]Voter turnout and votes for Democrats and Republicans are included for all election years from 1920 to 1932.

[24]Other studies that use FamilySearch data also face this issue and take the same approach (Lleras-Muney et al., 2022.

[25]The Life-M Project links by hand between 35.8% and 37.8% of men and 21.5% and 24.4% of women from birth certificate to death for a subsample of individuals in the States of Ohio and North Carolina. For the full sample, they link individuals to death 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 Censuses. Abramitzky et al. (2012) link 29% of men from the 1865 Norwegian Census to either the 1900 Norwegian or US Census. Craig et al. (2019) match 30% of married women of specific cohorts from marriage certificates in Massachusetts to the 1850, 1880, and 1900 US Censuses.

broadly representative of the 1930 population we target. Average New Deal relief per capita across the entire sample is $270, slightly more than the county-level average reported in Section 3.2.1. The average age of individuals in our sample in 1930 is 27. Although women are slightly underrepresented (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 their tree.

3.4 Matching and Sample Selection

Not all counties are equally represented in our sample. Match rates at county level are presented in Appendix Figure A.2, and 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 and southern of Texas. To address this problem, we weight our dataset at cohort and county level, and—as previously discussed and shown in Table 1.1—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 the 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 with the full-count census. This is shown in Appendix Figure **??** and could be due in part because individuals who are still alive do not have their death on the tree. Finally, FamilySearch's users tend to only enter 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 with Vital Statistics deaths for the 1929 cohort, our sample misses a large amount of infant and young deaths (Appendix Figure A.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 identify who has missing longevity information and whether individuals who lack this information differ from the general population. Table 1.2 presents estimates of the effects of different individual characteristics on an indicator for 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 and higher socioeconomic status, and live in areas in which the recession was less severe and that received less relief. Thus our analytic sample is a positively selected sample of individuals who would be expected to live longer than average. As stated above, to solve some of these issues we weight the population at county-cohort level and control for factors that affect the probability of being linked when conducting our analysis.

4. Empirical Strategy

To obtain the causal effects of New Deal relief and the Great Depression on longevity, we would like to estimate the following accelerated failure time (AFT) model of duration:[26]

$$Log(\text{Age at Death})_{ict} = \beta_0+\beta_1 Log(\text{Relief Spending})_c+\delta\text{Crisis Severity}_c+\alpha_1 X_i+\alpha_2 X_c+\gamma_t+\gamma_s+u_{ict} \tag{1.1}$$

where ict stands for an individual i living in county c and born in the year t. X_i are

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

individual covariates from the 1930 census: age, urban, married, schooled, employed, in the labor force, occupation score, family size, and number of children. X_c are county controls selected using LASSO: severity index, % black, % rural farm, farms per capita, % farm area, % farms 50-99, % farms 500-999. γ_t are cohort fixed effects and γ_s are birth state fixed effects.[27]

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

$$\mathbb{1}(\text{Survived to } m)_{ict} = \beta_0 + \beta_1 \text{Relief Spending}_c + \delta \text{Crisis Severity}_c + \alpha_1 X_i + \alpha_2 X_c + \gamma_t + \gamma_s + u_{ict} \tag{1.2}$$

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.[28] Thus $\mathbb{1}(\text{Survived to } m)$ =1 if the person died after the year m, and $\mathbb{1}(\text{Survived to } m)$ =0 if the person died the year m or before. ict denotes individual i living in county c and born in the year t. Covariates are the same as in equation 1. In both specifications, standard errors are clustered at county level.

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

[27] In Appendix Table A.11, we present results for the analysis of longevity using levels instead of logs.
[28] We grouped the youngest cohorts in intervals of 5 instead of 10 years, because under-5 mortality tends to differ from mortality at older ages.

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 of one standard deviation in the index on outcomes. The coefficient β_1 estimates the effect of \$1 more in New Deal relief on outcomes. For a causal interpretation of β_1 and δ, we further require that New Deal relief spending and crisis severity be orthogonal to other determinants of longevity that are not controlled for in the model. We do not have access to an instrument for severity, and thus the analysis of these effects will be descriptive.

However, we attempt to obtain causal estimates of the effects of the New Deal. Naive OLS estimates of the effects of New Deal relief on longevity from equation (1) might be biased for several reasons. First, there might be omitted variables related to crisis severity. Although we control for the severity of the Great Depression, this severity might be poorly measured. For example, there might be relevant variables that we can't observe, such as a change in personal income or individual wages, which we cannot include in our computation of the severity index. Second, different sources of measurement error can be related to both New Deal relief spending and crisis severity, leading to attenuation bias. Available data on New Deal spending provides information on funds from the federal government to counties but, for example, there could be missing transfers if there are independently funded programs at municipal or individual level. Finally, there could also be error from assuming that people suffering the recession and received relief in their county of residence in 1930. We will separate movers from non-movers in our robustness checks.[29]

4.1 Identification Strategy using IV-LASSO

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

[29]See Appendix Table **??**.

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 New Deal relief funds. Political models in the literature agree that the main variables that affected relief were voter turnout, support for Democrats, how tight 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 identify which political variables affected New Deal relief the most and how. Many of these variables could matter, and their interactions could also matter. Twenty-Five potential instruments have previously been used in the literature. If we allow for interactions and second-order terms, the set of potential instruments could include more than $1,000$ variables.

We use a sparse model that identifies and uses optimal and parsimonious controls to select our instruments from this set of potential instruments. We use a least absolute shrinkage and selection operator (LASSO) for instrumental variables to select the best predictors of 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 endogenous variables given the instruments can be well approximated by a parsimonious yet unknown set of variables and imposes a restriction where by only some of the variables have nonzero coefficients.[30]

[30]The potential set of county controls includes total population, population for different age intervals, population density, % black, % foreign born, % schooled in different age intervals, % urban and rural population, % people in urban and rural farms, % people not in farms in rural areas, illiteracy rates, manufacturing establishments per capita (pc.), % wage earners in manufacturing, average manufacturing wages, manufacturing product value, manufacturing added value, manufacturing added value pc., % gainful workers, % out of work, % layoff, whole establishments pc., whole average wages, % stocks, retail stores pc., % 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.

Thus, we select the instruments and controls by estimating

$$\hat{\beta} = argmin \sum_{i=1}^{n} (y_i - \sum_{j=1}^{n} x_{i,j})^2 + \lambda \sum_{j=1}^{p} |b_i| \gamma_j, \tag{1.3}$$

where λ is the "penalty level" and γ_j are the "penalty loadings." Penalty loadings are estimated from the data to ensure the equivalence of coefficient estimates to a rescaling of $x_i j$ and to address heteroskedasticity, clustering, and nonnormality in model errors. Similarly, standard errors are clustered at county level to address within-county correlation.

The algorithm for the IV-LASSO methodology does the following: First, it estimates a LASSO regression with New Deal relief as a dependent variable which includes all potential instruments (Z) and potential controls (X). From this first regression, we obtain a group of instruments and controls. Second, it estimates a LASSO regression with the outcome variable, longevity, and all 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 in which New Deal relief spending is the dependent variable and all controls (X) are the regressors. Finally, we estimate a 2SLS regression using the selected instruments in step 1 and the selected controls in steps 2 and 3, to get the post-LASSO IV estimator.[31] When using the LASSO algorithm, we partial out cohort fixed effects and state of birth fixed effects—in other words, we always include these controls.[32] The post-LASSO estimator refits the regression via 2SLS to alleviate LASSO's shrinkage bias.[33]

After this process, the LASSO algorithm selects one instrument which 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

[31]All county controls defined at the beginning of this section and crisis severity are selected using our IV-LASSO approach.

[32]We partial out fixed effects because they are important in our model from a theoretical point of view. We want to compare individuals born in the same year and same state, since both will affect the age at death.

[33]We use the *ivlasso* package to compute these estimators (Ahrens et al., 2020).

1932 presidential election voter turnout with the standard deviation of the 1928 Congressional election voter turnout.[34] By the nature of the standard deviation of voter turnout, our instrument will take values from 0 to 0.0625 (since each standard deviation takes values from 0 to 0.25). The instrument takes larger values when the county has a medium level of voter turnout and low values in areas with very low and very high turnout.

This instrument reflects voting culture exploitability in different areas—that is, how easy it is to obtain additional votes in a given location based on voting behavior. Places with very low turnout do not have voting culture, so obtaining an 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 turnout have a robust voting culture, and 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 who could potentially vote, obtaining more votes there is likely cheaper. Thus, it is efficient to allocate funds in places with medium-level turnout.

The key identification assumptions are that the IV is relevant and that the exclusion restriction holds. We will now discuss each assumption. Voting culture exploitability is strongly correlated with New Deal relief spending per capita, as shown in the binned scatter plot in Figure 1.4). Appendix Table A.2 shows that the instrument is strongly predictive of New Deal relief at both county level and individual level. The F-statistic has values of 108, 41.99, and 29.79 in the different specifications—well above the recommended cutoffs.[35] Figure 1.5) documents that there is substantial cross-county variation in the instrument. The South had the lowest values, since voter turnout was very low in the region. Interestingly, this area also received the lowest relief.

[34] Standard deviation is defined as turnout*(1-turnout).

[35] The highest F-statistic corresponds to the county-level specification without controls. Others correspond to individual-level specifications without and with controls, respectively. Appendix Figure A.7) shows the distribution of the voters' importance instrument. The instrument is concentrated between the values 0.04 and 0.06, with some counties having values between 0 and 0.2. Counties with lower values have very low or very high voter turnout.

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

5. Short- and Long-term 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 the effects on longevity. Table 1.3, column 4 reports 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 New Deal relief. Individuals who lived in places with a more severe depression had shorter lives. When we do not account for relief generosity, a 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 New Deal relief, the estimate is 60% larger in magnitude and the average decrease in longevity is 1.6 months.[36] Because more funds went to places with a deeper recession, when we do not account for 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 New Deal was not randomly allocated, these OLS estimates are still biased.

In Table 1.4 we present post-IV-LASSO estimates, in which we use an instrumental

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

variable for New Deal relief. The coefficient for the severity index almost doubles compared with the OLS coefficient. A one-standard-deviation increase in the severity index decreases longevity by 2.8 months on average. We are interested in how these effects differ by gender, as most relief recipients were men.[37] 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.[38]

The effects of the Depression could differ by age, since some groups could be more sensitive to the economic shock than others. Appendix Table A.4 shows the effects of the Great Depression on longevity by birth cohort, in which a cohort is defined as a 10-birth year group.[39] We find that individuals aged 10-20 have the largest effects and experience decreases in lifespan of 2.6 months for a one standard-deviation-increase in severity of 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 cohort. Since survival rates depend on the age of the individual, we study the effects on survival separately by cohort.[40] Figure 1.6) presents OLS and IV estimates for cohorts aged 16 to 25 in 1930, which are the most affected. 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 age 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: 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 crisis severity

[37] See Appendix Table A.1.

[38] Severity coefficients for men and women are statistically different in the IV specification.

[39] Estimates are conditional on surviving to age 20. They are not statistically different from the unconditioned estimates.

[40] To further account for trends in longevity, these regressions also control for cohort fixed effects.

decreased survival by 2.7%. Appendix Figure A.9) reports IV estimates on survival for the other groups of cohorts.[41]

We find a similar pattern for all cohorts: 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 time to accumulate and cause individuals to die. Schwandt and Von Wachter (2020) 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 (2022), who simulate the impact of temporary shocks to 20-year-olds on cohort mortality profiles.

If we disaggregate the effects by gender, we observe in Appendix Figures A.10) and A.11) that the magnitude of the effects for men is larger than for women in all cases. The largest effects for men are for the 1915-1924 birth cohorts in 1994, in which a one-standard-deviation increase in the severity of the crisis is related to a decrease in the survival rates of 7%. For women, this occurs in 2001 for the 1915-1924 birth cohort, in which an increase of one-standard-deviation in the severity of the Great Depression is associated with a decrease in survival rates of 6%.[42]

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, and teenagers, children, and men have larger effects. A possible reason young men have the largest effects is that they had the largest unemployment rates during the recession, so they were one of the groups that suffered the most in the 1930s. Also, they were finishing school and entering the labor market during a recession that has had long-term negative consequences for income and longevity (Schwandt and Von Wachter, 2019; Schwandt and Von Wachter, 2020).

[41] For the groups 0-5 and 6-15, we condition the sample on survival to 20 years to address the fact that young deaths are underrepresented in our sample.

[42] We repeat our estimation using mortality rates instead of survival rates, and the results are very similar. However, the effects on mortality are less precise.

6. Short and Long-term Effects of New Deal Relief

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

Table 1.3 shows OLS estimates of New Deal relief on longevity. Columns 1-3 present estimates without accounting for crisis severity, while columns 5-7 account for it. In the first column, we can see that New Deal relief is associated with negative effects on longevity. When we add county controls in column 2—to account for the fact that area that received relief were doing worse—the magnitude of the coefficient decreases by almost half. This is expected, since New Deal relief and the Great Depression are highly correlated. In column 5, when we add crisis severity, the relief coefficient decreases in magnitude by half compared with column 1. This indicates that OLS coefficients have some negative bias: When we don't account for severity, the effects are more negative.[43] To address potential bias in the OLS estimates, we now report 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 with individuals in counties with the same severity of the Great Depression but that received less money for political reasons. Table **??** presents post-IV-LASSO estimates of longevity from equation (5). Odd columns show first-stage estimates. As noted earlier, coefficients on the severity index are positive and statistically significant, which indicates that more New Deal funds went to areas 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.[44]

The coefficient on relief is now positive and statistically significant. Unlike the OLS estimates, these estimates imply that New Deal relief extended longevity. In column 2—

[43] By gender, OLS coefficients are not statistically different (Appendix Table A.3).

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

the specification without controls—the coefficient for New Deal relief is positive; compared with the same OLS estimate, its magnitude more than doubled. A one-standard-deviation increase in New Deal relief ($140) increased longevity by 9 months on average. When we include all controls in column 6, the coefficient is still positive and significant but slightly decreases in magnitude, which indicates an average extension in longevity of 6 months.[45]

Next, we investigate whether 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 Great Depression severity and compute the net effect. Figure 1.7) panel a presents 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 relief. Figure 1.7) panel b presents the density for computed net effects. On average, the New Deal more than offset the negative effects of the recession. On net, there is an average 1-month extension in longevity.

Heterogeneity

Understanding how the effects of New Deal relief on longevity differ across the population is crucial for policy evaluation and future policy design. Individuals who received relief during their working age can be affected differently than children: For instance, women and men worked in different industries and occupations and were affected differently by the Great Depression. They also received relief at different rates.To understand who was most likely to receive New Deal relief, we use the full-count 1940 Census, which included a question that asked whether the individual was working on or assigned to a public emergency project or local work relief. The main limitation of this source of information is that there were many fewer people on relief by 1939 than in previous years.

We find that only 2% of individuals were working on relief, and only 8% of households had

[45] $140 of New Deal relief is equivalent to approximately $2,000 in 2020$ for the full period 1933-1939. We could think about this as $285.7 a year for 7 years in 2020$.

a member receiving relief. Appendix Table A.1 presents the results from a regression of the indicator for receiving relief in the 1940 Census for both single individuals and households on individual characteristics. People in households who received relief are less likely to be white, married, have children, own a house, or 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 A.5) shows the age distributions of individuals who worked on relief in 1940, compared with those 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 children. In fact, most individuals working on relief were young adults, possibly just entering the labor market. Moreover, individuals receiving relief were poorer as, Appendix Figure A.6) shows, and had lower family wages. However, these differences do not seem to be statistically significant (Appendix Table A.1).

When we analyze the causal effects of New Deal relief on longevity by gender in Appendix Table A.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 New Deal relief ($140) extended longevity by 11.4 months.[46]

We next analyze the causal effects of the New Deal on longevity by cohort. Figure 1.8) presents post-IV-LASSO estimates for New Deal relief on longevity by cohort for the whole sample. The results are only significant for young individuals born between 1905 and 1915, who were teenagers in 1930. For them, an increase of one standard deviation in 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 the CCC. For men, in Appendix Figure A.13) we find positive

[46] In Appendix Table A.11, we present these estimates using specifications in levels instead of logarithms. The results are very similar: An increase of one standard deviation in New Deal relief per capita extended, on average, longevity by 5.7 months when we account for all of the white native population, and by 11.3 months for men. For women, the effects are not statistically significant. In Appendix Table A.8, we present the same results as in Table 1.4 but condition the sample to individuals surviving to 20 years to account for the fact that our dataset does not accurately report young deaths. The coefficients are not statistically different.

effects of the New Deal for all cohorts, but significant effect for most cohorts born before 1915. Point estimates are largest for those born between 1913 and 1915, for whom an increase of one standard deviation in New Deal relief ($140) extended their longevity by 23.5 months (1.95 years). For women, the magnitude of the effect is lower (Appendix Figure A.14)) and only statistically significant for women born between 1910 and 1912, who show increases in longevity of 12.6 months. For other cohorts, the effects appear to be zero.[47]

To study the dynamic effects of New Deal relief, we investigate the effects on survival. Figure 1.9) and Appendix Figures A.15) to A.18) show the dynamic effects for different groups of cohorts estimated by both OLS and IV-LASSO. We can see in the figures that OLS estimates for all cohorts are practically zero. However, when we look at IV estimates, New Deal relief has positive effects on survival rates for all cohorts, with larger magnitudes in the long run. The cohorts that benefited the most are individuals aged 16 to 25 and 6 to 15 in 1930. The effects are largest in 1988 and 1984, respectively, when the cohorts are around ages 60 to 80, which is again consistent with the model of cohort mortality of Lleras-Muney and Moreau (2022). For that period, a standard deviation increase in the New Deal relief results in an increase in survival rates of 18.3% and 11%, respectively. For the rest of the cohorts, the effects on survival are smaller.

Appendix Figures A.19) and A.20) confirm that men are much more affected than women. The figures present IV coefficients on survival by gender.[48] For men, we see the largest effect for cohorts aged 16 to 25 in 1930. Women also get the largest effects in the cohort 16 to 25, although the coefficients are smaller than for men.[49]

Summarizing, we find that men and teenagers were the most affected by New Deal relief. Two main hypothesis could explain the fact that men and teenagers are the main beneficiaries

[47]Since young cohorts are not that well represented in our data, especially if they died young, in Appendix Figure **??**) we show the same graph conditional on surviving to age 20. Coefficients do not differ from those in Appendix Figure A.14).

[48]OLS coefficients on survival by gender are available upon request.

[49]For men the largest coefficient is for survival to 1984, where a standard deviation increase in New Deal relief increases survival rates by 27.6%. For women it is to 1987, which increases survival rates by 10.2%.

of New Deal relief. First, they suffered more from negative effects of the crisis than women and other cohorts, so they obtained positive effects from being compensated by relief. Second, they were the main recipients of New Deal relief; a large share of men and teenagers received relief, compared with a lower share of women, as we have seen in Section 3.[50] These results are consistent with other studies that find that 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 finishing school and entering the labor market, and the benefits of getting a relief job in this situation might be larger.[51]

We also investigate whether there are other sources of heterogeneity. First, we want to know whether the relief compensated more for the poor. We divide the sample by occupation score in 1930, and our results in Appendix Table A.5 show that the lowest quartile was more affected by the recession and more compensated by New Deal relief than the upper quartile.[52] Since we do not find any effects for women, in Appendix Table A.6 we examine whether married women benefit from New Deal relief through their spouses. We do not find different effects. Finally, in Appendix Table A.7 we compare IV estimates for men who changed (or not) counties between 1930 to 1940.[53] Since we use their county of residence in 1930 when assigning New Deal relief and Great Depression values, if individuals move this could be a source of measurement error. Men who did not move were more affected by the recession, and slightly more affected by the New Deal. Considering that individuals in places where the recession was more severe moved more, this could explain why movers have smaller point estimates.[54]

[50]See Appendix Figure A.5).

[51]See Appendix Figure A.12).

[52]The occupation score in 1930 is an approximation for income, since the 1930 US Census did not include income information. We can't reject the null hypothesis that crisis severity coefficients are equal. Coefficients for New Deal relief are statistically different.

[53]About 20% of our linked sample moved from one county to another between 1930 and 1940.

[54]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.

7. Mechanisms

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

The Great Depression affected labor market outcomes, years of education, family composition, and cross-county mobility from 1930 to 1940. Table 1.5 presents IV results of the Great Depression and New Deal relief on different 1940 outcomes for our sample linked to FamilySearch deaths. The Great Depression negatively affected 1940 labor market outcomes. In places in which the crisis was more severe, people were less likely to be employed or in the labor force and they had lower incomes, although the effects are modest. The Great Depression also affected family composition: The Great Depression decreased the probability of being divorced and increased the probability of being widowed, which is expected if it increased mortality. Finally, more people in places in which the recession was more severe left their homes and moved to other counties.

New Deal relief also affected 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 New Deal also has positive point estimates for years of education, but they are not significant. It also affected family structure by 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, in Appendix Table A.8 we study the effects on 1940 outcomes for men. The Great Depression negatively affected 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 average did not affect 1940 outcomes for men. Since teenagers had the largest effects on longevity, in Appendix Figures A.21) and **??**)

we examine whether the effects on 1940 outcomes differ by cohort. Teenagers in 1930 are the only group to present positive and significant effects of the New Deal 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 relief, teenagers married more and stayed in school longer.[55]

8. Robustness Checks

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

The New Deal spending data available to us are at county level, so in our robustness checks we present estimates on longevity at county level.[56] OLS estimates for the New Deal and the Great Depression on average longevity in Appendix Table A.9 are smaller compared with individual-level estimates (Table 1.3). Now, however, they are positive for both the New Deal and the Great Depression in the joint and men specifications. It could be that when grouping information at 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 A.10 are more similar to individual-level estimates. The New Deal has positive effects on the full sample—men and women—and the coefficients for the Great Depression are negative in all specifications. However, we find larger effects in county level specifications. The magnitude of the coefficients almost doubles, and the effects become significant also for women.[57]

[55] Joint effects for women over 32 years on years of education are not statistically significant.

[56] In county-level specifications, the dependent variable is the average of the logarithm of the individual's age at death at county level. Besides the county controls, we also include individual covariates as county-level averages.

[57] A one-standard-deviation increase in New Deal relief extended longevity by 10.9 months overall, by 22.5

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 A.11 we present 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, since individuals who die young are less likely to be linked to their deaths. To address this issue, we estimate the main model restricting our sample to individuals who survived to 20 and report the estimates in Appendix Table A.12, which are consistent with our findings in the main specification.

9. Conclusion

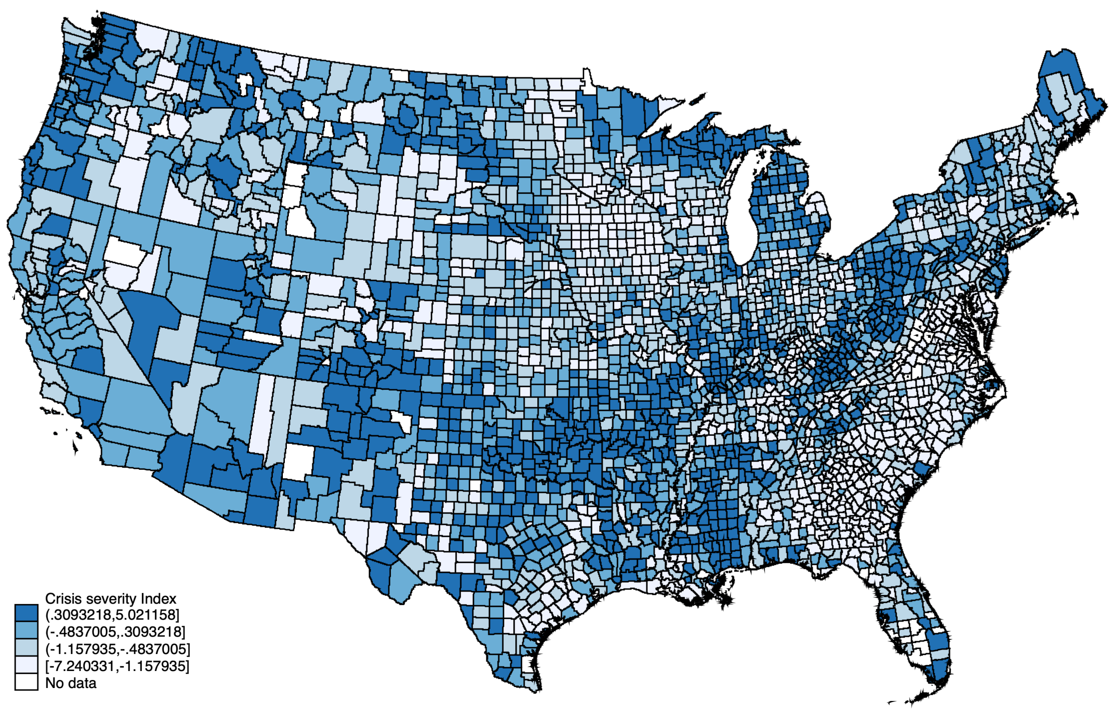
Using a large novel dataset that links the population alive in 1930 to their deaths, we provide evidence that the Great Depression was bad for people's health. Although we find negative effects in both the short and long run, the effects are larger in the latter. More importantly, we find that failing to account for the New Deal—the government's response to the economic crisis—results in biased estimates that underestimate the negative effects of the recession. This could partly explain why our results differ from the traditional literature, which finds short-run positive effects of recessions on health (Ruhm, 2000; Ruhm and Black, 2002; Dehejia and Lleras-Muney, 2004; Ruhm, 2005; Miller and Urdinola, 2010; Stevens et al., 2015; Strumpf et al., 2017; Tapia Granados and Ionides, 2017; Tapia Granados and Diez Roux, 2009; Stuckler et al., 2012). Another reason could be that we can follow individuals even if they moved (Arthi et al., 2022). We also present causal evidence that New Deal relief extended individuals' longevity, and the effects are also larger in the long run. On average, the New Deal extended longevity by 6 months. Our results on the effects of the New Deal are consistent with Fishback et al. (2007), who find reductions in infant mortality, and Aizer et al. (2020), who find positive effects of a specific New Deal program, the CCC, on

months for men, and 5.8 months for women.

longevity. New Deal relief more than compensated for the negative consequences of the Great Depression; we find a predicted average net effect of a 1-month increase in longevity. These findings are driven by men and teenagers; 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 results in this paper could have important implications when evaluating or designing public policy, since they provide evidence that both recessions and the policies designed to address them can have large effects on individuals' lives in the long run. For example, the US suffered two main recessions in the last two decades, in 2008 and 2020, during the financial crisis the covid pandemic, respectively. Our results could shed light on whom to target during an economic downturn, since we have seen that the most affected also benefit the most from relief. However, when trying to generalize these findings, we need to consider that in our setting a "social safety net" was nonexistent in the United States. Currently, there are several types of policies that may dampen the negative effects of a recession. In addition, our sample is positively selected toward individuals with above-average lifespans, which could cause our results to underestimate 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 outcomes other than lifespan. For example, when the full-count 1950 US Census becomes broadly available for research use, future researchers could replicate our methods and explore medium-term effects on income, employment, and so on. In addition, with improvements in matching rates, this analysis might be performed including populations we could not study, such as minorities.

10. Figures and Tables

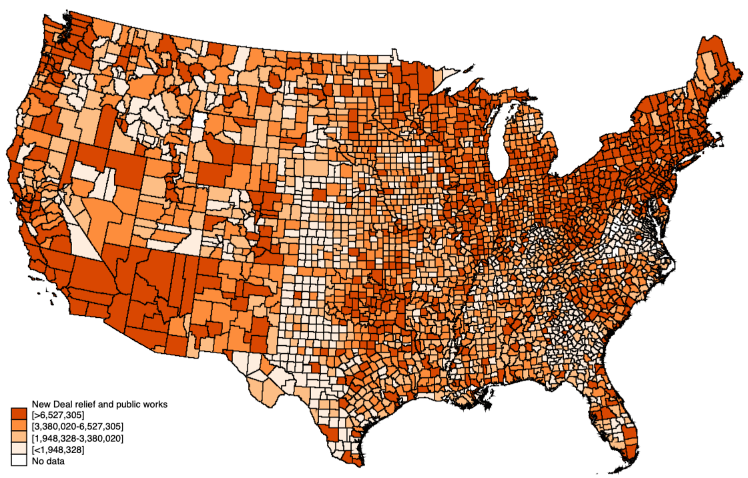
Figure 1.1: Variation of the Severity of the Great Depression by County



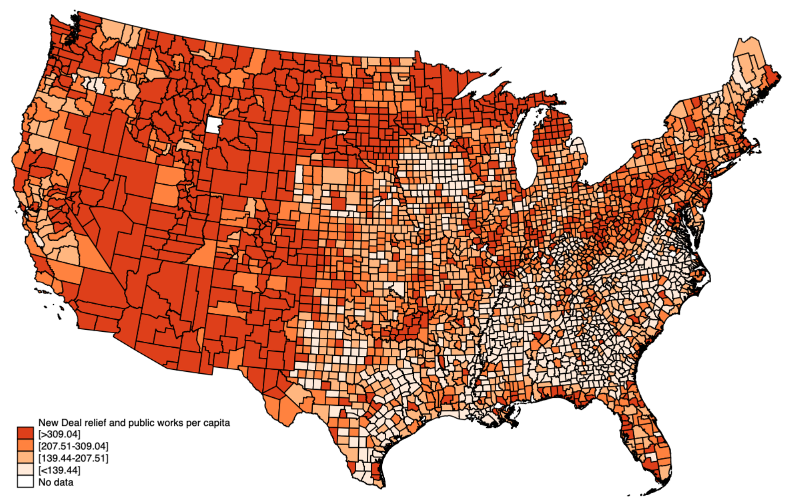
Notes: Black lines represent the limits of the counties in 1930. Counties are colored in blue scale to depict the severity of the crisis from 1929 to 1933. The data used to construct the index presented in this map include unemployment rates from the 1930 full-count US Census, the change in retail sales 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.

Figure 1.2: Geographic Distribution of New Deal Relief and Public Works

(a) Total Funds

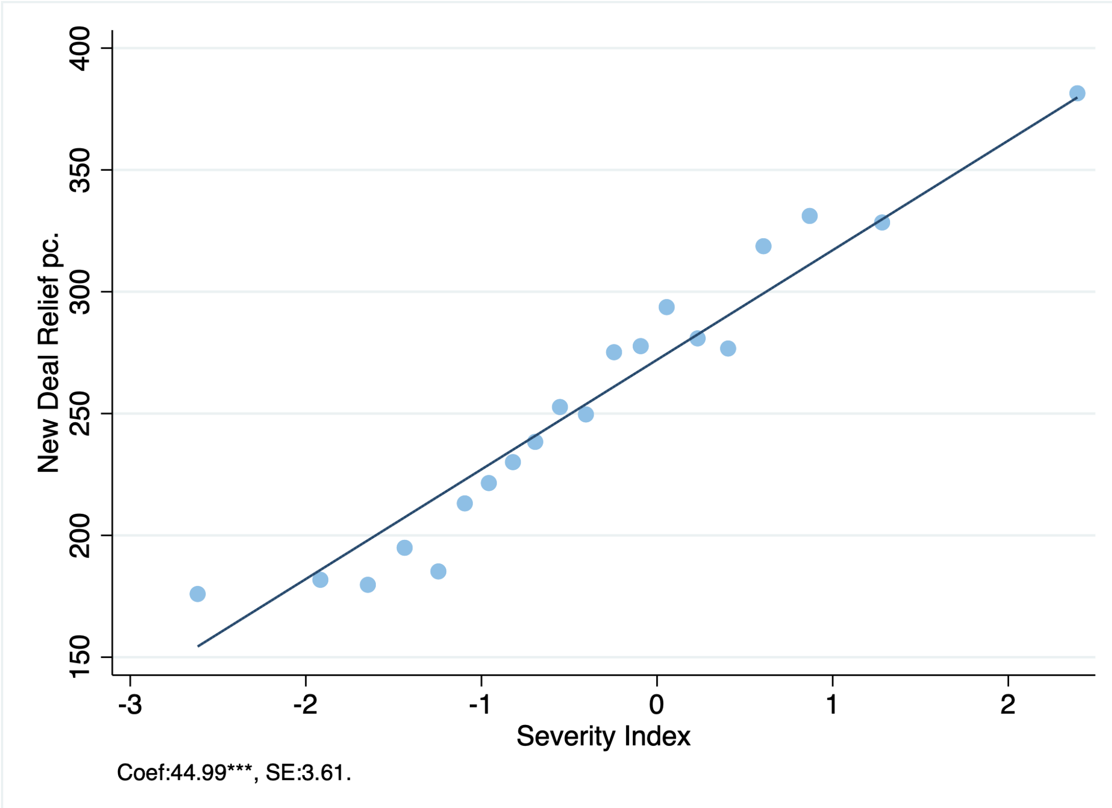


(b) Funds Per Capita



Notes: Black lines represent the limits of the counties in 1930. Counties are colored in orange scale to depict the amount of New Deal relief. New Deal relief data come from the Statistical Section of the Office of Government reports published in 1940, digitized by Fishback et al. (2005).

Figure 1.3: Relationship between New Deal Relief and the Severity Index



Notes: The figure is a binned scatter plot. New Deal relief data comes from the Statistical Section of the Office of Government reports published in 1940, digitized by Fishback et al. (2005). The severity index is the standardized and adjusted sum of unemployment rates from the 1930 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.

Table 1.1: Summary Statistics

	All Individuals				White and US-Born			
	1930 Census		**1930 Census - FS**		**1930 Census**		**1930 Census - FS**	
	(1)		(2)		(3)		(4)	
	mean	sd	mean	sd	mean	sd	mean	sd
Relief per Capita 1933-1939	279.26	139.04	264.81	140.05	270.19	134.50	269.80	134.17
Severity Index (County level)	-0.02	1.02	-0.10	1.01	-0.01	1.01	-0.01	1.01
Year of Birth	1901.17	19.78	1899.34	16.11	1902.89	19.57	1902.94	19.43
Year of Death	1973.60	20.27	1973.60	20.27	1974.07	20.20	1976.93	22.31
Age in 1930	28.83	19.78	30.66	16.11	27.11	19.57	27.06	19.43
Male	0.51	0.50	0.54	0.50	0.50	0.50	0.56	0.50
White	0.90	0.30	0.99	0.10	1	0	1	0
US-Born	0.88	0.32	0.97	0.17	1	0	1	0
Urban	0.56	0.50	0.44	0.50	0.52	0.50	0.49	0.50
Married	0.43	0.49	0.60	0.49	0.40	0.49	0.45	0.50
Schooled	0.23	0.42	0.17	0.38	0.26	0.44	0.26	0.44
Age at death	74.25	14.65	74.25	14.65	74.25	14.69	74.00	15.02
Northeast	0.28	0.45	0.17	0.38	0.25	0.43	0.24	0.43
Midwest	0.31	0.46	0.39	0.49	0.36	0.48	0.36	0.48
South	0.30	0.46	0.31	0.46	0.29	0.45	0.29	0.45
West	0.10	0.30	0.12	0.33	0.11	0.31	0.11	0.31
Observations	122,777,512		28,567,905		89,677,282		26,508,899	

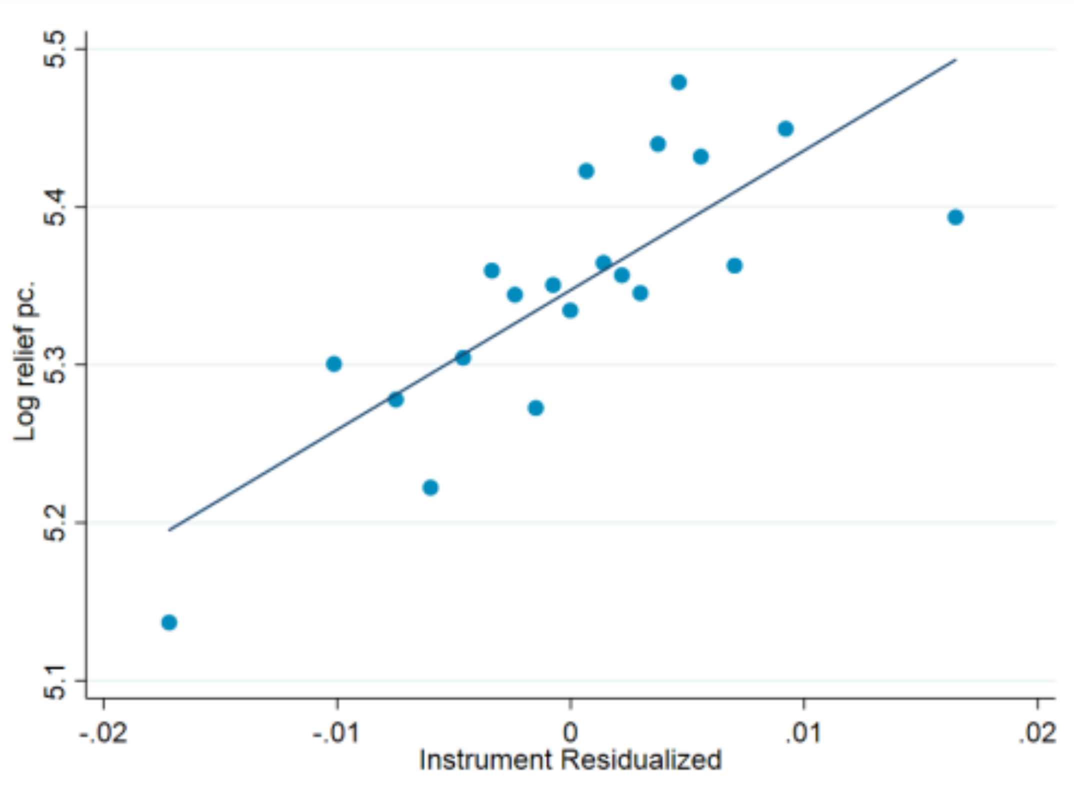
Notes: Summary statistics for all individuals and white and US-born. Column (1) reports summary statistics for the 1930 full-count US Census and column (2) for individuals linked to FamilySearch deaths. Column (3) presents summary statistics for all white and US-born individuals in the 1930 full-count US Census and column (4) the white and US-born individuals in the 1930 census linked to FamilySearch deaths weighted at cohort-county level.

Table 1.2: Analyzing Whom we Match from the 1930 US Census to the FamilySearch Deaths

Dep. Var.	1(Linked to FS deaths)
Family Size	0.0151***
	(0.0005)
Number of Children	0.0378***
	(0.0006)
Married	0.1246***
	(0.0023)
Student	-0.0171***
	(0.0015)
In the Labor Force	-0.0036**
	(0.0018)
Employed	0.0419***
	(0.0013)
Occupation Score	0.0003***
	(0.0001)
Age	0.0104***
	(0.0003)
Age^2	-0.0002***
	(0.0000)
Severity Index	-0.0243***
	(0.0036)
Relief pc.	-0.0001***
	(0.0000)
Constant	-0.1038***
	(0.0098)
Observations	70,043,541
R-squared	0.1418

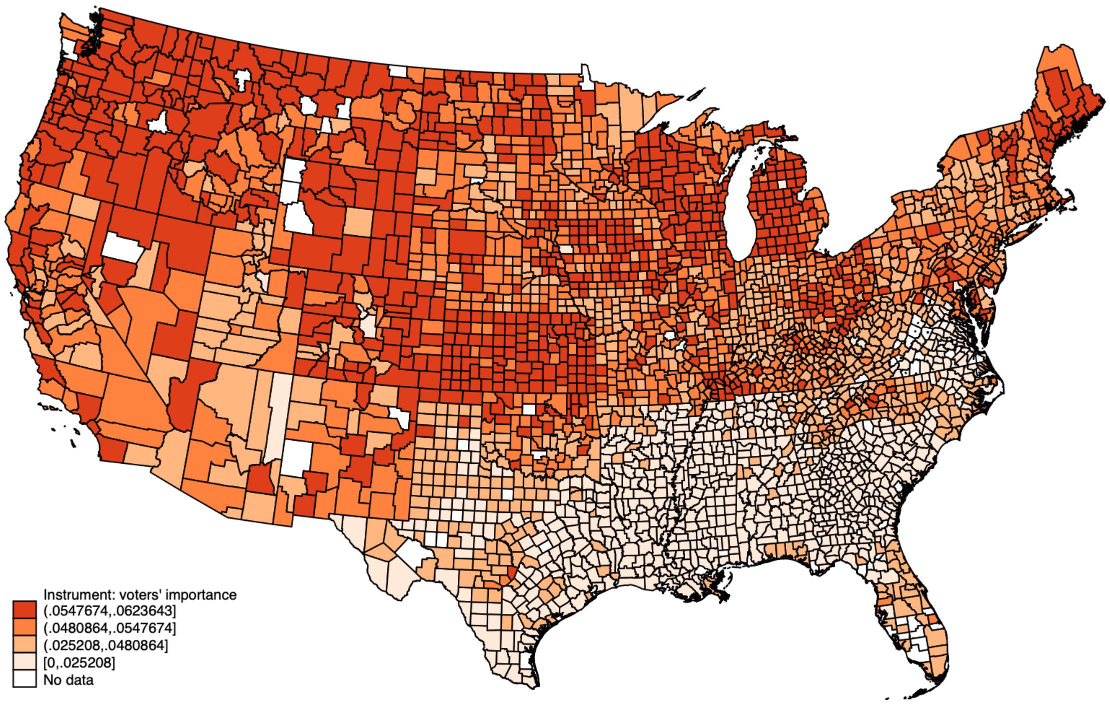
Note: The sample includes all white native individuals in the 1930 US Census. The dependent variable is an indicator whether the individual was linked to their FamilySearch death. The regression includes cohort and state of birth fixed effects. Standard errors are clustered at county level. 10\%*, 5\%**, 1\%***.

Figure 1.4: Relationship between Voting Culture Exploitability Instrument and New Deal Relief per Capita



Notes: Binned scatter-plot where the x-axis presents the residualized version of the voter's importance instrument and the y-axis presents New Deal relief per capita.

Figure 1.5: Geographic Distribution of Voting Culture Exploitability Instrument



Notes: Black lines represent the limits of the counties in 1930. Counties are colored in orange scale to depict distribution of the voters' importance instrument.

Table 1.3: OLS Estimates of the Great Depression and the New Deal on Longevity

Dep. Var. Log (Age at death)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log (Relief per capita $)	-0.0065***	-0.0035***	-0.0034***		-0.0036***	-0.0019***	-0.0019***
	(0.0002)	(0.0002)	(0.0002)		(0.0002)	(0.0002)	(0.0002)
Severity Index				-0.0012***	-0.0028***	-0.0019***	-0.0019***
				(0.0003)	(0.0001)	(0.0001)	(0.0001)
Constant	4.9600***	4.9131***	4.1418***	4.1447***	4.9462***	4.9037***	4.1312***
	(0.0242)	(0.0242)	(0.0039)	(0.4173)	(0.0241)	(0.0241)	(0.0039)
County Controls		x	x	x		x	x
Individual Covariates			x	x			x
Observations	26,508,335	26,429,219	26,429,219	26,429,219	26,508,335	26,429,219	26,429,219
R-squared	0.0413	0.0416	0.0422	0.0379	0.0414	0.0417	0.0422

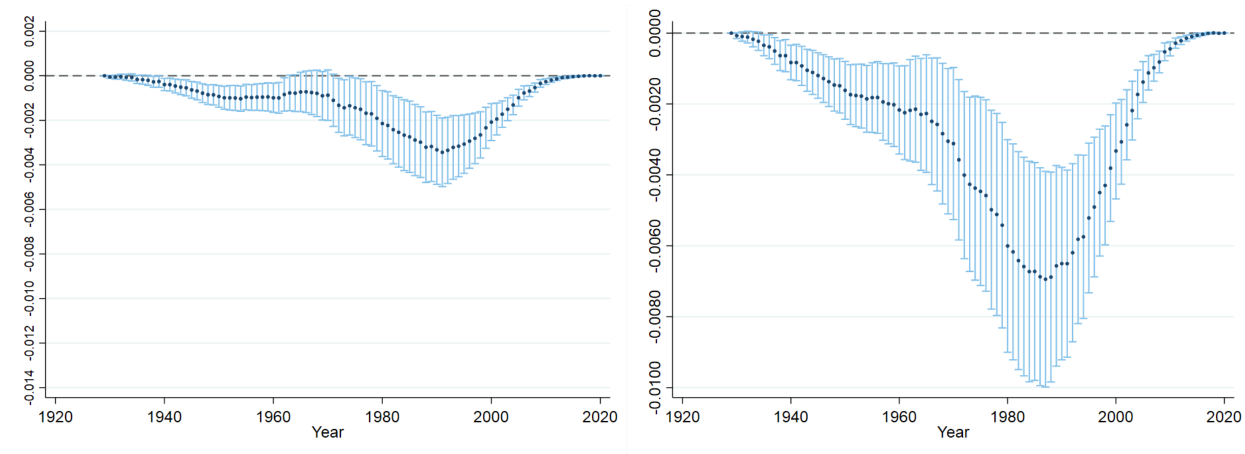
Notes: The sample includes all white native individuals in the 1930 US Census linked to their FamilySearch deaths. The first specifications (Columns 1-3) do not control for the severity of the recessions, column 4 does not include New Deal relief, and the last three (columns 5-7) include both relief and crisis severity. Standard errors clustered at county level. All specifications include state of birth and cohort fixed effects. 10\%*, 5\%**, 1\%***

Table 1.4: IV Estimates of the New Deal on Longevity

	All						Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var:	L(Relief pc)	L(Age at death)	L(Relief pc)	L(Age at death)	L(Relief pc)	L(Age at death)	L(Relief pc)	L(Age at death)	L(Relief pc)	L(Age at death)
Log(Relief pc)		0.0190***		0.0099*		0.0132**		0.0236***		0.0003
		(0.0058)		(0.0059)		(0.0067.)		(0.0087)		(0.0060)
Severity Index	0.2130***	-0.0081***	0.1374***	-0.0027***	0.1377***	-0.0032***	0.1368***	-0.0041***	0.1390***	-0.0013
	-0.0095	(0.0012)	(0.0093)	(0.0008)	(0.0093)	(0.0009)	(0.0095)	(0.0012)	(0.0092)	(0.0008)
Voting culture Instrument	7.2715***		4.6992***		4.6598***		4.7353***		4.7233***	
	-0.9691		(0.9623)		(0.9647)		(1.0206)		(0.9933)	
Constant	5.0038***	4.2066***	5.6207***	4.8938***	5.6456***	4.0616***	5.6494***	3.9915***	5.6018***	4.1955***
	-0.077	(0.0384)	(0.0592)	(0.0008)	(0.0694)	(0.0395)	(0.0678)	(0.0517)	(0.0750)	(0.0350)
County Controls			x	x	x	x	x	x	x	x
Individual Covariates					x	x	x	x	x	x
Observations	26,408,684	26,408,684	26,316,011	26,316,011	26,316,011	26,316,011	14,305,370	14,305,370	12,010,641	12,010,641
R-squared	0.47	0.03	0.56	0.03	0.56	0.03	0.55	0.05	0.56	0.02
F- Test	56.3		23.84		23.33		22.98		23.67	

Notes: The sample includes all white native individuals in the 1930 US Census linked to their FamilySearch deaths. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. 10\%*, 5\%**, 1\%***.

Figure 1.6: The Effects of the Great Depression on Survival for Cohorts Ages 16-25 in 1930

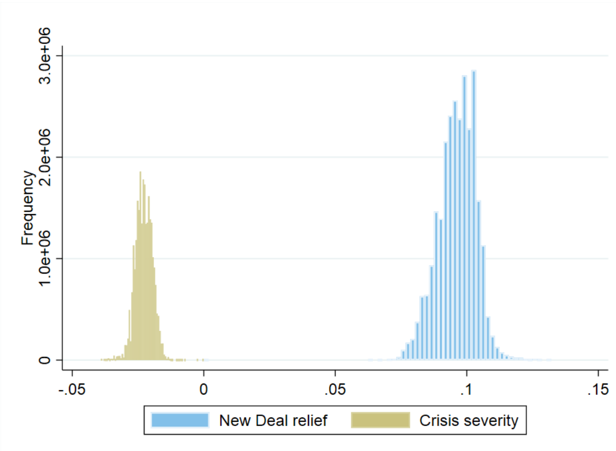


(a) OLS Estimates

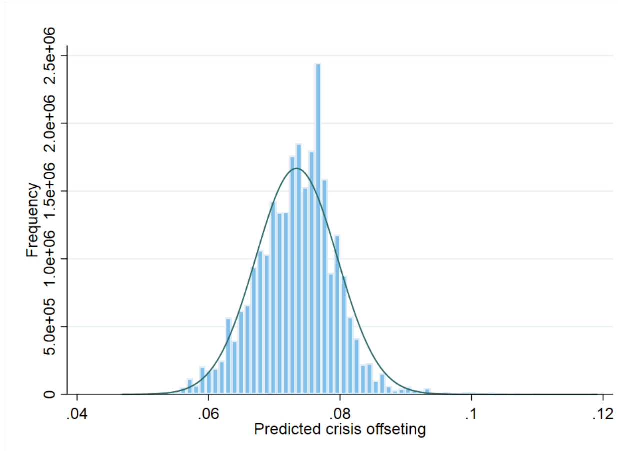
(b) IV Estimates

Notes: The figures show the OLS and IV coefficients and 95% confidence intervals, respectively, of the effects of crisis severity on survival from 1933 to 2020 for cohorts aged 16 to 25 in 1930. IV coefficients come from the regression in which we instrument New Deal relief, and the coefficients plotted are for the severity of the crisis without being instrumented. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Figure 1.7: IV-Predicted Effects of the Great Depression and New Deal Relief on Longevity



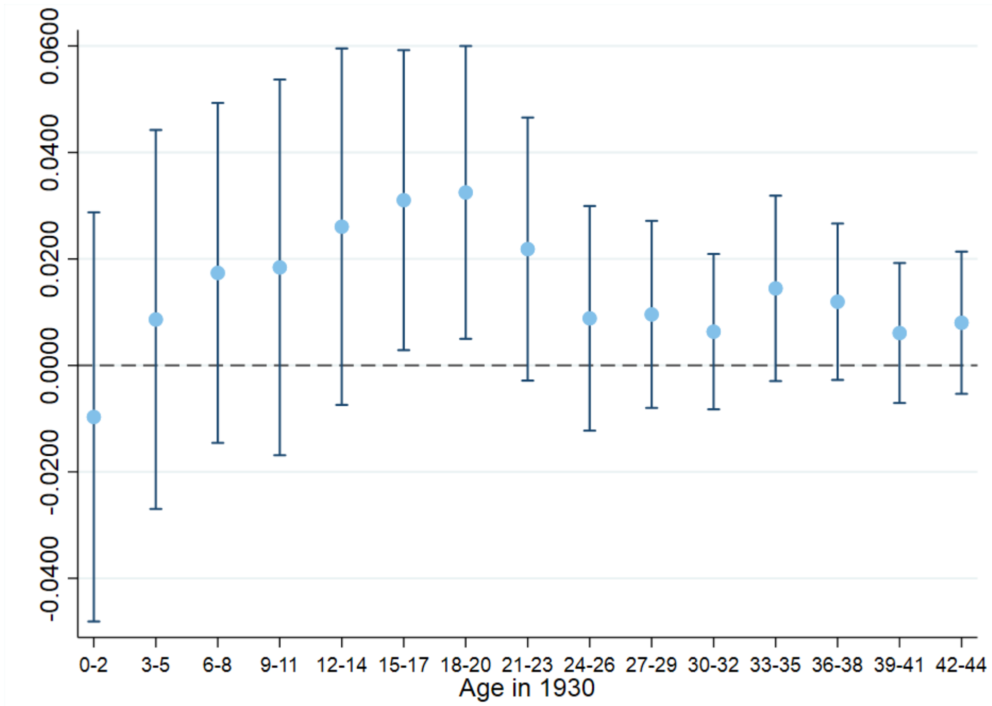
(a) Predicted IV Effects on Longevity



(b) Predicted IV crisis offsetting

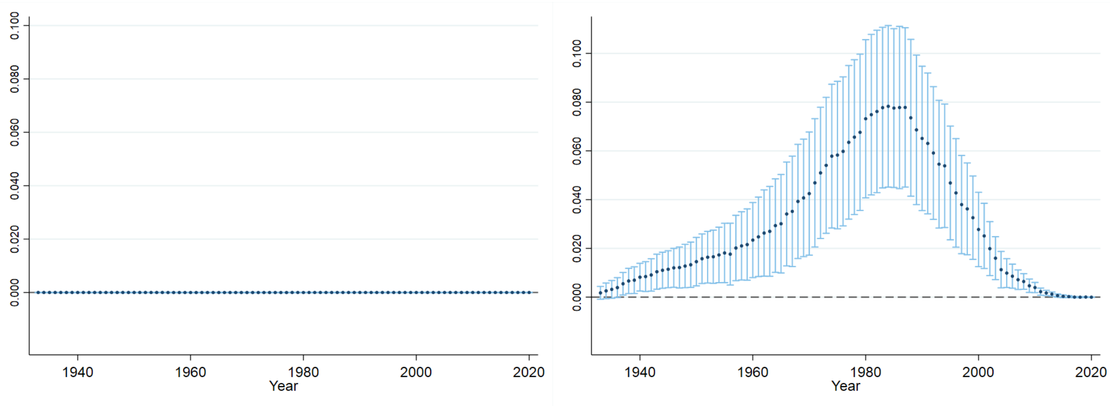
Notes: The figures present the IV predicted effects of the Great Depression and New Deal relief on longevity. The specification to predict effects include county controls selected by LASSO and individual covariates from the 1930 Census, as well as state of birth and cohort fixed effects. The sample includes all white, native individuals in the 1930 Census linked to their FamilySearch deaths.

Figure 1.8: IV Estimates of the New Deal Relief on Longevity by Cohort



Notes: The figure presents IV estimates and 95% confidence intervals of the post-IV-LASSO regression of the New Deal relief on longevity by cohorts. The regression accounts for the severity of the crisis and includes the county controls selected by LASSO and individual covariates from the 1930 census. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. The sample includes all white, native individuals in the 1930 Census aged 0-44 in 1930 linked to their FamilySearch deaths.

Figure 1.9: Effects of New Deal Relief on Survival for Cohorts 16-25



(a) OLS Estimates

(b) IV estimates

Notes: The figures present OLS (a), IV coefficients (b) and 95% confidence intervals of the effects of New Deal relief on survival from 1933 to 2020 for cohorts aged 16 to 25 in 1930. Regressions include county controls, individual covariates and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Table 1.5: IV Estimates of the Effects of the New Deal and the Great Depression on 1940 Outcomes

Dep. Variable	(1) Income	(2) Employed	(3) In Labor Force	(4) Education	(5) Married	(6) Divorced	(7) Widowed	(8) Moved
Relief pc.	0.1492	0.0001	0.0001**	0.002	0.00004	0.00001**	-0.00004***	-0.0002
	(0.2434)	(0.0001)	(0.0001)	(0.002)	(0.00003)	(0.00003)	(0.00002)	(0.0002)
Crisis Severity	-11.1370**	-0.0081***	-0.0045***	-0.166***	0.0005	-0.0012**	0.0007**	0.0223***
	(5.0645)	(0.0015)	(0.0014)	(0.056)	(0.0006)	(0.0005)	(0.0003)	(0.0047)
Constant	130.6288**	-0.0028	-0.008	3.546***	0.0044	-0.0113**	0.0078**	0.3533***
	(56.8772)	(0.0136)	(0.0125)	(0.542)	(0.0064)	(0.0044)	(0.0030)	(0.0429)
Observations	17,893,552	20,952,286	20,952,286	20,536,703	20,952,286	20,952,286	20,952,286	20,952,286
R-squared	0.12	0.12	0.14	0.21	0.54	0.001633	0.101741	0.04
Outcome Mean	497.33	0.5	0.54	9	0.72	0.01	0.04	0.21
Effect severity	-2%	-2%	-0.01%	-1.6%	0.07%	-12%	1.75%	10%
Effect relief	4.2%	2.8%	2.6%	3.5%	0.7%	14%	-14%	13%

Notes: The sample includes all white native individuals in the 1930 Census linked to their FamilySearch deaths and to the 1940 Census. For column 1, the sample is smaller because fewer individuals report information on their income. The table presents second-stage IV estimates for the effects of New Deal relief on different outcomes from the 1940 Census. The variable education is expressed in years. Moved is an indicator whether the individual moved counties from 1930 to 1940. All specifications include county controls selected by LASSO, individual covariates from the 1930 Census, and state of birth and cohort fixed effects. Standard errors are clustered at county level. The effects presented in the last two rows correspond to a standard deviation increase in severity and relief, respectively. 10\%*, 5\%**, 1\%***.

Chapter 2

Housing Wealth, Health and Deaths of Despair

1. Introduction

Do housing shocks affect population health? If so, does housing wealth play a role in the US opioid crisis that has tripled the drug-related death rate since 1999?[1] Although the relationship between wealth and health has been extensively reported in the literature, little is known about its causality. The attention on this relationship has been magnified by the recent dramatic increase in "deaths of despair", that is the increase in drug overdoses and alcohol-related deaths that have shortened the lifespan of white non-Hispanic Americans for the first time after decades of progress.[2] In this paper, we use unexpected shocks in housing wealth as an important unexplored driver that explains the effect of wealth on different measures of health such as self-reported health (SRH), limitations in activities of daily living (ADLs), drug-related mortality rates, suicide rates, and alcoholic-related liver

[1] Hedegaard et al. (2017) document that the age-adjusted rate of drug-overdose has increased from 6.1 per 100,000 in 1999 to 19.8 per 100,000 in 2016.

[2] Case and Deaton (2015, 2017) named this crisis "deaths of despair". They suggest that this increase has been due to difficult social and economic environments that have led to cumulative disadvantage over time.

mortality rates, as well as their socioeconomic and geographic differences.

We use household-level data from the Panel Study of Income Dynamics (PSID) to exploit a quasi-natural experiment to analyze the causal relationship between wealth and health. We use the fact that housing wealth is the most important part of households' wealth. It accounts for almost two thirds of the total wealth of the median household in the US (Federal Reserve Board) where the home ownership rate is 64.2% (Federal Reserve Bank of St Louis). To determine causality, we create a measure of unexpected shocks in housing wealth. This measure builds upon the fact that households tend to misestimate the value of their houses and they only discover their true market value when they sell them.[3] Therefore, households that overvalue (undervalue) their houses experience an unexpected negative (positive) shock in their housing wealth when they sell their houses. This unexpected shock in housing wealth is what we define as the "realization of housing wealth misestimation" (RHWM). The magnitude of this shock is very large: 25% of the households in our sample overvalue their house by 9% or more, while 25% of the households undervalue their houses by at least 11%.[4]

There could be four types of concerns related to the causal interpretation of a RHWM shock. First, one might worry that our results are driven by households that decide to move for health reasons. To address this concern, we perform a two sample t-test and find that those who move do not have a statistically different health status from those who do not move. Moreover, we control for the health of the head of the household in all our specifications.

Second, these unexpected wealth shocks must be truly unexpected. Maybe households that significantly overestimate their houses may not sell them because they are loss averse. Therefore, we test whether households only realize their house wealth misestimation when

[3] See Kish and Lansing (1954); Follain and Malpezzi (1981); Goodman Jr and Ittner (1992); Agarwal (2007); Benítez-Silva et al. (2015); Kuzmenko and Timmins (2011); Corradin et al. (2017).

[4] Housing wealth misestimation is large, even with the proliferation of online real estate appraisals such as Zillow, as well as the existence of real estate municipal tax assessments and appraisals for extracting home equity value. Zillow documents that 45.6% (25.5%) of Zillow's estimates are off by 5% (10%) or more (see https://www.zillow.com/zestimate). Moreover, the geographical variation is sizable. For example, 32.7% (14.7%) of Zillow's estimates are off by 5% (10%) or more in Phoenix, while 62.1% (44.9%) of Zillow's estimates are off by 5% (10%) or more in New York.

they sell their house and we show that RHWM is actually an unexpected shock.

Third, unexpected wealth shocks must be independent of any unobserved heterogeneity in health changes. For this reason, we control for variables such as initial health, housing wealth, number of family members in the household, and employment status to address reverse causality concerns. Moreover, house market changes may not only affect house prices but also correlate with prices of other wealth holdings. We show that our results are robust when controlling for the fraction of wealth held in stocks and in housing.

Fourth, it could be that those who move and sell their house at a large discount are different for those who move and sell at a profit due to unobservable variables. For example, those who are worse at bargaining or those who face liquidity constraints (e.g., those who lose their jobs and are forced to move) might sell their house at a large discount and, therefore, experience a negative wealth shock. We address this concern in three ways: (i) We perform tests to show that there are no significant differences among socioeconomic and demographic characteristics between households that experience a positive or negative wealth shock when moving; (ii) we control for socioeconomic and demographic characteristics at the household level that are correlated with bargaining and liquidity such as education, employment status, and income; and (iii) to account for other possible unobservable variables as well as some of the endogeneity issues explained above, we implement an instrumental variable (IV) strategy. Overall, our results suggest that there is causality between housing wealth and health outcomes.

We find that housing is an important channel to understand the causal effects of wealth on a broad range of health outcomes.[5] Our results show that a one standard deviation positive shock in housing wealth increases the probability of an improvement in SRH by 1.0 percentage points. A shock of the same size leads to a 1.10 percentage points decrease

[5]We define change in health outcome as the difference in health from two years after the unexpected wealth shock to the year of the wealth shock (i.e., when the household moves). This definition addresses a potential concern related to the fact that health shocks might trigger moving houses.

in the probability of increasing the number of limitations in mental ADLs suffered by an individual. Moreover, we find that a one standard deviation positive change in housing wealth decreases the change in drug-related mortality rate by 4.3 percent. We do not find significant equivalent results for alcohol or suicide death rates. We also show that these effects are different across geographical areas. Specifically, the impact of wealth on health is higher in Metropolitan Statistical Areas (MSAs) in which housing supply is more inelastic because unexpected shocks in housing wealth tend to be greater in those MSAs.

Our approach contributes to the previous research in three main ways. First, we contribute to the literature that investigates the causal link between wealth and health. RHWM provides a shock in wealth that is: (i) unexpected, (ii) sizable, and (iii) that affects a broad set of the population. Despite an extensive literature on the relationship between socioeconomic status (SES) and health (see Cutler, Lleras-Muney, and Vogl (2008) for an extensive summary of this literature[6]) the main difficulty is that SES can affect health and vice versa. On the one hand, lower income or wealth may lead to a decline in health through, for instance, a worsening of the individual's diet, or a reduction in access to medical care and a corresponding delay in the detection of medical conditions (Ettner (1996); Smith (1999); Currie et al. (2010)). On the other hand, people in worse health may find it difficult to go to work every day and, as such, are more likely to have low income or wealth (Wu (2003); Currie and Madrian (1999); McClellan (1998)).

The extant literature on the causal wealth-health link has used data on lottery winners (Lindahl (2005); Gardner and Oswald (2007); Apouey and Clark (2015); Brot-Goldberg et al. (2017)), inheritance (Meer et al. (2003); Kim and Ruhm (2012)), and changes in stock (McInerney et al. (2013); Schwandt (2018a)) and house prices (Fichera and Gathergood (2016)) to create settings as close to a natural experiment as possible. The main problem with studies of lottery winners is the low number of winners relative to the total population.

[6] Adler et al. (1994); Backlund et al. (1999); Chandola (2000); Contoyannis et al. (2004); Cutler et al. (2010); Cutler et al. (2016); Feinstein (1993); Golberstein et al. (2016); Humphries and Van Doorslaer (2000); Lewis et al. (1998); Lleras-Muney (2005); Meara (2001); Meer et al. (2003); Wilkinson and Marmot (2003)

The main concern with studies of inheritance is that an inheritance can be anticipated. An inheritance is not a random event. Households that receive a bequest are more likely to come from wealthy families and, hence, their health endowments might differ from those of households that do not inherit. Finally, the problem with studying changes in stock and house prices is that not all such changes come as unexpected shocks. In fact, the financial economics literature shows that investors are aware of return predictability and the existence of fat tails in stock returns (Bossaerts and Hillion (1999); Lettau and Ludvigson (2001)) and house prices are characterized by persistence and a high degree of predictability (Fischer and Stamos (2013); Corradin et al. (2013)).

To address potential endogeneity and measurement error concerns with our measure of RHWM, we provide a valid instrumental variable (IV) for wealth shocks based on the interaction of interest rates and the geographical determinants of elasticity of housing supply –calculated by Saiz (2010) using satellite-generated data on terrain elevation and presence of water bodies. The reasoning for the use of this interaction is as follows. When interest rates decrease, demand for housing increases. As markets can adjust prices and quantities, ceteris paribus, this increase in demand translates into higher real estate prices in areas where supply is more inelastic. This can translate into a larger underestimation of a house's true value and a larger positive wealth shock if the owners decide to sell. Although IVs based on housing-supply elasticity have previously been used in the literature to instrument local real estate prices (e.g., Himmelberg et al. (2005); Mian and Sufi (2011); Chaney et al. (2012); Cvijanović (2014)), they have never been used to analyze the impact of wealth on health status.

Our second contribution is the study of the impact of unexpected shocks in housing wealth on a broad range of health outcomes: SRH, total limitations in ADLs, limitations in mental ADLs, drug-related death rates, and alcohol and suicide related death rates. By looking at different measures of health outcomes, we can study the causes of the deaths of despair. Case and Deaton (2017) provide a first alternative explanation for the recent increase in deaths of despair. They suggest that deaths of despair respond more to prolonged economic conditions than to short-term fluctuations, and especially social dysfunctions that come with prolonged economic distress.

A second alternative explanation focuses on supply-side elements and on the fact that there might have been changes in the availability of risky drugs. In this regard, Ruhm (2018) finds that changes in the drug environment are an important aspect of the crisis. A distinguishing feature of the current epidemic of drug abuse is that many overdoses and deaths can be attributed to legal opioids that were prescribed by physicians.

In our paper, we explore another potential mechanism: unexpected shocks in housing wealth. To our knowledge, this paper is the first to document the impact of housing wealth shocks on the current US opioid crisis. To account for the two alternative explanations for the recent deaths of dispair, we control for various economic factors related to labor markets the economic environment and for several law changes in the US such as the introduction of the marijuana law, or the implementation of prescription drug monitoring programs.

Our third contribution is related to the study of the geographical variation of the effect of wealth on health outcomes and the "deaths of despair". Housing wealth is a channel through which macroeconomic shocks have different health outcomes across geographies. Ceteris paribus economic cycles have a more pronounced impact on health in MSAs where housing supply is more inelastic because unexpected shocks in housing wealth are larger in those MSAs. For example, a positive shock in demand experienced by households located in the most inelastic MSAs, such as Miami, Los Angeles-Long Beach, San Francisco, and New York, leads to a higher probability of a health improvement than a demand shock of the same magnitude experienced by those located in the top elastic MSAs, such as Cincinnati, Atlanta, San Antonio, and Oklahoma City.

The remainder of the paper is structured as follows. Section 2. describes the empirical data, which includes the description of our measure of unexpected shocks in wealth. Section 3. provides a detailed description of the empirical strategy. Section 4. presents the results. Finally, section 5. concludes.

2. Data

We use data from the Panel Study of Income Dynamics (PSID), which follows a nationally representative sample of U.S. households. The PSID contains data at the individual and family-unit levels.[7] Our dataset covers the characteristics of the head of household from 1984 to 2013.[8] Moreover, we link the PSID household-level data to health outcomes at the county-level (e.g., change in drug-induced, alcohol-induced, and suicide death rates) from the Center for Disease Control (CDC) for the analyses related to "deaths of despair". Table 2.1 presents the summary statistics and the description of the variables used in our analysis.

2.1 A measure of unexpected shocks in wealth: realization of housing wealth misestimation (RHWM)

We analyze whether a shock in wealth is related to a change in health. Ideally, this shock should be unexpected in order to determine causality. As housing wealth accounts for almost two thirds of the total wealth of the median household (Iacoviello (2012)), it is the most important part of households' total wealth. We create a measure of unexpected shocks in housing wealth that builds upon the fact that households tend to misestimate the value of their houses (Kish and Lansing (1954); Follain and Malpezzi (1981); Goodman Jr and

[7]Panel Study of Income Dynamics, restricted use dataset. Produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan, Ann Arbor, MI (2017). The collection of data used in this study was partly supported by the National Institutes of Health under grant number R01 HD069609 and R01 AG040213, and the National Science Foundation under award numbers SES 1157698 and 1623684.

[8]As we focus on the SRH of the head of the household, we drop observations that indicate a change in age of more than five years from one period to the next. We also remove observations with a negative change in age.

Ittner (1992); Agarwal (2007); Benítez-Silva et al. (2015); Kuzmenko and Timmins (2011); Corradin et al. (2017)) and they only discover their true market value when they sell them. Therefore, households that overvalue (undervalue) their houses experience an unexpected negative (positive) shock in their housing wealth when they sell their houses. This unexpected shock in housing wealth is what we define as the "realization of housing wealth misestimation" ($RHWM_{it}$) for a household i at time t.

We could simply measure $RHWM_{it}$ as the difference between the house selling price and the answer to the question in PSID (i.e., "Could you tell me what the present value of this house (farm) is? I mean about what would it bring if you sold it today?") in the previous period. However, PSID does not provide information on the selling price of the house.[9] Therefore, to calculate the $RHWM_{it}$, we need to build a measure of housing wealth misestimation. To do so, we follow Corradin et al. (2017) and we compare data on reported house values from the PSID –which includes the dollar amount of home improvements– to market house values calculated as the initial buying price of the house updated by the zip code level CoreLogic Home Price Index (HPI).[10] CoreLogic HPI is a repeated-sales index calculated using the market values for house transactions in the same zip code. We define housing wealth misestimation (HWM_{it}) for a household i at time t as the difference between the reported house value and its estimated market value. Hence, HWM is zero at the time of a housing transaction.[11]

If household i does not move in a given year t, then $RHWM_{it}$ takes a value of zero because the household is unaware of its misestimation (i.e., they only discover the true market value

[9]If a household sells its house and buys a new one between years $t-1$ and t, we can only obtain its declared value of the previous house at time $t-1$ (before selling it) and the transaction price of the new house at time t. This declared value at time $t-1$ may be misestimated.

[10]The main assumption is that house prices evolve the same way within the zip code. Notice that the impact of house specific characteristics is already included in the initial value. Therefore, if the house was originally purchased with attractive features, then it will be already accounted in the purchase price. As in Corradin et al. (2017), we adjust the house values reported in PSID for home-improvement expenses that households report in the same survey.

[11]In Appendix B, we show an analysis of the variation of HWM between and within households.

of the house when they sell it). Therefore, $RHWM_{it}$ is zero most of the time because most households do not move often. If household i moves in a given year t, then $RHWM_{it}$ is the difference between the market value at which the house is sold and the reported value of the house in the previous period. Therefore, $RHWM$ will be positive when the household undervalues its house (i.e., it experiences a positive unexpected shock on wealth when it sells the house) and negative when it overvalues its house (i.e., it experiences a negative unexpected shock on wealth when it sells the house.) $RHWM$ will only be different from zero for those households that move when they move. In summary, $RHWM$ represents an unexpected shock on the family's wealth. It is expressed in tens of thousands of dollars, and its mean value for our sample is 0.0047. Figure 2.1 presents a sketch of how our measure of $RHWM$ is created.

2.2 Health outcomes

We use different measures of health outcomes. The first one of them is the change in self-reported health (SRH). This variable takes a value of 1 if SRH improves two years after the unexpected wealth shock, a value of -1 if it worsens, and a value of 0 if there is no change. This approach follows previous literature (see Ruhm (2018), Kim and Ruhm (2012) or Schwandt (2018b), for instance)". SRH is obtained from the answer to the following question in the PSID: "Would you say your health in general is excellent, very good, good, fair or poor?". We code the answer using a 1 to 5 scale, with 5 being "excellent," 4 being "very good," 3 being "good," 2 being ''fair," and 1 being "poor." Previous research shows that SRH is a good predictor of mortality and of other health outcomes, with people who rate their health as poor being more likely to die or to have a bad health outcome (Long and Marshall (1999); Mossey and Shapiro (1982); Kaplan et al. (1988); Idler et al. (1990); McFadden et al. (2008)). We use a two-year period because of data restrictions—starting in 1999, the PSID was undertaken every two years instead of every year. The average change in SRH for a period of two years for the sample used in our study is -0.0204. Notice that we define change in health outcome as the difference in health from two years after the unexpected wealth shock to the year of the wealth shock (i.e., when the household moves). This definition addresses a potential concern related to the fact that health shocks might trigger moving houses.

We also include also some additional measures of health outcomes: the change in the number of limitations in activities of daily living (ADLs) and the change in mental ADLs.[12] These variables aim at measuring the difficulty an individual may have in executing common daily activities. The PSID questions in this regard are of the form "because of a health or physical problem, do you have any difficulty [doing an ADL]?"[13] We also include three limitations in mental capacities.[14] As before, the variable takes the value of 1 if the number of limitations increases, 0 if it stays the same and -1 if it decreases. These data come from the PSID and starts in 1999.

We also look at the impact on drug-related deaths, alcohol-related deaths and nondrug suicides. We obtain this data from the Multiple Cause of Death files (Center for Disease Control), that identifies death certificates with a single underlying cause of death.[15] We follow Ruhm (2018) to classify ICD-10 codes into the 3 different groups. Thus, drug poisoning deaths include ICD-10 codes X40-X44, X60-X64, X85, Y10-Y14 and Y352. Alcohol-related deaths through liver diseases are given by ICD-10 code K70, and nondrug suicides are defined as ICD-10 codes X65-X84, Y87.0 and *U03. Our analysis includes data at the county level from the year 2000 onwards, since earlier ICD-9 categories are not exactly equivalent to

[12]In the PSID data there are many variables related to health outcomes. For instance, there is information about specific health conditions such as strokes, cancer, high blood pressure, and diabetes in PSID. Instead, we use the most common composite measures of health status in the health economics literature: (1) SRH, (2) total ADLs, and (3) mental ADLs. Moreover, we have long time series of the variables that we need to calculate SRH and ADLs in the PSID data.

[13]The list of activities asked at the PSID are: bathing or showering, dressing, eating, getting in or out of bed or a chair, walking, getting outside, using the toilet, preparing own meals, shopping for personal toilet items or medicines, managing own money, using the telephone, doing heavywork, doing lightwork.

[14]"Has a doctor ever told you that you have... Any emotional, nervous, or psychiatric problems?"; "...loss of memory or loss of mental ability?"; "...a learning disorder?"

[15]See https://wonder.cdc.gov/mcd.html.

ICD-10 codes (Anderson et al. (2001)). We link this county-level data to each household in the PSID sample.

The number of deaths belonging to each group is converted into mortality rates per 100,000 people using Census population data. The number of deaths belonging to each group is converted into mortality rates per 100,000 people using population census data. Moreover, since our data include years where population changes could be significant due to shocks such as hurricanes Katrina and Rita in 2005, we also realize robustness checks using population data corrected by such shocks from the National Cancer Institutes Surveillance Epidemiology and End Results (SEER).[16]

2.3 Control variables

Healthy is a dummy variable created from the SRH variable. It takes a value of 1 if the individual's SRH is excellent, very good, or good. It takes a value of 0 if the individual's SRH is fair or poor. This allows us to control for the health of the individuals at the moment when the house is sold.

We include house value, which is the reported house value in PSID, in order to control for the initial wealth of the individuals. It is expressed in hundreds of thousands of dollars. We also include demographic and socioeconomic variables in our empirical analyses to control for income, age, gender, race, education, and employment status. We also use the number of family members living in the household. Finally, we add year and region (west, midwest, south and northeast) fixed effects. Table **??** provides the detail description and the main statistics of these variables. Note that we are interested in exploiting the geographical variation of our panel data. For this reason, we use an IV that is based on differences in the geographical housing supply across MSAs. Therefore, we use controls at the household level whenever is possible.[17]

[16]See https://seer.cancer.gov/popdata.

[17]We also show that our results are robust to the control for portfolio choice characteristics at the house-

To control for variables that might affect the supply-side of deaths of despair, we follow Ruhm (2018) and include the following controls. First, we control for the number of hospital beds from the Area Health Resource Files database.[18] Second, we control for changes in the effects of international trade are included through two variables of exposure to Chinese import competition. This measure was first constructed by Acemoglu et al. (2016), and is offered at the Commuting Zone level. Within a Commuting Zone, all counties are assumed to have the same level of import exposure.[19] Moreover, we use a dummy variable for the size of the county developed by the USDA Economic Research Service (ERS) County Level Data Sets for year 2013.[20]

Finally, we control for two dummy variables that serve as indicators of state-level legal framework related to drug use are also included in this category. One of them looks at the existence of a prescription drug monitoring program (PDMP), an electronic database that provides information about prescribing and patient behavior. The other dummy variable takes value 1 if marijuana has been legalized in a state at a certain year for medical or recreational purposes, and value 0 otherwise. Both indicators are obtained from the Prescription Drug Abuse Policy System.[21]

3. Identification Strategy and Empirical Approach

In this section we describe the identification strategy and our empirical approach. We want to test whether unexpected shocks in the wealth of individuals have an effect on their future health. Our main dependent variable is the change in SRH at the household level. As detailed in the previous section, this variable can take three values: -1 if there is a decline

hold level such as the ratio of housing to net wealth and stock holdings over total net wealth. Table B.1 in the Appendix reports these results.

[18]See http://www.arf.hrsa.gov.

[19]See http://www.ddorn.net/data.htm.

[20]See https://www.ers.usda.gov/data-products/county-level-data-sets/county-level-data-sets-download-data.

[21]See http://www.pdaps.org.

in SRH, 0 if SRH does not change, and +1 if SRH improves. As SRH is an interval-coded variable, our analysis is based on an ordered probit.[22]

We are interested in estimating $E(y^*|x) = x \cdot \beta$, where $a_1 \leq a_2$ are the known cell limits:

$$y = -1 \text{ if } y^* \leq a_1,$$
$$y = 0 \text{ if } a_1 \leq y^* \leq a_2, \text{ and}$$
$$y = +1 \text{ if } a_2 \leq y^*,$$

where we assume that $y^*|x \sim Normal(x\beta, \sigma^2)$ and that $\sigma^2 = Var(y^*|x)$ does not depend on x.

Our basic specification is the following:

$$\Delta H_{i,t+\tau} = \alpha + \beta RHWM_{it} + \delta H_{it} + \lambda W_{it} + \theta \Sigma X_{it} + \gamma_t + u_i + \epsilon_{ti}, \tag{2.1}$$

where i and t denote the head of the household and the time dimension, respectively. The dependent variable, $\Delta H_{i,t+\tau}$, is a measure of the change in health of the head of the household i from time t to time $t + \tau$.

Let $RHWM_{tj}$ denote the realization of housing-wealth misestimation in year t for head of family i. This is our variable of interest, as it captures the exogenous, unexpected shock in wealth. This variable will only be different than zero when households move. Therefore, $RHWM$ will only be different from zero for those households that move at the year that they move. This identification strategy allows us to identify the impact of house wealth misestimation for households that move.

In equation (2.1), H denotes the level of health just before the shock and W is the level of

[22] An alternative approach could be to use interval regressions. Both methodologies produce coefficients of the same significance and order of magnitude, and have a similar fit in terms of log-likelihood. Although our empirical analysis is based on an ordered probit approach, we present results for both methodologies in the next section.

housing wealth. X includes all relevant socio-demographic characteristics of the individuals that could have an impact on health status: age, sex, education, and race. We also include variables that could have an impact on the decision to move and, hence, on the realization of housing wealth misestimation, such as employment status and number of family members. γ_t refers to time effects, u_j denotes family fixed effects, and ϵ_{ti} is the error term.

The ordered probit estimation is then as follows:

$$P(\Delta H_{i,t} = -1|RHWM_{it}, H_{it}, W_{it,}X_{it}) = P(\Delta H^*_{i,t+\tau} \leq a_1|RHWM_{it}, H_{it}, W_{it,}X_{it}) =$$
$$= \Phi(a_1 \breve{} \beta RHWM_{it} + \delta H_{it} + \lambda W_{it} + \theta \Sigma X_{it}) \tag{2.2}$$

$$P(\Delta H_{i,t+\tau} = 0|RHWM_{it}, H_{it}, W_{it,}X_{it}) = P(\alpha_1 < \Delta H^*_{i,t+\tau} \leq a_2|RHWM_{it}, H_{it}, W_{it,}X_{it}) =$$
$$= \Phi(a_1 \breve{} \beta RHWM_{it} + \delta H_{it} + \lambda W_{it} + \theta \Sigma X_{it}) - \Phi(a_2 \breve{} \beta RHWM_{it} + \delta H_{it} + \lambda W_{it} + \theta \Sigma X_{it}) \tag{2.3}$$

$$P(\Delta H_{i,t} = +1|RHWM_{it}, H_{it}, W_{it,}X_{it}) = P(\Delta H^*_{i,t+\tau} > a_2|RHWM_{it}, H_{it}, W_{it,}X_{it}) =$$
$$= 1 - \Phi(a_2 \breve{} \beta RHWM_{it} + \delta H_{it} + \lambda W_{it} + \theta \Sigma X_{it}) \tag{2.4}$$

In some specifications, our dependent variable is quantitative (e.g., changes in drug related rates, and changes in alcohol or suicide related rates) rather than qualitative. In such cases, we use a standard panel OLS specification.

4. Empirical Results

4.1 Impact of an unexpected wealth shock on health: Baseline results

This section shows the baseline results of our study. First, Table 2.2 presents the estimates of the effect of levels and changes in housing wealth on the change in several health outcomes.

Results shows that there is a positive relationship between housing wealth and health. However, establishing a causal link requires the identification of an unexpected housing wealth shock. In the reminder of this section, we address the causal link between housing wealth and health.

Table 2.3 presents the baseline estimates of the effect of an unexpected housing wealth shock (i.e., RHWM) on the change in several health outcomes using different control variables in various specifications. Columns [1] and [2] in panel A show the ordered probit estimates on changes in SRH. The first column controls only for the main effects, i.e., initial house value, year and division fixed-effects. In column 2, we add a broad set of demographics.[23] Specifications [3] and [4] present the results of a RHWM shock on the change of the number of limitations in ADLs as well as on the number of mental impairments. Finally, the rest of expressions present the effects of the unexpected shock in housing wealth on the change of deaths of despair measured as drug-related deaths rates and alcohol and suicide death rates.

In specifications [1] and [2] the coefficient for RHWM is positive and statistically significant, indicating that a positive housing wealth shock leads to a significant change in self-reported health. Panel B in Table 2.3 shows that the corresponding marginal effect of a positive shock in housing wealth (i.e., an increment in RHWM) on the probability of a health improvement is 0.0042. In other words, if households experienced a one standard deviation positive shock in housing wealth, their probability of improving their health in the next period increases by 1.00 percentage points (=0.0042*0.5597/0.2347, where 0.2347 is the average probability of an improvement in health for our sample). In addition, the marginal effect of positive shock in housing wealth on the probability of a decline in health is -0.00485. In other words, if households experienced a one standard deviation positive shock in housing wealth, their probability of declining health in the next period decreases by 1.06 percentage points(=-0.00485*0.5597/0.2552, where 0.2552 is the average probability of a decline in health for our sample). The magnitude of these results is consistent to the previous litera-

[23] Our results are robust to the use of interval regressions.

ture that explores the effects of wealth shocks on health outcomes (Fichera and Gathergood (2016), Meer et al. (2003), Lindahl (2005), McInerney et al. (2013)). In specifications from [3] to [8], negative coefficients indicate a health improvement (i.e., fewer limitations of ADLs or lower death rates). In column [3], the impact of the RHWM in total ADLs is not statistically significant from zero, but the effect of an unexpected positive shock in house wealth has a strong negative impact on the number of mental conditions (column [4]). One explanation for the fact that total ADLs is not significant while mental health are, might be that the effects on total ADLs take longer to materialize than mental conditions because they imply much more severe impairments. Previous literature has also found significant negative effect of worsening economic conditions on mental health (Ruhm (2005); Ruhm (2015); Dávalos and French (2011); Dávalos et al. (2012); Golberstein et al. (2016)). Panel B in Table 2.3 shows that the corresponding marginal effect of a positive shock in housing wealth (i.e., an increment in RHWM) on the probability of decreasing the number of mental ADLs (i.e., improving their health) is 0.0008. In other words, if households experienced a one standard deviation positive shock in housing wealth, their probability of improving their health in the next period increases by 1.10 percentage points (=0.0008*0.5597/0.0406, where 0.0406 is the average probability decreasing the number of mental health complications for our sample). In addition, the marginal effect of positive shock in housing wealth on the probability of a worsening mental health is -0.00203. In other words, if households experienced a one standard deviation positive shock in housing wealth, their probability of declining mental health in the next period decreases by 2.3 percentage points(=-0.00203*0.5597/0.0491, where 0.0491 is the average probability of a decline in health for our sample).

Columns from [5] to [8] present the effects of the RHMW on deaths of despair. In columns [5] and [7] we use the same control variables as in expressions [2], [3] and [4]. The coefficient for RHWM is negative and statistically significant, indicating that positive housing wealth shocks decrease death rates. Our results show that an unexpected shock in housing wealth is negatively correlated with drug-related deaths. However, we do not find any significant effect on alcohol and suicide related death rates. In particular, a one standard deviation change in housing wealth leads to a 4.3 percent decrease in the drug-related death rate (i.e., 0.041/0.961, where 0.961 is the average change in drug-related death rate.) The effects on alcohol and suicide related deaths are not statistically significant. Columns [6] and [8] include additional controls to take into account elements that the previous literature has suggested could play a role on the recent rise of deaths of despair. Following Ruhm (2018), we control for various economic factors related to labor market outcomes -such as employment status and the change in manufacturing jobs- and international trade shocks. We also control for changes in the drug environment such as the introduction of Prescription Drug Monitoring Programs (PDMP) or marijuana laws. The implementation of these last programs and laws only start presenting some variation across states later on the sample. Therefore, we lose some observations in these latest specifications. The results are robust to the inclusion of these additional controls. A one standard deviation increase in RHWM, reduces the change in drug-related death rates by 5 percent (i.e., from 0.961 to 0.911). Specifications [6] to [8] use rates per 100,000 inhabitants according to the US Census population. Appendix table B.3 presents the coefficients for all the covariates for the same specifications. Alternatively, the Surveillance, Epidemiology, and End Results (SEER) Program provides population data designed to adjust for population shifts such as those resulting from the hurricanes Katrina and Rita. In the Appendix, we show that our results are robust to the use of SEER population data instead of US Census data. In the Appendix (table B.4) we also show the results for a four-years effect of $RHWM$ on health outcomes. The coefficients are consistent to our baseline results but less significant for some specifications.

Finally, for a causal interpretation of the results, housing wealth shocks must be independent of any unobserved heterogeneity in health changes. One could be concerned about the fact that housing market shocks could be correlated with other macroeconomic environment shocks affecting wealth, such as stock market value changes or changes in the employment status. For this reason, we run a robustness check where we also control for the proportion of total wealth held in stocks and the proportion of total wealth in housing in addition to our standard control variables. Results are robust and are presented in the Appendix.

4.2 RHWM as an unexpected shock in housing wealth

There could be three types of concerns regarding the unexpectedness of the RHWM shock. The first type of concern refers to the possibility that people who move have different health status from those who do not sell. Maybe those who are sick decide to change houses to adapt to their healthcare needs. To address this concern we perform two analyses: first, we control for the health of the head of the household in all our specifications. Second, we present a two sample t-test of the health status –measured as self-reported health and as limitations in ADLs– of two groups of people, those who move and those who do not move. The goal of this test is to check that there is not a significant difference between the health status of heads of household who move prior to moving and the health status of those who do not move. Table 2.4 panel A presents the results and it shows that there are not significant differences in health status between the two groups.

The second question refers to the possibility that households that move and sell their house at a large discount are different for those that move and sell at a high price. For example, those who lose their jobs might be forced to move and sell their house at a large discount and, therefore, have a negative wealth shock. To address this issue, we have performed t-tests to study whether there are significant differences among socioeconomic and demographic characteristics between households that experience a positive or negative wealth shock when moving. We find that there are no such differences in variables that could trigger housing moves such as employment, number of family members, and marital status.[24]

The third type of concern refers to the fact that households that significantly overestimate their houses may not sell them because they are loss averse. This concern is already addressed

[24]The p-values for the t-tests on employment status, number of family members, and marital status are 0.28, 0.78, and 0.61.

in the type of data that we use because households included in PSID report what they believe is the value of their houses.[25] Nevertheless, we test whether households only realize their house wealth misestimation when they sell their house, in other words, whether RHWM is actually an unexpected shock.

The economic intuition behind this test goes as follows. If misestimation is truly something that homeowners only realize when they sell their house, then the effects of housing wealth misestimation (HWM) on health should not be significant prior to selling the house. This should hold for two groups of people: (i) those who never sell the house and (ii) those who decide to sell it before selling and realizing their misestimation. The first column of Table 2.4 panel B includes all households that never realized their house wealth misestimation. Therefore, we include all the observations related to households that never moved and the observations of households that moved up to the period before moving. Column [1] shows that there is no effect on SRH if the household does not realize its house wealth misestimation in any period before selling the house. We obtain the same result for the two subgroups: first, observations of households that never moved (column [2]) and observations of households that moved until the period before moving (column [3]). In summary, these results suggest that RHWM is an unexpected shock in housing wealth.

4.3 Instrumental variable results

There could be some unobserved variables that affect both health status and realized housing wealth misestimation. For example, when a family members dies, an individual might be more likely to move to a smaller house and might also feel more depressed. Or an individual with lower health status might have worse bargaining skills and, therefore, may sell her house at a lower price. To address reverse-causality concerns, all the analyses in the paper control

[25]Even if they do not sell, they would report a lower value of their house if they found that it was worth less because the question in PSID states "Could you tell me what the present value of this house (farm) is? I mean about what would it bring if you sold it today?"

for variables such as initial health, housing wealth, the number of family members in the house and employment status. Moreover, we run an extra analysis and we implement an IV strategy for robustness.

Our instrumental variable is the interaction between local supply elasticity in the housing market and the interest rates for the market yield on US Treasury securities at 10-year constant maturity. Therefore, our instrument is time-varying.[26] To our knowledge, this is the first time that this instrument is used in the health economics literature. The economic intuition behind this interaction goes as follows. When interest rates decrease, demand for housing increases. As markets can adjust prices and quantities, ceteris paribus, this increase in demand translates into higher real estate prices in areas where supply is more inelastic. As there is persistence in housing-wealth perceptions (Kuzmenko and Timmins (2011)), misestimations will be greater in more inelastic supply areas where house prices vary the most. We use the elasticity of supply of housing as estimated in Saiz (2010), who employs satellite-generated data on the slope of the terrain, and the presence of rivers, lakes, and other water bodies to estimate the amount of developable land at the MSA level. We use data on yields of US Treasury securities at 10-year constant maturity from the Federal Reserve website.[27] [28]

This instrument has been extensively used in the finance and real estate economics literature to address endogeneity issues related to real estate prices. Himmelberg et al. (2005) instrument local house prices using the interaction of local housing-supply elasticity and long-term interest rates to study housing bubbles. Mian and Sufi (2011) use the same instrument for house prices to analyze household leverage. Chaney et al. (2012) and Cvijanović (2014)

[26]Changes in the elasticity of supply at the MSA level are large in the cross-section but small in the time-series since we consider time lags of 2 years for changes in health outcomes in our panel. Recent studies that consider changes in the house price elasticity do not find relevant changes over short periods of time (e.g., Kirchhain et al. (2018)). Furthermore, there are no available time-varying measures of land elasticity at the MSA or city level that cover our period of study (1986-2015). For instance, Kirchhain et al. (2018) cover the time period 2014-2016.

[27]See http://www.federalreserve.gov/.

[28]When limiting the specification to only those who move, results are consistent since RHWM will only be different from zero for those households that move when they move.

use this instrument for commercial real estate prices in their study of firms' investments and leverage, respectively. However, this is the first time that the interaction between the local supply elasticity of individual housing markets and long-term interest rates is used as an instrumental variable for an unexpected shock in wealth.

This is a good instrument for our empirical strategy for two reasons. First, the IV is highly correlated with RHWM. In other words, this IV has a strong first stage. The results of the first-stage regression are presented in Table 2.5 Panel B. The instrument is strongly statistically significant and, as expected, has a negative sign. Second, both the amount of developable land and the interest rates are exogenous to changes in health status.[29]

Table 2.5 Panel A presents the estimates of the effect of a shock on wealth (i.e., RHWM) using the IV described above on the change in SRH, change on the number of mental impairments and change on drug and alcohol and suicide-related death rates. We also use different control variables in each specification. Panel C presents the estimated marginal effects on the change in SRH for specification [1] in Panel A.[30]

In specification [1] in Table 2.5 panel A, the coefficient for the instrumented RHWM is positive and statistically significant. This indicates that a positive wealth shock leads to a significant positive change in SRH. The corresponding marginal effect of a positive shock in housing wealth on the probability of a health improvement is 0.0057. In other words, if households experience a one standard deviation shock in their housing wealth, the probability of an improvement in their health in the next period increases by 1.36% (=0.0057*0.5597/0.2347, where 0.2347 is the average probability of an improvement in health

[29] Davidoff (2016) criticizes the use of housing-supply constraints as IVs for house prices in studies in which the dependent variable has an economic component, such as consumption growth, leverage, or investments, because some demand factors that could affect both house prices and the dependent variable of interest might have been omitted. This is not the case in our study, as the dependent variable is change in health status.

[30] We estimate this model using maximum likelihood. The estimation is performed using the CMP user-provided package in STATA. See https://ideas.repec.org/c/boc/bocode/s456882.html and Roodman (2009). This approach has been used extensively in the literature (e.g., Einav et al. (2012); Cullinan and Gillespie (2016)).

for our sample). In addition, the marginal effect of a positive shock in household wealth on the probability of a decline in health is -0.0065. Therefore, if households experience a one standard deviation shock in their housing wealth, the probability of a decline in their health in the next period decreases by 1.43% (=-0.0065*0.5597/0.2552, where 0.2552 is the average probability of a decline in health for our sample).

The measures reported in specifications from [2] to [4] in Table 2.5panel A correspond to a worsening of health conditions. Therefore, the coefficient for the instrumented RHWM is negative, as expected. The effect is only statistically significant for the case of a change in drug-related death rates (specification [3]). As before, a shock on RHWM has not a significant effect on alcohol and suicide related deaths (specification [4]). Moreover, we lose significance on its effect on the change in the number of mental health problems (specification [5]) when using an IV. Our results show that a one standard deviation increase in housing wealth leads to a 6.995 decrease in changes in drug-related death rates.

4.4 Differential effects across geographical areas

In this section we study how the effect of housing wealth on health varies across geographical areas. Figure 2.2 shows an exploratory analysis of the effect of a sharp growth (and decrease) of house prices in health outcomes related to "deaths of despair" for the U.S. MSAs with more than 100,000 inhabitants. The top (bottom) left figure shows the relationship between changes in house prices and changes in the drug-related (alcohol and suicide) death rates for the recent period of sharp increase in house prices 2003-2007. The top (and bottom) right figure exhibits the same figure for the recent period of sharp decrease in house prices 2007-2010. All figures show a negative relationship between growth in house prices and growth of death rates related to "deaths of despair".

The instrumental variable approach that we developed in the previous section implies that, ceteris paribus, the RHWM is, on average, larger in those areas where housing supply is constrained. Hence, an increase in demand should translate into a higher positive change in health in areas where housing supply is more inelastic. For instance, a demand shock experienced by households located in the most inelastic MSAs, such as Miami, Los Angeles-Long Beach, San Francisco, and New York, leads to a higher probability of a health improvement than a demand shock of the same magnitude experienced by those located in the top elastic MSAs, such as Cincinnati, Atlanta, San Antonio, and Oklahoma City.

We study the differential effects of unexpected housing wealth shocks on health across different geographies. We want to understand if the economic cycles have a differential effect across different geographical areas. To do so, we classify the households in our sample in the ones that live in a housing supply inelastic area and the ones that live in an elastic area. We define a dummy variable $Inelastic_{P33}$ that takes the value of 1 of the household lives in an area located in the top 33% of housing inelastic cities according to the measure in Saiz (2010) and zero otherwise.[31] We also separate the sample in periods of housing boom and periods of housing bust. Housing boom (bust) includes the years with growth in house prices at least one standard deviation above (below) their historical mean.

Table 2.6 reports the results of this analysis. All the specifications in this table control for demographic and economic characteristics. The coefficients for $Inelastic_{P33}$ are statistically significant for changes in SRH and changes in drug-related death rates. Table 2.6 shows that for the boom periods (i.e., when households are more likely to experience positive housing wealth shocks), the improvement on health outcomes is larger on MSAs with a more inelastic housing supply market. During recessions (i.e., when households are more likely to experience negative housing wealth shocks), health is likely to worsen more in these areas with inelastic housing supply.

[31] This choice of 33% divides our sample in about half, that is, 50% of the households in our sample live in the top 33% inelastic MSAs. Our results are robust to the choice of 33% as the threshold between elastic and inelastic cities. In the Appendix, we also report these results using a continuous measure of elasticity. These results are also robust, but less significant.

5. Conclusion

Several studies have documented the positive effect of changes in wealth on health. To analyze this causal relation, the extant literature has used either shocks in wealth that affect only a small part of the population (e.g., lottery winners) or shocks that can be expected, at least to some extant (e.g., an inheritance). In contrast, we develop a new measure of unexpected wealth shocks: realizations of housing wealth misestimations (RHWM). Our results show that a positive, unexpected shock in wealth increases the probability of an improvement in self-reported health, a decrease in the drug-related mortality rate and a reduction in mental health problems. The opposite effect also holds, such that a negative shock on wealth increases the probability of a decline in health. However, we find that unexpected shocks in wealth have no effect on alcohol- or suicide-related deaths.

Our results provide important policy implications to the set of initiatives provided by the President´s Commission on Combating Drug Addiction and the Opioid Crisis. If the economy is the main cause of this crisis, one should look for measures to stimulate worst-off communities. But, if the crisis is mostly drug supply-driven, then one should implement measures such as the promotion of opioid prescription guidelines, physicians' education, and a stricter control of illegal drug supply. However, we are probably facing a multidimensional challenge. In this paper, we show that there is an additional driver that should be taken into account: housing wealth. Our results also emphasize the different effects that booms and crisis can have in areas where the housing supply is more inelastic. Further efforts should be devoted to the study of housing-related policies, such as affordable housing plans, and their impact on health outcomes.

6. Figures and Tables

Table 2.1: Descriptive Statistics.

Panel A. Head of household level data

	Description	Mean	SE	Observations	Min	Max
Health outcomes						
Change in SRH	Measure of change in self-reported health (SRH) for a two year period. If SRH increases it takes value 1, if it decreases value -1, and if it remains the same, value 0.	-0.0204	0.700	93,807	-1	1
Change in total ADLs	Change in the total number of limitations of activities of daily living (ADLs).	0.8653	0.3658	165,861	-1	1
Change in mental ADLs	Change in the number of limitations of activities of daily living (ADLs) classified as mental.	0.0085	0.299	36,170	-1	1
Measure of wealth shock						
Realization of housing wealth misestimation	Shock in housing wealth when the household sells its current house. It is expresed in $10,000.	0.00431	0.560	153,112	-41.24	49.19
Health, wealth and sociodemographic controls						
Healthy	Health control variable. It takes value 1 if the individual's Self-Reported Health is good, very good or excellent. It takes value zero otherwise.	0.580	0.494	103,248	0	1
House value	Reported house value in hundreds of thousands of dollars.	1.364	1.403	51,514	1.00e-05	35
Family income	Total family income in hundreds of thousands of dollars.	0.467	0.600	105,765	-0.993	55
Age	Age of the head of the household in years.	41.18	13.13	106,157	16	100
Male	Gender of the head of the household. It takes the value of 1 if male and 0 if woman.	0.685	0.465	106,168	0	1
Non-white	Race of the head of the household. It takes value 1 if the individual is nonwhite and 0 if the individual is white.	0.456	0.498	105,989	0	1
High school	Indicator whether the head of the household completed high school.	0.775	0.418	115,324	0	1
Employed	Dummy variable equal 1 if the head of household is employed and 0 otherwise.	0.788	0.409	106,137	0	1
Married	Dummy variable that takes the value 1 if the head of the household is married and 0 if not.	0.506	0.500	123,760	0	1
Family members	Number of members in the household.	3.388	1.716	106,949	0	14
Year	Year of the data collection.	1,996	8.561	123,760	1,984	2,013
Division	US Census division of the household.	5.349	2.656	150,194	1	9
Housing supply elasticity and interest rates						
Supply elasticity (SE) of the house market	Housing supply elasticity as estimated in Saiz (2010).	1.658	0.904	84,915	0.600	5.450
10-year interest rate (IR)	Yield of the U.S. Treasury bond at the 10-year maturity.	6.567	2.457	123,760	2.350	12.46
SE*IR	Interaction between housing supply elasticity and the 10-year interest rates.	10.84	7.529	69,925	1.410	49.84

Table 2.1: Descriptive Statistics (cont.)

Panel B. County level data

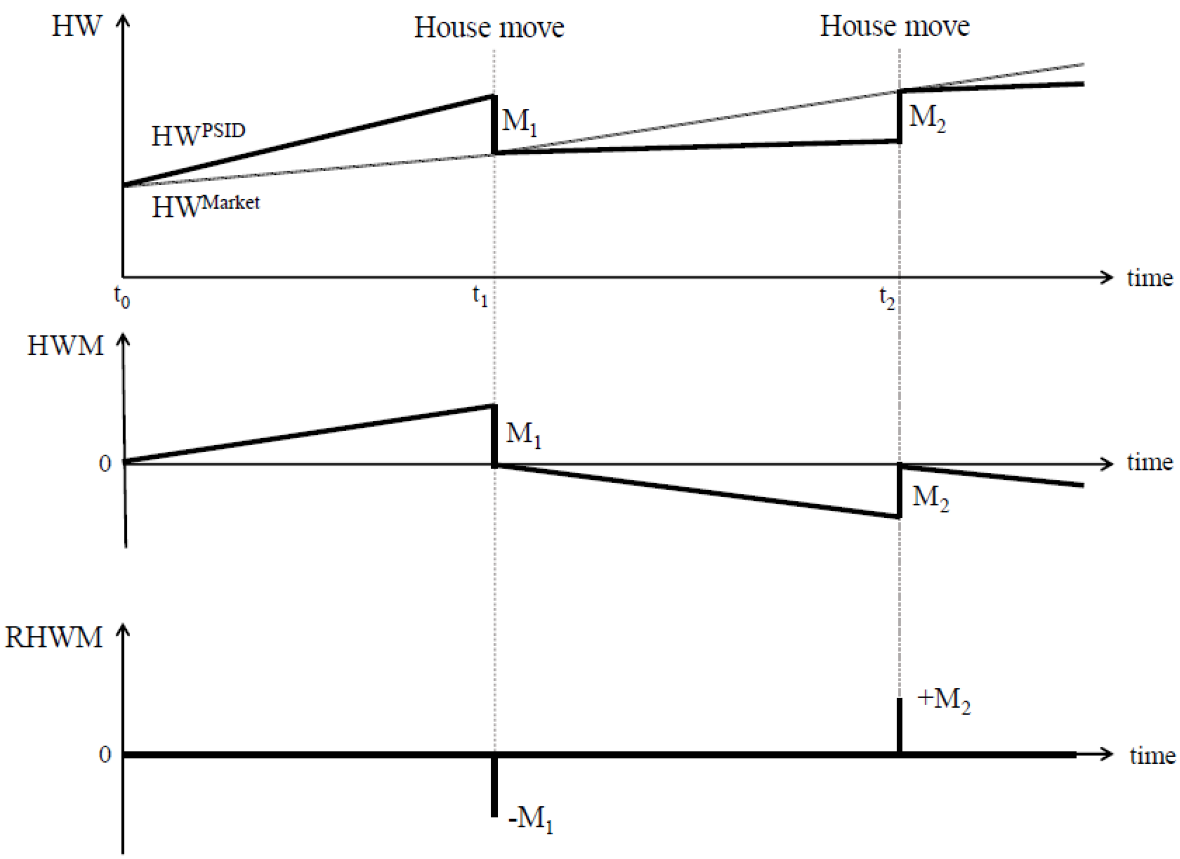
	Description	Mean	Std. Dev.	Observations	Min	Max
Health outcomes						
Change in drug-induced death rate	Change in deaths by unintentional drug poisoning per 100,000 people, as cataloged by ICD-10 codes X40 to X44.	0.9614	3.8169	20,591	-12.5298	18.7494
Change in alcohol and suicide-induced death rate	Change on alcohol and suicide-induced causes of death per 100,000 people, as cataloged by ICD-10 codes X45, X65, Y15 and others.	0.3419	2.1328	14.946	-8.8995	11.8846
Number of hospital beds	Number of hospital beds in the county.	2.767664	2.369005	20,852	8	23,094
Change in manufacturing employers	Change in percentage of manufacturing employers in working age population	-3.036349	3.251128	25,378	-23.4701	6.5501
Change in import exposure	Change in import exposure per worker	3.652997	3.584937	25,378	1.57e-06	49.0050
Legal environment controls						
PDMP operational	Takes the value of 1 from the first year in which Prescription Drug Monitoring Program (PDMP) was implemented onwards.	0.6094	0.4879	28,323	0	1
First marijuana law	Takes the value of 1 from the first year in which the marijuana law was implemented in its state onwards.	0.6749	0.4684	28,332	0	1

Table 2.2: Effects of change in house wealth on health outcomes.

	SRH		Δ(SRH)		Mental ADLs	Δ(Mental ADLS)	Drug death rates		Alcohol or suicide death rates	
	OLS	Ord. probit	OLS	Ord. probit	OLS	OLS	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Log House Wealth	0.04478***	0.1552***			-0.01093**		-0.7223***		-0.3107***	
	(7.8511)	(7.4532)			(-2.2175)		(-4.8444)		(-4.2954)	
Δ(Log House Wealth)			0.03052*	0.05160*		-0.02765***		0.02173		-0.1777
			(1.6425)	(1.6416)		(-2.9352)		(0.1328)		(-1.6172)
Healthy	1.4586***	16.280***	-0.07422***	-0.1248***	-0.03983***	3.188e-04	-0.002004	0.06402	-0.1164**	-0.1189*
	(202.41)	(134.25)	(-9.5247)	(-9.4453)	(-7.8108)	(0.07697)	(-0.02051)	(0.6940)	(-2.3155)	(-1.8596)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45,425	45,425	31,643	31,643	18,702	14,020	11,021	6,869	8,636	4,855

Notes: This table reports estimates of the effect of housing wealth in terms of Log(House Wealth) and Δ(Log(House Wealth)) on health outcomes. Specifications [1] and [2] show the effect of the logarithm of housing wealth on self-reported health. Column [3] and [4] report the effects of the change on the log of housing wealth on the change in SRH. In specifications [5] and [6], our dependent variable is mental ADLs and change in mental ADLs respectively. Specifications [7]-[8] and [9]-[10] show the effect of house wealth on drug death rates and alcohol death rates, respectively. t-statistics are reported in parentheses. All specifications include age, gender and socioeconomic controls, as well as division fixed effects and year fixed effects.

Figure 2.1: Sketch of the misperception mechanism.



Notes: This figure details the definition of $RHWM$ from the house wealth reported in PSID, HW^{PSID}, and the house wealth in market value, HW^{Market}. The figure on the top plots a sketch of a path for a household's reported housing wealth from PSID, HW^{PSID}, and a sketch of the path for the housing wealth in market value, HW^{Market}, of the same house. In this sketch, the household moves to a different house at times t_1 and t_2. In these specific times, the household realizes the market value of its house and, therefore, its housing wealth misestimation (e.g., M_1 and M_2 is the housing wealth misestimation at times t_1, and t_2, respectively). The plot in the middle exhibits the resulting path of house wealth misestimation, HWM, for the top figure. Notice that the household in this sketch is overvaluing its housing wealth from time t_0 to t_1 (i.e., its HW^{PSID} is above its HW^{Market}), hence HWM is positive during this period. At time t_1, the household realizes its overvaluation of size M_1 and experiences a negative housing wealth shock of size M_1. The household is undervaluing its housing wealth from time t_1 to t_2 (i.e., its HW^{PSID} is below its HW^{Market}), hence HWM is negative during this period. At time t_2, the household realizes its undervaluation of size M_2 and experiences a positive housing wealth shock of size M_1. The figure in the bottom plots realized housing wealth misestimation, $RHWM$, which takes always the value of zero, except at times t_1 and t_2 when it takes the values of $-M_1$ and M_2, respectively.

Table 2.3: Baseline. Effects of shocks in wealth on changes in health.

Panel A. Baseline regressions for the different health outcomes.

	Δ(SRH)		Δ(Total ADLs)	Δ(Mental ADLs)	Δ(Drug death rates)		Δ(Alcohol or suicide death rates)	
	Ord. probit	Ord. probit	Ord. probit	Ord. probit	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
RHWM	0.0102*	0.01720**	0.00048	-0.02450**	-0.07416***	-0.08943***	0.00666	-0.01110
	(1.81)	(2.39)	(0.06)	(-2.07)	(-2.69)	(-2.60)	(0.36)	(-0.47)
Healthy	-1.147***	-1.1741***	-0.2311***	-1.7867***	0.08683	0.07252	-0.1340**	-0.1538**
	(-6.67)	(-46.35)	(-9.89)	(-9.02)	(1.08)	(0.66)	(-2.43)	(-1.98)
House value		0.0477***	-0.0033	-0.0503***	0.0051	0.0137	-0.0222	-0.0078
		(7.41)	(-0.39)	(-5.14)	(0.22)	(0.46)	(-1.34)	(-0.34)
PDMP Operational						0.2712		0.3598***
						(1.62)		(2.72)
First marihuana law						1.1720***		-0.0071
						(6.17)		(-0.05)
Hospital beds rate						-0.0436		0.0591
						(-1.07)		(1.57)
Δ manufact. employers						0.0514		0.0143
						(1.06)		(0.39)
Δ import exposure						-0.0434		-0.0768**
						(-1.00)		(-2.47)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,506	35,427	12,069	15,294	8,879	4,979	6,300	3,535

Panel B. Marginal effects. Ordered probit specifications [2] and [4].

	[2]			[4]		
	Decrease	No change	Increase	Decrease	No change	Increase
RHWM	-0.00485**	0.00062**	0.00423**	0.00085**	0.00118**	-0.00203**
	(-1.90)	(-1.75)	(1.75)	(2.08)	(2.06)	(-2.08)

Notes: This table reports estimates of the effect of Realization of Housing Wealth Misestimation (RHWM) on the change in health outcomes. All specifications include age control, year fixed effects and division fixed effects. Specifications [1] and [2] show the estimates of an ordered probit model for self-reported health, Δ(SRH). Specification [1] only includes as control variables House value. Specification [2] adds health level (SRH), as well as all the demographic controls, which include family income, race (Non-white), education (High school or more), employment (Employed), marital status (Married), and Family members. Specifications [5] and [6] report the estimates for change in drug death rates. Specifications [7] and [8] report the estimates for the change in alcohol or suicide death rates. t-statistics are reported in parentheses. All the specifications include year and division fixed effects and all errors are clustered at the family level.

Table 2.4: RHWM as an unexpected shock in housing wealth.

Panel A. Two sample t-test of health status by movers and non-movers.

	SRH			Total ADLs		
	Moved	Did not move	t-test	Moved	Did not move	t-test
Age 25-40	0.6350	0.6095	5.3913***	0.1578	0.2038	-2.5376***
	(0.4814)	(0.4879)		(0.8492)	(0.9474)	
Age 41-55	0.5617	0.4604	13.8784***	0.2744	0.3673	-3.7124***
	(0.4962)	(0.4985)		(1.1068)	(1.2523)	
Age >55	0.4639	0.3734	6.4427***	0.5947	1.0671	-7.1878***
	(0.4987)	(0.4839)		(1.5890)	(2.4071)	

Panel B. Two sample t-test.

	Households that did not move during the previous period [1]	Households that never moved [2]	Households that had moved until the period before moving [3]
HWM	-0.05238	-0.00049	0.00037
	(-0.10)	(-0.52)	(0.63)
Healthy	-1.2549***	-1.4034***	-1.1634***
	(-20.36)	(-11.65)	(-16.27)
House Value	0.06359***	0.07754**	0.04900*
	(2.85)	(2.18)	(1.69)
Controls	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes
Observations	8,351	2,692	5,659

Notes: Panel A reports the results of two sample t-tests of the differences of health variables for households that moved and did not move. Health variables are self-reported health (SRH) and Total ADLS. It displays the mean of the health outcomes for movers and non-movers, as well as the t-test for different age ranges. Standard deviations are reported in parentheses. Panel B reports the estimates of the determinants of house wealth misestimation, HWM, and moving, using in all the cases ordered probit regressions. All specifications include age and gender controls, socieconomic controls, year fixed effects and division fixed effects. In model [1], we only take into account individuals who did not move during the previous two-year period. Model [2] takes into account individuals who never moved, and model [3] individuals who sometime moved but not during the previous period. Errors are clustered at the family level in all the specifications.

Table 2.5: Effects of shocks in wealth on changes in health. Instrumental variables.

Panel A. Second stage regressions.

	Δ(SRH)	Δ(Mental ADLs)	Δ(Drug death rates)	Δ(Alcohol or suicide death rates)
	Ord. probit	Ord. probit	OLS	OLS
	[1]	[2]	[3]	[4]
RHWM	0.02165*	0.00892	-6.99570***	-0.07796
	(1.76)	(0.67)	(-4.24)	(-0.17)
Controls	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes
Observations	38,256	29,991	7,597	5,627

Panel B. First stage regressions.

	RHWM	RHWM	RHWM	RHWM
	[1]	[2]	[3]	[4]
SE*IR	-0.00304***	-0.00303***	-0.01594***	-0.01252***
	(-2.98)	(-2.97)	(-4.45)	(-3.08)
Controls	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes
Observations	38,256	29,991	7,597	5,627

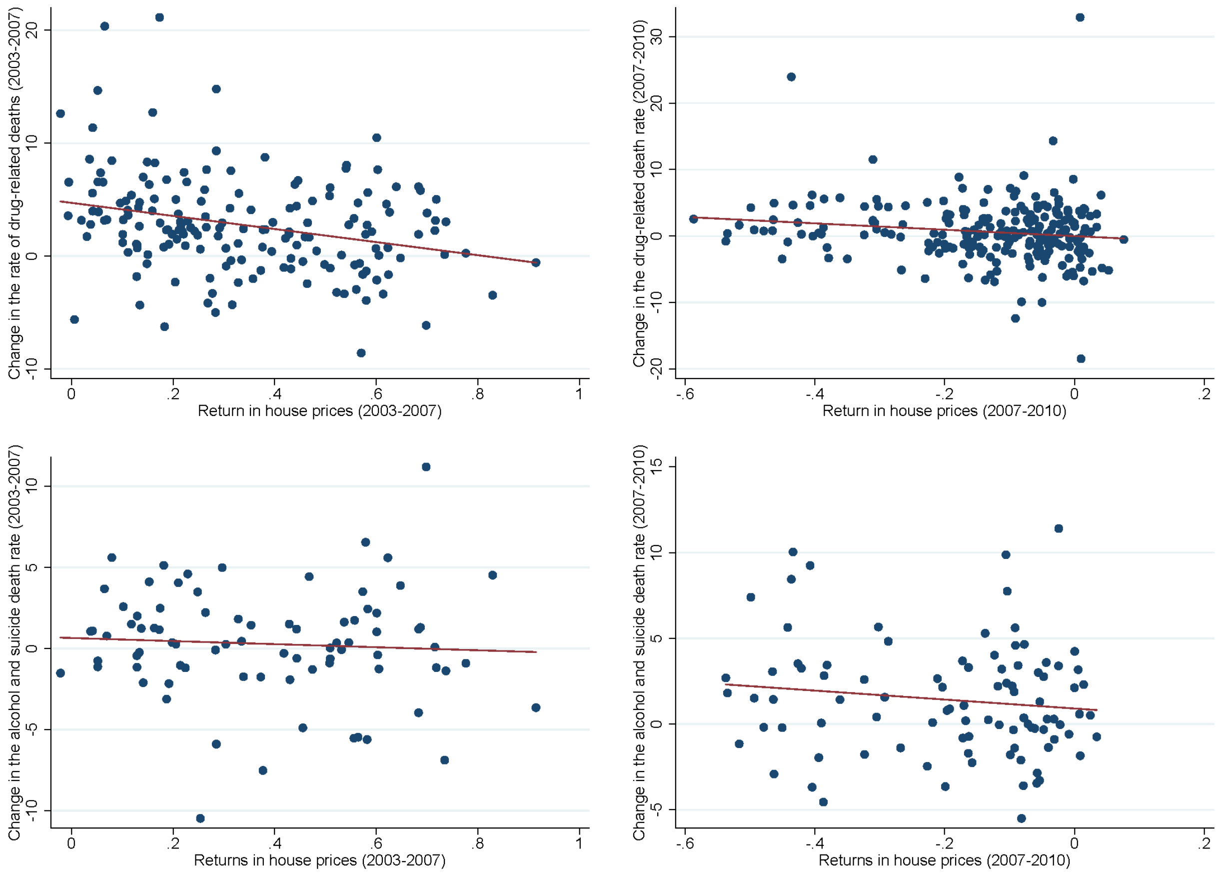
Note: Columns [1]-[4] correspond to the first stages for Panel A specifications [1]-[4] respectively.

Panel C. Marginal effects. Ordered probit IV specification [1] in Panels A and B.

	Decrease	No change	Increase
RHWM	-0.00648*	0.00079	0.00568*
	(-1.75)	(1.32)	(1.75)

Notes: This table reports the ordered probit estimates of the second stage (Panel A) and the first stage (Panel B) of the instrumental variable model. Panel C exhibits the marginal effects of the ordered probit IV specification in column [1]. The dependent variable is the change in SRH (column [1]), change in mental ADLs (column [2]), change in drug death rates (column [3]), and change in alcohol or suicide death rates (column [4]). The variable RHWM has been instrumented by the interaction between housing supply elasticity (SE) and the interest rate at 10 years (IR). Errors are clustered at the family level in all the specifications.

Figure 2.2: Changes in house prices and changes in health outcomes.



Notes:The top two figures report the effect of returns in house prices in the change in the rate of drug-related deaths. The bottom two figures report the effect of returns in house prices in the change in the alcohol and suicide of drug-related deaths. Every dot represents an MSA with more than 100,000 inhabitants. The two figures in the left show the effects for the recent period of sharp increase in house prices 2003-2007. The two figures in the right exhibit the effect for the recent period of sharp decrease in house prices 2007-2010.

Table 2.6: Effects of housing supply constraints and the housing market cycles.

	Increasing house prices			Decreasing house prices		
	Δ(SRH)	Δ(Mental ADLs)	Δ(Drug death rates)	Δ(SRH)	Δ(Mental ADLs)	Δ(Drug death rates)
	Ord. probit	Ord. probit	OLS	Ord. probit	Ord. probit	OLS
	[1]	[2]	[3]	[4]	[5]	[6]
Inelastic_{P33}	0.1567**	-0.0558	-1.8050***	-0.0844*	0.0197	0.4940***
	(2.02)	(-0.53)	(-9.83)	(-1.73)	(0.27)	(3.36)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,252	1,193	2,148	2,763	2,631	2,349

Notes: This table reports the effects of housing supply constraints during periods of sharp increasing house prices (booms) and periods of sharp decreasing house prices (busts). Errors are clustered at the family level in all the specifications.

Chapter 3

Has the College Premium really Flattened?

1. Introduction

The statistics commonly known as the college wage premium has received great attention in the past fifty years.[1] Its charm comes from highlighting movements in wage inequality (Freeman, 1976; Katz and Murphy, 1992; Card and Lemieux, 2001; Goldin and Katz, 2008; Acemoglu and Autor, 2011) and for serving as reference for public and private investments in higher education (Rodriguez et al., 2016; Bank, 2019). While the college premium has increased in developed countries, it has flattened or even decreased in many Latin American countries during the last 20 years (Fernandez and Messina, 2018). At the same time, there was a great expansion in the number of college graduates and college institutions in the region. Although it is usual to interpret the effects of such expansion on the college wage premium through the lenses of market equilibrium, the characteristics of workers with a college degree have considerably changed over time and possibly affected this measure.

[1]The college premium is the ratio between the average wages of all workers with a college degree and all workers with a high school degree.

In this paper, we study whether a reduction in the average quality of college institutions and students is responsible for the flattening/decrease in the college premium in Brazil.[2] First, we show that students from older and more established institutions perform better in the end-of-degree standardized exams, which suggests that these are better schools. Second, we show that the college premium has increased when we hold constant this set of universities. Combining these two facts, we infer that a composition change is driving the flattening of the college premium. There are more workers with a college degree but lower quality degrees, which reflects lower average wages.

To perform this analysis, we create a long panel with college data matched with labor market data. First, we constructed novel data of one million students that graduated between 1990 and 2020 in 42 Brazilian universities.[3] The data were constructed from FOIL requests (Freedom of Information Law) and include information on students' names, their institution, major, and year of graduation. To get information on students' wages, age, and other characteristics, we use the Brazilian employer-employee matched dataset (RAIS). We match both datasets by name from 2007 to 2018. We use a machine learning approach to decide which is the best match for student-workers with multiple matches. As a result, we have annual wage data on workers of different ages and different cohorts that graduated from a constant set of universities.

We first document an increase in the supply of college workers over the last decades. The number of people graduating from any college institution increased by three times between 2000 and 2018, from 400 thousand to 1.2 million people. The stock of college workers in the formal labor market increased as well, from 4 million to 12 million in the same period. These trends are not specific to Brazil. In fact, college enrollment in upper-middle-income countries—a group of 54 countries as defined by the World Bank—has increased by a similar magnitude during the same period as Brazil.[4]

[2]This is also referred to as the degraded tertiary hypothesis (Camacho et al., 2017)
[3]The universities in our sample are public and are representative of the best universities in the country.
[4]UNESCO Institute for Statistics (uis.unesco.org). School enrollment, tertiary (% gross) - Brazil, Upper

Second, we present evidence that this growth in enrollment came from new and possibly worse-quality college institutions. For example, 80% of the students that graduated in 2018 were in a major founded after the year 2000. We show that newer universities and majors are often lower ranked, and their students perform worse in standardized exams. Furthermore, an increasing share of students graduates from distance education (INEP, 2020), which could presumably be of lower quality. Institution quality is not the only thing that changed in the period: new cohorts of students have a lower socio-economic background when compared to older cohorts (ANDIFES, 2019). We interpret this as preliminary evidence that the skill level of the median college graduate decreased in the last two decades.

The increase in labor supply and the changes in the median quality of workers can both play a role in the evolution of the college premium—the challenge is to decompose these effects. We show that the college premium was flat between 1998 and 2005 and decreased after that.[5] Given an increase in the labor supply of college workers, there are two opposing explanations for the flattening and decline in the college premium trends: (i) labor demand for college workers remained fixed, and the decline in returns is defined by a movement along the labor demand; or (ii) skill-biased technological change increases the labor demand for college workers for any skill level, increasing returns to skill. However, the reduction in the median skill level of college workers has a stronger effect, reducing the college premium measure. Which explanation is more appropriate is an empirical question.

We identify the change in returns to skills by fixing one of the quality components: universities and majors. For this analysis, we use the RAIS dataset matched to our sample of college graduates. We observe each worker's labor market outcome for multiple years, which allows us to separately identify the year of graduation effects (cohorts) from year effects. Furthermore, workers graduate at different ages, such that we are not constrained by the age-cohort-year identification problem. Under the assumption that the average quality

middle income. Data as of June 2022, accessed through https://data.worldbank.org/.

[5]We use the nationally representative household survey (PNAD). Similar trends are found using the universe of formal workers in Brazil (RAIS).

of universities in our sample is stable, we identify the change in returns to skill for high-skill workers.

Our main results show that the college premium restricted to workers from a set of high-quality universities has increased. We find that the college wage premium increased by 23% between 2007 and 2018—an average increase of 2% per year. When we consider the full sample of college workers—i.e., including changes in composition—we find that the college premium decreased by 12%.

A story in which all universities—including lower ranked institutions—have increasing college premium trends that aggregate into a decreasing overall college premium trend is consistent with our results. This conclusion depends on three facts: (i) The share of students graduating from lower ranked universities increased over time; (ii) At any given year, students that graduated from higher ranked universities earn higher wages; and (iii) All universities have an increasing college premium trend. We have already mentioned that (i) holds in our data. We present evidence that (ii) holds by documenting a positive correlation between earnings and university ranking. For example, students from the top 10 universities receive 20% higher wages than students from universities ranked around the 100th place. Additional data is required to prove that (iii) holds, which should be the object of future research.

This paper contributes to a few strands of the literature. We are the first to our knowledge to show that changes in the quality composition of higher education institutions are responsible for the trends in the college premium in Brazil. Although this hypothesis has been brought up by several studies, the literature had not reached a consensus—probably due to data limitations.[6] Rodriguez et al. (2016) and Camacho et al. (2017) show that returns to college are heterogeneous in Chile and Colombia, respectively, with recently created programs having lower returns.[7] However, due to data limitations, they cannot explain the

[6] This hypothesis is sometimes referred to as "degraded tertiary hypothesis". See Messina and Silva (2017) for a review on the topic.

[7] Camacho et al. (2017) argue that the effects come from self-selection.

changes in the college premium trends. We contribute to the discussion by presenting evidence that uses a long panel with matched information on workers' wages and universities for different cohorts of workers.

Our results are also relevant to the growing literature that looks at the causes of the decline in earnings inequality in Latin America. Barros et al. (2010) argue that half of the decline in inequality was caused by an acceleration of educational progress. Ferreira et al. (2017), Alvarez et al. (2018) and Fernandez and Messina (2018) disagree with the latter statement and argue the decrease in earnings inequality is due to a compression of returns to firm and worker characteristics, such as experience and education We argue that decompositions of certain measures of inequality mistakenly attribute decreasing returns to education to a group that had increasing returns, possibly biasing the results.[8]

The results in this paper have strong implications regarding the public decision to invest in higher education. Previous studies found that the labor demand for college workers is relatively inelastic and that firms can easily substitute skilled for non-skilled workers (Katz and Murphy, 1992; Acemoglu, 2002; Ciccone and Peri, 2005;Haanwinckel, 2020). This implies that public investments that increase the supply of workers have the unintended effect of reducing relative wages for all workers with college. Our results show that returns to high quality education have continued to increase, consistent with skill-biased technological change and/or a more elastic labor demand. As a consequence, investments in higher education had increasing returns in the past, a trend that may continue in the future.

The chapter proceeds as follows. Section 2. presents the data sources and shows that there was a decrease in the college premium measure during the last two decades. In section 3., we present preliminary evidence that the skill level of the median college graduate decreased over time. Section 4. presents the decomposition strategy and the main results of the paper.

[8]Common measures of inequality are the ratio of log earnings of the 90th and 50th or 10th percentile in the wage distribution.

Section 5. discusses the adjusted college premium trends. 6. concludes.

2. Data Sources and Descriptive Statistics

We gathered information on college graduates from a selected sample of universities and match them to the Brazilian linked employer-employee dataset in order to compute the college premium.[9]

A. Linked Employer-Employee dataset (RAIS) and Household Survey (PNAD)

In our main analysis, we use earnings and schooling data from the *Relação Anual de Informações Sociais* (RAIS) from 2007 to 2018. RAIS is an administrative dataset that covers the universe of formal employees and firms in the private and public sectors, with the exception of domestic service workers. The data is administered by the Ministry of Labor and has restricted access. RAIS has information on individuals (CPF, full name, age, gender, race, schooling), on firms (CNPJ and sector), on establishments (county, zip code, and name), and on the employer-employee match (wages, occupation, tenure, dates of hiring and firing/separation).

To circumvent the fact that RAIS is not representative of the entire Brazilian population, we also use data from the *Pesquisa Nacional por Amostra de Domicílios* (PNAD). PNAD is the Brazilian household survey (PNAD), which was collected annually between 1970 and 2015. The survey was later replaced by PNAD-Continua, which is collected quarterly and provides a better representation of the Brazilian population. PNAD-Continua is available between 2012 and 2021.

B. College Graduates Sample

[9]College premiums are usually computed using household surveys, which are representative at the national level. However, such surveys do not include information on worker's schools.

We gathered data on college graduates from public universities in Brazil through FOIL requests (Freedom of Information Law). For all universities, the data includes full name, university, major, year of admission, and year of graduation. Some universities provided more information, such as national identification number, gender, etc. Appendix Table C.1 lists the sample of universities that agreed with providing the basic information. We have information on 1.2 million students that graduated from 42 federal/state universities between 1990 and 2020.[10]

Below, we describe the procedure to match the college graduates' sample with the employer-employee dataset (RAIS) and clean the data. We first match the college graduate sample with the RAIS using student/worker's full names. This procedure leads to multiple matches. Secondly, we use a machine learning algorithm to select the best match. Third, we impose some sample restrictions.

The raw college graduates sample includes information of 1,217,440 students that graduated from 42 universities. After removing special characters, we are able to match 74% of these students to one or more workers with the exact same full name in the RAIS dataset. As a result, 2,093,069 workers are matched to 906,420 students. Out of these students, 78% are matched to a single worker and 22% are matched to at most 20 workers with the exact same name. We drop students with very common names that are matched to more than 20 workers.

Among students with multiple matches, we select the best match using a machine learning procedure. One university provided us with students' identification number such that, for this university, we can match students and workers by both name and identification number. We proceed by matching students by name, and, for a training sample, we estimate a model that uses student and worker's characteristics to predict whether the match is correct—as defined by the match using the identification number.[11]

[10] The variables and the number of cohorts available vary across universities.

[11] We use the following characteristics that are specific to the student-worker match: five indicators for

We estimate two different models—a logit regression, and a random forest model—using the training sample. Using the model's estimates, we calculate a score for each match. We define the correct match based on 3 rules: (i) The match has the highest score of all matches; (ii) The score of the match is sufficiently large (greater than 5%); and (iii) The score of the top match is sufficiently large relative to the second-best match (the ratio of scores is greater than 1.1).

Appendix Table C.3 compares the results of each model using two metrics: the positive predictive value (PPV or accuracy) and the true positive rate (TPR or efficiency). While the random forest model has better PPV and TPR rates in the training sample (96.6% and 97.2%, respectively), these numbers do not produce better results in the test sample. Therefore, we decide to use the logit approach due to the consistency of training sample and test sample metrics (PPV of 87.1% and 87.2%). Appendix Table C.4 presents the sample sizes used for the in sample PPV and TPR calculations.

The machine learning algorithm finds a match for 806,893 students/workers. We further restrict the sample to students that graduated between 2000 and 2017. The final dataset includes 17,455,296 employment observations from 545,478 students/workers, that graduated from 42 universities. In the rest of the paper, we refer to students in this sample as the "college graduates' sample" and the universities as "sample universities". All the other universities in Brazil are referred to as "out-of-sample universities".

C. Higher Education Census and University Rankings

We use four additional data to complement the analysis. First, we use the Brazilian census of higher education to document the growth in enrollment by type of institution. The census

age at college admission (< 17, > 20, > 25, > 35, > 45 years old) where age is determined by the worker variable; difference between worker's earliest year on the data with year of college admission; an indicator for whether the individual had a full time job during college; an indicator for whether the schooling variable at RAIS reports an educational achievement inferior to college after the year of graduation; the number of worker observations at RAIS; and indicators for the maximum schooling from all worker's observations.

is available for every year between 1995 and 2018 and covers all universities and majors in Brazil.[12] It includes information such as total enrollment, number of graduates, and each major's date of foundation. In 1995, there were 884 higher education institutions and around 7,000 majors in Brazil. There was strong growth in the sector, such that in 2019 there were 2,608 higher education institutions and more than 40,000 majors.

Secondly, we use national examination data from Exame Nacional de Desempenho dos Estudantes (ENADE), and two rankings of higher institutions that are published online (RUF, from Folha de Sao Paulo newspaper; and Web ranking) to rank universities. Using ENADE's data from 2014, we create a university score by aggregating scores from all students and majors from each university. In Section 3, we combine these data to describe the changes in the composition of college graduates over time in terms of school age and ranking.

Using this data, we show that sample universities are older and better ranked. Table 3.1 presents the age distribution of sample universities and out-of-sample universities. The data comes from the RUF ranking and is limited to 194 universities. The table shows that most universities in our sample were founded more than 50 years ago (59.5%). In comparison, only 37.7% of out-of-sample universities were founded more than 50 years ago.[13]

Table 3.2 presents the RUF score and ranking, and the web ranking for the two samples. Universities in our sample have better scores and as a consequence are better ranked, according to the RUF ranking. The Web ranking includes more universities (1,285) and shows an even larger discrepancy between the sample universities and the out-of-sample universities. The median ranking in the sample is 37, and 663 out-of-sample. In summary, our sample includes many of the best and oldest universities in the country.

D. Age-adjusted College Premium

[12] University identification numbers are only matched across years after the year 2000.

[13] This number is probably overestimated since the RUF ranking only includes 194 universities out of 2000 higher education institutions.

We define age-adjusted college premium as the weighted ratio of earnings for workers with a college degree and workers with a high school degree. Similar to Fernandez and Messina (2018), premiums are constructed using a fixed-weight average of every age subgroup, for workers of ages between 21 and 65 years old. The weights are equal to the mean employment share of each subgroup across all years. We present the weighted average by aggregating all groups.

Figure 3.1 presents the evolution of the college premium over time using the nationally representative household survey (PNAD, in Panel A) and the employer-employee matched dataset (RAIS, in Panel B). Panel A shows that, on average, college workers' wages were 123% higher than the wages of workers with only a high school degree (2.23 times higher). The college premium increased by around 20p.p. between 1997 and 2004, a 3p.p. annual growth rate. Between 2004 and 2015, the college premium decreased by 19p.p., or -2p.p. a year. Fernandez and Messina (2018), describe a similar picture for Brazil and other countries in Latin America.

Panel B of Figure 3.1 shows that in 1995, conditional on having formal employment, workers with a college degree earned 86% higher wages than workers with only a high school degree. There was a strong increase in the college premium between 1994 and 2002 when the difference in wages was 137%. This represents a 6p.p. annual growth. The college premium has a more moderate growth between 2002 and 2012 when it reaches the peak of a 150% difference in average wages (1p.p. per year). Finally, the college premium decreases to a 129% difference between 2012 and 2019 (-3p.p. per year).

In summary, the age-adjusted college premium had a ceiling during the last two decades. This is described both by the nationally representative household survey and by the census of formal employees, even though they have very distinct samples.

E. Changes in labor supply

During the same period, there was a large increase in the supply of college workers and in the supply of high school workers. Figure 3.2 shows that the number of formal workers with a college degree has substantially increased between 1995 and 2019 (RAIS). There were 3 million formal workers with a college degree in 1995 and 12 million in 2020. Using the census of higher education, we show that the number of people graduating from any college institution increased from 400 thousand to 1.2 million people per year. These trends are not specific to Brazil. In fact, college enrollment in upper-middle-income countries—a group of 54 countries as defined by the World Bank—has increased by a similar magnitude during the same period as in Brazil.

The number of workers with a high school degree has also increased but the quality of primary and secondary education remained low. Appendix Figure C.1 shows that there were 5 million formal job relations with positive wages in which workers had a high school degree in 1995. In 2019, there were almost 30 million. This is a consequence of public policies such as compulsory school laws and also the increase in formal employment. However, public investments in education did not target the quality of education. In 2000, Brazil was ranked 30th out of 30 countries in the Programme for International Student Assessment (PISA). In 2018, Brazil was ranked 65th of 78 countries, with little evolution in scores.

We disagree with the standard economic analysis that uses these trends in college premium and labor supply to make inferences regarding returns to skill. Previous studies have associated a decreasing college premium with decreasing returns to skill. In this context, the labor supply curve for skilled workers shifted to the right while the labor demand curve remained fixed, resulting in decreasing returns to skill. In contrast, we argue that there were profound transformations in the higher education sector in Brazil. These transformations led to changes in the quality composition of college workers, such that trends in college premium cannot be interpreted as trends in returns to skills. In the rest of the paper, we make this point by comparing the college premium trends presented in panel B of Figure 3.1 with measures that account for changes in university composition.

3. Preliminary Evidence of Changes in Graduates' Skill Composition

We argue that the average skill of college workers decreased in the last 20 years. This was not driven by a reduction in each degree quality, but by an increase in the number of graduates with lower-quality degrees. This has been referred to as the Degraded Tertiary Hypothesis (Camacho et al., 2017).

Two federal legislations had an important impact on Brazil's education sector. Law number 9394, from 1996, authorizes and promotes distance learning. By 2019, 20% of students graduated with distance learning degrees. Executive order 2207, from 1997, allowed private higher education institutions to establish as for-profit institutions. Previous to that, private higher education institutions were non-for-profit only. Appendix A describes other programs and legislations that affected the higher education sector in Brazil.

As a consequence, most of the growth in the supply of college workers is due to the introduction of new majors and institutions. Using annual data from the higher education census, Figure 3.3 shows that the number of students that graduated from a bachelor's program increased from around 400 thousand people in 2000 to 1.2 million people in 2018. Yet, the number of graduates from majors that were created before the year 2000 is steady over time and possibly decreasing. The growth in the number of graduates comes from majors founded in the last two decades and not from increases in class size from majors founded before the 2000's.[14]

[14]Appendix Figure C.2 shows similar trends categorized by the age of the oldest major in the higher education institution. The figure shows that half the growth in the number of college graduates comes from institutions that were founded between 1990 and 2000 and after the year 2000.

New institutions are lower ranked, and their students perform worse on national exams. Figure 3.5 presents a nonparametric regression and shows that ENADE scores and University age are negative correlated. We also regress the average ENADE score and the RUF ranking on a categorical variable of institutions' age, as defined by the university's first major foundation year. Table 3.3 shows that students from universities founded between 1980 and 2000 perform worse in the overall ENADE exam (-6.1 points or -1.1 standard deviations), in the specific knowledge exam (-6 points), and in the general knowledge exam (-6.5 points) when compared to students from universities founded before 1940. We also use the RUF ranking and scores, which have a smaller sample of universities. Columns 4 and 5 show a similar negative relation between university ranking and age.

In summary, older universities are better ranked and they used to represent a higher share of graduates. Figure 4 makes this point by showing that the share of graduates from Top 25 universities according to ENADE ranking has substantially decreased.

4. College Premium Decomposition

We decompose the college premium into two samples: (A) the college graduates' sample from FOIL requests matched to RAIS, and (B) all other workers in the RAIS dataset with a college degree. Estimates from both samples are relative to wages of all workers with a high school degree in the RAIS data. We include all workers in the RAIS data between ages 21 and 65 that worked 40 hours a week, were employed on December 31st , had positive wages, and had either complete high school or college education. The resulting sample includes 263 million worker-wage observations.

To compute and adjust the college premium, we regress log wages of individual i and year t, on a set of fixed effects. There are a few reasons for adjusting the college premium. First, like discussed in Section 2., demographic changes in the age composition of the labor force could affect the college premium measure since older workers tend to receive higher wages. Secondly, in Sample A, the number of observations by cohort of graduation varies over time. For example, in 2007 we have observations from cohorts that graduated between 2000 and 2007, but in 2018 we include individuals that graduated between 2000 and 2018. Older cohorts tend to receive higher wages, even after controlling for age, which would affect the college premium. For last, even though Sample A includes observations from a fixed number of universities, the composition of majors and universities—which have different returns—changed over time. For all these reasons, when computing the college premium, we control for age, cohort of graduation, university and major, as shown by the equation below:

$$ln(wage)_{it} = \delta_t + \delta_{ts} + \alpha_{ae} + \psi_c + \eta_{um} + \varepsilon_{it} \tag{3.1}$$

where δ_t are year fixed effects, δ_{ts} are year by sample fixed effects, $s \in \{A,\ B\}$, α_{ae} are age by schooling fixed effects; ψ_c are cohort of graduation fixed effect; η_{um} are university by major fixed effects, and ε_{it} is a residual component.[15] We set ψ_c and η_{um} to zero for high school workers and for college workers whom we do not have university data. We omit year fixed effects for the first year in the data due to collinearity. Therefore, all estimates are relative to the 2007 college premium. We are interested in the estimates of δ_{ts}, which represent the changes in log wages of each sample relative to the wages of high school workers, taking the year of 2007 as the benchmark.

5. The Adjusted College Premium Trends

The unconditional college premium has a decreasing pattern. Figure 3.6 presents the estimates of δ_{ts} from Equation 3.1. The dashed line represents the college premium for Sample B, i.e., all workers in the RAIS dataset with a college degree, excluding workers from Sample

[15] We can separately identify age, cohort, and year effects because we observe workers of different ages in the same cohort of graduation and over time. I.e., age, cohort of graduation, and year do not form a colinear relation.

A. The figure shows that the college premium remained constant between 2007 and 2011 and decreased by 14.5% between 2011 and 2018—a 1.8% annual decrease.

However, when fixing the sample to students that graduated from the same group of universities, the college premium has actually increased. The black line in Figure 3.6 presents the college premium for Sample A, i.e., the college graduate's sample from FOIL requests. The figure shows that the college premium increased by 19% between 2007 and 2011—a 4% annual increase. The college premium increases at a slower pace between 2011 and 2018, by 5% in total or 0.6% annually.

In theory, all universities could have an increasing college premium that aggregates into a decreasing overall college premium. Table 3.4 presents the correlation between the university fixed effect from Equation 3.1 with university rankings (where #1 is the best). The table shows that a move in ten positions in the university ranking is associated with a wage difference of 2%. For example, students from universities ranked in the 1st place receive 20% higher wages than students from universities ranked in 100th place. The point is that all these universities could have increasing trends in the college premium but at different levels. However, the number of students graduating from lower ranked universities (and lower fixed effect) has increased. As a result, the overall college premium gives stronger weight to workers from lower ranked universities by the end of the period, showing a decreasing trend.

6. Conclusion

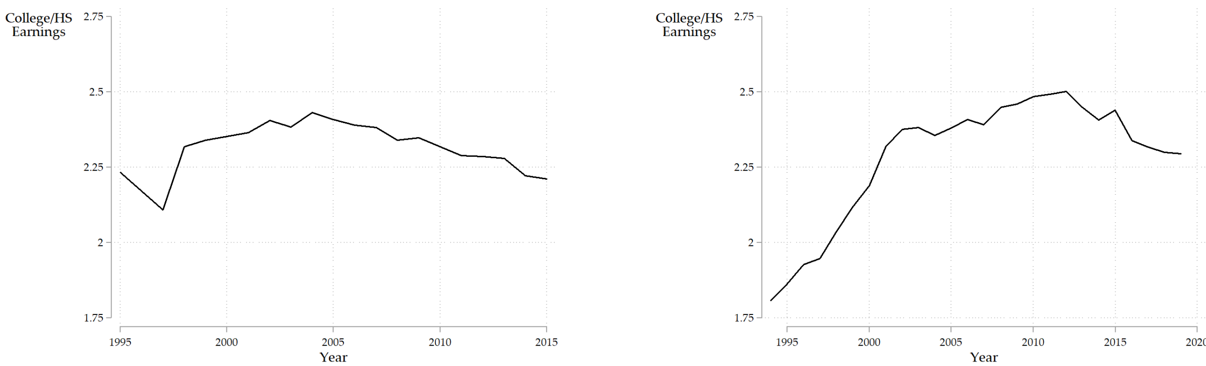
We presented evidence that the college premium in Brazil has increased, opposing previous results in the literature. The difference in results comes from the construction of a new dataset that identifies worker's university. We find that the college wage premium increased by 23% between 2007 and 2018 when holding constant the set of universities for which we have data and decreases by 12% in the overall sample. In addition, we showed that the supply of workers with college degree has significantly increased, but much of this increase came from newer, lower ranked and lower wage-premium universities.

The results are relevant for the estimation of labor demand elasticities and to calculate the importance of skill biased technological change. Future research should try to include measures of skill in such models in order to account for changes in skill composition of workers in the same schooling group. The results also inform individuals and policymakers regarding the decision to invest in higher education. That said, future research should focus on verifying if these trends are similar for all universities in Brazil and in other countries.

7. Figures and Tables

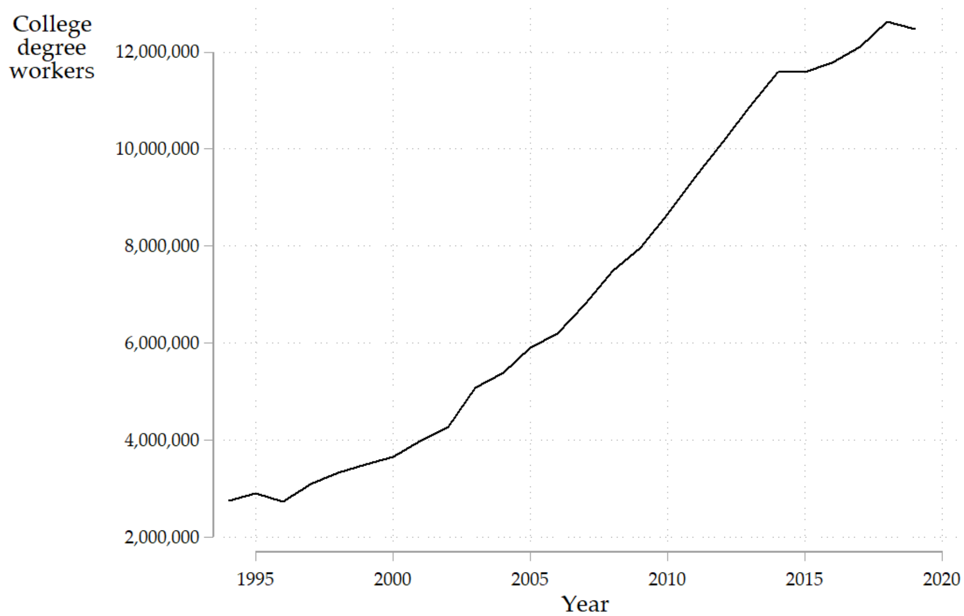
Figure 3.1: College/High School wage premium

(a) PNAD **(b)** RAIS



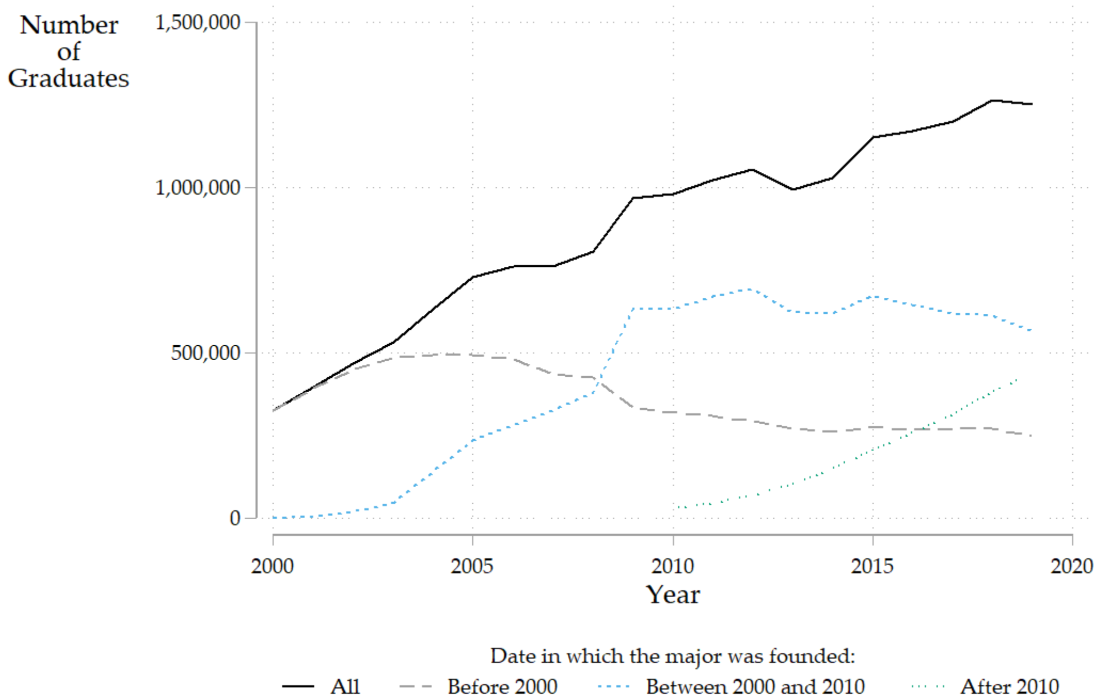
Note: The graph plots the ratio between wages of workers with college degree and workers with high school degree adjusted for age composition. The sample is restricted to workers between 21 and 65 years old. Panel A uses the Brazilian household survey (PNAD). Panel B uses the Brazilian matched employer-employee data (RAIS).

Figure 3.2: Formal workers with a college degree



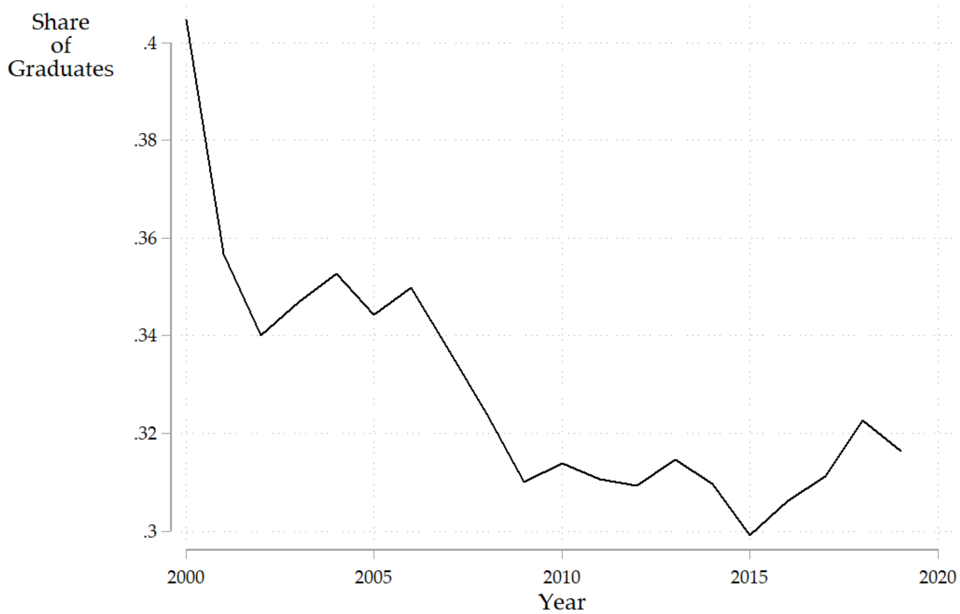
Note: The figure plots the trends of the number of formal job relations with positive wage in which workers had a college degree. Data: RAIS 1994-2019.

Figure 3.3: Number of graduates by major's date of foundation



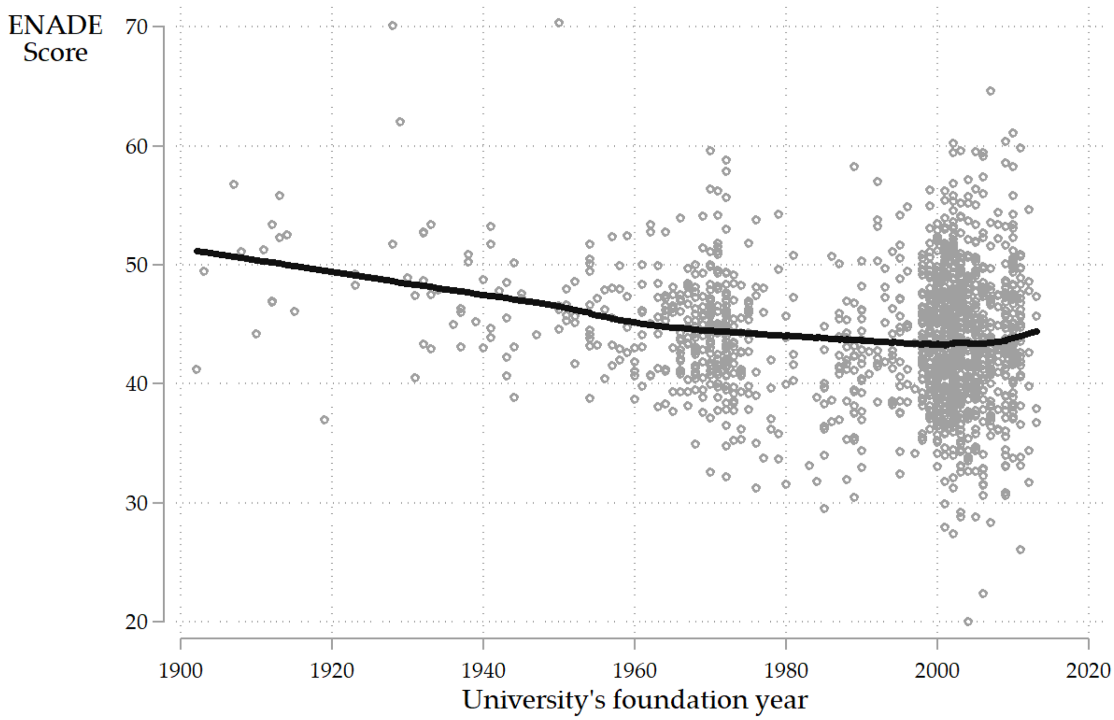
Note: The figure presents the number of students that graduated in each year. Categories are defined by the year in which the major was founded. Source: Higher Education Census (2000-2018)

Figure 3.4: Share of graduates from Top 25 Universities according to ENADE ranking



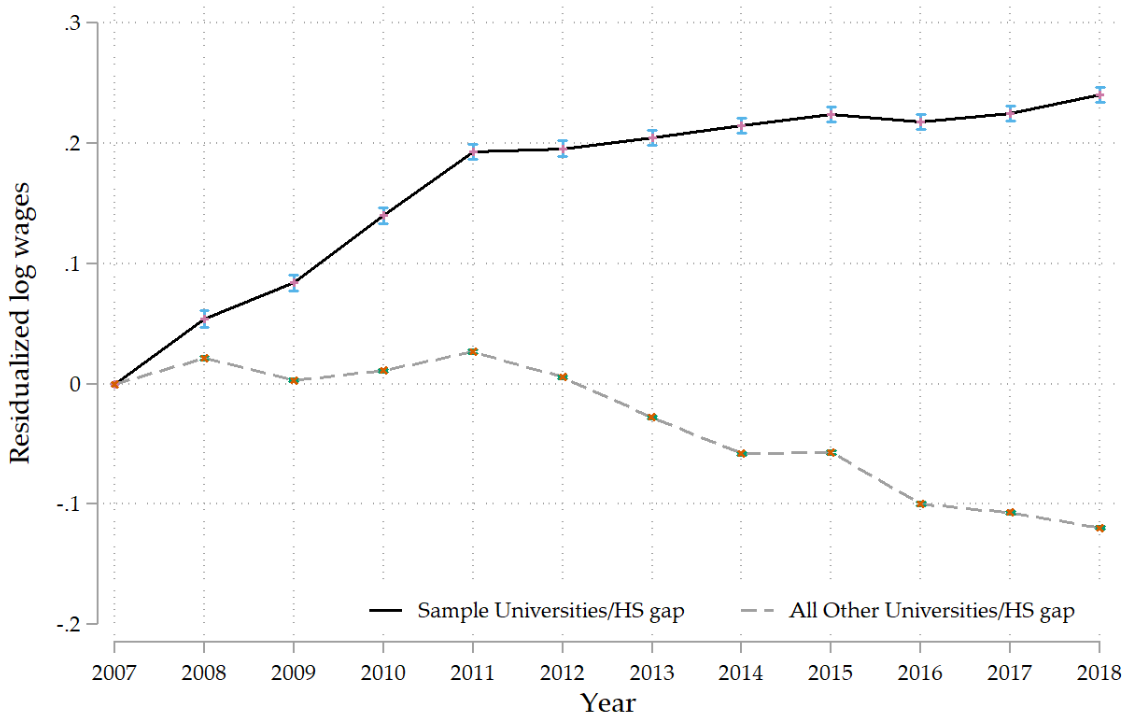
Note: The number of graduates from each university comes from the higher education census.

Figure 3.5: ENADE Score and University's age. Nonparametric regression



Note: ENADE scores per university are constructed as the average across all students in all majors that had an ENADE exam in 2014. University's foundation year is the year of the foundation of the earliest major of the university. The bandwidth for the nonparametric regression is 0.8.

Figure 3.6: Trends in residualized log wages (college premium)



Note: The figure plots the evolution of the college premium, relative to 2007 values, for the college graduate's sample from FOIL requests (Sample Universities) and for the sample of all workers in the RAIS dataset with a college degree, excluding workers in the Sample Universities (All Other Universities). Both curves are relative to the same trends in the high school residualized wages. Estimates come from the estimation of Equation 1 over a sample of 263,181,905 observations. The figure plots 95% confidence intervals for each point estimate.

Table 3.1: University's age

University's Age	Sample	Out of Sample
< 30 years	10.80%	31.20%
30 to 50 years	29.70%	33.10%
> 50 years	59.50%	35.70%
N	37	157

Note: The first column refers to the universities from the college graduates' sample. The second column refers to all other universities in the RUF ranking.

Table 3.2: Institutions ranking and score

	Mean	S.D.	Median	Min	Max	N
RUF score						
Sample	69.6	21.7	42.1	4.8	98	37
Out of sample	43	21	74	4.2	97	157
RUF ranking						
Sample	48.3	48.7	33	3	197	37
Out of sample	110.2	52.6	111	1	196	157
Web ranking						
Sample	52.7	50.4	37	3	219	41
Out of sample	662.5	361	663.5	1	1285	1244

Note: RUF range between 0 and 100. The RUF ranking includes 194 universities and the Web ranking includes 1,285 universities.

Table 3.3: Association between university's age and quality

Dependent variable:	ENADE score			RUF	
	All	Specific knowledge	General knowledge	Ranking	Score
	(1)	(2)	(3)	(4)	(5)
Year in which the institution's first major was founded:					
$1940 < year < 1960$	-2.77**	-2.95**	-2.25*	42.84***	-18.36***
	(1.15)	(1.35)	(1.26)	(11.13)	(4.21)
$1960 < year < 1980$	-4.96***	-5.03***	-4.76***	69.38***	-29.64***
	(0.95)	(1.12)	(1.04)	(9.76)	(3.65)
$1980 < year < 2000$	-6.12***	-5.99***	-6.54***	94.03***	-36.41***
	(0.96)	(1.13)	(1.05)	(14.63)	(5.97)
$2000 < year < 2014$	-5.70***	-5.40***	-6.59***	106.68***	-41.61***
	(0.92)	(1.08)	(1.00)	(14.98)	(6.43)
Constant	49.15***	45.78***	59.22***	45.03***	73.27***
	(0.90)	(1.05)	(0.98)	(8.36)	(3.09)
Observations	1,457	1,457	1,457	185	161
R-squared	0.04	0.02	0.05	0.31	0.36

Note: Standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1)

Table 3.4: Correlation between University ranking and wages

Dependent Variable:	University fixed effect	
	(1)	(2)
Ranking RUF	-0.0016**	
	(0.0008)	
Ranking Web		-0.0021**
		(0.0008)
Observations	33	36

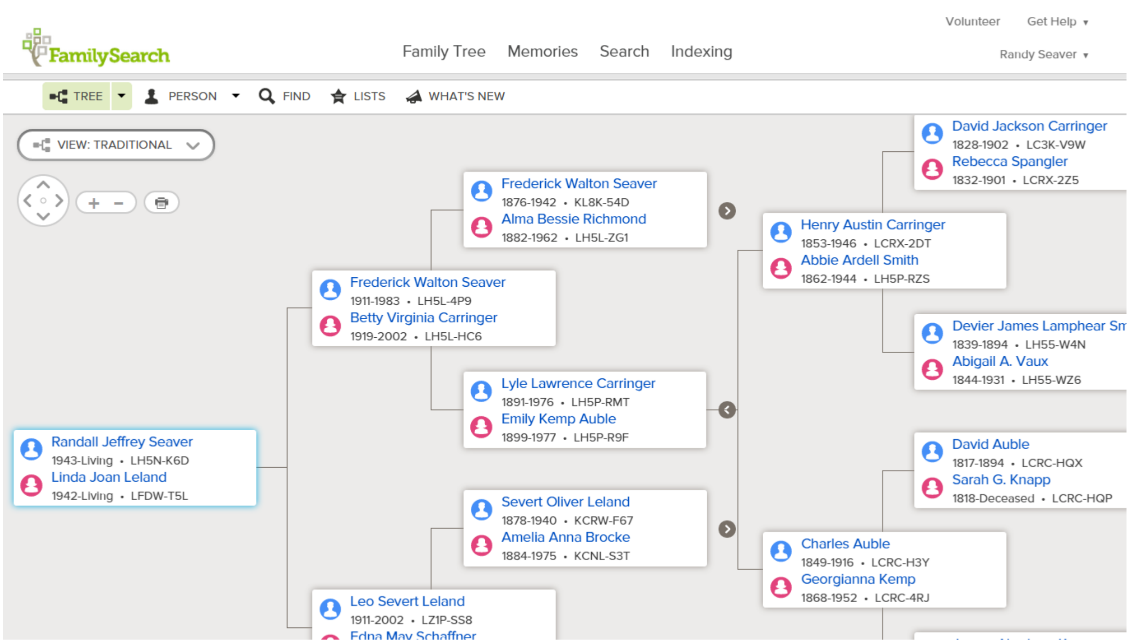
Note: The dependent variable is the fixed effects from the estimation of Equation 1, with the exception that we include university fixed effects and do not include major by university fixed effects. Standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1)

Appendix A

Appendix Materials for Chapter 1

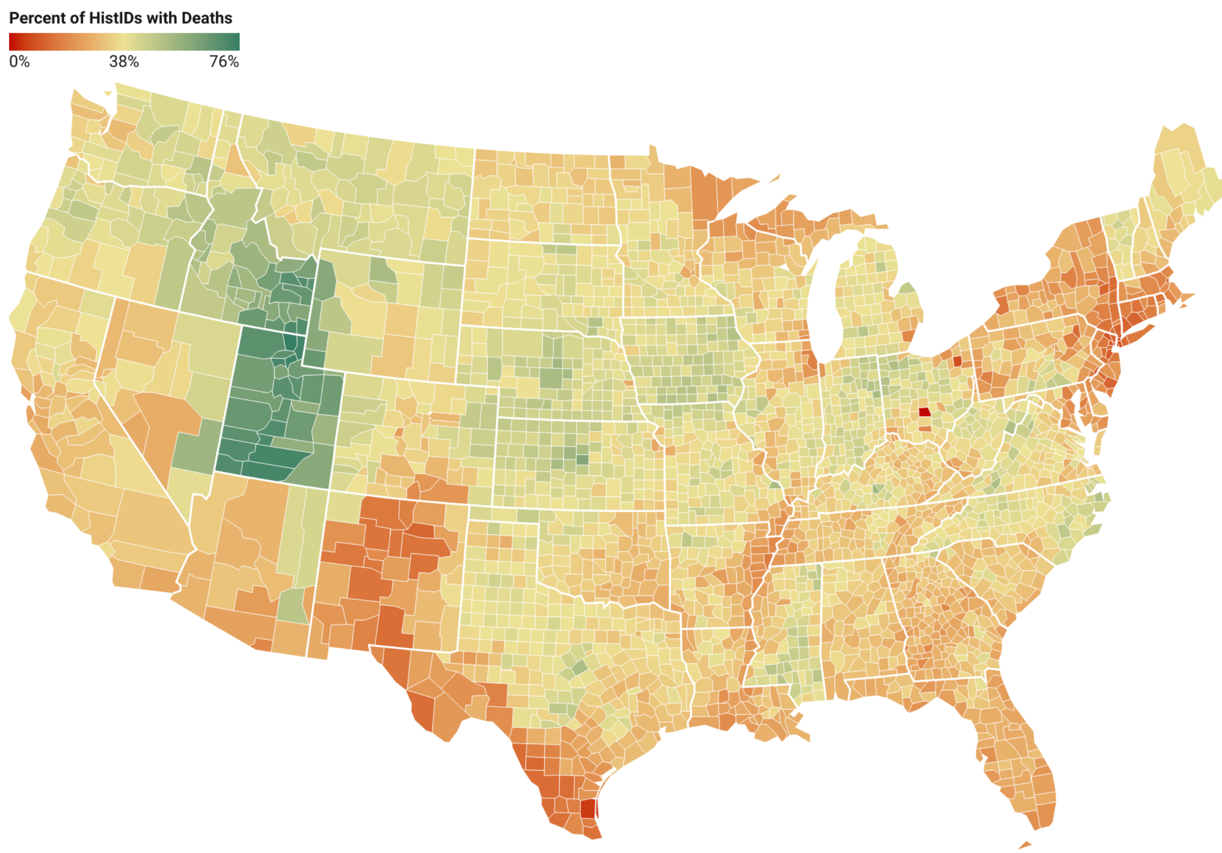
1. Appendix Figures and Tables

Figure A.1: FamilySearch Tree from the Point of View of a Regular User



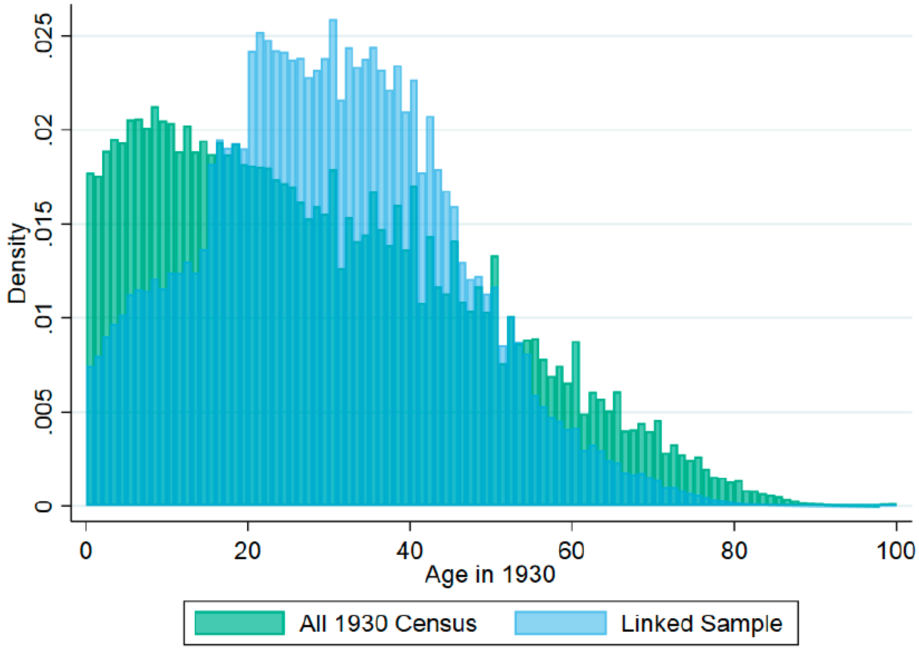
Note: The figure presents an example of a FamilySearch Tree from the point of view of a regular user.

Figure A.2: Match Rates from the White Native Population in the 1930 Census to their FamilySearch Deaths



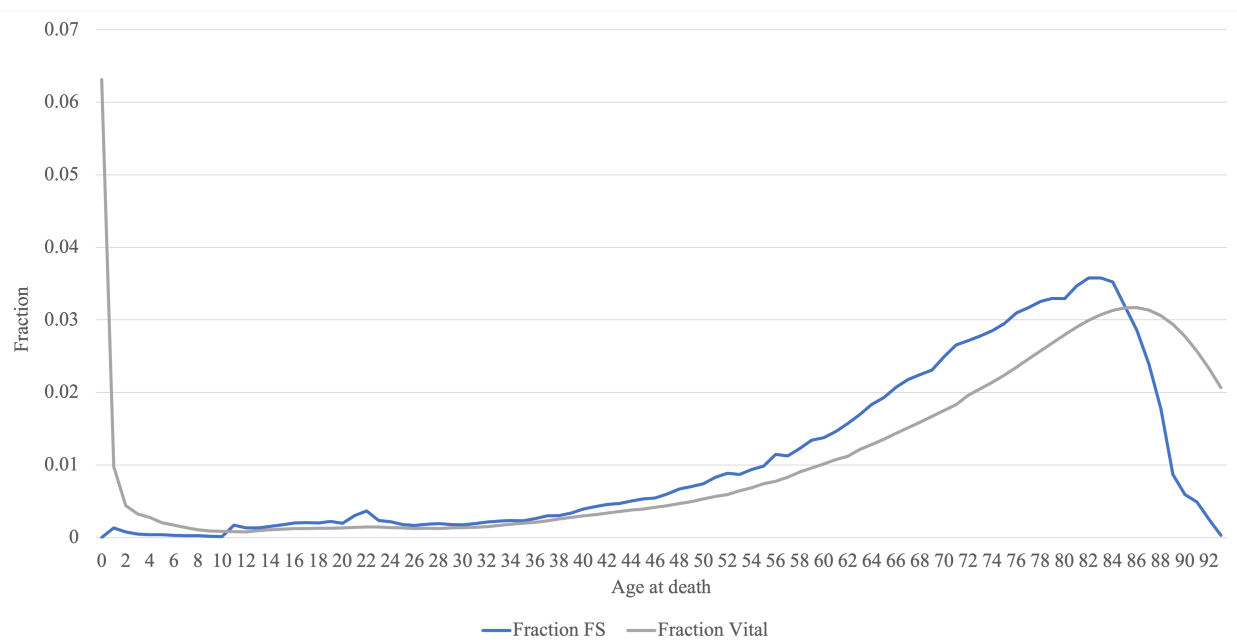
Notes: White lines represent the limits of the counties and states in 1930. Counties are colored in orange to green scale to depict the level of match rates for the linkage from the white native population in the 1930 Census to their FamilySearch deaths.

Figure A.3: Age Distribution in the 1930 Census Sample and the FamilySearch Linked Sample



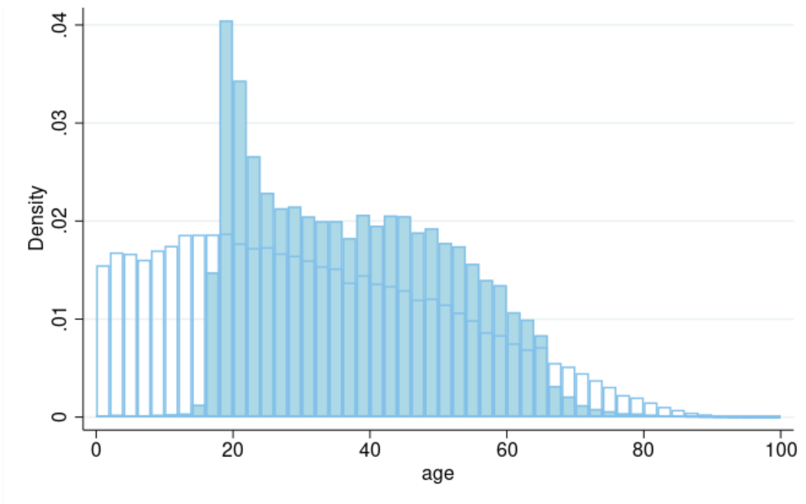
Notes: The histogram presents the distribution of age in 1930 of the two samples of interest: the white native US population in the 1930 Census in green and our linked sample to FamilySearch deaths in blue.

Figure A.4: Distribution of the Age of Death for the 1930 Cohort Using Our Linked Sample and the Vital Statistics Data



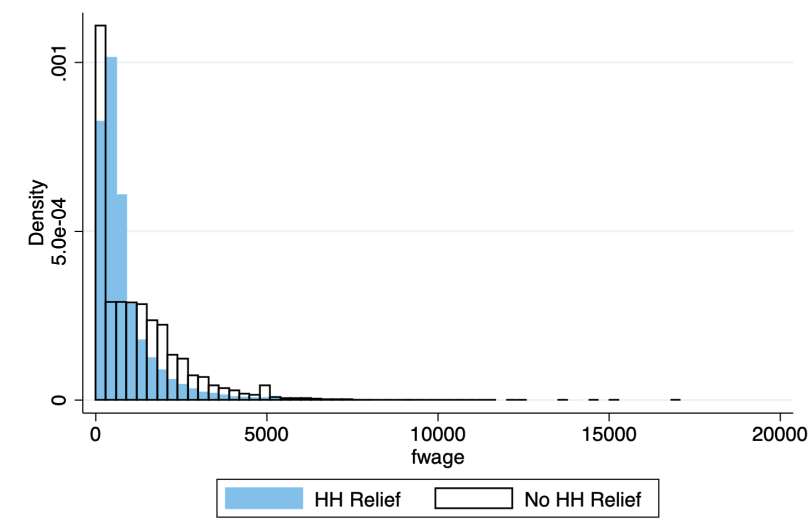
Note: The graph presents the distribution of the age at death for individuals born in 1930. The blue line represents the fraction of deaths at each age in our 1930 Census sample linked to FamilySearch deaths. The grey line represents the fraction of deaths from Social Security Life Tables.Since some individuals born in 1930 are missing form our sample, in blue we report the fraction of deaths for the 1929 cohort.

Figure A.5: Age Distribution of the Relief Recipients in the 1930 Census



Note: In blue, we present the age distribution in 1930 of relief receivers and in white for non-receivers. The sample includes the population in the 1940 US full-count Census.

Figure A.6: Family Wage Distribution of the relief Recipients in the 1940 Census



Note: In blue, we present the family wage distribution in 1940 of households that had at least one relief recipient in 1940 and in white for households that had no relief recipients. The sample includes the population in the 1940 US full-count Census.

Table A.1: Households Receiving and Non-Receiving Relief in 1940

	HH no relief (1)	HH Relief (2)	Difference (3)
Male	0.500	0.521	0.0211***
White	0.902	0.872	-0.0304***
Black	0.094	0.122	0.028***
Spouse	0.436	0.357	-0.0786***
Children	0.340	0.332	-0.0073***
Farm HH	0.235	0.193	-0.0414***
Urban pop.	3376.255	2849.551	-526.704
Owner	0.450	0.335	-0.1141***
Family Wage	1262.914	1163.712	-99.2026
Family Size	4.277	5.298	1.0211***
Income	456.039	270.420	-185.6187
Same Country	0.868	0.905	0.0364***
Urban pop.	1.557	1.538	-0.0187***
Foreign Born	0.092	0.065	-0.0266***
New England	0.064	0.068	0.0038***
Middle Atlantic	0.212	0.177	-0.0344***
East North Central	0.201	0.222	0.0211***
West North Central	0.102	0.111	0.0094***
South Atlantic	0.136	0.131	-0.0043***
East South Central	0.081	0.087	0.0055***
West South Central	0.099	0.102	0.0033***
Mountain	0.031	0.043	0.0127***
Pacific	0.075	0.058	-0.0171***
Observations	121,670,326	10,233,584	131,903,910

Notes: The table compares the means of individuals' characteristics in households receiving and non-receiving relief in the US full-count Census. The sample in column (1) includes all individuals in the US full-count 1940 Census in households with no individuals receiving relief. The sample in column (2) includes all individuals in the US full-count 1940 Census in households with someone receiving relief. Column (3) reports the differences in means. We classify individuals as receiving relief if they answer yes to the 1940 Census question asking "Was the person at work on, or assigned to, public Emergency Work (WPA, NYA,CCC, etc.) during the week of March 24-30?" 10\%*, 5\%**, 1\%***.

Table A.2: County-level First Stage: Voting Culture Exploitability Instrument and New Deal Relief

Dep. Var: Log(Relief pc)	(1)	(2)	(3)
Voting Culture Instrument	9.236***	6.043***	5.050***
	(0.889)	(0.933)	(0.925)
Severity Index	0.164***	0.114***	0.121***
	(0.008)	(0.008)	(0.008)
Constant	5.118***	5.614***	5.795***
	(0.261)	(0.291)	(0.320)
County Controls		x	x
Average of individual covariates			x
Observations	2,983	2,961	2,961
R-squared	0.455	0.551	0.568
F-Test	108	41.99	29.79

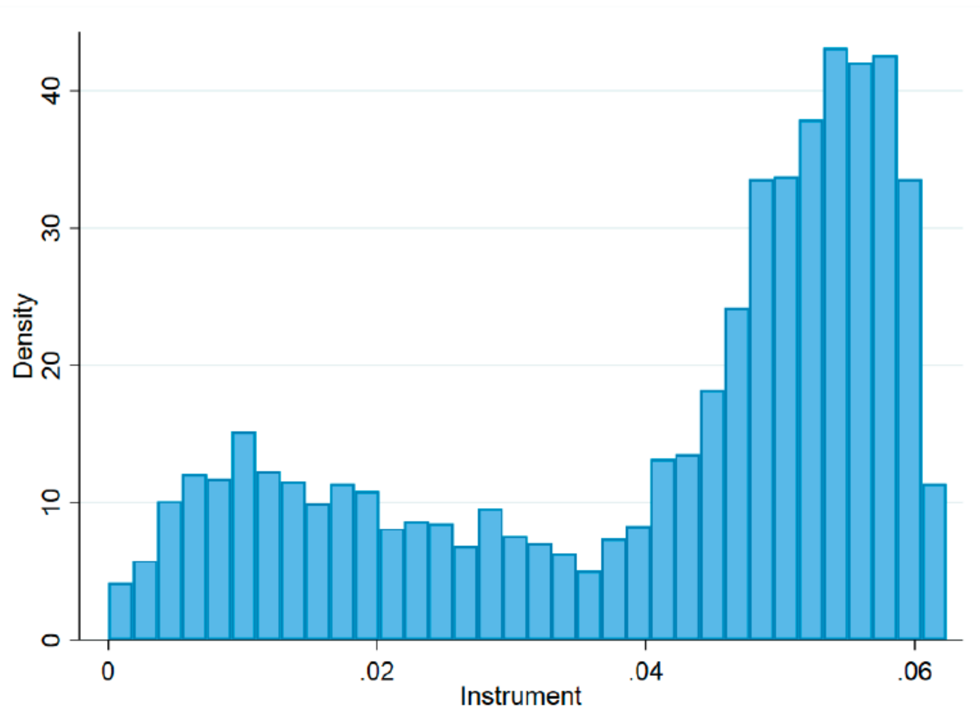
Note: The sample includes all counties for which we have all variables of interest. The dependent variable is an indicator whether the individual was linked to their FamilySearch death. The regression includes cohort and state of birth fixed effects. Standard errors are clustered at county level. All specifications include state fixed effects. Robust standard errors in parentheses. 10\%*, 5\%**, 1\%***

Table A.3: OLS estimates of the Great Depression and the New Deal on Longevity by Gender

		Men			Women	
Dep. Var. Log (Age at death)	(1)	(2)	(3)	(4)	(5)	(6)
Log (Relief per capita $)	-0.0049***	-0.0017***	-0.0018***	-0.0027***	-0.0008	-0.0008*
	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0005)
Severity Index	-0.0030***	-0.0016***	-0.0014***	-0.0028***	-0.0018***	-0.0017***
	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Constant	4.9672***	4.9343***	4.1177***	4.8262***	4.2661***	4.2483
	(0.0247)	(0.0269)	(0.0100)	(0.0029)	(1.3061)	-1.6152
County Controls		x	x		x	x
Individual Covariates			x			x
Observations	14,409,631	14,366,330	14,366,330	12,098,702	12,062,887	12,062,887
R-squared	0.0474	0.0482	0.0490	0.0159	0.0162	0.0162

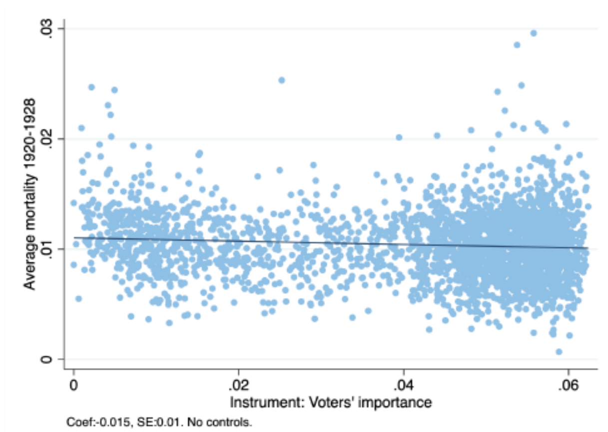
Notes: The sample in the first three columns (1-3) include all white, native, men in the 1930 US Census linked to their FamilySearch deaths. The sample in the last three columns (4-6) include all white, native, women in the 1930 US Census linked to their FamilySearch deaths. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at the county level. 10\%*, 5\%**, 1\%***

Figure A.7: Distribution of the Voting Culture Exploitability Instrument

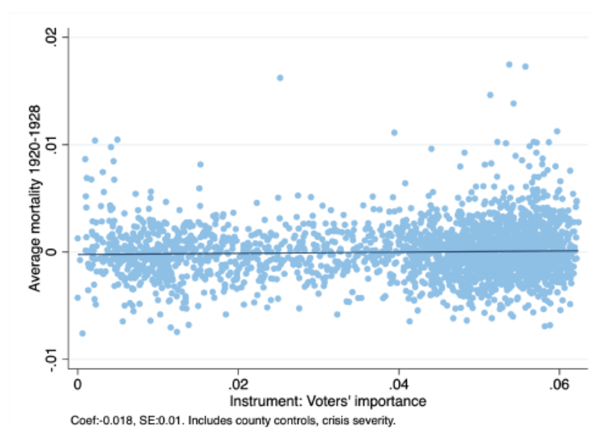


Notes: The histogram presents the distribution of the voting culture exploitability instrument.

Figure A.8: Relationship of Average Mortality Rates 1920-1928 and Voting Culture Instrument



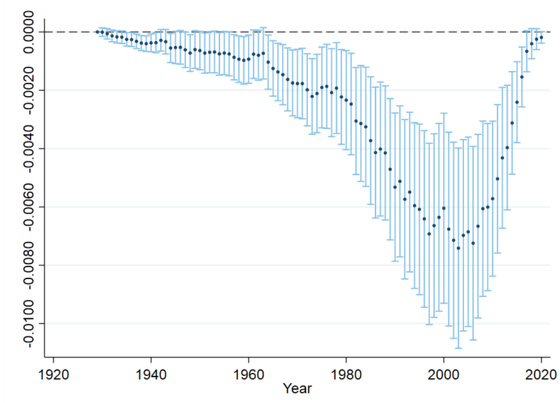
(a) Without Controls



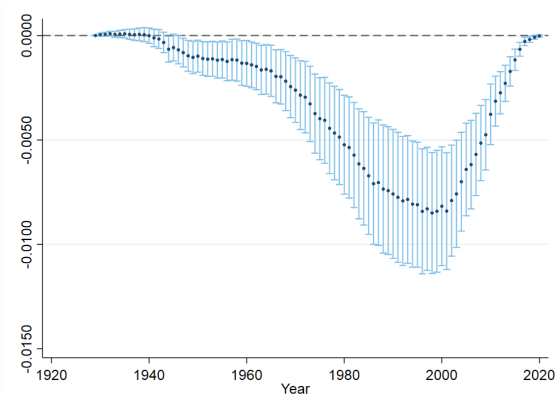
(b) With Controls

Notes: The graphs plot the county-level relationship between the voting culture exploitability instrument and average mortality from 1920 to 1928. Figure A shows the relationship without controls and Figure B shows the residualized version that account for the severity of the crisis and county-level controls selected by LASSO. We report the coefficient and standard errors for the estimates of the effects of the instrument on average mortality rates.

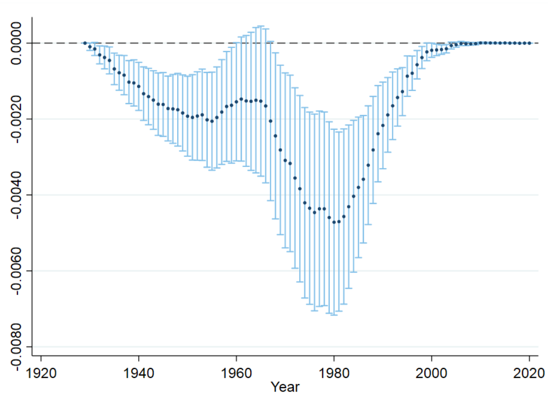
Figure A.9: IV Estimates of the Effects of the Great Depression on Survival



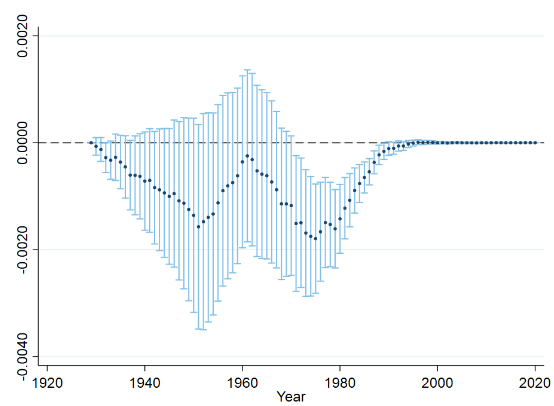
(a) Ages 0 to 5 in 1930



(b) Ages 6 to 15 in 1930



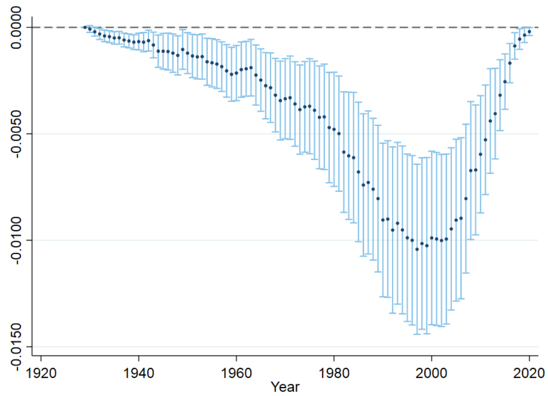
(c) Ages 26 to 35 in 1930



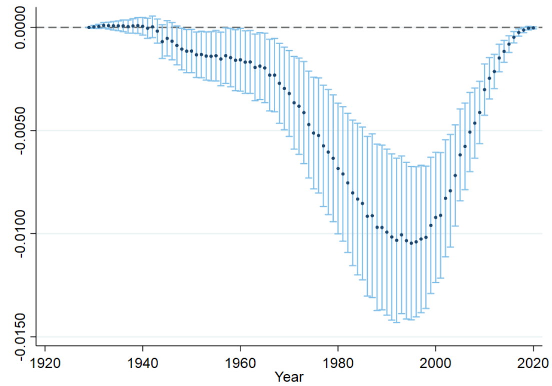
(d) Ages over 35 in 1930

Notes: The figures present IV coefficients and 95% confidence intervals, of the effects of crisis severity on survival from 1933 to 2020 for different groups of birth cohorts. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

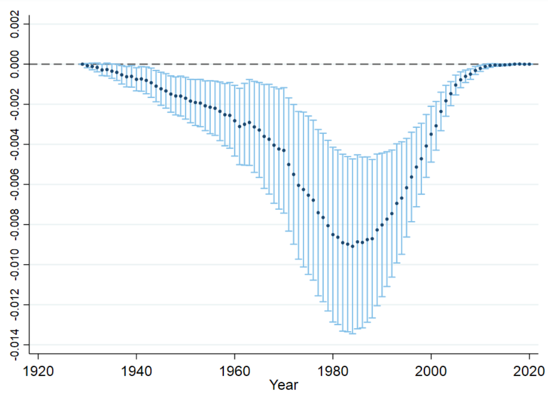
Figure A.10: IV Estimates of the Effects of the Great Depression on Survival for Men



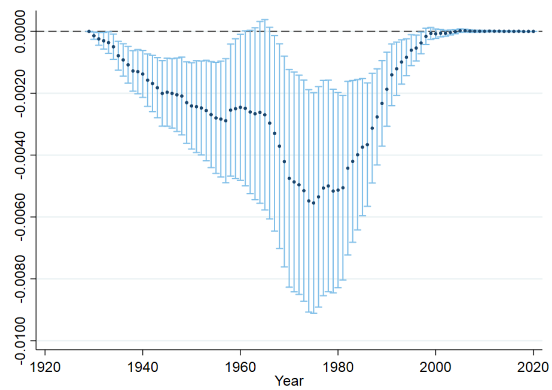
(a) Ages 0 to 5 in 1930



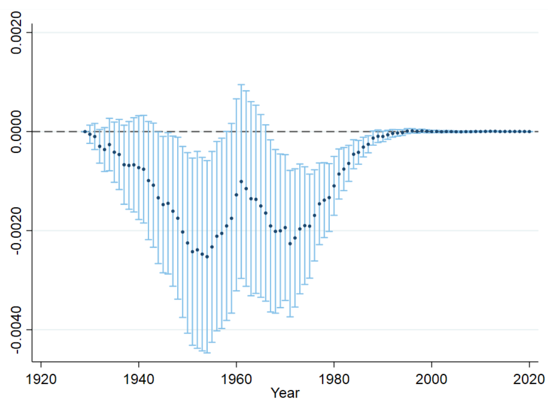
(b) Ages 6 to 15 in 1930



(c) Ages 16 to 25 in 1930



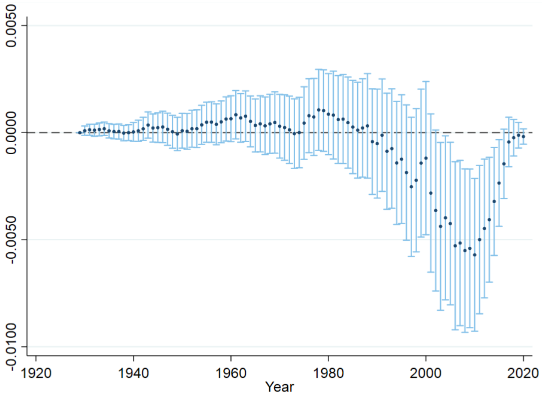
(d) Ages 26 to 35 in 1930



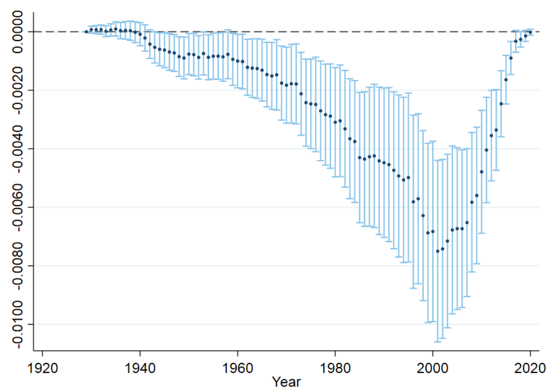
(e) Ages over 35 in 1930

Notes: The figures present IV coefficients and 95% confidence intervals of the effects of the severity of the Great Depression on survival from 1933 to 2020 for men of different ages in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

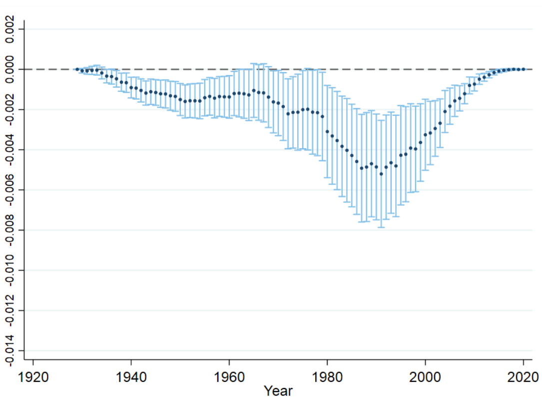
Figure A.11: IV Estimates of the Effects of the Great Depression on Survival for Women



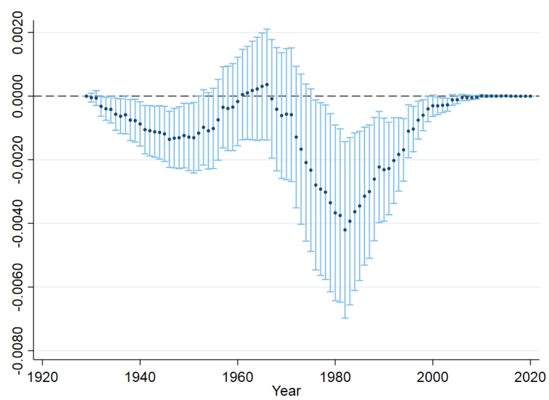
(a) Ages 0 to 5 in 1930



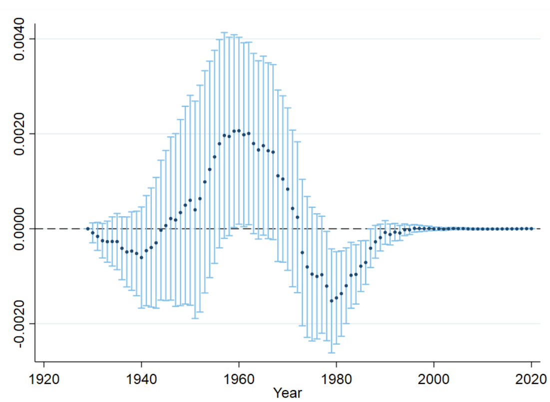
(b) Ages 6 to 15 in 1930



(c) Ages 16 to 25 in 1930



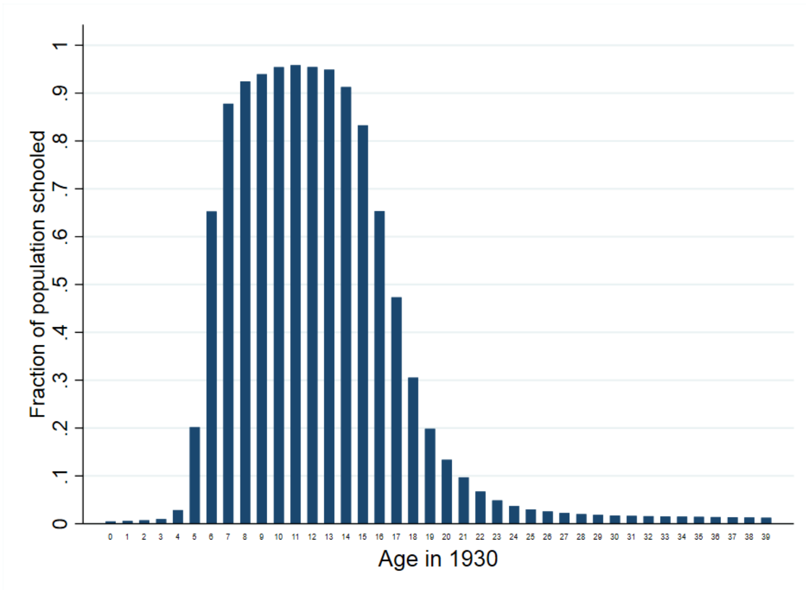
(d) Ages 26 to 35 in 1930



(e) Ages over 35 in 1930

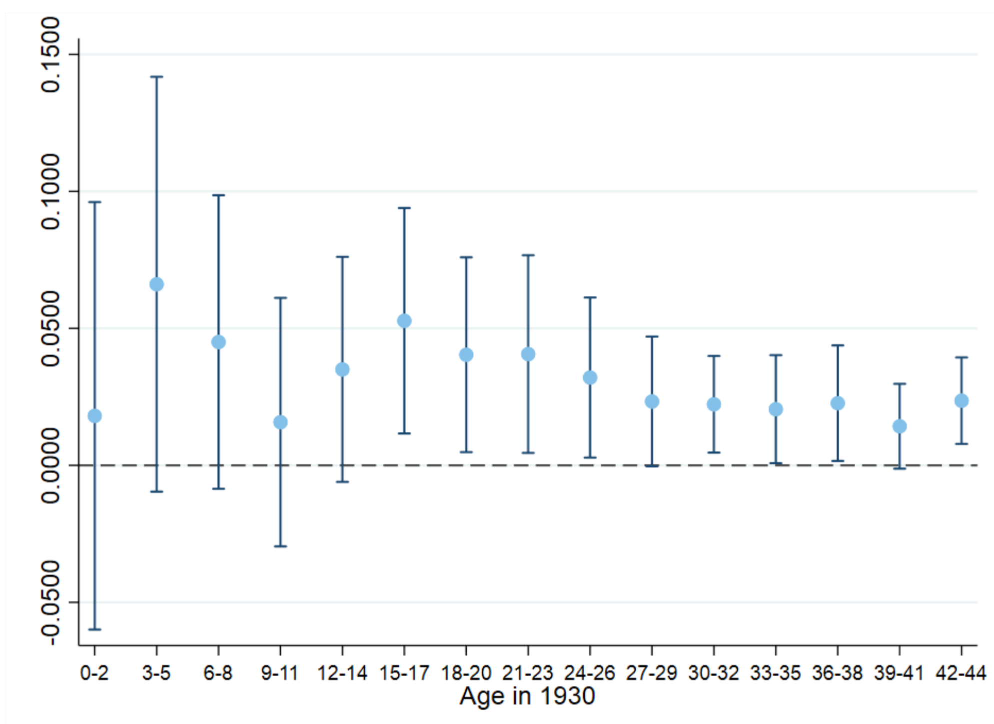
Notes: The figures present IV coefficients and 95% confident intervals, of the effects of the severity of the Great Depression on survival from 1933 to 2020 for women of different ages in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Figure A.12: Fraction of Individuals in School in the 1930 Census by Age



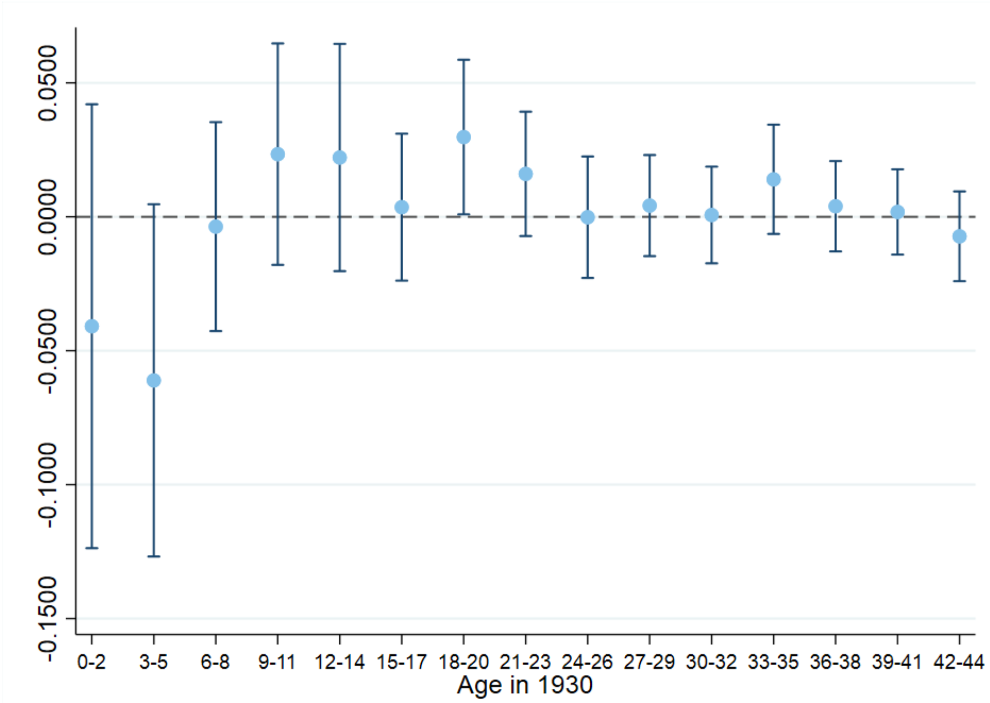
Notes: The sample includes all individuals in the 1930 full-count US Census.

Figure A.13: IV Estimates of the New Deal Relief on Longevity by Cohort for Men



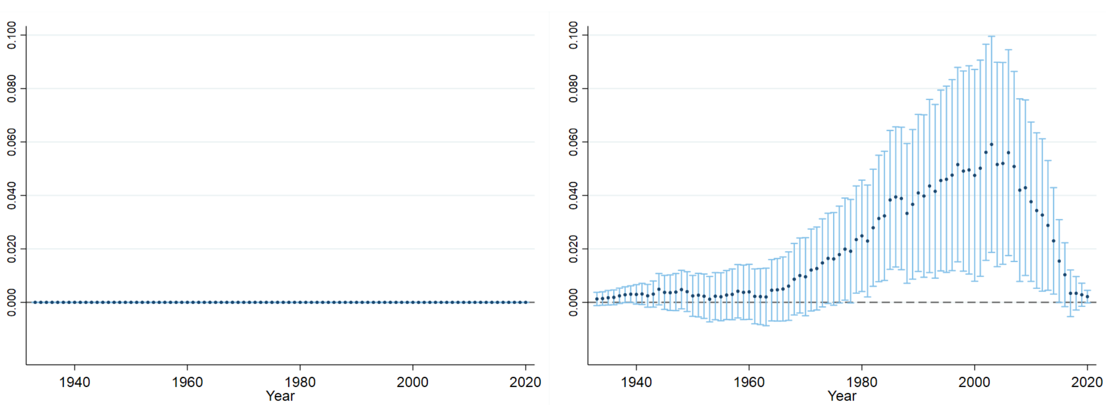
Notes: The figure presents IV estimates and 95% confidence intervals of the post-IV-LASSO regression of the effect of New Deal relief on longevity by cohort. The regression accounts for the severity of the crisis and includes the county controls selected by LASSO and individual covariates from the 1930 Census. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. The sample includes all white native men in the 1930 Census aged 0-44 in 1930 linked to their FamilySearch deaths.

Figure A.14: IV Estimates of the New Deal Relief on Longevity by Cohort for Women



Notes: The figure presents IV estimates and 95% confidence intervals of the post-IV-LASSO regression of the effects of New Deal Relief on Longevity by cohort. The regression accounts for the severity of the crisis and includes the county controls selected by LASSO and individual covariates from the 1930 Census. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. The sample includes all white native women in the 1930 Census aged 0-44 in 1930 linked to their FamilySearch deaths.

Figure A.15: The Effects of New Deal Relief on Survival for Cohort 0-5

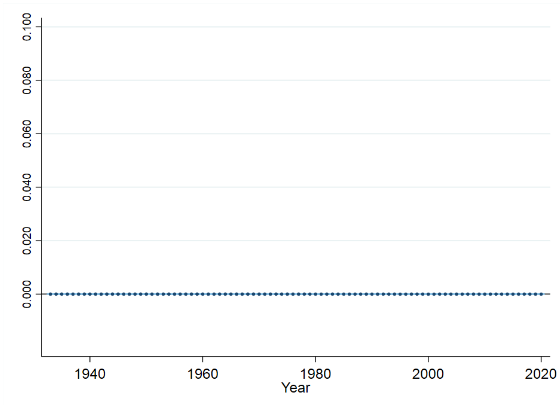


(a) OLS Estimates

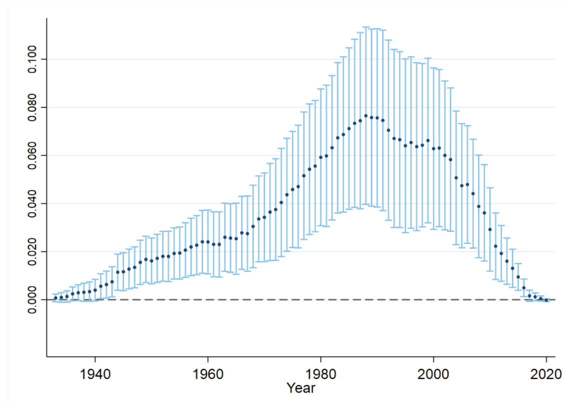
(b) IV estimates

Notes: The figures present the OLS (a), IV coefficients (b), and 95% confidence intervals of the effects of New Deal relief on survival from 1933 to 2020 for the cohort aged 0 to 5 in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Figure A.16: The Effects of New Deal Relief on Survival for Cohorts 6-15



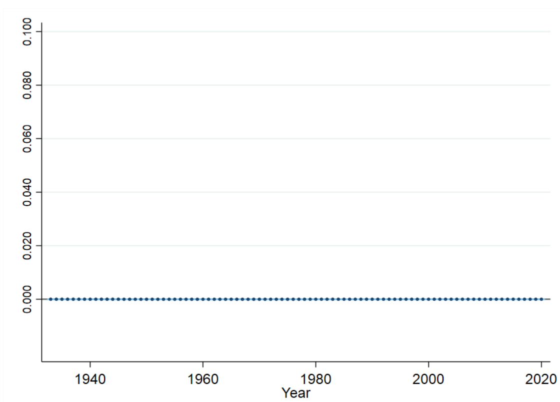
(a) OLS Estimates



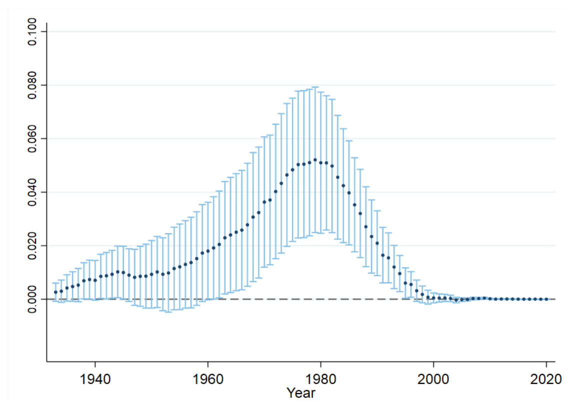
(b) IV estimates

Notes: The figures present the OLS (a), IV coefficients (b), and 95% confidence intervals of the effects of New Deal relief on survival from 1933 to 2020 for cohort aged 6 to 15 in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Figure A.17: The Effects of New Deal Relief on Survival for Cohort 26-35



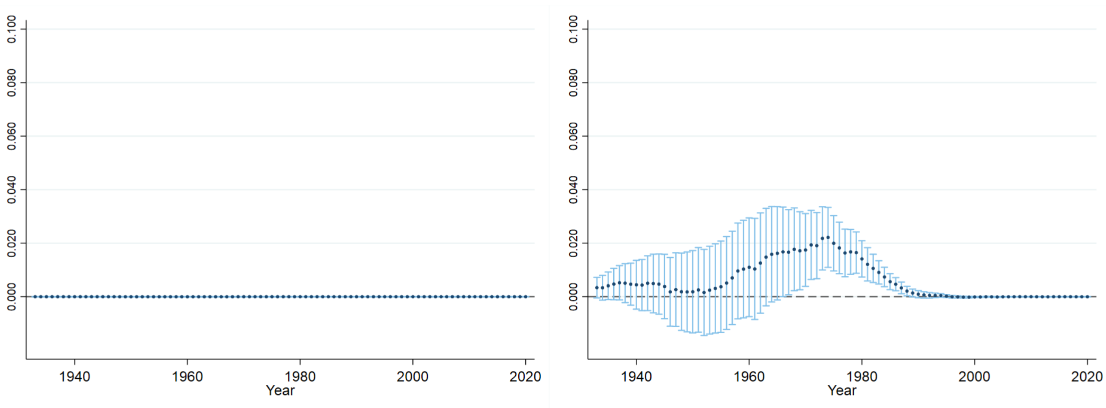
(a) OLS Estimates



(b) IV estimates

Notes: The figures present the OLS (a), IV coefficients (b), and 95% confidence intervals of the effects of New Deal relief on survival from 1933 to 2020 for cohort aged 26 to 35 in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Figure A.18: The Effects of New Deal Relief on Survival for Cohort +35

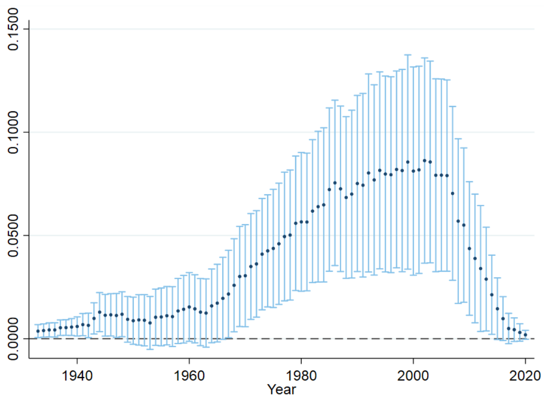


(a) OLS Estimates

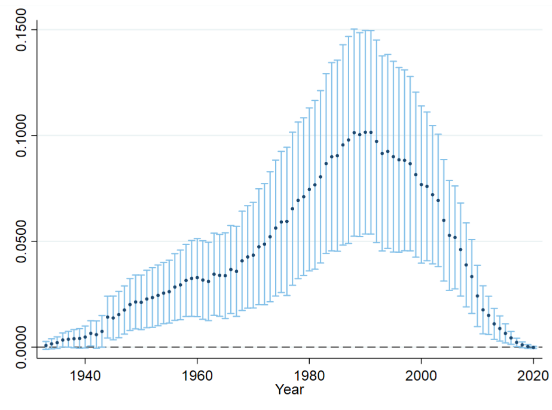
(b) IV estimates

Notes: The figures present the OLS (a), IV coefficients (b, and 95% confidence intervals, of the effects of New Deal relief on survival from 1933 to 2020 for cohort aged over 35 in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

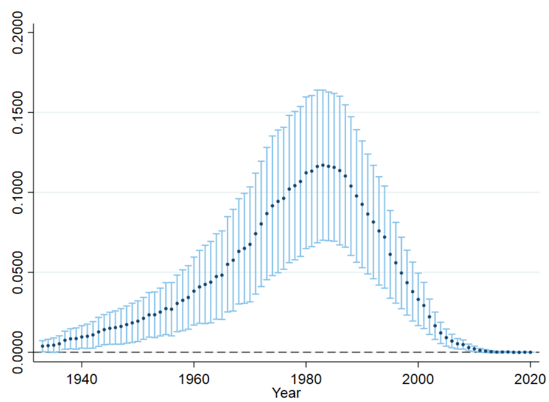
Figure A.19: IV Estimates of the Effects of New Deal Relief on Survival for Men



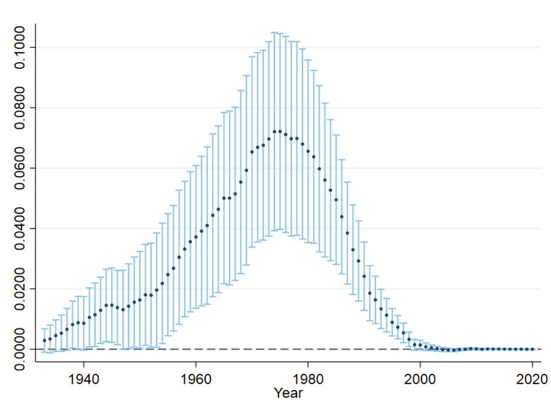
(a) Ages 0 to 5 in 1930



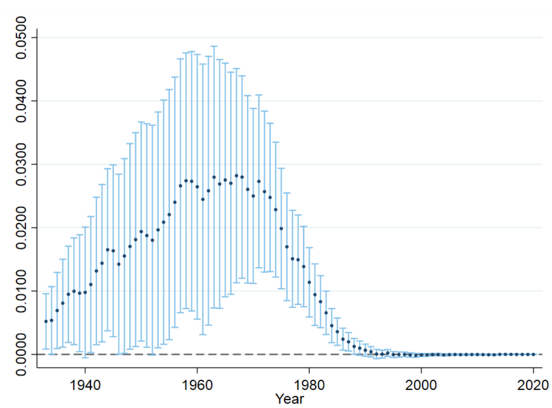
(b) Ages 6 to 15 in 1930



(c) Ages 16 to 25 in 1930



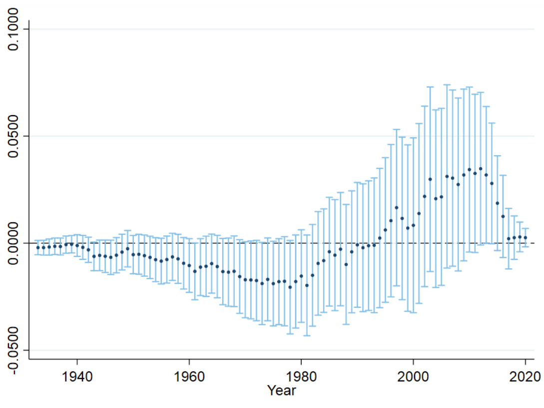
(d) Ages 26 to 35 in 1930



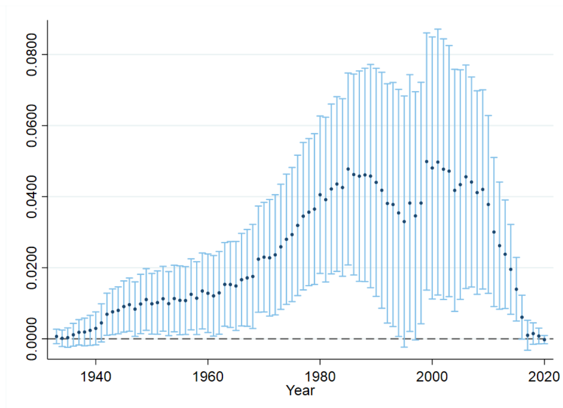
(e) Ages over 35 in 1930

Notes: The figures present IV coefficients and 95% confidence intervals of the effects of New Deal relief on survival from 1933 to 2020 for men of different ages in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute the confidence intervals are clustered at county level.

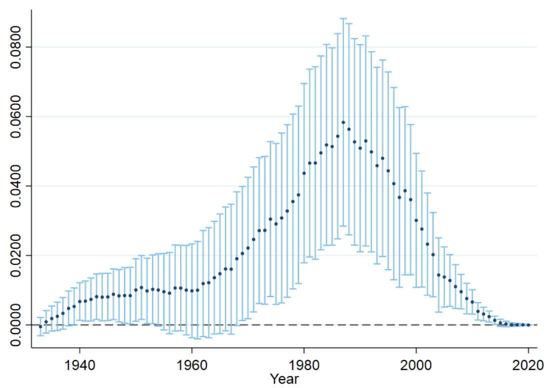
Figure A.20: IV Estimates of the Effects of New Deal Relief on Survival for Women



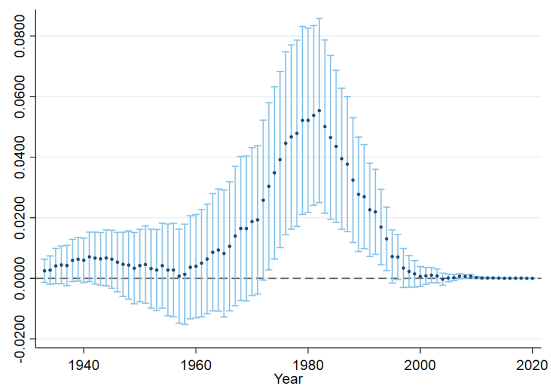
(a) Ages 0 to 5 in 1930



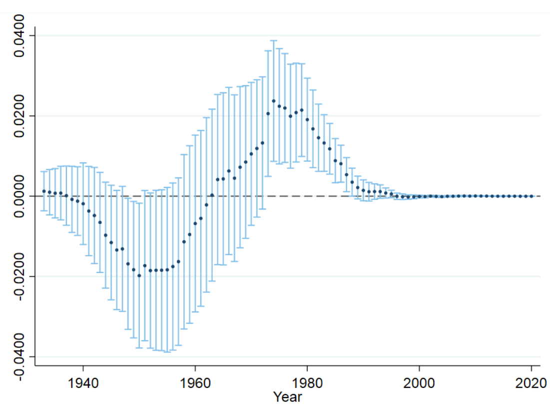
(b) Ages 6 to 15 in 1930



(c) Ages 16 to 25 in 1930



(d) Ages 26 to 35 in 1930



(e) Ages over 35 in 1930

Notes: The figures present IV coefficients and 95% confidence intervals of the effects of New Deal relief on survival from 1933 to 2020 for women of different ages in 1930. Regressions include county controls, individual covariates, and state of birth and cohort fixed effects. Standard errors used to compute confidence intervals are clustered at county level.

Table A.4: IV Estimates of the Effects of the Great Depression and the New Deal on Longevity by Cohort

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age in 1930	0-10		10-20		20-30		30-40		40+	
Dep. Variable	L(Relief pc.)	L(Longevity)	L(Relief pc.)	L(Longevity)	L(Relief pc.)	L(Longevity)	L(Relief pc.)	L(Longevity)	L(Relief pc.)	L(Longevity)
Log(Relief pc.)		0.0105		0.0256**		0.0157		0.0103		0.0021
		(0.0138)		(0.0116)		(0.0102)		(0.0070)		(0.0045)
Severity Index	0.0597***	-0.0022**	0.0608***	-0.0029***	0.0598***	-0.0020***	0.0637***	-0.0014**	0.0717***	-0.0004
	(0.0103)	(0.0010)	(0.0103)	(0.0008)	(0.0114)	(0.0007)	(0.0116)	(0.0005)	(0.0110)	(0.0004)
Voting Culture	3.3071***		3.8050***		4.6220***		4.8548***		5.1824***	
	(1.1123)		(0.9505)		(0.9223)		(0.9192)		(0.8737)	
Constant	5.3026***	4.1245***	5.3340***	4.0466***	5.3049***	4.2132***	5.2488***	4.1099***	5.2788***	4.0895***
	(0.0822)	(0.0766)	(0.0750)	(0.0644)	(0.0701)	(0.0578)	(0.0758)	(0.0406)	(0.0740)	(0.0288)
Observations	2,706,851	2,706,851	4,233,345	4,233,345	6,138,841	6,138,841	5,926,705	5,926,705	7,257,055	7,257,055
R-squared	0.6116	0.0041	0.6047	0.0036	0.5857	0.0049	0.5717	0.0064	0.5634	0.0639
Mean Longevity		71.63		74.14		73.6		73.82		74.25
Effect severity (mos)		-1.9		-2.6		-1.7		-1.2		-0.4
Effect relief (mos)		5		12.3		7.3		4.8		1

Notes: The sample includes all white native individuals in the 1930 Census linked to their FamilySearch deaths who survived to age 20. All specifications include county controls selected by LASSO, individual covariates from the 1930 Census, and state of birth and cohort fixed effects. Standard errors are clustered at county level. Magnitudes correspond to changes in longevity in months for a standard-deviation increase in severity and relief, respectively. 10\%*, 5\%**, 1\%***.

Table A.5: IV Estimates of the Effects of the Great Depression and the New Deal on Longevity by 1930 Occupation Score

	(1)	(2)
Dep. Var. Log(Age at death)	Lower quartile	Upper quartile
Log(Relief pc.)	0.0231***	0.0140*
	(0.0073)	(0.0078)
Severity Index	-0.0029***	-0.0026**
	(0.0010)	(0.0010)
Constant	3.9560***	4.1068***
	(0.0483)	(0.0708)
Observations	3,761,723	2,863,020
R-squared	0.05	0.05
Mean Longevity	74.14	72.3
Effect severity (months)	-2.5	-2.2
Effect relief (months)	13.3	5.5

Notes: The sample includes all white native individuals in the 1930 Census linked to their FamilySearch deaths which are in the first or last quartile for occupation score in 1930. We restrict our classification to individuals with a positive occupation score. All specifications include county controls selected by LASSO, individual covariates from the 1930 Census, and state of birth and cohort fixed effects. Standard errors are clustered at county level. 10\%*, 5\%**, 1\%***.

Table A.6: IV Estimates of the Effects of the Great Depression and the New Deal on Longevity by Gender and 1930 Marital Status

	Men		Women	
IV estimates	Married	Single	Married	Single
Dep. Var. Log(Age at death)	(1)	(2)	(3)	(4)
Log(Relief pc.)	0.0142**	0.0364**	-0.0014	-0.0006
	(0.0055)	(0.0176)	(0.0053)	(0.0097)
Severity Index	-0.0021***	-0.0069***	-0.0007	-0.0018
	(0.0008)	(0.0024)	(0.0008)	(0.0013)
Constant	3.9757***	3.9138***	4.2987***	4.1992***
	(0.0509)	(0.1044)	(0.0469)	(0.0572)
Observations	8,081,344	6,224,026	7,504,891	4,505,750
R-squared	0.07	0.02	0.01	0.02

Notes: The sample includes all white native individuals in the 1930 Census linked to their FamilySearch deaths. All specifications include county controls selected by LASSO, individual covariates from the 1930 Census, and state of birth and cohort fixed effects. Standard errors are clustered at county level. 10\%*, 5\%**, 1\%***.

Table A.7: IV Estimates of the Effects of the Great Depression and the New Deal on Longevity by Men Movers between 1930 and 1940

	Movers	Non-Movers
Dep. Var. Log (age at death)	(1)	(2)
Log(Relief pc.)	0.0136***	0.0162***
	(0.0050)	(0.0062)
Severity index	-0.0024***	-0.0045***
	(0.0008)	(0.0011)
Constant	4.0396***	4.0315***
	(0.0281)	(0.0380)
Observations	2,476,909	9,190,724
R-squared	0.04	0.07

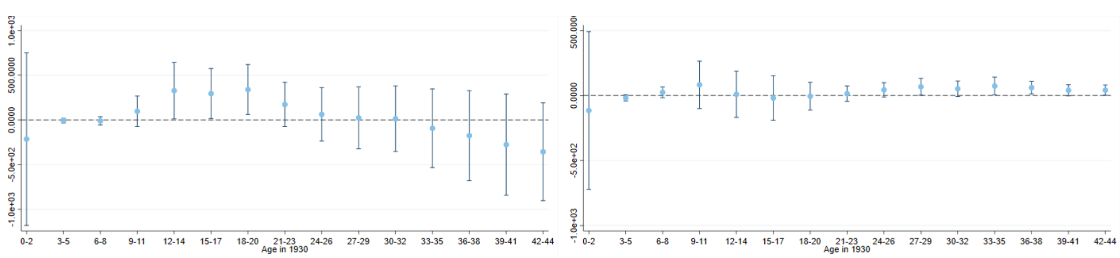
Notes: The sample includes all white native men in the 1930 Census linked to their FamilySearch deaths. All specifications include county controls selected by LASSO, individual covariates from the 1930 Census, and state of birth and cohort fixed effects. Standard errors are clustered at county level. 10\%*, 5\%**, 1\%***.

Table A.8: IV Estimates of the Effects of New Deal Relief on 1940 Outcomes for Men

Dep. Variable	(1) Income	(2) Employed	(3) In labor force	(4) Education	(5) Married	(6) Divorced	(7) Widowed	(8) Moved counties
Relief per capita	0.010125	-0.000107*	-0.000083*	0.002148	-0.000010	0.000067***	-0.000006	-0.000180
	(0.385166)	(0.000063)	(0.000050)	(0.001531)	(0.000028)	(0.000025)	(0.000010)	(0.000244)
Severity Index	-13.818007*	-0.005589***	0.000305	-0.051245	0.000964*	-0.001246**	-0.000024	0.022392***
	(7.511056)	(0.001497)	(0.001149)	(0.031384)	(0.000579)	(0.000499)	(0.000218)	(0.004771)
Constant	223.393604**	0.020525	0.012479	4.045916***	0.021537***	-0.011931***	-0.002047	0.360853***
	(86.853466)	(0.013525)	(0.010645)	(0.326174)	(0.005854)	(0.004373)	(0.002070)	(0.045534)
Observations	10,289,961	11,695,703	11,695,703	11,455,826	11,695,703	11,695,703	11,695,703	11,695,703
R-squared	0.229248	0.388440	0.492030	0.216850	0.563451	0.000066	0.101677	0.037490
Outcome Mean	497.33	0.5	0.54	9	0.72	0.01	0.04	0.21
Effect severity	-2.8%	-1.12%	0.01%	-0.5%	0.13%	-12.46%	-0.06%	10.7%
Effect relief	0.28%%	-3%	2.15%	3.3%	-0.2%	94%	-2.1%	-12%

Notes: The sample includes all white native men in the 1930 Census linked to their FamilySearch deaths and to the 1940 Census. For column 1, the sample is smaller because fewer individuals report information on their income. The table presents second-stage IV estimates for the effects of New Deal relief on different outcomes from the 1940 Census. The variable Education is expressed in years. All specifications include county controls selected by LASSO, individual covariates from the 1930 Census, and state of birth and cohort fixed effects. Standard errors are clustered at county level. The effects are expressed for a one-standard-deviation increase in severity and relief. 10\%*, 5\%**, 1\%***.

Figure A.21: The IV Estimates of New Deal Relief on 1940 Income Wage by Gender

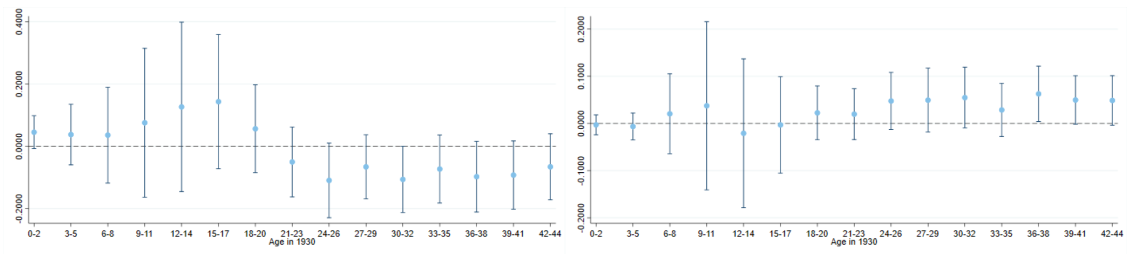


(a) Men

(b) Women

Notes: The figures present IV estimates and 95% confidence intervals of the post-IV-LASSO regression of New Deal relief on 1940 income wage by cohorts for men and women. All specifications account for the severity of the crisis and include the county controls selected by LASSO and individual covariates from the 1930 Census. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. The sample in Figure a (b) includes all white native men (women) in the 1930 Census aged 0-44 in 1930 linked to their FamilySearch deaths and to the 1940 Census.

Figure A.22: IV Estimates of the Effects of New Deal Relief on 1940 Employment by Gender



(a) Men **(b)** Women

Notes: The figures present IV estimates and 95% confidence intervals of the post-IV-LASSO regression of New Deal relief on 1940 employment by cohort for men and women, respectively. All specifications account for the severity of the crisis and include the county controls selected by LASSO and individual covariates from the 1930 Census. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. The sample in Figure a (b) includes all white native men (women) in the 1930 Census aged 0-44 in 1930 linked to their FamilySearch deaths and to the 1940 Census.

Table A.9: OLS estimates of the Effects of the New Deal and the Great Depression on Longevity at County Level

	Everyone	Men	Women
Dep. Var: Log(Age at death)	(1)	(2)	(3)
Log(Relief pc)	0.001260**	0.001973	0.002458**
	(0.000495)	(0.001455)	(0.001100)
Severity Index	0.000345	0.000129	-0.000144
	(0.000245)	(0.000756)	(0.000578)
Constant	4.333828***	4.199061***	4.332166***
	(0.010025)	(0.015269)	(0.013244)
Observations	2,975	2,975	2,975
R-squared	0.533626	0.513859	0.315212

Notes: The sample includes data on all white native individuals in the 1930 US Census linked to their FamilySearch deaths summarized at county level. Columns (2) and (3) include data for men and women, respectively. In all specifications, the dependent variable is the logarithm of the average age at death at county level. All specifications include county controls and individual covariates. Individual covariates are the averages at county level. Robust standard errors reported in parentheses. 10\%*, 5\%**, 1\%***.

Table A.10: IV Estimates for the Effects of New Deal Relief on Longevity at County Level

	Everyone		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var:	Log(Relief pc)	Log(Longevity)	Log(Relief pc)	Log(Longevity)	Log(Relief pc)	Log(Longevity)
Log(Relief pc)		0.022176***		0.047395***		0.011437*
		(0.004410)		(0.011021)		(0.006260)
Severity Index	0.081500***	-0.001342***	0.082692***	-0.003586***	0.076255***	-0.000820
	(0.010282)	(0.000463)	(0.010500)	(0.001084)	(0.010972)	(0.000749)
Voting Culture Instrument	5.521569***		4.935077***		7.508647***	
	(0.809733)		(0.848812)		(0.785691)	
Constant	3.870722***	4.253995***	5.727014***	3.933686***	6.003612***	4.276287***
	(0.526868)	(0.020572)	(0.150360)	(0.067186)	(0.149695)	(0.037452)
Observations	2,961	2,961	2,961	2,961	2,961	2,961
R-squared	0.535080	0.177591	0.536011	-0.458701	0.513307	0.245561

Notes: The sample includes data on all white native individuals in the 1930 US Census linked to their FamilySearch deaths summarized at county level. Columns (2) and (3) include data for men and women, respectively. In all specifications, the dependent variable is the logarithm of the average age at death at county level. All specifications include county controls and individual covariates. Individual covariates are the averages at county level. Robust standard errors reported in parentheses. 10\%*, 5\%**, 1\%***.

Table A.11: IV Estimates for the Effects of New Deal Relief on Longevity Using Levels

	Everyone		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var:	Relief pc	Age at death	Relief pc	Age at death	Relief pc	Age at death
Relief per Capita		0.0034*		0.006719**		0.001166
		(0.0018)		(0.002615)		(0.001631)
Severity Index	17.0893***	-0.1078***	0.064224***	-0.155378***	0.065874***	-0.045119
	(3.7704)	(0.0372)	(0.011007)	(0.055955)	(0.010688)	(0.030386)
Instrument	1170.579***		4.742996***		4.669500***	
	(278.8096)		(0.917934)		(0.892396)	
Constant	240.8327***	65.0870***	5.313950***	63.961102***	5.285936***	69.394595***
	(22.4628)	(0.5815)	(0.069992)	(0.831235)	(0.070903)	(0.500789)
Observations	26,316,569	26,316,569	14,305,370	14,305,370	12,010,641	12,010,898
R-squared	0.3963	0.0282	0.577522	0.047364	0.585258	0.017237
F-Test	17.63		16.6		18.75	

Notes: The sample includes all white native individuals in the 1930 US Census linked to their FamilySearch deaths. All specifications include county controls selected by LASSO, individual covariates, and state of birth and cohort fixed effects. Standard errors are clustered at the county level. 10\%*, 5\%**, 1\%***.

Table A.12: IV Estimates for the Effects of New Deal Relief on Longevity for Individuals Who Survived to Age 20

	Everyone		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var:	Log(Relief pc)	Log(Longevity)	Log(Relief pc)	Log(Longevity)	Log(Relief pc)	Log(Longevity)
Log(Relief pc)		0.012274*		0.023945***		0.004062
		(0.006266)		(0.008188)		(0.005834)
Severity Index	0.065005***	-0.001521***	0.064238***	-0.002177***	0.065892***	-0.000660
	(0.010845)	(0.000476)	(0.011012)	(0.000669)	(0.010689)	(0.000413)
Voting Culture Inst.	4.710916***		4.743662***		4.669262***	
	(0.905531)		(0.917978)		(0.892459)	
Constant	5.308176***	4.115610***	5.314023***	4.043570***	5.286042***	4.217644***
	(0.069115)	(0.034848)	(0.069994)	(0.045715)	(0.070916)	(0.032238)
Observations	26,262,797	26,262,797	14,270,707	14,270,707	11,992,090	11,992,090
R-squared	0.580991	0.026405	0.577450	0.045435	0.585218	0.013388
F- Stat	27.06		26.7		27.37	

Notes: The sample includes all white native individuals in the 1930 US Census linked to their FamilySearch deaths who survived to age 20. All specifications include state of birth and cohort fixed effects. Standard errors are clustered at county level. 10\%*, 5\%**, 1\%***.

2. 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 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 those data to create the datasets we used for our analysis. It will also describe match-rate outcomes at several levels, including geographic breakdowns at 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 high-quality, pre-cleaned, full-count US Census datasets from which we obtain the majority of our useful variables, such as a person's birth year and place of residence. Their full-count Census datasets identify individuals within a given 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 on large websites like FamilySearch.org, but it creates difficulties 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 data in 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).

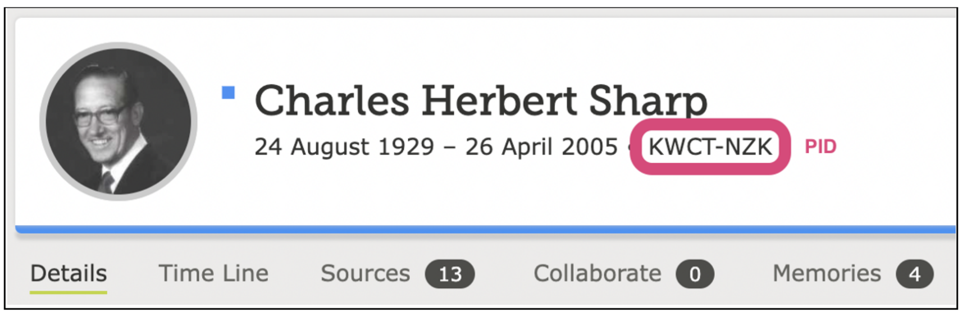
2. Use the Census Tree links to link the 1930 FamilySearch identifiers to the 1940 FamilySearch identifiers (ARK1930 → ARK1940).

3. Use another HIST-ARK crosswalk developed by the RLL to link 1940 FamilySearch identifiers to the 1940 IPUMS dataset (ARK1940 → HISTID1940).

Those three steps result in a linking process that uses RLL crosswalks and Census Tree links to go from HISTID1930 → ARK1930 → ARK1940 → HISTID1940, and thereby link 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, several 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 that show general success rates in matching at state and county levels are also presented in that section.

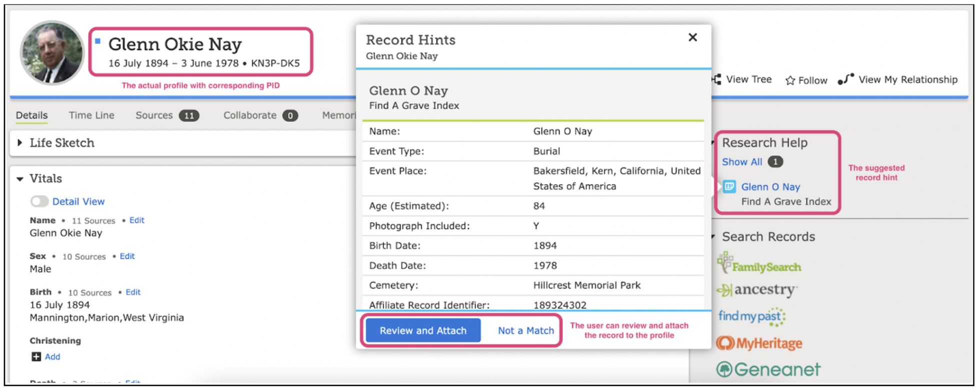
II. Linking individuals in the 1930 Census to their death information

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 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 such death information. As described above, FamilySearch indexes their Census records at 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 individuals, 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, which allows users to find and verify potential record matches with minimal effort. An example of one such record "hint" is presented below:

Importantly, the records a user might attach to a given profile can include both death records and ARK-indexed Census records, which yields 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 linking steps:



1. Use a HIST-ARK crosswalk developed by the RLL to link the 1930 IPUMS dataset to 1930 FamilySearch individual identifiers (HISTID1930 $\rightarrow$ 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 1930 FamilySearch identifiers to those people's profiles on the Family Tree (ARK1930 $\rightarrow$ 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 $\rightarrow$ 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 $\rightarrow$ ARK1930 $\rightarrow$ PID $\rightarrow$ 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 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 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. 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, 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; i.e., 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:

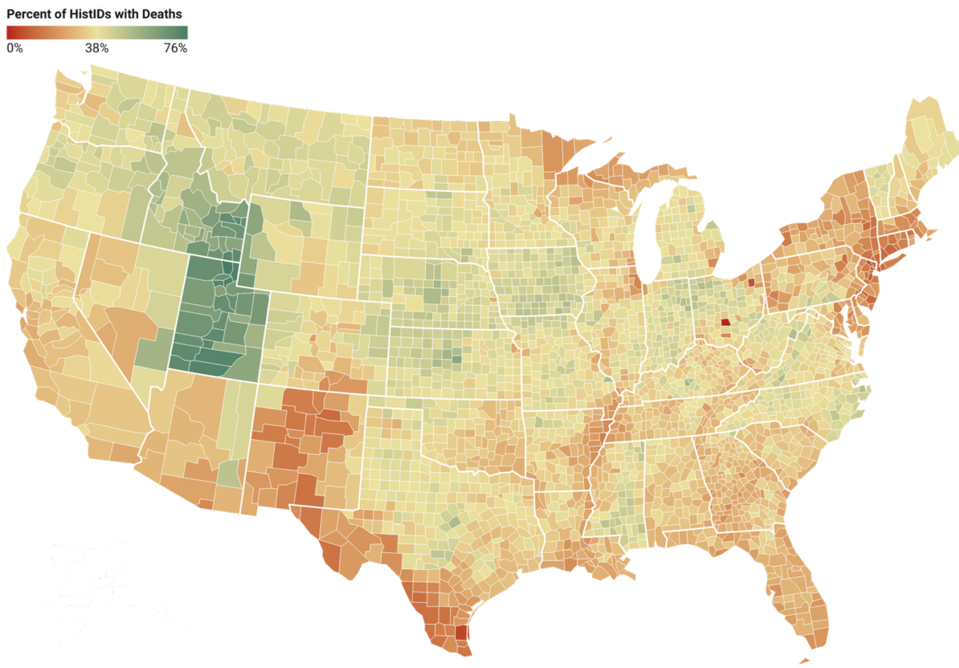
Matching Process	*Matching Success Rate*
HISTID1930 → HISTID1940	60.323%
HISTID1930 → Death Year	28.489%
HISTID1930 → HISTID1940 & Death Year	**22.659%**
Product of rows 1 & 2	**17.185%**

The fact that our HISTID1930 → HISTID1940 and 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 of the probability that a person matches to a death year.

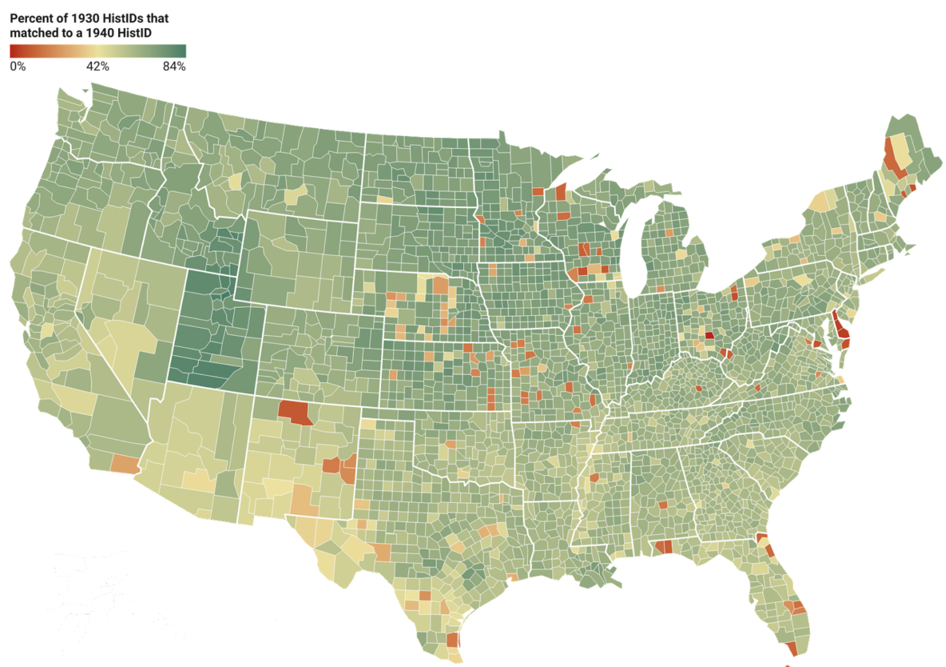
IV. Match rate breakdowns by county and birth year cohort

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.

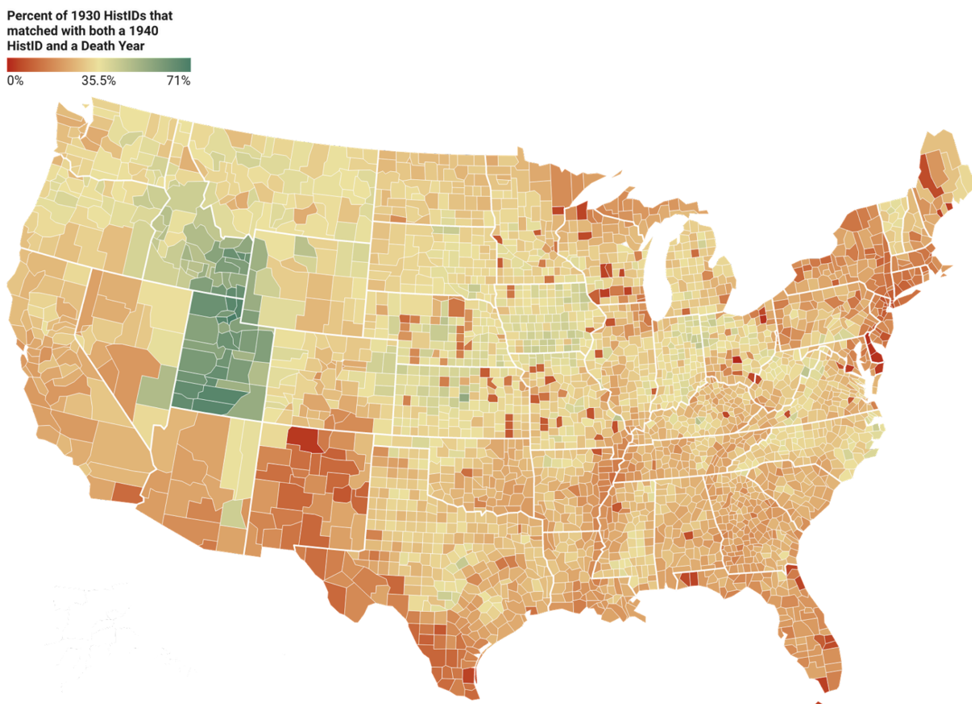
We also present a chart of match rates by birth year cohort beginning in 1880. We first examine variation in match rates at county level. Below are three choropleth maps showing match rates from HISTID1930 to death years, HISTID1930 to HISTID1940, and HISTID1930 to both death years and HISTID1940, respectively, 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 with other counties in the country. Next, we have 0% at the left 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 concern, we reason that the extremely 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 for our sample, even with relatively low match rates. For the other two areas of concern, 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 who trace 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:

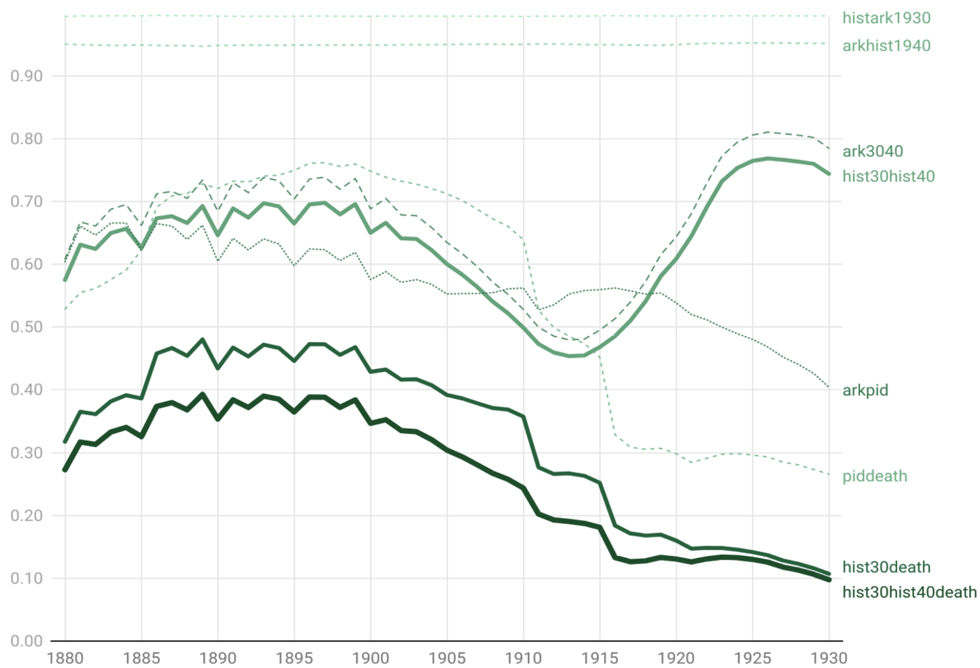


This map presents fewer immediate problems for our sample, though it is not free from areas of concern. The scattered distribution of red and orange counties on this map suggests that their lower match rates are more random than selected. Delaware's three counties are an obvious exception; for some reason,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

Appendix B

Appendix Materials for Chapter 2

1. Appendix Figures and Tables

This section of the appendix presents several robustness checks and full table versions of our main specifications in the paper. First, we show that our results are robust when we control for asset allocation controls. Table A-1 exhibits the estimates of the effect of RHWM on the change in the different health outcomes when controlling for the ratio of housing wealth over total net wealth and stock holdings over total new wealth in addition to the baseline demographic controls. Overall, this table shows that our results are robust to the inclusion of this controls.

Table B.1: Robustness control for asset allocation.

	Δ(SRH)	Δ(SRH)	RHWM	Δ(Mental ADLs)	Δ(Drug death rates)	Δ(Alcohol or suicide death rates)
	Ord. probit	IV (2nd stage)	IV (1st stage)	Ord. probit	OLS	OLS
	[1]	[2]	[3]	[4]	[5]	[6]
RHWM	0.02531***	0.02499*		-0.008965	-0.07399**	0.04086
	(2.87)	(1.84)		(-0.88)	(-2.06)	(1.47)
SE*IR			-0.006145***			
			(-3.2487)			
Healthy	-1.0082***	-1.0082***	-0.02141	-1.6479***	0.1379	-0.1235*
	(-44.66)	(-44.66)	(-1.05)	(-12.54)	(1.39)	(-1.66)
House value	0.03838***	0.03838***	0.007474	-0.03982***	0.05782*	0.07472***
	(5.36)	(5.36)	(0.37)	(-4.06)	(1.87)	(3.23)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Asset allocation controls	Yes	Yes	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,098	15,219	15,219	11,150	5,163	3,633

Notes: This table reports estimates of the effect of Realization of Housing Wealth Misestimation (RHWM) on the change in health outcomes when controlling for the ratio of housing wealth over total net wealth and stock holdings over total new wealth. All specifications include year fixed effects and division fixed effects. Specifications [1]-[3] show the estimates for self-reported health, Δ(SRH). Specification [1] is equivalent to the baseline ordered probit specification. Specifications [2] and [3] show the second and first stage IV regressions. Specification [4] shows the estimates for mental ADLs, Δ(Mental ADLs). Specifications [5] and [6] report the estimates for change in drug death rates and change in alcohol or suicide death rates, respectively. t-statistics are reported in parentheses. All the specifications include year and division fixed effects and all errors are clustered at the family level.

Second, we show that our results are robust when using SEER-adjusted population data instead of U.S. Census data.

Table B.2: Robustness check using population SEER data.

	Δ(Drug death rates)	Δ(Drug death rates)	Δ (Alcohol or suicide death rates)	Δ (Alcohol or suicide death rates)
	[1]	[2]	[3]	[4]
RHWM	-0.0771***	-0.0935***	0.00657	-0.0112
	(0.0277)	(0.0346)	(0.0187)	(0.0237)
Healthy	0.0919	0.0740	-0.134**	-0.154**
	(0.0807)	(0.111)	(0.0550)	(0.0779)
House Value	0.00313	0.0124	-0.0224	-0.00792
	(0.0235)	(0.0297)	(0.0166)	(0.0228)
PDMP Operational		0.297*		0.364***
		(0.168)		(0.132)
First marihuana law		1.177***		-0.00518
		(0.190)		(0.132)
Hospital beds rate		-0.0414		0.0588
		(0.0409)		(0.0375)
Δ manufact. employers		0.0452		0.0136
		(0.0485)		(0.0371)
Δ import exposure		-0.0475		-0.0776**
		(0.0434)		(0.0311)
Demographic Controls	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes
Observations	8,868	4,973	6,300	3,535

Notes: This table reports estimates of the effect of Realization of Housing Wealth Misestimation (RHWM) on the change in health outcomes when controlling for SEER-adjusted population data. Specifications [1]-[2] report the estimates for change in drug death rates and [3]-[4] report the change in alcohol or suicide death rates. Robust standard errors are reported in parentheses. All the specifications include year and division fixed effects and all errors are clustered at the family level.

Third, we show the full Table 3 including the coefficients of all the covariates.

Table B.3: Full Table 3 including all the covariates coefficients.

	Δ(SRH)	Δ(SRH)	Δ(Total ADLs)	Δ(Mental ADLs)	Δ(Drug death rates)		Δ(Alcohol or suicide death rates)	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
RHWM	0.0102*	0.0172**	0.000476	-0.0245**	-0.0742***	-0.0894***	0.00667	-0.0111
	(0.00562)	(0.00719)	(0.00867)	(0.0118)	(0.0276)	(0.0344)	(0.0186)	(0.0237)
Healthy	-1.147***	-1.174***			0.0868	0.0725	-0.134**	-0.154**
	(0.0172)	(0.0253)			(0.0808)	(0.111)	(0.0550)	(0.0778)
HVnew		0.0477***	-0.231***	-1.787***	0.00514	0.0137	-0.0222	-0.00776
		(0.00643)	(0.0234)	(0.198)	(0.0235)	(0.0297)	(0.0166)	(0.0228)
PDMP Operational						0.271		0.360***
						(0.168)		(0.132)
First marihuana law						1.172***		-0.00714
						(0.190)		(0.132)
Hospital beds rate mean						-0.0436		0.0591
						(0.0409)		(0.0375)
Δ manufacturing employers						0.0513		0.0143
						(0.0485)		(0.0371)
Δ import exposure						-0.0434		-0.0768**
						(0.0433)		(0.0311)
Age	-0.00560***	-0.00572***	0.00742***	-0.00772***	-0.00336	-0.000718	0.000436	0.00181
	(0.000505)	(0.000744)	(0.00127)	(0.00158)	(0.00363)	(0.00482)	(0.00250)	(0.00343)
Family Income	0.0803***	0.0359***	-0.0195	-0.00328	-6.32e-07	-1.18e-08	2.51e-07	5.85e-07
	(0.0167)	(0.0122)	(0.0137)	(0.0209)	(4.71e-07)	(6.29e-07)	(3.30e-07)	(4.89e-07)
Male	0.0249	-0.0424	0.105*	-0.102	-0.0327	-0.335	0.0542	0.0423
	(0.0219)	(0.0356)	(0.0582)	(0.0688)	(0.159)	(0.213)	(0.107)	(0.148)
Marital Status	-0.00150	0.0478	-0.0362	0.0954	0.0119	0.171	0.0334	0.0120
	(0.0219)	(0.0334)	(0.0549)	(0.0651)	(0.152)	(0.203)	(0.102)	(0.141)
Family Members	-0.0152***	-0.00956*	-0.00128	-0.00463	-0.0100	-0.00257	-0.00516	-0.0167
	(0.00363)	(0.00519)	(0.0100)	(0.0130)	(0.0297)	(0.0449)	(0.0197)	(0.0304)
Employed	0.127***	0.165***	-0.0902*	-0.397***	-0.109	-0.207	-0.0161	-0.0653
	(0.0153)	(0.0237)	(0.0478)	(0.0591)	(0.118)	(0.155)	(0.0807)	(0.111)
High School	0.130***	0.144***	-0.111**	-0.0632	-0.271*	-0.418**	-0.157	-0.382**
	(0.0151)	(0.0244)	(0.0503)	(0.0566)	(0.139)	(0.204)	(0.0984)	(0.149)
Nonwhite	-0.136***	-0.0858***	-0.0824**	-0.200***	-0.259***	-0.320**	-0.0643	-0.00184
	(0.0147)	(0.0209)	(0.0324)	(0.0441)	(0.0967)	(0.141)	(0.0669)	(0.0996)
Observations	62,449	35,427	12,069	15,294	8,879	4,979	6,300	3,535

Notes: This table reports estimates of the effect of Realization of Housing Wealth Misestimation (RHWM) on the change in health outcomes. All Specifications. include age control, year fixed effects and division fixed effects. Specifications [1] , [2] and [3], [4] show the estimates for the health outcome change in self-reported health (SRH), total ADLS and Mental ADLS using an ordered probit model. Specification [1] only includes as control variables the health status, age, and gender of the head of the household. Specifications [2], [3] and [4] add the house value as control, as well as all the demographic controls, which include family income, race (i.e., non-white dummy), education (i.e., dummy high school or more), employment (i.e., dummy employed), marital status (i.e., dummy married), and family members. These two ordered probit specifications include errors clustered at the family level. Specifications [5] and [6] report the estimates for the health outcome change in drug death rates. Specifications [7] and [78 report the estimates for the health outcome change in drug death rates. Specifications [5]-[8] control for urban-rural categories. Standard errors are reported in parentheses.

Fourth, the following table shows results with the choice of a four-year lag to explore longer run effects as opposed to the two-year changes that we use in our main analyses.

Table B.4: The effects of RHWM on four-year health outcomes.

	Δ(SRH)	Δ(SRH)	Δ(Total ADLs)	Δ(Mental ADLs)	Δ(Drug death rates)		Δ(Alcohol or suicide death rates)	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
RHWM	0.00836	0.0154**	0.00332	-0.0181**	-0.0452	-0.0305	-0.00197	-0.0292
	(0.00556)	(0.00709)	(0.00979)	(0.00854)	(0.0320)	(0.0390)	(0.0227)	(0.0287)
Healthy	-1.370***	-1.409***	-0.488***	-2.637***	-0.0544	-0.0697	-0.00246	0.0277
	(0.0181)	(0.0262)	(0.0717)	(0.249)	(0.103)	(0.127)	(0.0664)	(0.0856)
HVnew		0.0489***	-0.0366***	-0.0636***	-0.0149	-0.0229	-0.0655***	-0.0372
		(0.00791)	(0.0118)	(0.0133)	(0.0299)	(0.0341)	(0.0197)	(0.0252)
PDMP Operational						1.104***		1.165***
						(0.191)		(0.146)
First marihuana law						0.171		-0.939***
						(0.214)		(0.142)
Hospital beds rate mean						-0.338***		0.0460
						(0.0469)		(0.0394)
Δ manufacturing employers						0.105*		-0.228***
						(0.0565)		(0.0397)
Δ import exposure						-0.0175		-0.162***
						(0.0501)		(0.0337)
Observations	53,582	30,369	7,227	13,284	7,438	4,909	5,379	3,522

Notes: This table reports estimates of the effect of Realization of Housing Wealth Misestimation (RHWM) on the change in health outcomes after 4 years. All Specifications. include age control, year fixed effects and division fixed effects. Specifications [1] , [2] and [3], [4] show the estimates for the health outcome change in self-reported health (SRH), total ADLS and Mental ADLS using an ordered probit model. Specification [1] only includes as control variables the health status, age, and gender of the head of the household. Specifications [2], [3] and [4] add the house value as control, as well as all the demographic controls, which include family income, race (i.e., non-white dummy), education (i.e., dummy high school or more), employment (i.e., dummy employed), marital status (i.e., dummy married), and family members. These two ordered probit specifications include errors clustered at the family level. Specifications [5] and [6] report the estimates for the health outcome change in drug death rates. Specifications [7] and [78 report the estimates for the health outcome change in drug death rates. Specifications [5]-[8] control for urban-rural categories. Standard errors are reported in parentheses.

Fifth, we run a robustness check of the effect of the housing market cycles on changes in health using the continuous measure in Saiz (2010).

Table B.5: Effects of housing supply constraints and the housing market cycles using the continuous measure of housing supply elasticity in Saiz (2010).

	Increasing house prices			Decreasing house prices		
	Δ(SRH)	Δ(Mental ADLS)	Δ(Drug death rates)	Δ(SRH)	Δ(Mental ADLS)	Δ(Drug death rates)
	Ord. Probit	Ord. Probit	OLS	Ord. Probit	Ord. Probit	OLS
	[1]	[2]	[3]	[4]	[5]	[6]
Inelasticity, negative elasticity	0.1011	-0.05761	-0.9320***	-0.04488	0.06287	0.03484
	(-1.1267)	(-0.9107)	(-9.1846)	(-1.5734)	(1.5530)	(0.4430)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year and division FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,252	1,193	2,148	2,763	2,631	2,349

Notes: This table reports the effects of housing supply constraints during periods of sharp increasing house prices (booms) and periods of sharp decreasing house prices (busts). Errors are clustered at the family level in all the specifications. This table presents the equivalent results to Table 2.6 when using the continuous measure of housing supply elasticity in Saiz (2010). The estimates are robust to our main specifications, but less statistically significant.

In this section of the Appendix, we discuss the results of the analysis of the variance in household misestimation explained by household (family) fixed effects and by year fixed effects. Our goal is to understand how much of the variation in house wealth misestimation comes from *within* versus *between* households. First, Table B.6 shows that the standard deviation of misestimation is much larger by individual household ID than by year.

Additionally, Table B.7 shows the R-squares of different OLS specifications using (or not) covariates, individual FE, and year FE. These results show that the proportion of the variance in misestimation explained when using individual household fixed effects is more than 20 times larger than when using only year fixed effects. This result remains true when we add the covariates.

Finally, we perform an ANOVA analysis of house value misestimation. Table B.8 shows that the variation across family IDs is substantial. F indicates that the variation between is 5.98 times the variation within household ID. Therefore, we include fixed effects in all our specifications in order to take into account these large variation among families (i.e., the *between* effect).

Table B.6: Analysis of the variation in house value misestimation. Summary statistic average of misestimation (by household ID and year).

	Mean	St.Dv.	Obs.
House Wealth Misestimation (HWM)	0.768	21.965	58,701
Average HWM by ID	0.462	11.089	116,413
Average HWM by Year	0.623	2.168	123,760

Notes: This table reports the summary statistics of the variable house wealth misestimation (HWM) in aggregate terms, by household ID, and by year.

Table B.7: Analysis of the variation in house value misestimation. Analysis of within and between R-squared using our simple OLS approach (no panel).

	[1]	[2]	[3]	[4]	[5]	[6]
R-squared	0.012	0.374	0.386	0.017	0.394	0.405
Covariates	No	No	No	Yes	Yes	Yes
Individual FE	No	Yes	Yes	No	Yes	Yes
Year FE	Yes	No	Yes	Yes	No	Yes
Observations	58,701	58,701	58,701	53,196	53,196	53,196

This table shows the results of the analysis of the variation in House Wealth Misestimation (HWM) using an OLS approach. The dependent variable in all the specifications is HWM. Specifications [1]-[3] do not include covariates, while [4]-[6] do include them. Specifications [2]-[3] and [5]-[6] include household individual fixed effects (FE). Specifications [1], [3], [4], and [6].

Table B.8: Analysis of the variation in house value misestimation. ANOVA.

Source	Partial SS	df	MS	F	Prob.>F
Model	10,602,331	5,338	1,986.20	5.98	0
Residual	17,717,600	53,362	332.02		
Total	28,319,931	58,700	482.45		
Observations	58,701				
R-squared	0.374				

This table shows the results of the one-way ANOVA test using individual household ID. It compares the variance between and within individual households. Let F denote the variation *between* the sample mean squares, MS, of the model divided by the variation *within* the sample MS.

Appendix C

Appendix Materials for Chapter 3

1. Higher Education Programs and Legislation

Below, we summarize four important programs that made changes to the higher education sector in Brazil in the last two decades.

Fundo de Financiamento ao Estudante do Ensino Superior (FIES) is a federal program that gives loans with subsidized interest rates to students attending on-site undergraduate courses at private universities. The resources are transferred directly to the universities to pay for tuition. The program was regulated in 1999 and expanded in 2010.

Programa Universidade para Todos (PROUNI) is a federal program that provides full and partial scholarships in private universities to low-income students. Part of the scholarships is reserved for people with disabilities, indigenous and black or brown. The program was established in 2005.

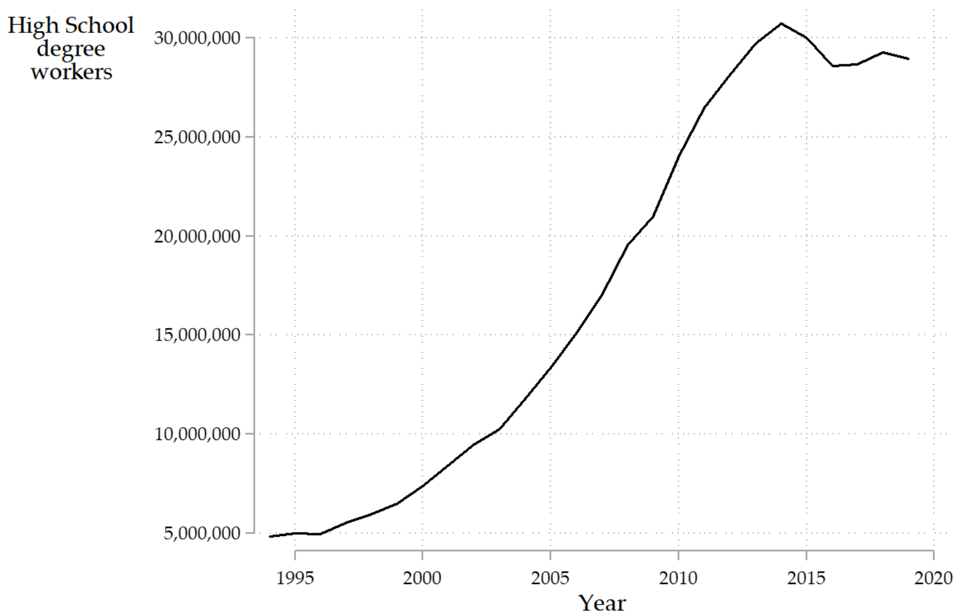
Lei de Cotas is a legislation that implemented racial and social quotas in all federal universities. The law establishes that 50% of the vacancies in federal universities are reserved to people with disability, low-income people and indigenous, black and brown people, accordingly to the proportion of each group in the university's state. The law was passed in

2012.

Programa de Apoio a Planos de Reestruturação e Expansão das Universidades Federais (REUNI) was a federal program that aimed the creation of more vacancies in federal universities. The program financed the renovation of university buildings and the construction of new buildings. The program started in 2007.

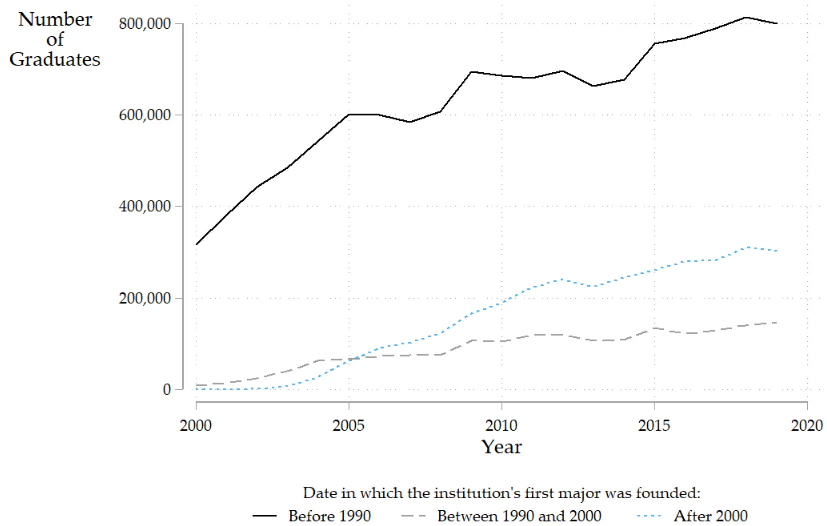
2. Appendix Figures and Tables

Figure C.1: Formal workers with a high school degree



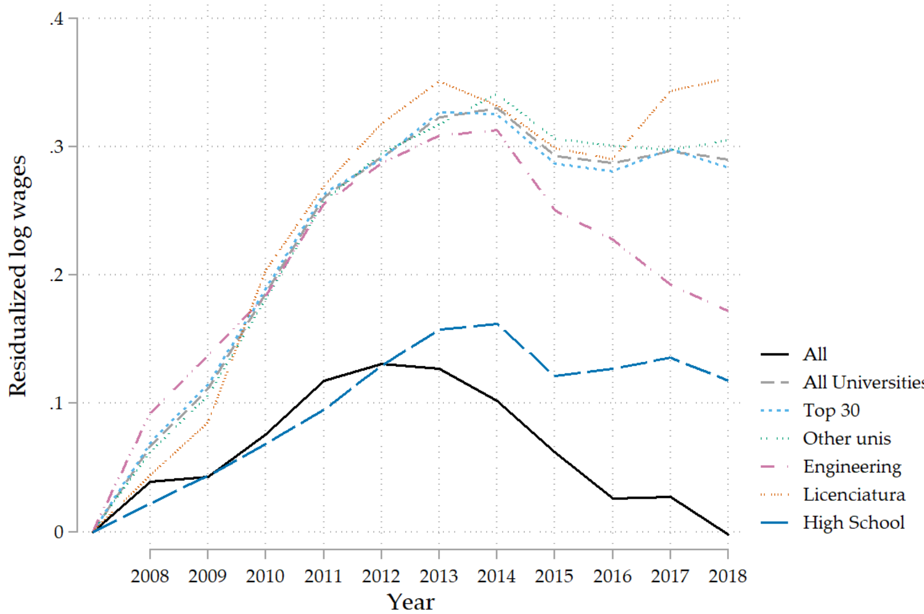
Note: The figure plots the trends of the number of formal job relations with positive wage in which workers had a high school degree. Data: RAIS 1994-2019.

Figure C.2: Number of graduates by institution's date of foundation



Note: The figure presents the number of students that graduated in each year. Categories are defined by the year in which the institution's first major was founded. Source: Higher Education Census (2000-2018)

Figure C.3: Trends in residualized log wages



Note: The figure plots the evolution of earnings, relative to 2007 values, for different samples. Estimates come from the estimation of Equation 1 over a sample of 263,181,905 observations.

Table C.1: Schools with access to the data

Universidade/ Instituto	Acronym
Centro Federal de Educação Tecnológica Celso Suckow da Fonseca	CEFET-RJ
Fundação Universidade do Amazonas	UFAM
Fundação Universidade Federal de Mato Grosso	UFMT
Fundação Universidade Federal de Ouro Preto	UFOP
Fundação Universidade Federal de Pelotas	UFPel
Fundação Universidade Federal de Rondônia	UNIR
Fundação Universidade Federal de Roraima	UFRR
Fundação Universidade Federal de São João Del Rei	FUNRei
Fundação Universidade Federal de Sergipe	UFS
Fundação Universidade Federal do ABC	UFABC
Fundação Universidade Federal do Acre	UFAC
Fundação Universidade Federal do Maranhão	UFMA
Fundação Universidade Federal do Tocantins	UFT
Fundação Universidade Federal do Vale do São Francisco	UNIVASF
Instituto Federal de Educação, Ciência e Tecnologia do Rio de Janeiro	IFRJ
Instituto Federal de Educação, Ciência e Tecnologia Fluminense	IFF
Instituto Militar de Engenharia	IME
Universidade de Brasilia	UNB
Universidade do Estado do Rio de Janeiro	UERJ
Universidade Estadual de Maringa	UEM

Table C.1: Schools with access to the data (cont.)

Universidade/ Instituto	Acronym
Universidade Estadual Paulista Júlio de Mesquita Filho	UNESP
Universidade Federal de Alagoas	UFAL
Universidade Federal de Alfenas	UNIFAL
Universidade Federal de Campina Grande	UFCG
Universidade Federal de Goiás	UFG
Universidade Federal de Itajubá	UNIFEI
Universidade Federal de Juiz de Fora	UFJF
Universidade Federal de Lavras	UFLA
Universidade Federal de Minas Gerais	UFMG
Universidade Federal de Santa Catarina	UFSC
Universidade Federal de Santa Maria	UFSM
Universidade Federal de Uberlândia	UFU
Universidade Federal de Vicosa	UFV
Universidade Federal do Ceará	UFC
Universidade Federal do Espírito Santo	UFES
Universidade Federal do Estado do Rio de Janeiro	UNIRIO
Universidade Federal do Pará	UFPA
Universidade Federal do Rio de Janeiro	UFRJ
Universidade Federal do Rio Grande do Norte	UFRN
Universidade Federal do Sul e Sudeste do Pará	UNIFESSPA
Universidade Federal Rural do Rio de Janeiro	UFRRJ
Universidade Tecnológica Federal do Paraná	UTFPR
Total	42

Table C.2: Sample size by university

University	Students	Matched to RAIS	% Matched
CEFET-RJ	30,446	23,029	75.60%
FUNRei	2,971	2,370	79.80%
IFF	16,387	10,682	65.20%
IFRJ	1,896	1,336	70.50%
IME	2,558	2,042	79.80%
UEM	53,160	40,605	76.40%
UERJ	72,562	56,330	77.60%
UFA	439	331	75.40%
UFAL	37,418	28,050	75.00%
UFABC	2,271	1,864	82.10%
UFAM	2,544	1,975	77.60%
UFC	65,155	51,206	78.60%
UFCG	32,647	23,678	72.50%
UFES	49,408	39,269	79.50%
UFG	46,335	36,233	78.20%
UFJF	31,459	23,471	74.60%
UFLA	13,231	9,480	71.60%
UFMA	1,851	1,511	81.60%
UFMG	99,656	75,839	76.10%
UFMT	45,547	35,156	77.20%
UFOP	27,024	21,423	79.30%
UFPA	47,025	35,051	74.50%
UFPel	24,189	16,107	66.60%
UFRJ	105,380	78,696	74.70%

Table C.2: Sample size by university (cont.)

University	Students	Matched to RAIS	% Matched
UFRN	81,761	50,166	61.40%
UFRR	325	259	79.70%
UFRRJ	13,395	10,003	74.70%
UFS	37,705	29,139	77.30%
UFSC	53,713	41,741	77.70%
UFSM	7,272	5,457	75.00%
UFT	1,537	1,101	71.60%
UFU	25,170	13,893	55.20%
UFV	26,383	18,231	69.10%
UNB	62,106	45,813	73.80%
UNESP	3,077	2,639	85.80%
UNIFAL	10,573	6,766	64.00%
UNIFEI	6,553	5,070	77.40%
UNIFESSPA	4,701	3,398	72.30%
UNIR	18,813	15,192	80.80%
UNIRIO	19,464	14,887	76.50%
UNIVASF	4,405	3,138	71.20%
UTFPR	28,928	23,793	82.20%
Total	1,217,440	906,420	74.50%

Table C.3: Comparing Matching Algorithms

Algorithm	Hyper Parameters		Algorithm Quality			
			Training sample (50%)		Test sample (50%)	
	b1	b2	PPV	TPR	PPV	TPR
Logit	0.1	1.25	87.10%	92.00%	87.20%	92.20%
	0.05	1	86.70%	92.50%	87.10%	93.00%
Random Forest	0.1	1.25	96.60%	97.20%	88.80%	90.30%
	0.05	1	96.00%	98.70%	86.70%	92.70%

Note: Hyper parameters b1 and b2 are the threshold for whether the match's score is sufficiently large and the threshold for whether the ratio between the best and the second-best scores is sufficiently large, respectively. Positive Predictive Value (PPV or Accuracy): number of true positives over total number of positives. True Positive Rate (TPR or Efficiency): number of true positives over total number of correct cases.

Table C.4: Confusion Matrix—Out of Sample Predictions

Algorithm Prediction	True Status		Total
	False	Correct	
Not Matched	13,613	691	14,304
Matched	1,362	8,900	10,262
Total	14,975	9,591	24,566

Note: The table presents the sample sizes from the logit model with b1=0.05 and b2=1.1. Positive Predictive Value (PPV or Accuracy): number of true positives over total number of positives = 8900/(8900+1362) = 86.7%. True Positive Rate (TPR or Efficiency): number of true positives over total number of correct cases = 8900/(8900+691)= 92.8%.

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