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Social insurance programs and later-life mortality: Evidence from new deal relief spending☆

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ABSTRACT

A growing body of research explores the long-run effects of social programs and welfare spending. However, evidence linking welfare support in early life with longevity is limited. We add to this literature by evaluating the effect of in-utero and early-life exposure to the largest increases in welfare spending in the US history under the New Deal programs. Using Social Security Administration death records linked with the 1940-census and spending data for 115 major cities, we show that the spending is correlated with improvements in old-age longevity. A treatment-on-treated calculation focused on a period when spending rose by approximately 1900 percent finds that a 100 percent rise in municipal spending in the year of birth is associated with roughly 3.5 months higher longevity. We show that these effects are not driven by endogenous selection of births, selective fertility, endogenous migration, and sample selection caused by endogenous data linking. Additional analysis suggests that rises in education and socioeconomic status are likely channels of impact.

1. Introduction

Economic depression can have long-lasting effects, particularly for vulnerable populations. Infants and those in-utero are specifically more susceptible to external stressors. Dire economic situations and adverse shocks can influence their initial health endowment, which, in turn, changes the trajectory of their later-life outcomes (Almond et al., 2018; Almond and Currie, 2011b; Barker, 1994, 1997; Currie, 2009, 2011). Studies find that adverse economic conditions during in-utero, infancy, and childhood influence adult health, hospitalization, education, labor market outcomes, and old-age health and mortality (Flores and Kalwij, 2014; Freedman et al., 2008; Lillard et al., 2015; Montez and Hayward, 2011; Scholte et al., 2015; Sotomayor, 2013). Social welfare programs have the potential to offset the negative shocks and boost short-term and long-term health outcomes. Some social insurance programs, such as Medicaid, are designed as a health policy intervention. Others may target economic security, yet their spillover and unintended externalities may also be observed in health outcomes (Boyd-Swan et al., 2016; Hoynes et al., 2015; Markowitz et al., 2017; Nelson and Fritzell, 2014;

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Noghanibehambari and Salari, 2020; Salm, 2011). These studies usually find evidence that the interventions are more effective among populations of color, people of lower socioeconomic status, and those who are unemployed, and that the effects are more pronounced when the economy is going through a downturn (Kuka, 2020; Wehby et al., 2020).

During the 1930s, Americans experienced the greatest downturn in their economic history. The infamous Great Depression left millions of people unemployed and caused sharp and unprecedented reductions in income. The deep recession limited access to necessary material resources, as was evident by reports on children's malnutrition and bread lines on the streets (McElvaine, 1993). The federal government responded to the situation by establishing programs to stimulate the economy, boost confidence in the market, and provide material assistance to unemployed persons and impoverished families. The Roosevelt administration took a series of initiatives to promote social welfare. The Social Security Act of 1935 complemented earlier small-scale programs to support infants, children, and older adults. Welfare spending under Roosevelt's New Deal programs revolutionized the structure of social insurance and safety net in the US. Per capita spending increased by twenty-fold between the years 1929–1938. Despite the enormous scale of the Great Depression and unparalleled increases in welfare spending, the economics literature on the health-related effects of New Deal spending is quite limited (see Fishback (2017), for a review). Furthermore, very few studies explore the longer-run effects of welfare spending on health outcomes and specifically old-age mortality. This study fills this gap in the literature by documenting the association between in-utero and early-life exposure to the New Deal relief spending on subsequent longevity.

We use death records from the Social Security Administration linked with the full-count 1940 census. The data provides us with granular and detailed geographic level data for the early-life period in addition to a full battery of family characteristics. We also employ city-level data on welfare spending and grants allocation before and during the New Deal era (1929–1940) for 115 major cities across the US. The spending data cover programs providing assistance to poor families, unemployed workers, and other populations in need via the Old-Age Assistance (OAA) program, Aid to Dependent Children (ADC), Aid for the Blind, private donations, direct relief assistance, work relief assistance, and payments through the Works Progress Administration (WPA). These combined programs constitute an increase of twentyfold in total assistance per capita between the years 1929–1938, an unprecedented rise in welfare spending in American history. We implement panel data fixed effect models and take advantage of city-level differences in deviations from city-specific trends to compare the longevity of individuals born within the same region and birth year. The results suggest considerable improvements in longevity. Conditional on a full set of covariates, an increase of 100 percent in municipal per capita spending is correlated with 1 month higher longevity for those born in the city in that year.

We argue that these effects are not driven by selection of births based on observable individual and parental characteristics. We show that the effects are not driven by changes in the composition of births and selective fertility. Additional tests do not provide concerning evidence that Numident-census linkage of observations induces an endogenous demographic change in the final matched sample. We also implement a series of placebo tests in which we assign spending to people aged 10 and 15, children whose longevity may have benefited less than those who experienced increased spending in-utero and during early-life. These tests search for the association between welfare spending and overall trends in population health, medical care use, and other health-related technologies that could be detected in old-age longevity. The placebo tests fail to provide evidence of such associations, supporting the robustness of the effect of in utero exposure to higher social welfare spending on subsequent mortality.

To search for pathways, we implement two sets of analyses. First, we complement the literature that documents the early-life influences on adult outcomes, including education and income (Almond et al., 2018; Almond and Currie, 2011b; Currie, 2009; Garces et al., 2002; Goodman-Bacon, 2021). We use census and American Community Survey data and focus on similar birth cohorts and spending at the state-year-of-birth. We show that in-utero and early-life spending is associated with significant improvements in educational outcomes, measures of the socioeconomic index, and family income. Second, we posit that these improvements could lead to better health outcomes throughout one's life cycle and be detected in old-age mortality outcomes. While this link can be inferred from the bulk of studies that establish a link between education-income profiles and old-age mortality, we implement a series of two-sample two-stage-least-square analyses that reveal the importance of improvements in education-income as channels of impact.

This paper makes several important contributions to the literature. First, to our knowledge, this is among the first studies to examine the effects of welfare spending and the introduction of New Deal programs during in-utero and early-life on old-age longevity, and the first to do so in a geographically and demographically diverse sample (Modrek et al., 2022). As most of welfare spending during the Great Depression did not specifically target pregnant women and new-born children, our findings shed light on the potential long-run externalities of broad welfare programs and add to the growing literature exploring the health externalities of non-health social spending (Bailey et al., 2016; Braga et al., 2020; Classen and Dunn, 2012; Duflo, 2000; Hoynes et al., 2015; Kuka, 2020; Noghanibehambari and Salari, 2020; Tefft, 2011; Wehby et al., 2020) and the long-term economic and health outcomes of early-life exposures (Almond et al., 2018; Almond and Currie, 2011a, 2011b; Barker, 1990, 1994, 1997, 2004; Barker et al., 1989, 1993, 2002; Campbell et al., 2014; Goodman-Bacon, 2021; Hayward and Gorman, 2004; Heckman, 2007; Hoynes et al., 2016). The Great Depression exercised a universal and deep effect on local economies across the nation, and the cohorts of the 1930s were uniquely exposed to both the long-term economic hardships associated with the Great Depression and the unprecedented scale of the New Deal policy response during a critical period of their development. This economic shock and the accompanying social spending were the largest in American history and provide an unprecedented and newly relevant opportunity for examining the long-term impacts of large-scale government relief spending on a key summary measure of population health – longevity. Mortality is among the most accurately captured measures of health and several studies suggest that it is correlated with other old-age health measures (Buchman et al., 2012; Lubitz et al., 2003; Mathers et al., 2001). Exploring the linkages between spending and subsequent longevity also provides opportunities to extend research into the Fetal Origins Hypothesis by documenting the relevance of exposure to social spending while in-utero and in early-life on later-life health and longevity.

The rest of the paper is organized as follows. In section 2, we provide a background on New Deal programs. In section 3, we review the literature. In section 4, we review the data and sample construction. In section 5, we discuss the empirical method. In section 6, we go over the results. In section 7, we conclude the paper.

2. Background: the great depression and the new deal

The Wall Street Crash of 1929 marked the beginning of the darkest period of US economic history. By 1933, real GDP had dropped by more than 30 percent, and the unemployment rate reached 20 percent (Fishback, 2010, 2017). As shown in Fig. 1, commodity prices and real GDP fell sharply during this period, and it took the economy almost a decade to return to the pre-1929 values. The dire economic conditions sharply increased the demand for local government aid and private donations. As unemployment rose and the recession reached its trough, the capacity of local welfare spending was capped. The situation called for federal interventions, and the Republican Hoover administration responded by expanding grants, loans, and welfare spending. A prominent example was the introduction of the Reconstruction Finance Corporation, which provided loans to banks, railroads, and mortgage agencies with the purpose of stimulating the economy. As a result, per capita welfare spending increased from \$50 in 1929 to \$506 in 1932 (in 2020 dollars, see Table 1).

However, it was Roosevelt and the accompanying Democratic congress that engineered New Deal welfare programs and established a spending structure and eligibility rules that are recognized as a landmark in the welfare history of America. Roosevelt's administration introduced various relief programs primarily targeting unemployed workers, nonworking poor, and needy families through cash transfers and spending on essential items such as food and health care. While several programs were created by the federal government, others were extensions of established local and state programs that operated on smaller scales prior to the New Deal.

Between 1933–1935, during the so-called first New Deal, the federal government established the Federal Emergency Relief Administration (FERA), through which funds and grants were distributed to state authorities. The distribution of funds depended on states' economic conditions, their leaders' political connections, and each state's own contribution to relief spending. FERA offered direct relief that provided cash and in-kind assistance to the poor in the forms of food stamps, food distribution, and school lunches. FERA also triggered Work Relief Assistance (WRA) which provided temporary aid to those in the labor force and unemployed. Although federally funded, these programs were administered locally. Local officials examined applications on a case-by-case basis, and the provision of assistance was based on a *budget-deficit* rule. Officials operationalized this principle by evaluating the required subsistence budget for specific family size and calculating the deficit the family face according to their income. Between the years 1933–1934, FERA's initiatives were supplemented by Civil Works Administration (CWA) funding with the purpose of job creation,

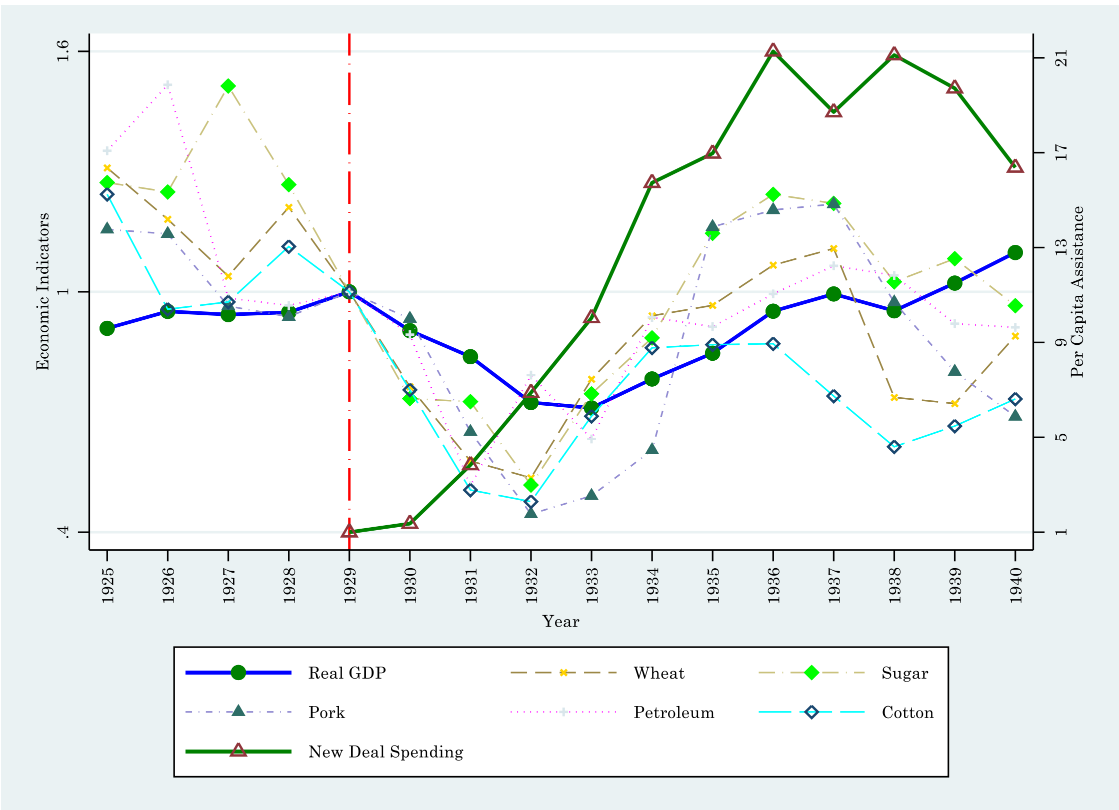

Fig. 1. Changes in economic conditions and new deal relief sending during the great depression.

Table 1
- Elements and Trends in Relief Spending in 115 Major Cities between the Years 1929-1940.

Year	***Per capita relief spending in 2020 real dollars ($100)*** Direct Relief Assistance	Work Relief Assistance	Civil Work Administration	Work Progress Administration	Old-Age Assistance	Aid to Dependent Children	Aid to Blind	Private Assistance	Total (public + private) assistance	Log Total Assistance
1929	.05	0	0	0	0	.04	0	.04	.19	2.47
1930	.13	0	0	0	.01	.05	.01	.05	.33	2.87
1931	.23	.08	0	0	.03	.07	.01	.12	.64	3.53
1932	.56	.16	0	0	.05	.09	.01	.16	1.17	4.18
1933	1.24	.66	.45	0	.07	.09	.02	.08	2.78	5.04
1934	1.56	.87	1.27	0	.05	.08	.01	.04	4.04	5.57
1935	2.04	1.09	.03	.72	.18	.08	.01	.04	4.45	5.54
1936	.78	.01	0	3.04	.24	.09	.02	.03	4.56	5.72
1937	.88	0	0	3.88	.59	.25	.02	.08	6.55	5.66
1938	0	0	0	3.8	1.07	.24	.03	.04	7.41	6.02
1939	0	0	0	2.77	.67	.21	.04	.03	5.59	5.94
1940	0	0	0	2.04	.69	.25	.03	.03	4.73	5.8

mostly for blue-collar workers. In 1935, the federal government centralized these programs under the Works Progress Administration (WPA), which granted assistance to those deemed *employable*. Those in need and not eligible for the WPA could still benefit from other federal/state/local funding that were administered locally.

The passage of the Social Security Act in 1935 created new welfare programs and extended smaller-scale earlier programs, including Aid to Dependent Children (ADC) (which replaced states' mothers' pensions or cash transfers to impoverished single mothers), Old Age Assistance, and Aid to the Blind. These programs were administered by states, with federal funds in the forms of grants-in-aid poured into states' reserves on a matching basis. Unlike other relief spending programs that were tied to unemployment rates and diminished as the economy recovered toward the end of the 1930s, these welfare spending remained in effect.

Overall, total spending rose from roughly $50 per capita in 1929 to a peak of about $1000 per capita in 1938 and then declined slightly to approximately $800 in 1940 (in 2020 dollars, see Table 1).

3. Welfare spending and subsequent health: literature review

A growing literature emphasizes the relevance of in-utero and early-life shocks for health outcomes in infancy, childhood, and later-life (Caruso, 2017; Chevalier and Marie, 2017; Cobb-Clark and Zhu, 2017; Cook et al., 2019; Elgar et al., 2017; Fletcher, 2018a, 2018b; Fletcher et al., 2010; Margerison-Zilko et al., 2017; Noghanibehambari, 2022; Sanders, 2012; Strand and Kunst, 2006; Van Den Berg et al., 2006, 2009, 2011, 2015). For instance, Scholte et al. (2015) documented that those who experienced the Dutch Hunger Winter of 1944–1945 while in-utero had a lower probability of being employed in adulthood and higher hospitalization rates at older ages. Almond and Mazumder (2011) showed that infants of Muslim mothers who were exposed to Ramadan, the holy month in which Muslims abstain from eating from sunrise to sunset, have lower birth weight compared with Muslim infants not exposed to intermittent fasting in utero. They argued that both the alteration in nutritional intake and the reduction in overall caloric intake may be responsible for affecting fetal health and development. Sotomayor (2013) explored the effect of exposure to tropical storms in-utero on later-life health outcomes. Consistent with the Fetal Origins Hypothesis, they found that exposed cohorts are more likely to be diagnosed with diabetes and hypertension. Although many studies highlight the negative impact of deleterious early-life exposures, other studies fail to detect an association (Cutler et al., 2007; Myrskylä, 2010). For instance, Cutler et al. (2007) explored the effects of large and unprecedented drops in income during America's Dust Bowl Era on later-life health outcomes. They did not find an association between in-utero exposure to income shocks and later-life disability and chronic disease.

Social welfare spending and safety net programs have the potential to alleviate the negative impact of damaging exposures either directly by the program's design or indirectly as spillover effects since the strong kinship ties and family bonds allow for spending in one member to benefit other members. Therefore, investments in different populations through welfare programs should not be viewed as a zero-sum game. For instance, Noghanibehambari and Salari (2020) showed that payments through Unemployment Insurance program has positive effects on birth outcomes. Hoynes et al. (2015) documented that tax rebates under the Earned Income Tax credit program improve birth outcomes. Duflo (2000) investigated the spillover effects of expansions in an Old Age Pension program in South Africa, a policy tool to help economically disadvantaged old-aged people, on children's nutrition and health. They found that the assistance increased available material resources which, in turn, increased children's nutritional intake and improved their health outcomes. Almond et al. (2011) showed that the introduction of Food Stamp program during the 1960s-70 s as an initiative to combat poverty improved infants' health outcomes. Hoynes et al. (2016) documented that early-life exposure to the introduction of the Food Stamps program is associated with reductions in metabolic syndrome and improvements in women's self-sufficiency. However, some research also suggests that the spillover effects of social spending on children's later-life outcomes may be negative. For instance, Dahl and Gielen (2021) investigated the intergenerational spillover effects of a Dutch reform that restricted eligibility for and generosity of a Disability Insurance (DI) program. They found that reductions in payments increased children's education, raised their earnings during adulthood, reduced their use of prescribed mental health drugs, and reduced the probability of being incarcerated. They posited that the reductions in DI payments to parents may have changed children's views towards work and assistance which led to improved labor market outcomes.

Studies of the New Deal's health benefits – particularly over the longer run – are limited. One notable exception is the study of Modrek et al. (2022). They employed the Wisconsin Longitudinal Study (WLS) data and explored the effect of neighborhood work-relief activities in 1940 as a proxy for the concentration of the New Deal relief spending on later-life outcomes. The authors focused on exposure of children aged 0–3 and a wide array of adult outcomes. They found significant impacts on IQ scores and education. However, their findings on midlife income were mixed. Moreover, they fail to detect significant impacts on mortality. The WLS data focuses on white people who were in Wisconsin in 1940 and who had at least a high school education. Therefore, they are relatively better educated and come from higher socioeconomic families. This homogeneity in WLS is probably one reason the authors could not observe an effect on mortality outcomes. In contrast, our sample covers a more demographically, geographically, and socioeconomically diverse sample, allowing us to detect significant effects. Arthi (2018) documented the long-lasting negative effects of in-utero and early childhood exposure to the Dust Bowl. She found that exposed cohorts have higher rates of poverty and disability during adulthood, but spending during New Deal mitigates the long-term negative effects. Galofré Vilà (2020) explored the effects of payments under Aid to Dependent Children (ADC) during the Great Depression on mortality outcomes. He found that expansions in ADC from mothers' pensions decreased infant and adult mortality rates. Fishback et al. (2007) explored the effects of cumulative welfare spending in major American cities during the Great Depression. They found sizeable improvements in infant mortality rates, reductions in all adult suicide rates, and increases in fertility rates. Kitchens (2013) found that roughly 44 percent of observed reductions in Georgia's malaria rates can be attributable to the Works Progress Administration (WPA) spending.

However, other studies of welfare yielded inconclusive evidence. For instance, Stoian and Fishback (2010) investigated the effects of means-tested old-age assistance funding before and after the Social Security Act of 1935 on mortality among older adults and found no meaningful associations. In contrast, Balan-Cohen (2009) employed an instrumental variable strategy and focused on Old Age Assistance payments during the years 1930–1950 and found sizeable reductions in old age mortality associated with the benefit receipts.

Welfare spending-induced early-life improvements can operate through several channels to affect longevity and mortality. Several studies show that health endowment at birth and cumulated acquired childhood health can affect later-life education and income (Behrman and Rosenzweig, 2004; Black et al., 2007; Case et al., 2005; Cook et al., 2015; Goodman et al., 2011; Maruyama and Heinesen, 2020; Royer, 2009; Smith, 2009, 2015). For instance, Royer (2009) implemented a twin fixed-effect strategy to explore the association between birth weight and adult outcomes. She finds that an increase of 250 grams in birth weight leads to roughly 0.1 years of additional schooling. Higher education and income can be translated into higher access to material resources, better quality health insurance, better access to health-related knowledge, and better health behavior, all of which could be pathways through which welfare spending in early life may be linked to higher longevity and better health in later life (Balia and Jones, 2008; Demakakos et al., 2015; Gong et al., 2019; Halpern-Manners et al., 2020; Koch, 2011; Manzoli et al., 2007; Marmot, 2002; Stringhini et al., 2017).

4. Data source

The primary source of data is Numident death records of the Social Security Administration linked with 1940 census records from the CENSOC project (Goldstein et al. 2021). The Numident data covers death that occurred between the years 1988–2005. The data include the exact dates of death and birth and limited information on places of birth and death. There are two advantages of the Numident-linked sample. First, it provides detailed publicly-available below-state geographic information that allows us to infer county-of-birth. Second, it offers a full battery of information on parental characteristics, including education and socioeconomic background.

The 1940 census contains information on migration since 1935 and place-of-residence in 1935. We use the county-of-residence in 1935 as the proxy for county-of-birth. In cases where individual reports no migration since 1935 and the county-of-residence in 1935 is missing, we use county-of-residence in 1940 as the proxy for county-of-birth. In cases where a county covers more than one city, and the individual has not moved since 1935, we use city-of-residence in 1940. For those movers whose 1935 county covers more than one city, we use 1935 county and aggregate the city-level spending at the county level.[1] Although we try to minimize the measurement error of city/county-of-birth due to migration, there could still be issues regarding endogenous migration (Boustan et al., 2010; Fishback et al., 2006). We extensively discuss this in Appendix E and provide empirical evidence that migration induces a downward bias in our estimates. We show that using only 1940 county/city (with potentially more measurement error) provides smaller coefficients. We find an insignificant but positive association between migration and spending and between migration status and lower parental socioeconomic status. To the extent that the low socioeconomic status of parents during childhood affects old-age longevity (Hayward and Gorman, 2004; Montez et al., 2014), these two results suggest that migration would exert a downward bias in the relationship between relief spending and longevity.

We link this data with New Deal welfare spending during the years 1929–1940 for 115 major cities extracted from Fishback et al. (2007). The merging is done based on the city/county and year of birth. This data reports disaggregated spending for various welfare programs during the Great Depression. While we aggregate all categories and focus on total spending in the main results, we also show the results for several disaggregated categories in Appendix G.

We use decennial census data (1920–1940) extracted from Ruggles et al. (2020) to construct a series of city-level covariates and linearly interpolate the values for inter-decennial years. We also use city-level reports of infectious diseases extracted from Tycho (2021) to compute city-by-year total disease rates as an additional control. In addition, to account for the general health environment during the birth year, we calculate the infant mortality rate, stillbirth rate, and total mortality rate extracted from Manson et al. (2017).

The city-level spending data limits the final sample to birth cohorts of 1929–1940. The final sample includes 442,929 observations. Summary statistics for the final sample are reported in Table 2. Age at death, the primary outcome variable of this study, has an average value of 791 months (roughly 66 years). Approximately 90 percent of individuals in the sample are white, and 41 percent are female. Since the linking technique between Numident and census is primarily based on name commonality and women of this generation most commonly changed their last names after marriage, they are underrepresented in the sample. Fig. 2 shows the geographic distribution of cities in our final sample.[2] While there is no specific regional clustering of cities, the major cities in the study sample are primarily located in the West, Midwest, and Northeast regions.

In Appendix B, we also report summary statistics of individual and family covariates in our Numident-census-linked sample and the original population of 1940.

5. Econometric method

Our econometric method exploits deviations from the city-specific trend in welfare spending across cities within each census region

[1] Overall, in our final sample, about 24 percent are stayers, i.e., they reside in the same house as five years ago. About 18 percent of observations are non-migrants who live in counties that cover more than one city. And about 49 percent are migrants whose county-of-residence in 1935 covers more than one city.

[2] Appendix A provides a list of cities in the final sample.

Table 2
- Summary Statistics.

Variable	Mean/Proportion/Rate	SD	Min	Max
Individual/Family Variables:				
Death Age (Months)	790.659	63.172	566	923
Female	.412	.492	0	1
White	.904	.295	0	1
Black	.094	.292	0	1
Other Races	.002	.047	0	1
Hispanic (ethnicity)	.021	.142	0	1
Log Real per Capita Relief Spending	5.151	1.696	-1.529	14.123
Father's Socioeconomic Index < Median	.448	.497	0	1
Father's Socioeconomic Index ≥ Median	.448	.497	0	1
Father's Socioeconomic Index Missing	.103	.304	0	1
Mother's Education< High School	.515	.5	0	1
Mother's Education=High School	.377	.485	0	1
Mother's Education> High School	.053	.225	0	1
Mother's Education Missing	.055	.228	0	1
City-Level Controls:				
Percentage Urbanized	.891	.147	-.034	1
Share of Institutionalized People	.007	.006	0	.31
Share of Home Owners	.402	.123	.016	.879
Average Male Socioeconomic Index	32.603	3.167	13.524	39.749
Share of Literate Females	.526	.247	0	.872
Share of Literate Males	.526	.247	0	.872
Male Labor Force Participation Rate	.874	.021	.071	.946
Average Fam Size	4.27	.367	3.095	6.616
Share of Farmers	.037	.078	0	.936
Share of Children Aged 0-4	.077	.011	.047	.168
Share of Children Aged 5-10	.1	.014	.055	.189
Share of Children Aged 11-18	.135	.013	.09	.224
Real Per Capita Retail Sale	15.917	2.987	7.324	28.083
Disease Case Rate	794.294	393.81	.867	6371.724
Stillbirths Rate	60.175	19.162	26.495	153.044
Infant Death Rate	94.589	28.183	49.436	415.743
Deaths Rate	1129.982	82.265	756.904	1596.665
Number of Individuals	442,929			

Notes. City-level controls are constructed using full-count decennial censuses (1920-1940) and linearly interpolated for inter-decennial years. Share of *urbanized* represents share of households in urban areas. Based on the 1940 census definition, cities and incorporated places with a population of at least 2,500 residents are considered urabn places. Moreover, townships and political subdivisions which contain a population of at least 10,000 are also considered urban places. Share of *institutionalized* people is constructed based on the type of group quarter in which an individual lives at the time of the census. The variable is defined as the share of people in institutional group quarters to the total population. Group quarters include institutions (correctional institutions, mental institutions, institutions for elderly, institutions for disabled people, and institutions for poor people) and non-institutional group quarters (non-institution households, military, dormitory, and all other types). The variable *Socioeconomic Index* represents Duncan's Socioeconomic Index. The index is a composite measure of occupational status that is assigned to each occupation (hence the individual holding the occupation) based on income and educational level associated with the occupation in 1950. Retail sale is the aggregate sale of products to consumers. It excludes goods sold as intermediary goods, inputs for final production, and wholesale sales. Disease cases are built based on eight common infectious diseases (smallpox, polio, measles, mumps, rubella, hepatitis A, whooping cough, and diphtheria) compiled by Project Tycho and are deflated by 100,000 state-level population. Stillbirth and infant death rates are also computed as cases per 100,000 city-level population.

and year, conditional on fixed effects and covariates. We operationalize this strategy using the following ordinary-least-square formulations:

$$DA_{icrb} = \alpha_0 + \alpha_1 LogWS_{crb} + \alpha_2 X_i + \xi_c \times T_b + \zeta_{rb} + \varepsilon_{icrb} \quad (1)$$

Where *DA* is age at death (in months) of individual *i* who is born in city/county *c* in census region *r* and year *b*. The parameter *X* includes individual race, ethnicity, and gender dummies. It also includes a series of dummies for maternal education and paternal socioeconomic status (and missing indicators for missing values). We interact city/county fixed effects by a linear trend in birth year (represented by $\xi \times T$).[3] The parameter ζ represent census-region-of-birth-by-birth-year fixed effects to account for cross-cohort movements in longevity that vary across census regions. Finally, ε is a disturbance term.

The variable *LogWS* is log of per capita welfare spending. We focus on the cumulative welfare spending (aggregating all spending in Table 1) as families could benefit from various programs simultaneously, and focusing on one single payment could mismeasure the

[3] There are cities that are located in more than one county. For instance, New York City comprises five counties. In these cases, we also add fixed effects for each county and interact them with a linear birth cohort trend.

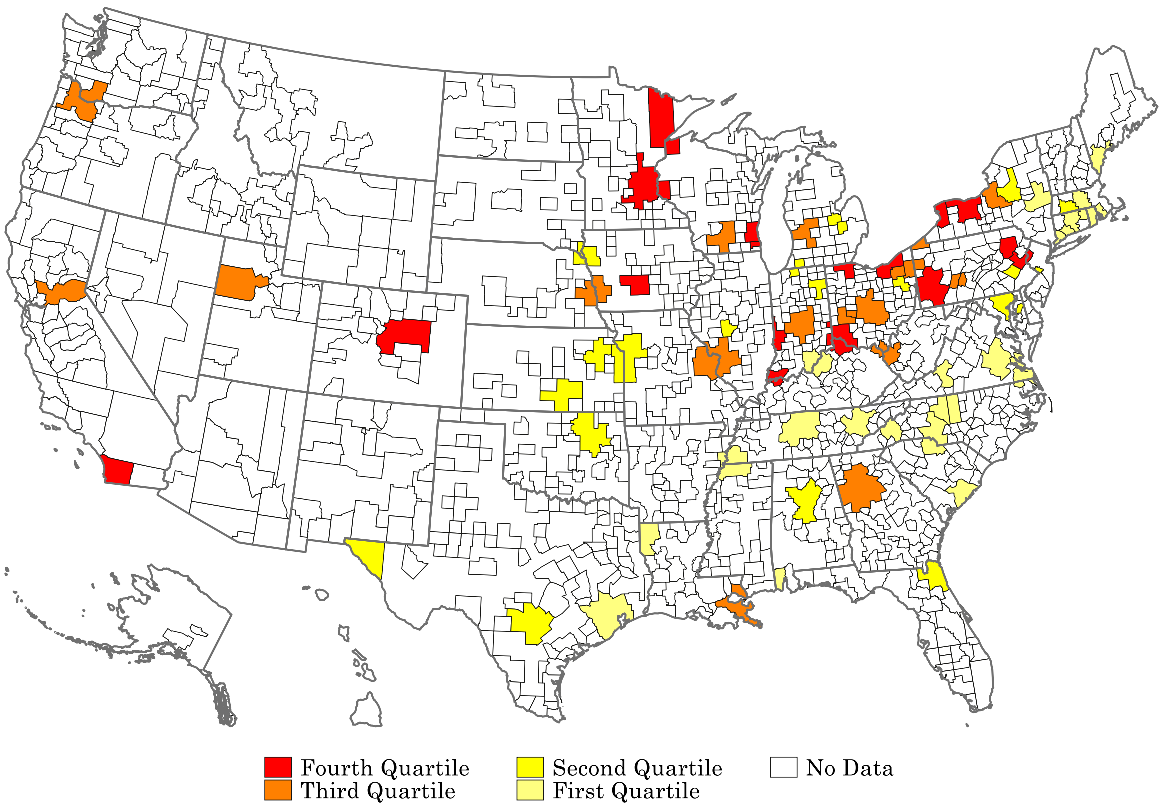

Fig. 2. Geographic distribution of metropolitan areas that contain cities in the final sample based on city-level relief spending during 1929–1940.

spending's impacts (Fishback et al., 2007).[4] Therefore, the coefficient α_1 is our parameter of interest, which shows the effect of a 100 percent increase in the welfare spending per capita on individuals' later-life longevity. We cluster standard errors at the city level. Moreover, since there are differences between the original population of 1929-1940 birth cohorts and those linked to Numident death data, we weight the regressions to render results representative of the original population of 1929-1940 cohorts in the 1940 census. We employ inverse probability weighting, where weights are the inverse of the probability of successful merging the Numident death records with the 1940-census data (Halpern-Manners et al., 2020). The probabilities are drawn from probit regressions that control for individuals' race, gender, age, and parental characteristics.

6. Results

6.1. Balancing Tests

Welfare spending may prompt differential migration among people of different demographic and socioeconomic backgrounds. In this case, changes in population composition may bias estimates in equation as people of different races, ethnicity, and socioeconomic backgrounds may vary in longevity for various unmeasured reasons. We explore this source of endogeneity by regressing a wide array of demographic and socioeconomic characteristics on log of welfare spending per capita, conditional on a full set of fixed effects and trends.[5] As shown in Table 3, there is no significant association between spending and individuals' race, ethnicity, gender, paternal socioeconomic status, father's wage, father's weeks of employment in 1939, father's hours of employment during the last week, father's schooling, mother's schooling, father's age at the child's birth, mother's age at child's birth, and dummies for the presence of father and mother in the household. The marginal effects are small in magnitude, in most cases suggesting changes of less than 1 percent relative to the mean of the outcome (as a result of 100 percent change in per capita spending), and statistically insignificant in all cases. The observed null results on the association between pre-determined observables provide a benchmark for the fact that there

[4] However, we also examine the effects in three sub-categories of spending: total work-related public assistance, total private assistance, and total non-work-related public assistance (ADC, old-age assistance, and aid to blind). These results are reported and discussed in Appendix G.

[5] The importance of including region-by-cohort fixed effects and city-by-cohort linear trend is evident in the balancing tests. In the absence of these interactions and trends, Appendix C replicates the balancing test and shows that these tests fail for parental characteristics as outcomes.

Table 3
- Balancing Test.

	Outcomes: Female (1)	White (2)	Black (3)	Other Race (4)	Hispanic (5)	Father's Socioeconomic Score (6)	Father's Socioeconomic Index below Median (7)	Father's Socioeconomic Index above Median (8)	Father's Socioeconomic Index Missing (9)	Father's Wage Income (10)
Log Real per Capita Relief Spending	.00487	-.00085	.00134	-.00049	.00057	-.27955	.0033	-.00527	.00197	-12.60464
	(.0043)	(.00498)	(.00483)	(.00071)	(.00118)	(.23876)	(.00612)	(.00506)	(.0041)	(12.20764)
Observations	442929	442929	442929	442929	442929	442929	442929	442929	442929	387920
R-squared	.02032	.14643	.15185	.03438	.16177	.04859	.03963	.04245	.04942	.06508
Mean DV	0.634	0.880	0.116	0.003	0.019	23.836	0.517	0.349	0.134	1166.376
%Change	0.768	-0.097	1.158	-16.340	2.992	-1.173	0.639	-1.510	1.467	-1.081

	Father's Weeks of Work Last Year (11)	Father's Hours Worked Last Week (12)	Father's Years of Schooling (13)	Father's Education Missing (14)	Mother's Years of Schooling (15)	Mother's Education Missing (16)	Father's Age at Child's Birth (17)	Mother's Age at Child's Birth (18)	Father is Present in Household (19)	Mother is Present in Household (20)
Log Real per Capita Relief Spending	-.07467	.09692	-.07796	-.00158	-.04823	-.00066	.07583	.20134*	-.00197	-.00043
	(.12695)	(.11835)	(.04813)	(.0012)	(.04154)	(.00274)	(.08084)	(.10587)	(.0041)	(.00233)
Observations	365982	314909	389455	442929	418310	442929	396859	425440	442929	442929
R-squared	.04069	.05475	.10035	.07391	.11743	.05398	.02694	.08648	.04942	.04204
Mean DV	43.933	43.925	8.659	0.019	8.909	0.071	31.781	26.996	0.866	0.949
%Change	-0.170	0.221	-0.900	-8.312	-0.541	-0.924	0.239	0.746	-0.227	-0.045

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4
- Exploring Selection of Numident Data: The Association between Spending and Successful Merging of Numident-1940-Census Records.

	Outcome: Successful Merging between Numident and 1940-Census						
	Full Sample	***Subsamples:***					
		Whites	Blacks	Hispanics	Females	Mother's Schooling<HS	Father's SEI<Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Real per Capita Relief Spending	.00102	.00108	-.00034	.00215	.00086	.00129	.00141
	(.00072)	(.00072)	(.00145)	(.00357)	(.00075)	(.00085)	(.0009)
Observations	5906998	5285135	600649	161923	2968860	4161621	3017944
R-squared	.00793	.00825	.00708	.00778	.00724	.00817	.00814
Mean DV	0.064	0.065	0.061	0.055	0.057	0.067	0.065
%Change	1.594	1.663	-0.560	3.910	1.505	1.932	2.167

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5
- Exploring Endogenous Fertility.

	Outcomes:						
	Log Births	Fertility Rate	Log Fertility Rate	Share of Births to Whites	Log Share of Births to Whites	Share of Births to Blacks	Log Share of Births to Blacks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Real per Capita Relief Spending	0.02296	2.72286	0.02282	-0.89939	-0.03485	-0.45161	-0.01686
	(0.0222)	(2.23462)	(0.02224)	(1.45226)	(0.04842)	(0.98515)	(0.07233)
Observations	1436	1436	1436	377	377	377	377
R-squared	0.99618	0.89628	0.93214	0.96563	0.82632	0.98297	0.96873
Mean DV	9.360	59.251	4.048	83.118	-0.203	16.712	-2.115

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include county fixed effects, birth year fixed effects, region-of-birth-by-birth-year fixed effects, county-specific linear trend, and county controls. County-by-year covariates include share of homeowners, share of literate females, share of literate males, average male socioeconomic index, male labor force participation rate, share of farmers, share of people aged 0-4, share of people aged 5-10, share of people aged 11-18, share of urban area, share of people living in institutions, average retail sale per capita, total disease rate, total mortality rate, infant mortality rate, and stillbirth rate. All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

is also no association between spending and unobservables, which lends to the validity of our OLS estimations (Altonji et al., 2005; Fletcher et al., 2021).

6.2. Endogenous data linking

One may argue that the criteria for linking Numident death records with the 1940 census could induce bias by differentially selecting individuals of certain demographic groups, rendering the results non-representative of the population even with the inverse probability weighting scheme. For instance, if the successful merging is more likely among white Americans, their overrepresentation would challenge the generalizability of the results as white Americans are more likely to live longer than their non-white counterparts for reasons not captured in the data. The inverse probability weights are constructed from regressions using observables and thus fail to account for these unobservable determinants of longevity. We explore this issue by regressing a binary variable that indicates the successful linking between Numident and the 1940-census records on log of per capita spending. The results are reported in Table 4 using the full sample in column 1 and subsamples based on demographic characteristics in columns 2-7. The marginal effects imply quite small associations between spending and the probability of Numident-1940-census linking. Moreover, all the coefficients are statistically insignificant at conventional levels.

6.3. Endogenous fertility

Another potential concern is that changes in welfare spending may induce changes in fertility behaviors. Selective fertility would pose an endogeneity challenge to the estimations of Eq. 1 if certain parental demographic or socioeconomic traits both increase the likelihood of childbirth in response to municipal spending increases and are correlated with children's health endowment as observed in later-life longevity. We implement additional analyses to explore whether there are differential fertility responses to welfare

Table 6
- Placebo Tests: Assignment of Spending at Ages 10 and 15.

	Outcome: Age at Death (Months)					
	Assignment of Spending at Age 10			**Assignment of Spending at Age 15**		
	(1)	(2)	(3)	(4)	(5)	(6)
Log Real per Capita Relief Spending	.28209	.27721	.18421	-.47889	-.32394	-.1751
	(.28125)	(.28032)	(.4521)	(.70943)	(.72708)	(.75455)
Observations	668213	668213	661259	292785	288438	288438
R-squared	.1486	.1491	.14909	.03555	.03609	.03669
City/County and Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓
Family Controls	✓	✓	✓	✓	✓	✓
City/County-by-Birth-Year Trend		✓	✓		✓	✓
Region-by-Birth-Year FE			✓			✓

Notes. Standard errors, clustered at the city level, are in parentheses. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

spending. These tests are relevant to our identification assumption – that spending changes are uncorrelated with other determinants of longevity–. A change in cohorts' demographic composition could bias the estimates and birth year fixed effects, and race/ethnicity dummies would not fully account for structural differences in longevity across demographic groups.

We use birth, fertility, and female population data at the county-by-year (and by race where available) level from Manson et al. (2017) for the period 1929-1940. We include the same set of controls, fixed effects, and trends as in equation and regress fertility outcomes on spending. The results, shown in Table 5, suggest that concerns about selective fertility are not warranted. Specifically, there is no association between spending and log of birth (column 1), fertility rate (column 2), share of white births (column 4), and share of black births (column 6). Thus, under the full specification of our model, the city and demographic-group-specific trends in fertility are arguably controlled for.

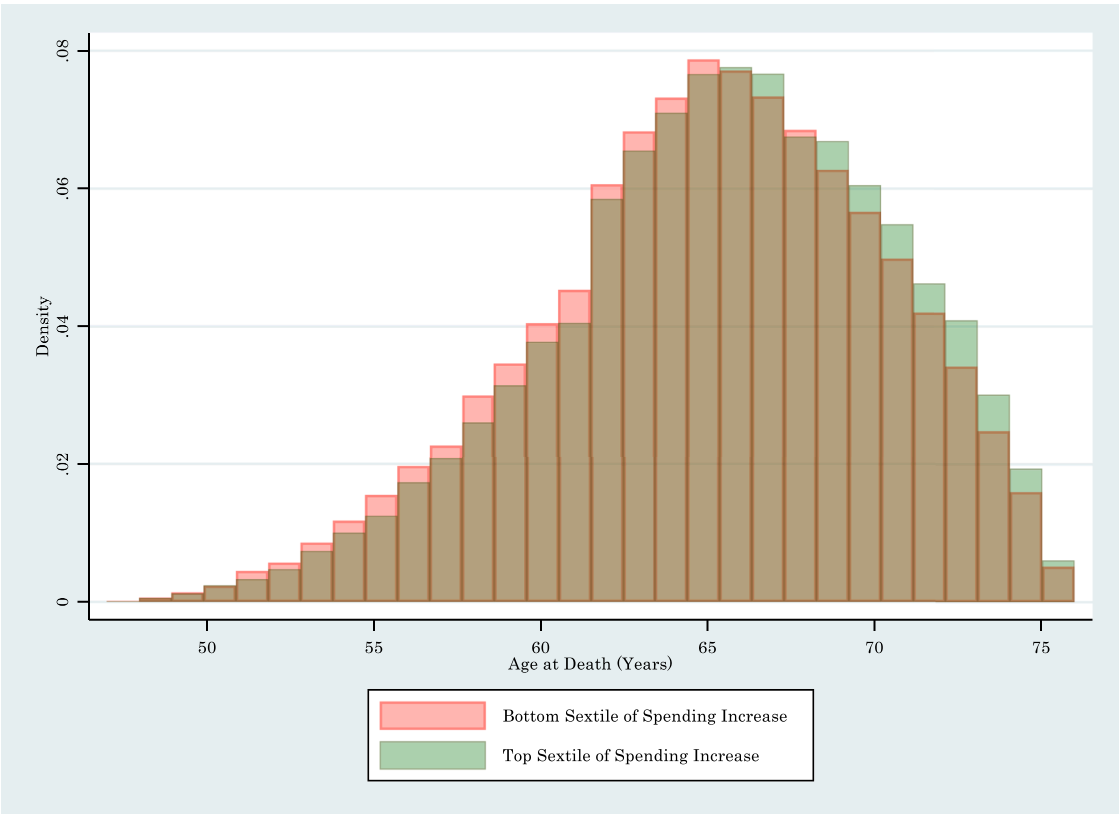

Fig. 3. Density distribution of age at death in cities at the top and bottom sextiles of increases in spending during the years 1929–1940.

A similar concern may arise for selection due to mortality among infants and children. In the absence of relief spending, mortality selection may lead fitter individuals to survive economic hardships. The rise in spending may contribute to reductions in infant/child mortality and change the composition of surviving individuals. Those who would have died in the absence of social aid may now reach older ages in poorer health and potentially reduce subsequent cohort longevity. Therefore, we may expect a downward bias in the estimated effects of Eq. 1. We empirically examine this confounding source in Appendix F. We show that spending (conditional on fixed effects and trends) is not statistically correlated with infant mortality, stillbirth, and all-age mortality rates. Moreover, the estimated coefficients are virtually unchanged when we include these covariates in our regressions.[6] However, we should note that these cohorts benefited from subsequent medical innovations, improvements in healthcare industry, and the introduction of new vaccines which may have added to their expected life expectancy at all ages. Therefore, the potential disparate impact of social aid that has not been observed in early-life measures could be detected in later life longevity (Engelman et al., 2010).

6.4. Placebo tests

One may argue that changes in spending may reflect increases in health outcomes and health-related technologies over time. Also, spending may be particularly concentrated in areas that are already experiencing improvements in public health infrastructures, increased access to medical resources, and expansions in disease prevention campaigns. Therefore, the estimated effects of equation may simply pick up on such preexisting trends rather than reflect the outcome of increased spending. We argue that if this is indeed the case, we should be able to observe a general improvement in health outcomes among other presumably less affected populations as a response to expansions in welfare spending. This conclusion offers a placebo test in which we assign the spending during postnatal ages, specifically ages 10 and 15. We report the results in two panels of Table 6 across specifications with and without region-by-cohort fixed effects and city-trend. The results produce no discernible effect of spending on longevity of these cohorts. Small-sized and insignificant marginal effects rule out the concern that the coefficients only reveal over-time trends in health outcomes.[7]

6.5. Main results

Longevity is slightly higher in areas with a higher amount of relief spending per capita. Fig. 3 shows the differences in the density distribution of individuals at the top versus bottom sextiles of spending. Visually, the distribution is left-skewed for those with higher relief exposure and vice versa. To move from these visual links toward direct associations, we run various specifications of equation . We report the results in Table 7. We start with a parsimonious model that only includes city and birth year fixed effects (column 1) and sequentially add additional covariates across consecutive columns. In the fully parameterized model of column 5, a 100 percent increase in spending is associated with a roughly 1-month increase in age at death. This effect is about 21 percent of female-male gap in the outcome and about 87 percent of white-nonwhite difference in longevity.[8] As shown in Table 1 and Fig. 1, between the years 1929 to its peak in 1937, total relief spending per capita experienced a twentyfold increase. This sharp rise, combined with the results of Table 7, suggest an increase in lifespan by roughly 1.6 years. This is equivalent to about a 2.3 percent rise from the mean of age at death in our sample.

In interpreting the results, we note that the period of 1929-1940 encompasses two shocks occurring simultaneously, i.e., the local economic hardships due to the Great Depression and the establishment of social insurance under the New Deal programs. Table 7 suggests an average effect of 1 month across all cities, regardless of how hard they were hit by the recession. In Appendix J, we show that the longevity effects of relief spending were larger in places with more severe economic disruption. As discussed in that appendix, we use the drop in state-level income between the years 1929-1936 to group cities based on state-level income reductions. We show that the effects vary between 0.7 and 3.3 for the first and fourth quartiles of income drop, respectively. Note that a higher coefficient could be due to a higher spending concentration due to a higher drop in income, not necessarily a higher return to spending. However, to the extent that states' economic conditions are connected within the same region, region-by-birth-year fixed effects account for these changes in income. Moreover, in section 6.6, we also show the robustness of the results to adding state-by-birth-year fixed effects so that the variation reflects differences in spending across cities/counties within the same state and year. Therefore, our estimation method compares individuals born in a city-year with a higher spending level (not due to its economic hardship) to those born in a city-

[6] These results contradicts findings of Fishback et al. (2007), who find significant changes in births and deaths. The primary reason for this difference is the implementation of region-by-cohort fixed effects and city-specific linear trends. We can replicate Fishback et al.'s main findings when estimating models with only city and year fixed effects. However, once we include region-cohort fixed effects to control for the cross-cohort differences in health across census regions, the coefficients for fertility and infant mortality become small and non-significant. Moreover, if we only add a city-trend to the main set of fixed effects, again the effects likewise become non-significant. In section 6.1 and Appendix C, we argue that the inclusion of these fixed effects and trends is necessary to balance the sample, i.e., to absorb potential confounders so that the spending becomes orthogonal to observable characteristics of individuals, hence (arguably) to unobservables (Altonji et al., 2005).

[7] In this analysis, we focus on cohorts born between 1919-1930 (those aged 10 between 1929-1940) and cohorts born between 1914-1925 (those aged 15 between 1929-1940). In Appendix D, we use cohorts of 1929-1940 and calculate the leads of spending, i.e., spending in one to three years after birth. Since the spending is only available between 1929-1940, lead values are computable only for cohorts born between the years 1929-1937. Focusing on these cohorts, we explore the association between spending in year of birth and up to three years after birth on longevity. These results confirm our placebo tests in this section and suggest that spending is not associated with other health improvement factors, as the coefficients of postnatal ages are insignificant.

[8] This is based on coefficients of female (4.7, se=0.21) and white (1.03, se=0.31) that are not reported in this table.

Table 7
- The Effects of in-Utero and early-Life Exposure to Relief Spending between the Years 1929-1940 on Old-Age Longevity.

	Outcome: Age at Death (Months)				
	(1)	(2)	(3)	(4)	(5)
Log Real per Capita Relief Spending	.73427**	1.06398***	1.05459**	1.03181**	1.04356**
	(.31718)	(.30217)	(.41234)	(.413)	(.41346)
Observations	442929	442929	442929	442929	442929
R-squared	.35558	.35591	.36104	.3624	.36252
City/County and Birth Year FE	✓	✓	✓	✓	✓
Region-by-Birth-Year FE		✓	✓	✓	✓
City/County-by-Birth-Year Trend			✓	✓	✓
Individual Controls				✓	✓
Family Controls					✓

Notes. Standard errors, clustered at the city level, are in parentheses. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

year with lower spending (unrelated to its economic downturn).

These effects are in line with studies that reveal the relevance of early-life economic conditions and a healthy environment on later-life longevity and mortality outcomes (Beltrán-Sánchez et al., 2021; Dewey and Begum, 2011; Hayward and Gorman, 2004; Johnson and Schoeni, 2011; Ko and Yeung, 2019; Van Den Berg et al., 2006). For instance, Arthi (2018) documents how New Deal spending ameliorated the negative effects of the Dust Bowl. She finds that a one-standard-deviation rise in per capita federal loans during early childhood is associated with 66 basis points higher likelihood of completing college. Fletcher and Noghanibehambari (2021) use Numident data to explore the effects of college opening on mortality and find that attending college is associated with 1-1.6 years higher age-at-death. Noghanibehambari et al. (2022) explore the impacts of local economic conditions during in-utero on old-age longevity. They proxy local economic and financial conditions with bank deposits and find significant associations. They show that a drop of roughly \$300 in income (off a mean of \$600, equivalent to the drop in state-level income during 1929-1933) during prenatal development is associated with about 8 months lower longevity. Based on our intent-to-treat effects, a similar rise in relief spending leads to about 3 months longer lives. Overall, the findings of Table 7 are in line with these studies and add to the ongoing research by showing the reduced-from effects of in-utero and early-life safety net spending on old-age longevity.

6.6. *Sensitivity analysis*

In Table 8, we explore the robustness of the main results to alternative specifications. In columns 1-9, we add to the full specification of Table 7 additional covariates and fixed effects.

In the main analysis, we avoid including city controls as they could endogenously move with spending. However, we examine the robustness of the results to several potential confounders in column 1. We add a series of city, county, and state level controls, including share of homeowners, share of literate females, share of literate males, average male socioeconomic index, male labor force participation rate, share of farmers, share of people aged 0-4, share of people aged 5-10, share of people aged 11-18, share of urban area, share of people living in institutions, average occupational income score, average occupational education score, average family size, share of whites, share of blacks, share of Hispanics, share of females[9], total disease rate[10], total mortality rate, infant mortality rate, stillbirth rate[11], child labor law dummies, compulsory schooling dummies[12]. In Appendix F, we also show the sensitivity of the results to additional city-county-level covariates, including school spending, school quality index, bank deposits per capita, and retail sale per capita. We also show that relief spending is not statistically correlated with any of these potential county-city confounders.

Since the choice of place of residence at the time of death is endogenous and potentially correlated with the health status of individuals, we avoid including it in the main results. We show that the effects rise by 7 percent when we include state of death fixed effects (column 2).

To control for potential seasonality in death, we add month of death fixed effects (column 3). Similarly, to control for seasonality in birth outcomes, we add birth month fixed effects interacted with birth year fixed effects (column 4). The results are almost identical to the main results.

In addition, we add a city-specific quadratic trend (column 5), interact city/county fixed effects with race dummies (column 6), and interact parental dummies with city/county fixed effects (column 7). The estimated coefficients are quite comparable to the main results.

[9] These covariates are constructed using available information in the full-count censuses 1920-1940. We linearly intraplate the values for inter-decennial years. The full-count censuses are extracted form Ruggles et al. (2020).
[10] The data is built based on city-state reports of disease from Tycho (2021).
[11] Death rate data is extracted from Manson et al.(2017).
[12] Child labor laws and compulsory schooling are extracted from Acemoglu and Angrist (2000).

Table 8
- Robustness Checks to Alternative Specifications.

	Additional City and State Controls	Adding State of Death FE	Adding Month of Death FE	Interacting Birth Month FE with Birth Year FE
	(1)	(2)	(3)	(4)
Log Real per Capita Relief Spending	.77898*	1.11082***	1.08481***	1.08108***
	(.4341)	(.41664)	(.41323)	(.41082)
Observations	442929	441626	442929	442929
R-squared	.36266	.36465	.36482	.36515
	Adding a Quadratic Cohort Trend in City and County	Interacting City FE with Race/Ethnicity Dummies	Interacting City FE with Parental Characteristics Dummies	Adding Family FE
	(5)	(6)	(7)	(8)
Log Real per Capita Relief Spending	.97652*	1.03838**	1.06022**	1.9028
	(.50395)	(.41498)	(.41865)	(2.30002)
Observations	442929	442917	442929	73650
R-squared	.36543	.36324	.36442	.72001
	Adding Birth-State-by-Birth-Year FE	Above-Median Drop in Income between 1929-1936	Huber-White Robust SE	Clustering SE at City and Region-Cohort Level
	(9)	(10)	(11)	(12)
Log Real per Capita Relief Spending	1.30406**	2.12235*	1.04477*	1.04477***
	(.63908)	(1.11913)	(.55362)	(.39215)
Observations	442917	56172	442929	442929
R-squared	.36371	.25298	.36252	.36252
	Log Age at Death	Age at Death>60	Age at Death>65	Age at Death>70
	(13)	(14)	(15)	(16)
Log Real per Capita Relief Spending	.00137**	.00651**	.00698**	.0016
	(.00055)	(.00316)	(.00319)	(.00253)
Observations	442929	442929	442929	442929
R-squared	.35255	.16315	.28603	.25077

Notes. Except for columns 11-12, standard errors are clustered at the city level and reported in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. All regressions include individual covariates and parental controls. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics. Except for columns 15-18, the outcome in all regressions is age-at-death in month.
*** p<0.01, ** p<0.05, * p<0.1

Table 9
- Sensitivity to Selection of Death Window: Comparing the Results with NCHS Death records Using State-Aggregated Spending Measures.

	Outcome: Age at Death (Months)			
	Numident Data, Death Window: [1988-2005] (1)	NCHS Data, Death Window: [1988-2005] (2)	NCHS Data, Death Window: [1979-2005] (3)	NCHS Data, Death Window: [1979-2017] (4)
State-Aggregated Log Real per Capita Relief Spending	.4172*	.36672***	.52621***	.95848***
	(.22468)	(.11495)	(.15901)	(.17819)
Observations	1847162	5381982	6507506	13117881
R-squared	.19894	.31028	.17286	.09365
Mean DV	761.419	768.518	739.115	838.529
%Change	0.055	0.048	0.071	0.114

Notes. Standard errors, clustered at the birth-state level, are in parentheses. All regressions include state-of-birth fixed effects, year-of-birth fixed effects, region-of-birth-by-year-of-birth fixed effects, and individual controls. Individual covariates include dummies for race, ethnicity, and gender. State-by-year covariates include share of homeowners, share of literate females, share of literate males, average male socioeconomic index, male labor force participation rate, share of farmers, share of people aged 0-4, share of people aged 5-10, share of people aged 11-18, share of urban area, share of people living in institutions, average retail sale per capita, total disease rate, total mortality rate, infant mortality rate, and stillbirth rate. All regressions are weighted using state-level population.
*** p<0.01, ** p<0.05, * p<0.1

Furthermore, we add family fixed effects to control for all unobserved family confounders (column 8). The coefficient of column 8 suggests that we observe larger effects when controlling for shared environment during childhood and comparing sibling longevity. However, the effects are imprecisely estimated, and we cannot add any additional comments. In column 9, we add state-by-cohort fixed effects to control for any changes in state policies or economic conditions that affect all cities in a given year. The effects are very similar to the main findings of Table 7.

In column 10, we show the results for a subsample of states that revealed larger drops in state-level income for the years 1929-1936, the period of falling income during the Great Depression. The effects suggest an increase of 2.1 months in longevity, more than twice the effects in the main results.

In columns 11-12, we explore the sensitivity of the standard errors to using Huber-white robust rather than clustering at the city level and to two-way clustering at the city and region-cohort level. The standard errors are comparable to the main results.

Moreover, we explore the nonlinearities in the effects in two ways. First, we replace the outcome with log of age at death (column 13). Second, we replace the outcome with a series of dummies that indicates the age at death is larger than 60, 65, and 70 years (columns 14-16). It is worth noting that the average outcomes in these columns are 0.78, 0.43, and 0.13, respectively. Compared to the mean of the outcomes, the coefficients suggest that the effects become relatively larger around age 65 (0.8, 1.6, and 1.2 percent change as a 100 percent change in spending).

Although we add a wide array of fixed effects and covariates in this section and find relatively stable coefficients, there is still room for unobservables to bias the results towards zero. Using the method in Oster (2019) we calculate the degree of selection based on unobservables that are necessary to bias the results to a specific value (denoted by δ). We find a required δ of roughly 1.9, which suggests that selection based on unobservables must be almost twice as large as selection based on observables (fixed effects and controls in our regressions) to lead the estimate towards zero. It is difficult to envision an unobservable that is correlated with spending and affects longevity, its effect is not captured in any observables, and its influence on longevity is twice as much as all the observables and fixed effects combined. Therefore, we believe the effects are likely not sensitive to selection based on unobservable.

6.7. Selection of death window

Another concern in interpreting the results is the limited death coverage of our Numident data. The observations in the Numident file are those that have a Social Security number (as they are reported by Social Security Administration) and are linkable to the 1940-census. In addition, the Numident file available to us covers deaths that took place between 1988-2005. Thus, if the mortality gains from in-utero and early-life social spending appear early in adulthood and old age, the Numident results will be downwardly biased.

To explore this selection problem, we supplement our analysis via Vital Statistics, specifically death records extracted from the National Center for Health Statistics (NCHS). The advantages of NCHS data are their relatively long period of death coverage and virtually universal coverage of deaths in the country. The disadvantage of NCHS is the lack of a geographic identifier for the place of birth below the state level. Moreover, for the data files prior to 1979, there is no information on the place of birth. To overcome this limitation, we aggregate the spending data at the state level and implement a series of analyses with state and cohort fixed effects.

First, we start by assigning spending at the state and year of birth to Numident data and replicate the main results. We focus on all Numident records born between 1929-1940 with available state-aggregated spending information. These estimates are reported in column 1 of Table 9. In these regressions, we include state-of-birth fixed effects and region-of-birth-by-birth-year fixed effects. We also include individual controls and a series of state-by-birth-year controls. Compared to the marginal effects of Table 7, the estimated marginal effect suggests a smaller effect of 0.4, implying that aggregation attenuates the correlation between spending and longevity.

Next, we merge state-by-year spending data with the NCHS data based on state-of-birth and year-of-birth for the years 1979-2017. The cross-state migrations (from birth to death) induce a measurement error that is possibly correlated with early-life exposures (Xu et al., 2020, 2021). Therefore, we report the results in year groups and exercise caution in interpreting the results of this section. In column 2 of Table 9, we use NCHS death records of 1988-2005 (similar death window to Numident) and replicate the regression of column 1. The estimated marginal effect is quite similar to that of the state-aggregated Numident results. This fact suggests that Numident data fairly represents the universe of birth records for the cohorts and states of this exercise.

We then expand the death window to cover the universe of death records between the years 1979-2005. Therefore, we add 9 earlier death years. The results, reported in column 2, imply an increase of about 40 percent in the magnitude of the effect. We also observe a fairly similar change when we look at the implied change with respect to the mean of the outcome (reported in the last row).

Next, we include deaths that occurred after 2005. Column 4 replicates the results for the NCHS sample of deaths between the years 1979-2017. The implied marginal effect points to a 1-month increase in longevity for a 100 percent rise in spending. Compared to the estimated effect of column 2 (NCHS 1988-2005), this suggests an increase in the magnitude of about 2.6 times.

The big picture of Table 9 reveals three aspects related to our main results. First, a more granular geographic level (i.e., city/county versus state) provides larger estimates. Second, we observe an almost identical coefficient when including deaths that are not in Numident data but occurred during the Numident death window. Third, we observe coefficients that are roughly 2.6 times larger when we include earlier and later deaths. Therefore, the estimates obtained from Numident provide a lower bound for the true correlations. Since we observe a similar effect between columns 1 and 2 for Numident and NCHS in similar death windows, we would expect to observe an inflated effect size of about 2.7 months had we had access to earlier and later death windows.[13]

6.8. Heterogeneity analysis

Our analysis employs an intention-to-treat perspective as spending is measured for the total population. We expect to see relatively larger effects among poor and otherwise disadvantaged sub-population as they are more likely to have received and benefited from the spending intervention. To explore how the effects vary by demographic and socioeconomic characteristics, we interact the log of spending with dummy variables for Black race, female gender, an indicator low (less than high school) maternal education, and an indicator for father's low (below median) socioeconomic index (SEI score). The results are reported in columns 1–4 of Table 10. The effects are considerably larger among Black persons. The interaction term suggests that doubling the spending per capita raises longevity of Black persons by 0.7 months more than it does among non-Black individuals. The effects are also smaller among females, although the interaction coefficient is not significant.

As expected, the interaction of parental characteristics suggests that the effects of increased spending are more pronounced among families with low-educated mothers and low-SEI fathers. For instance, children of fathers with a low SEI score (SEI score below the median of the sample) lived 2.7 months less than those whose fathers had a high SEI score (SEI score above the median of the sample). On average, a 100 percent rise in spending increases the longevity of children of low-SEI fathers by 0.5 months more than children of high-SEI fathers.

Studies show that, in addition to states' economic distress, their leaders' political connections also played a role in the allocation of funds and grants (Fishback, 2017; Fishback et al., 2007; Fleck, 2015; Wallis, 1998; Wright, 1974). As a final check, we show the heterogeneity in the effects by the county's number of labor committee members in the House of Representatives. Column 5 of Table 10 shows that the effects are larger when the county/city's political connection is stronger, suggesting that political connection is instrumental for attracting the funds.

6.9. Mechanism channel

As we discussed in section 3, improvements in education and income are testable pathways between early-life shocks and old-age longevity. However, the Numident data does not report the education and income of the deceased. To overcome this issue and to explore potential mechanism channels, we use census data and American Community Survey data extracted from Ruggles et al. (2019). The advantage of census-ACS, besides its relatively large sample size, is that it reports income, measures of socioeconomic score, and detailed information on education. The disadvantage of these data is that they report the birth state, and there is no more granular geographic variable for the birthplace. To address this issue, we aggregate the spending of major cities at the state-level and merge it with the census-ACS sample based on state and year of birth.[14] We focus on post-1980 data years to be consistent with the Numident death period. We modify the empirical strategy of equation to replace city fixed effects with state-of-birth fixed effects. We replace the

[13] this is computed using columns 4 and 2 of Table 9 and column 5 of Table 7: $\frac{0.958}{0.367} \times 1.04\langle /END\rangle$ In section 6.7 and specifically column 1 of Table 9, we show that the longevity estimate of Numident data is smaller but statistically significant when we aggregate spending at the state-level and implement state and birth-year fixed effects. In Appendix H, we replicate the main results of Table 7 for the case where spending data is aggregated at the state-level and a full specification of equation is estimated. In line with the results of Table 9, the marginal effects are smaller in magnitude but remain statistically significant.

[14] As we showed in this section, early-life welfare spending effect on later-life mortality could cover a number of pathways. This is the primary reason that we avoid using alternative strategies such as two-stage least-square estimation. For instance, if we focus on education as the endogenous variable and the spending as the IV we observe strong first-stage and reduced-form effects. However, spending also affects income (columns 8-9, Table 11) which is documented to affect mortality. Therefore, the exclusion restriction assumption is violated. While we are aware of this fact, it helps understanding the magnitude of the results using similar strategies. Since our first-stage and second-stage samples are drawn from two different samples, we can implement two-sample two-stage least-square estimation strategies to explore the magnitude of the spending-induced increases in education and adulthood socioeconomic status on old-age longevity (Angrist and Krueger, 1992; Inoue and Solon, 2010). These results are reported and discussed in Appendix I.

Table 10
- Heterogeneity Analysis.

	Outcome: Age at Death (Months)				
	(1)	(2)	(3)	(4)	(5)
Black × Log Real per Capita Relief Spending	.69461**				
	(.27516)				
Black	-5.97694***				
	(2.21454)				
Female × Log Real per Capita Relief Spending		-.24103			
		(.1522)			
Female		6.19403***			
		(.74961)			
Mother's Education<HS × Log Real per Capita Relief Spending			.20139		
			(.14573)		
Mother's Education<HS			-1.36148		
			(.92213)		
Father's SEI<Median × Log Real per Capita Relief Spending				.53317**	
				(.21496)	
Father's SEI<Median				-2.76011***	
				(1.01525)	
No Labor Committees in House of Representatives × Log Real per Capita Relief Spending					.51051***
					(.18201)
No Labor Committees in House of Representatives					-2.94375**
					(1.31327)
Log Real per Capita Relief Spending	.9793**	1.25639***	1.04058**	.76844	1.07726**
	(.44232)	(.42725)	(.43963)	(.47478)	(.44527)
Observations	442929	442929	442929	442929	442929
R-Squared	.36259	.36257	.36257	.36261	.36257

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

Table 11
- Exploring Mechanism Channel: The Effects of in-Utero and Early-Life Relief Spending Exposure on Education and Socioeconomic Status.

	Outcomes: Education<HS	Education=HS	Education=HS Graduate	Education: Some College	Education≥4 Years of College	Socioeconomic Index	Occupational Education Score	Total Family Income	Log Total Family Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log Real per Capita Relief Spending	-0.0045***	-0.00326***	0.00501***	0.00256*	0.00018	0.3048**	0.38781***	2440.5113***	0.02385***
	(0.00141)	(0.00117)	(0.0017)	(0.00142)	(0.00142)	(0.11854)	(0.11587)	(558.33784)	(0.0057)
Observations	8583383	8583383	8583383	8583383	8583383	3352229	3342866	7752463	7749960
R-squared	0.05602	0.02083	0.02306	0.01103	0.03425	0.03771	0.04894	0.04104	0.03824
Mean DV	0.076	0.103	0.455	0.165	0.202	44.050	53.989	7.2e+04	10.721
%Change	-5.920	-3.165	1.101	1.554	0.091	0.692	0.718	3.390	0.223

Notes. Standard errors, clustered at the birth-state level, are in parentheses. All regressions include birth-state fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, state-specific linear trend, and individual controls. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). Birth-state-by-year covariates include share of homeowners, share of literate females, share of literate males, average male socioeconomic index, male labor force participation rate, share of farmers, share of people aged 0-4, share of people aged 5-10, share of people aged 11-18, share of urban area, share of people living in institutions, average retail sale per capita, total disease rate, total rate, infant mortality rate, and stillbirth rate. All dollar figures are in 2020 dollars to reflect real values.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

outcome with a series of education dummies, measures of socioeconomic index, and family income. The results, reported in Table 11, suggest improvements in all outcomes. For education, the effects are stronger at the lower levels of education. Specifically, spending induces larger reductions in low-educated individuals (less than high school) than increases in high-educated people (more than high school). For instance, a 100 percent rise in spending at the state-of-birth and year-of-birth is associated with 45 basis points reduction in the likelihood of less than high school education, 33 basis points reduction in the likelihood of high school education, 50 basis points increase in the likelihood of high school graduation, and 26 basis points rise in the likelihood of having some college education.

Furthermore, we observe considerable improvements in later-life socioeconomic index and total family income. For instance, a 100 percent rise in spending at the state-year-of-birth is associated with a 0.3 units rise in later-life socioeconomic index, an increase of 0.7 percent from the mean of the outcome.[15]

Chetty et al. (2016) examine the relationship between income and life expectancy in the US between the years 2001–2014. They find that an increase of 5 income percentiles (regardless of the baseline income) is associated with roughly 0.9 years of additional life among people aged 40–76. In our census-ACS sample, a rise of 5 income percentiles from the mean equals a rise in income of about $7 K, equivalent to about 2.9 times the coefficient in column 8 of Table 11. Thus, an increase of 2.9 units in log of spending is needed to reach such an income change. Using the same shock on spending in Table 7, we observe an increase in longevity of about 2.9 months. However, as suggested by the state-aggregated effects discussed in Appendix H and column 1 of Table 9, the longevity effect in state-aggregated spending is about 40 percent of city-level spending. Therefore, we expect an increase of about 7.25 months in longevity. This longevity gain is about 70 percent of the gains documented by Chetty et al. (2016). Hence, a considerable portion of the effects could operate through education-income channels.

7. Conclusion

There is a growing body of research exploring the early-life determinants of later-life longevity (Brandt et al., 2012; Brown et al., 2020; Goodman-Bacon, 2021; Janssen et al., 2006; Karas Montez et al., 2014; Montez and Hayward, 2011; Schellekens and van Poppel, 2016). Within this literature, a narrow strand of studies explores the long-term effects of welfare spending on adult outcomes (Almond et al., 2018; Deming, 2009; Goodman-Bacon, 2021). These long-term effects provide important policy implications, adding to the usually unobserved and overlooked social programs' benefits. The current study adds to this ongoing research by documenting the longevity improvements that accrued to those who experienced expansions in welfare spending under the New Deal programs while they were in-utero and in early-life.

We find that doubling the spending (in a period when per capita spending increased by twentyfold) is associated with roughly 1 additional month of life. We show that these effects are not driven by endogenous selection of births, selective fertility, endogenous migration, and sample selection caused by endogenous data linking. We implement placebo tests to rule out the concern that the effects are picking up the overall health improvement trends. As expected, the effects are more pronounced among disadvantaged individuals. Additional analysis using census and American Community Survey data suggests that improvements in education and socioeconomic index are likely channels of impact.

We should note that these effects are only intention-to-treat effects and provide a lower bound for the programs' true effects as the spending is assigned at the population level rather than welfare recipients. It is estimated that the average unemployment rate during the 1930s was between 15–19 percent (Chatterjee and Corbae, 2007; Smiley, 1983). Also, non-marital fertility (eligible for the ADC program) in the 1930s accounts for about 10 percent of all births. Suppose we assume that the assistance is primarily received by the full population of unemployed workers in all years and all unmarried/widow mothers. In that case, the results suggest improvements in longevity of about 3.3 months as a result of a 100 percent rise in spending, in an era when spending per capita increased by roughly 1900 percent. We note that relief spending spillovers could affect non-welfare recipients. For instance, Neumann et al. (2010) show the effects of New Deal spending on employment in the private sector. Fishback et al. (2001) show the spillovers of spending in the retail sale market of neighboring counties. To the extent that these spillovers are operating, the calculated treatment-on-treated effects (extracted from intent-to-treat effects) could be attenuated.

Life expectancy at birth for cohorts born in 1930 and 1940 was 59.7 and 62.9, respectively. Therefore, the rises in longevity as a result of a 100 percent rise in welfare spending during this period accounts for roughly an 8.5 percent rise in life expectancy across cohorts in our sample. Our findings thus show that the impact of increased municipal spending during the New Deal had an impact that went beyond economic stimulation to encompass improved population health.

Another way to understand the magnitudes of the results we report is to conduct a back-of-an-envelope calculation converting them into dollars per additional life-year. If we assume a 100 percent spending increase and further assume that the effects are similar if we had data on the original population of 1940 born between 1929–1940 in the major cities in the final sample, then we can reach

[15] For comparison, Goodman-Bacon (2021) examines the impact of in-utero and childhood exposure to Medicaid introduction in the 1960s on adult labor market and mortality outcomes. That cost-benefit analysis suggests that each additional quality-adjusted life-year (an additional year of life in perfect health) saved by Medicaid costs about $10 K. This number is used for the cost-benefits for all individuals reported in the first column of table 9 of his paper.

511,940 life-years saved. An increase of 100 percent from the mean of spending is roughly $104 (in 2020 dollars). Therefore, the cost of each additional life-year is about $5 K.[16]

Appendix A

Appendix Table A-1 shows the cities and observation-per-city in the final sample. There are 115 cities included in the final sample.

Appendix Table
A-1. Cities and per-City Number of Observations in the Final Sample.

City	Obs.	City	Obs.	City	Obs.	City	Obs.
Akron, OH	3074	Flint, MI	2651	New Orleans, LA	6864	South Bend, IN	1334
Albany, NY	1250	Fort Wayne, IN	1686	New Rochelle, NY	603	Springfield, IL	898
Allentown, PA	1162	Fort Worth, TX	1401	New York, NY	72,257	Springfield, MA	1883
Altoona, PA	1073	Grand Rapids, MI	2085	Newark, NJ	4804	Springfield, OH	949
Asheville, NC	633	Greensboro, NC	648	Newton, MA	755	Syracuse, NY	2456
Atlanta, GA	3671	Hartford, CT	1761	Niagara Falls, NY	1037	Tacoma, WA	1232
Baltimore, MD	11,652	Houston, TX	2527	Norfolk, VA	1776	Terre Haute, IN	824
Bethlehem, PA	663	Huntington, WV	1337	Oakland, CA	2948	Toledo, OH	3872
Birmingham, AL	3192	Indianapolis, IN	5873	Omaha, NE	3218	Topeka, KS	860
Boston, MA	8964	Jacksonville, FL	2175	Philadelphia, PA	20,814	Trenton, NJ	1436
Bridgeport, CT	1888	Jersey City, NJ	4085	Pittsburgh, PA	8187	Tulsa, OK	1952
Brockton, MA	790	Johnstown, PA	875	Pontiac, MI	892	Utica, NY	1117
Buffalo, NY	7319	Kansas City, KS	1672	Portland, ME	1117	Wichita, KS	1429
Cambridge, MA	1277	Kansas City, MO	4483	Portland, OR	2983	Wilkes-Barre, PA	1074
Canton, OH	1308	Kenosha, WI	641	Providence, RI	3281	Wilmington, DE	1463
Charleston, SC	938	Knoxville, TN	1777	Racine, WI	967	Winston-Salem, NC	1033
Charlotte, NC	1195	Lawrence, MA	975	Reading, PA	1301	Worcester, MA	2204
Chester, PA	848	Los Angeles, CA	13,545	Richmond, VA	2667	Yonkers, NY	1751
Chicago, IL	39,270	Louisville, KY	4552	Roanoke, VA	972	Youngstown, OH	2214
Cincinnati, OH	5533	Lowell, MA	1444	Rochester, NY	3405		
Cleveland, OH	10,541	Lynn, MA	1353	Sacramento, CA	1011		
Columbus, OH	3932	Madison, WI	885	Saginaw, MI	1310		
Dallas, TX	2144	Malden, MA	743	Saint Louis, MO	10,692		
Dayton, OH	2815	Memphis, TN	3359	Saint Paul, MN	3591		
Denver, CO	4095	Miami, FL	1640	Salt Lake City, UT	2021		
Des Moines, IA	2292	Milwaukee, WI	8726	San Antonio, TX	1883		
Detroit, MI	22,641	Minneapolis, MN	5403	San Diego, CA	2073		
Duluth, MN	1215	Mobile, AL	988	San Francisco, CA	5150		
El Paso, TX	865	Nashville, TN	2354	Scranton, PA	1478		
Erie, PA	1384	New Bedford, MA	1415	Seattle, WA	3605		
Evansville, IN	1439	New Britain, CT	849	Shreveport, LA	1013		
Fall River, MA	1608	New Haven, CT	1823	Sioux City, IA	1180		

Appendix B

The Numident-census-linked sample contains individuals that are different in observable characteristics from the similar cohorts in the original population of the 1940 census. In section 6.2 and Table 4, we show that these linking selections are not endogenous to changes in relief spending. In Appendix Table B-1, we provide summary statistics of the final sample of the paper next to similar statistics of the original population of cohorts in the 1940 census (from the same cities as in the final sample). Compared with the original population, females are underrepresented in the final sample by about 10 percentage-points. The difference in the mean of this variable in the two samples (reported in the third panel) is statistically significant.

Although T-tests show that differences in the means of other variables across two samples are also statistically significant, the magnitudes of the differences are very small in many cases. For instance, the share of whites in the final and original sample is 90 and 89 percent, respectively. Relief spending in the final sample is lower than the original population, by about 89 dollars (in 2020 dollars). The share of fathers with the below median socioeconomic score in the final sample and the original population is 45 and 46 percent, respectively.

[16] For comparison, Goodman-Bacon (2021) examines the impact of in-utero and childhood exposure to Medicaid introduction in the 1960s on adult labor market and mortality outcomes. That cost-benefit analysis suggests that each additional quality-adjusted life-year (an additional year of life in perfect health) saved by Medicaid costs about $10 K. This number is used for the cost-benefits for all individuals reported in the first column of Table 9 of his paper.

Appendix Table
B-1 - Comparing the Characteristics of the Final Linked Sample with the Original Sample of Cohorts in the 1940 Census.

	Numident-Census-Linked Sample		Original Sample of Cohorts in 1940 Full-Count Sample		Difference between (3)-(1)	
	Mean	SD	Mean	SD	Value	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)
Female	.412	.492	.5101	.4999	.098	.001
White	.904	.295	.8945	.3072	-.009	.0005
Black	.094	.292	.1017	.3022	.0075	.0005
Other Races	.002	.047	.0038	.0617	.0015	0
Hispanic (ethnicity)	.021	.142	.0285	.1664	.0045	.0005
Real per Capita Relief Spending	.5775	1.6934	.6671	1.7803	.0895	.0025
Father's Socioeconomic Index < Median	.448	.497	.4524	.4977	-.02	.001
Father's Socioeconomic Index ≥ Median	.448	.497	.4302	.4951	.0075	.001
Father's Socioeconomic Index Missing	.103	.304	.1174	.3219	.013	.0005
Mother's Education< High School	.515	.5	.4894	.4999	-.0225	.001
Mother's Education=High School	.377	.485	.3803	.4855	.0025	.0005
Mother's Education> High School	.053	.225	.0642	.2452	.0095	.0005
Mother's Education Missing	.055	.228	.0661	.2484	.0105	.0005
Observations	442,929		5,495,307		5,938,236	

Appendix C

In equation, we include a city-specific linear trend as well as a series of region-of-birth-by-birth-year dummies. We argue that the inclusion of these additional covariates is a necessary aspect of our methodology as they help us to eliminate the role of endogenous migration and selection of births by observable characteristics. To show the importance of these additional fixed effects and trends, we replicate the balancing tests of Table 3 in the absence of these controls. The results are reported in Appendix Table C-1. There is significant evidence of an association between spending and selection based on parental characteristics. These meaningful coefficients fail the balancing test in the absence of region-cohort fixed effects and city-specific trends.

Appendix Table
C-1 - Balancing Test in the Absence of City-Trend and Region-Cohort Fixed Effects.

	Outcomes: Female	White	Black	Other Race	Hispanic	Father's Socioeconomic Score	Father's Socioeconomic Index below Median	Father's Socioeconomic Index above Median	Father's Socioeconomic Index Missing	Father's Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log Real per Capita Relief Spending	-.00076	-.00386	.0038	.00007	-.00206*	-.27624	.01489***	-.00666**	-.00823***	-15.85576
	(.00279)	(.00326)	(.00326)	(.00022)	(.00109)	(.16913)	(.00462)	(.0031)	(.00272)	(10.1387)
Observations	449688	449688	449688	449688	449688	449688	449688	449688	449688	393418
R-squared	.01215	.13207	.13735	.0301	.19701	.03936	.03211	.03422	.03934	.05594
Mean DV	0.635	0.880	0.117	0.003	0.021	23.795	0.517	0.348	0.135	1164.361
%Change	-0.120	-0.439	3.244	2.211	-9.794	-1.161	2.880	-1.914	-6.093	-1.362
	Father's Weeks of Work Last Year	Father's Hours Worked Last Week	Father's Years of Schooling	Father's Education Missing	Mother's Years of Schooling	Mother's Education Missing	Father's Age at Child's Birth	Mother's Age at Child's Birth	Father is Present in Household	Mother is Present in Household
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Log Real per Capita Relief Spending	-.28663*	-.01318	-.14158***	-.00037	-.12792***	-.00568***	-.1009	.0572	.00823***	.006***
	(.15993)	(.1011)	(.03176)	(.00201)	(.03547)	(.00202)	(.09392)	(.1016)	(.00272)	(.00189)
Observations	371234	319406	394994	449688	424551	449688	402483	431744	449688	449688
R-squared	.03182	.04707	.09311	.06668	.11032	.04334	.01903	.07852	.03934	.03009
Mean DV	43.932	43.963	8.647	0.019	8.900	0.071	31.775	27.024	0.865	0.949
%Change	-0.652	-0.030	-1.637	-1.928	-1.437	-7.999	-0.318	0.212	0.951	0.632

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects and birth-year fixed effects. All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

Appendix D

In section 6.4, we show that assigning spending to those aged 10 and 15 does not produce meaningful coefficients. We deduce that municipal spending at those ages is not associated with the overall improvement in subsequent health outcomes and health-related technologies, including the invention of new vaccines and the introduction of new drugs. We complement the placebo analysis of that section by showing the effects of spending at year of birth and up to three years after birth. The results are reported in Appendix Table D-1. The effects are insignificant, both economically and statistically, for postnatal ages and remain stable and statistically significant for year of birth.

Appendix Table
D-1 - Assignment of Spending at the Birth-Year and Postnatal Ages.

	Outcome: Age at Death (Months)			
	(1)	(2)	(3)	(4)
Log Real Per Capita Spending Assigned at:				
Year of Birth	.94133**	.82373*	1.04999**	.95806*
	(.42846)	(.429)	(.43508)	(.56563)
Year of Birth + 1	-.24587	-.33163	-.37748	-.50208
	(.51339)	(.53094)	(.58453)	(.71459)
Year of Birth + 2	.02461	.23254	.31679	.48071
	(.60135)	(.64784)	(.70471)	(.83212)
Year of Birth + 3	-.25475	.07864	.19892	.00618
	(.57996)	(.5641)	(.61907)	(.79209)
Observations	380200	380200	380200	380200
R-squared	.26379	.26407	.26438	.27133
City and Birth Year FE	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
City-Level Covariates		✓	✓	✓
Family Controls		✓	✓	✓
State-Level Covariates		✓	✓	✓
City-by-Birth-Year Trend			✓	✓
Region-by-Birth-Year FE				✓

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

Appendix E

In the main text, we use the information on migration of individuals since 1935 to infer the county-of-birth. Specifically, we assume that the county-of-residence in 1935 is the county-of-birth (though with some additional modifications). In this appendix, we implement several additional tests to examine how the migration patterns could induce bias in our estimations. We start by assigning

Appendix Table
E-1 - Replicating the Main Results for Assigning based on 1940 City-County Information.

	Outcome: Age at Death (Months)				
	(1)	(2)	(3)	(4)	(5)
Log Real per Capita Relief Spending	.72756***	.80702**	.79191**	.85192**	.86062**
	(.24892)	(.34383)	(.34255)	(.34945)	(.34941)
Observations	451864	451864	451864	451864	451864
R-squared	.34452	.34476	.34606	.34608	.34623
City and Birth Year FE	✓	✓	✓	✓	✓
Region-by-Birth-Year FE		✓	✓	✓	✓
City-by-Birth-Year Trend			✓	✓	✓
Individual Controls				✓	✓
Family Controls					✓

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table
E-2 - The Association between Relief Spending and Migration Status in 1940 Relative to 1935.

	Outcomes: Cross-State Migrant (1)	Cross-County Within-State Migrant (2)
Log Real per Capita Relief Spending	.00106	.00776
	(.00511)	(.00527)
Observations	442929	442929
R-squared	.25707	.24999
Mean DV	0.032	0.415
%Change	3.297	1.870

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table
E-3 - The Association between Relief Spending and Longevity Controlling for Cohort-State Specific Migration Measures.

	Outcomes: Adding Cohort-by-Birth-State Migration Control (1)	Adding Cohort-by-Current-State Migration Control (2)
Log Real per Capita Relief Spending	1.04356**	.99698**
	(.41346)	(.42175)
Observations	442929	442929
R-squared	.36252	.36252

Notes. Standard errors, clustered at the city level, are in parentheses. Column 1 controls for migration per capita in the state-of-birth of each cohort. Column 2 controls for migration per capita in the state-of-residence in 1940. These measures are built from the full-count 1940 census records. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

the spending based on the county in 1940. The results are reported in Appendix Table E-1. We observe an effect size of 0.86 months (column 5). This is slightly smaller than the effect size of 1 month in the main results. If we assume that the 1935 county is a better proxy for the county-of-birth than the 1940 county, then the measurement error of the county-of-birth proxy biases the results downward. Thus, in case we had the county-of-birth without any measurement error, the effect size would be larger than 1 month.

In Appendix Table E-2, we investigate the association between spending and migration status of individuals. We build two dummies to capture migrants using the information on migration in 1940 relative to 1935. The first is an indicator of cross-state migration, i.e., equals one if the person's state-of-residence in 1935 is different than in 1940. The second indicates within-state migration, i.e., equals one if the person's 1935 and 1940 states are the same, but the county of 1935 is different from than county of 1940. We implement regressions similar to the full specification of the main results. The estimated effects are reported in Appendix Table E-2. The implied percent changes suggest a 3.3 and 1.9 percent increase in cross-state and cross-county migration outcomes for a 100 percent rise in spending, respectively. However, both marginal effects are statistically insignificant.

We also test the sensitivity of the results to migration controls. In so doing, we use all records of the full-count 1940 census. We build a binary variable indicating whether an individual has moved from their state-of-birth. We then collapse the sample by state and year of birth. We merge this data with our final sample based on state and year of birth to measure the share of people who migrated for each cohort and their birth state. We replicate this procedure for state-of-residence in 1940 and build a variable to measure net migration to the 1940 state for each cohort. We then add them in our full specification models and report the results in Appendix Table E-3. Column 1 adds the cohort-birth-state migration control and column 2 adds the cohort-by-current-state migration measure. Both columns report almost identical effects to the main results.

We explore the association between migration status and other outcomes in Appendix Table E-4. For migration status, we build a dummy variable indicating both cross-county and cross-state migration. We focus on age-at-death, individual characteristics, and family covariates as outcomes of interest. We do not observe differences in longevity or likelihood of being female, white, or Hispanic for movers versus stayers. Movers are more likely to have fathers with below median socioeconomic scores and mothers with less than high school education. Appendix Table E-2 suggests that relief spending raises migration, though the results are insignificant. Considering these findings, the higher prevalence of low socioeconomic status among migrants, and the fact that parental socioeconomic measures are positively associated with longevity, we can deduce that our main results regarding the association between spending and longevity understate the true effects.

Appendix Table

E-4 - The Association between Migrant Status and Individual/Family Characteristics.

	Outcomes: Death Age	Female	White	Hispanic	Father's SEI<Median	Father's SEI>Median	Father's SEI Missing	Mother's Education < High School	Mother's Education = High School	Mother's Education College	Mother's Education Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Migrant Status	-.10279	.00227	.00382	-.00018	.01446***	-.00912**	-.00534**	.03487***	-.0209***	-.00746***	-.00651***
	(.33715)	(.00222)	(.00485)	(.00077)	(.0046)	(.00377)	(.00225)	(.00347)	(.00313)	(.00199)	(.00221)
Observations	442929	442929	442929	442929	442929	442929	442929	442929	442929	442929	442929
R-squared	.36251	.02032	.14645	.16176	.03971	.04248	.04945	.0976	.08284	.06372	.05405
Mean DV	783.698	0.634	0.880	0.019	0.517	0.349	0.134	0.479	0.374	0.076	0.071
%Change	-0.013	0.358	0.434	-0.935	2.796	-2.612	-3.984	7.279	-5.588	-9.816	-9.168

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix F

In the main text and specifically in column 1 of Table 8, we added a wide array of county, city, and state covariates. However, we avoided including them in our main regressions as they are probably endogenous to spending and considered *bad controls*. We could further expand our control set to include other potential city-level confounders. However, limited data sources during the specific period covered by our analysis report county-by-year and city-by-year variables. Those data that do contain these measures do not cover all counties and years. In this appendix, we use several data sources and examine the sensitivity of the results to these controls.

The data sources that we use are as follows. We extract measures of school quality and school spending from Carruthers and Wanamaker, (2017). Since there is a growing literature on education and mortality, school spending and school quality could influence longevity and confound our results (Fletcher, 2015; Halpern-Manners et al., 2020; Lleras-Muney, 2005; Modrek et al., 2022). Data on bank deposits come from Manson et al. (2017). While we do not have measures of income-employment for the period of the study, retail sales and bank deposits could operate as a proxy for general local economic conditions (Calomiris and Mason, 2003; Noghanibehambari et al., 2022; Stuckler et al., 2012). The rest of the data sources are explained in section 4.

We start our analyses by showing the correlation between these covariates and spending, conditional on fixed effects and trends. The results are reported in Appendix Table F-1. We should note that for several of the outcomes of this table, we do not have full county-by-year values between 1929–1940. Columns 1–3 show the association between log spending and measures of school quality, including the number of teachers per pupil, log per pupil school spending, and an overall school quality index. Column 4 shows the correlation between spending and log retail sales per capita. In column 5, we show the results for log total bank deposits per capita. We do not observe a significant association for any of these outcomes.

In columns 7–17, we use city controls that are constructed using values of full-count decennial censuses 1920–1940 and interpolated for inter-decennial years. We do not find any significant change in these controls as a result of changes in spending. In column 19, we examine the total disease rate. In columns 20–22, we show the correlations with death rates. None of these outcomes appear to be statistically correlated with spending. Moreover, for all the outcomes, the coefficients are statistically insignificant.

An interesting finding is the lack of significant associations between spending and mortality rates for both infants and non-infants

Appendix Table
F-1 - The Association between Relief Spending and City/County Characteristics.

	Outcomes: Number of Teachers per Pupil	Log per Pupil School Expenditure	School Quality Index	Log Retail Sale per Capita	Log Bank Deposits per Capita	Share of Urbanized	Share of Institutional
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Real per Capita Relief Spending	.00036	-.04006	-.0048	.00438	.02697	.00004	-.00001
	(.00049)	(.03556)	(.03905)	(.007)	(.05284)	(.00016)	(.00002)
Observations	1977	1317	1977	7916	10429	12636	12636
R-squared	.95635	.97416	.93992	.99819	.96895	.99999	.99981
Mean DV	0.032	2.141	0.357	5.402	4.908	0.518	0.007
%Change	1.124	-1.871	-1.345	0.081	0.549	0.007	-0.084
	Share of Homeowners	Average Socioeconomic Score	Share of Female Literate	Share of Male Literate	Male Labor Force Participation Rate	Average Family Size	Average Farmers
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Log Real per Capita Relief Spending	.00037*	-.00002	.00043	.00043	-.00021	.00039	-.00006
	(.0002)	(.00402)	(.00033)	(.00033)	(.00018)	(.0004)	(.00008)
Observations	12636	12636	12636	12636	12636	12636	12636
R-squared	.99992	.99998	.99998	.99998	.99907	.99997	.99999
Mean DV	0.476	26.355	0.492	0.492	0.862	4.559	0.264
%Change	0.079	-0.000	0.088	0.088	-0.024	0.009	-0.022
	Share of Children 0-4 Years Old	Share of Children 5-11 Years Old	Share of Children 12-18 Years Old	Disease Rate	Stillbirth Rate	Infant Death Rate	Total Death Rate
	(15)	(16)	(17)	(19)	(20)	(21)	(22)
Log Real per Capita Relief Spending	0.00001	.00005	-.00001	-6.71889	-.34016	.59704	-4.13588
	(.00002)	(.00003)	(.00003)	(25.96706)	(.47431)	(1.32375)	(4.13433)
Observations	12636	12636	12636	12636	12636	12636	12636
R-squared	.99992	.99994	.9999	.87141	.9866	.97242	.95492
Mean DV	0.089	0.115	0.151	807.654	63.248	104.270	1109.323
%Change	0.001	0.039	-0.004	-0.832	-0.538	0.573	-0.373

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table
F-2 - Robustness of Results to Additional Control Sets.

	Outcome: Age at Death (Months) Similar Sample to Column 2	Adding County-Level Teacher per Pupil	Similar Sample to Column 4	Adding Log Total Per Pupil School Spending	Similar Sample to Column 6	Adding School Quality Index	Similar Sample to Column 8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Real per Capita Relief Spending	1.72159 (1.32392)	1.67484 (1.37295)	.48519 (1.76775)	.2954 (1.91241)	1.72159 (1.32392)	2.34282* (1.16348)	1.06023** (.40928)
Observations	34326	34326	20354	20354	34326	34326	442929
R-squared	.32472	.32476	.19197	.19199	.32472	.32479	.36351
Mean DV	0.032	2.141	0.357	5.402	4.908	0.518	0.007
%Change	1.124	-1.871	-1.345	0.081	0.549	0.007	-0.084
	Adding Log Retail Sale per Capita	Similar Sample to Column 10	Adding Log Bank Deposit Per Capita	Similar Sample to Column 12	Adding City Controls from Decennial Censuses	Similar Sample to Column 14	Adding Disease and Death Rates
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Log Real per Capita Relief Spending	.99294** (.42162)	1.08583** (.47864)	1.07179** (.47937)	1.06023** (.40928)	1.12494*** (.41893)	1.06023** (.40928)	1.03655** (.42241)
Observations	442929	282837	282837	442929	442929	442929	442929
R-squared	.36352	.21429	.21429	.36351	.36354	.36351	.36353
Mean DV	0.476	26.355	0.492	0.492	0.862	4.559	0.264
%Change	0.079	-0.000	0.088	0.088	-0.024	0.009	-0.022

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

(columns 21–22). The implied percent change from the mean of the outcomes reveals quite small effects, changes around 0.5 percent. This fact alleviates concern about selective infant/child survival into adulthood that is correlated with spending and could confound the long-run effects on longevity.

In Appendix Table F-2, we add these control sets into our main regressions. First, we implement regressions for samples that exclude missing values of each control set. We report the results in odd columns. The following columns add the control set. The main purpose is to observe the results in similar samples with and without controls. In columns 2, 4, and 6, we add school spending and quality measures. Although the effects are mostly insignificant in both samples with and without controls due to very small sample sizes, the marginal effects are quite comparable with those in their respective excluded-control-sample results (comparing with columns 1, 3, and 5, respectively).

We observe very similar effects to the main results of the paper when we add retail sales per capita (column 8), bank deposit per capita (column 10), city controls (column 12), and disease-death rates (column 14). Overall, the available data do not provide any evidence for city-county level confounders of the key relationship.

Appendix G

In the main results, we use the total amount of spending as there could be spillovers in each category of spending to individuals' health that can appear in their old-age longevity. In Appendix Table G-1, we show the results for three disaggregated spending

Appendix Table
G-1 - Heterogeneity in the Effects by Type of Spending.

	Outcome: Age at Death (Months) Public Assistance Related to Work Programs	Private Assistance	Public Assistance (Aid to Blind, Old-Age, ADC)
	(1)	(2)	(3)
Log Real per Capita Relief Spending	.5442* (.27902)	.66433** (.29429)	.65292*** (.2278)
Observations	442929	442929	442929
R-squared	.36254	.36254	.36255

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** p<0.01, ** p<0.05, * p<0.1

measures, including total work-related public assistance and grants, total private contributions and assistance, and total public welfare assistance. The results suggest significant effects on longevity from all three measures. However, compared with work-related public assistance, the estimated coefficients are slightly larger for public assistance to Aid to Dependent Children, Old-Age Assistance, and Aid to Blind.

Appendix H

To explore the importance of spending at alternative levels of governance, we use census and American Community Survey data on state-of-birth, which we merge with state-aggregated spending data (see section 6.9). In this appendix, we show that the main results are indeed robust to state-aggregated spending. The results, reported in Appendix Table H-1, suggest that the marginal effects drop by almost 50 percent (compared to the reported effects of Table 7) but remain statistically significant and robust across models.

Appendix Table H-1
- Robustness of the Results to Collapsing the Spending at the Birth-State Level.

	Outcome: Age at Death (Months)		
	(1)	(2)	(3)
Log Real per Capita Relief Spending	.69145**	.69145**	.4172*
	(.27951)	(.27951)	(.22468)
Observations	1847162	1847162	1847162
R-squared	.19893	.19893	.19894
Mean DV	761.419	761.419	761.419
%Change	0.091	0.091	0.055
Birth-State FE	✓	✓	✓
Region-by-Birth-Year FE	✓	✓	✓
Individual Controls		✓	✓
State Controls			✓

Notes. Standard errors, clustered at the birth-state level, are in parentheses. All regressions include birth-sate fixed effects, birth-year fixed effects, and region-of-birth-by-birth-year fixed effects. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using state-level population.
*** p<0.01, ** p<0.05, * p<0.1

Appendix I

In section 6.9, we offered empirical evidence suggesting that in-utero and early-life exposure to increased relief spending improves subsequent educational and socioeconomic outcomes, potential channels linking spending and longevity. In this appendix, we implement a series of Two-Sample Two-Stage Least-Square (TS2SLS) estimations in which the endogenous regressors are education/ socioeconomic outcomes, and the exogenous regressor is the log of real per capita relief spending. Since we do not have city-of-birth data in the census-ACS sample for the first stage, we follow the same strategy as in section 6.9 and Table 11 to aggregate the spending data at the birth-state level and include birth-state fixed effects, birth-year-by-region fixed effects, and birth-state interacted with a

Appendix Table
I-1 - The Results of Two-Sample Two-Stage Least-Square Estimations.

	Outcome: Age at Death (Months)			
	(1)	(2)	(3)	(4)
Education< High School	-6.28646***			
	(2.18313)			
Education= High School Graduate		1.627469***		
		(0.562326)		
Education>High School			0.732814	
			(0.476156)	
Socioeconomic Index				.0798426***
				(.0271699)
Observations	8,737,930	8,737,930	8,737,930	8,737,930
R-squared	0.1914	0.1914	0.1914	0.1914
Mean DV	772.8799	772.8799	772.8799	772.8799

Notes. Standard errors, clustered at the birth-state level, are in parentheses. All regressions include state-of-birth fixed effects, year-of-birth fixed effects, region-by-year-of-birth fixed effects, and individual controls. Individual controls include dummies for gender, race, and ethnicity. The results are from Two-Sample Two-Stage-Least-Square (TS2SLS) regressions in which the exogenous variable is log of real per capita relief spending and endogenous regressors are reported in rows.
*** p<0.01, ** p<0.05, * p<0.1

linear cohort trend. We also collapse the spending at the birth state for the second stage regressions and follow the same strategy as in the first stage. The results are reported in Appendix Table I-1. Having less than high school education (i.e., years of schooling less than 9) is associated with roughly 6 months lower longevity compared with those who have at least a high school education. The marginal effect of column 2 implies a 1.6 months gain in longevity for high school graduates.

Appendix J

In this appendix, we show the heterogeneity in the results across states that experienced larger drops in per capita income during the years 1929–1936. We chose these years as, in most states, income per capita experienced a constant fall for this period. We show the results by quartiles of long-difference in income and report them in Appendix J. the marginal effects suggest larger impacts as we go up the quartiles of income drop. However, since these drops in income could have absorbed higher levels of spending, it could be the case that the effects only reveal a higher concentration of spending rather than the impact of spending across less/more affected places. This is the primary reason that we do not interact these state measures with our treatment variable in the main analyses of the paper. (Table J-1)

Appendix Table
J-1 - Heterogeneity in the Effects by State-Level Drop in Income per Capita over the Years 1929-1936.

	Outcome: Age at Death (Months), Subsamples based on drop in state-level income 1929-1936			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	(1)	(2)	(3)	(4)
Log Real per Capita Relief Spending	.60072	1.92104	1.53118*	3.77482**
	(.51204)	(1.99688)	(.9015)	(1.56451)
Observations	303128	54365	63487	20503
R-squared	.3606	.36148	.36901	.38239

Notes. Standard errors, clustered at the city level, are in parentheses. All regressions include city/county fixed effects, birth-year fixed effects, region-of-birth-by-birth-year fixed effects, and city/county-specific linear trend. Individual covariates include dummies for race, ethnicity, and gender. Parental controls include dummies for maternal education and paternal socioeconomic score (and missing indicators for missing values). All regressions are weighted using the inverse probability weighting method, where weights are the probability of linkage between Numident and 1940-census using probit regressions conditional on individual and parental characteristics.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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