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BIRTHS, DEATHS, AND NEW DEAL RELIEF DURING THE GREAT DEPRESSION

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Abstract—The article examines the impact of New Deal relief programs on infant mortality, non-infant mortality, and general fertility rates in major U.S. cities between 1929 and 1940. Effects are estimated using a variety of specifications and techniques for a panel of 114 cities that reported information on relief spending between 1929 and 1940. The significant rise in relief spending during the New Deal contributed to reductions in infant mortality, suicide rates, and some other causes of death, while contributing to increases in the general fertility rate. Similar to Ruhm's (2000) findings for the modern United States, the article finds that many types of death rates were pro-cyclical during the 1930s. Estimates of the relief costs associated with saving a life (adjusted for inflation) are similar to those found in studies of modern social insurance programs.

I. Introduction

HOW well do social welfare programs mitigate the effect of economic disasters on demographic outcomes? To answer this question, this article examines the effect of New Deal relief programs on fertility, infant deaths, non-infant deaths, suicides, homicides, and other causes of death during the Great Depression. During the 1930s Americans experienced an economic disaster that lasted an entire decade, as unemployment rates peaked at well over 20% of the labor force and remained over 10% throughout. Sharp reductions in income and consequent inadequate access to nutrition, housing, and medical care put people of all ages at greater risk of death and disease. Economic problems potentially fueled social and psychological stresses that contributed to more suicides and homicides. Meanwhile, the greater uncertainty about the future led many couples to delay starting or adding to their families.

To combat the Depression, the federal government expanded its social welfare spending in a dramatic and unprecedented fashion. In the early 1930s the burden of providing relief rested on state and local governments and private charitable organizations. The extraordinary unemployment eventually overwhelmed their resources, despite loans offered by President Hoover in late 1932. President Franklin Roosevelt's New Deal revolutionized welfare spending both in the immediate and longer terms. The federal government poured resources into direct relief from 1933 to 1935 and emergency work relief from 1933 through 1942, leading to an immediate doubling and ultimately a tripling of per capita relief spending by the late 1930s, even though unemployment rates were substantially below the 1932 level.

To examine the impact of relief on economic welfare, the authors follow other economists in examining birth and death rates. Infant mortality rates, deaths from a variety of causes, and fertility rates are typically associated with socioeconomic status and poverty and are now commonly used to measure aspects of economic welfare not fully captured by income measures.[1] This is particularly important during the 1930s when good estimates of the incomes of the poor and unemployed are scarce and New Deal relief was designed to help people in the lower tail of the income distribution. The authors have developed a new panel that combines data on relief spending by all levels of government, birth and death rates, and other socioeconomic information for 114 cities annually from 1929 through 1940.[2] The panel captures the dramatic increases in relief spending during the 1930s, and variation in those increases across cities is used to measure the impact of relief spending on demographic

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[1] See Morris (1979) and OECD (2001) on life expectancy and infant mortality as measures of well-being. See Antonovsky and Bernstein (1977), Clifford and Brannon (1978), Waldmann (1992), Paxson and Waldfogel (1999), and Case, Fertig, and Paxson (2003) for discussions of childhood health, socioeconomic status of parents, and later success in life.

[2] The U.S. Children's Bureau published annual information on public assistance from all levels of government for 1929 through 1935 (Winslow, 1937). The U.S. Social Security Board then updated the series and carried the data forward through 1940 (Baird, 1942).

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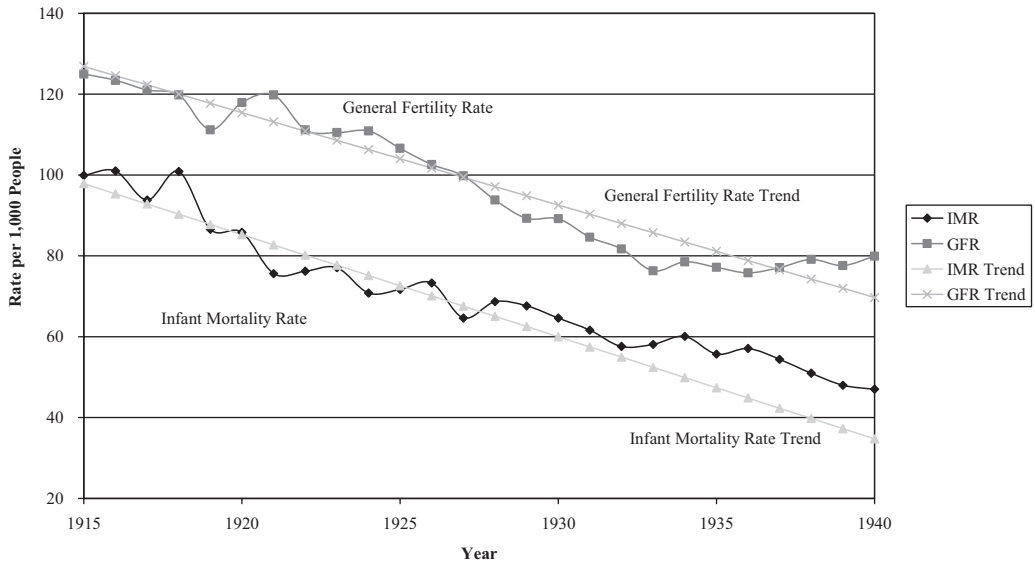
FIGURE 1.—INFANT MORTALITY AND GENERAL FERTILITY RATES, 1915–1940, AND TREND BASED ON 1915–1917, 1919–1929



outcomes. Analysis using fixed effects to control for unmeasured heterogeneity and instrumental variables to control for endogeneity shows that New Deal relief spending was associated with lower infant mortality, lower suicide rates, fewer deaths from diarrhea and infectious diseases, and higher birth rates. The New Deal relief programs thus contributed to returning society toward more normal demographic patterns during the Depression.

II. Demographic Trends and the Depression

The Great Depression depressed birth rates and elevated death rates relative to trends established between 1915 and 1930. Figures 1 and 2 show the national rates from 1915 to 1940, as well as trend lines derived from linear regressions of the rates on year for the periods 1915–1917 and 1919–1929. We eliminated 1918 in determining the trend due to the sharp spike associated with the Great Influenza and U.S. entry into World War I.[3] The general fertility rate (GFR), defined as the number of live births per female aged 15–44, displayed a downward trend between 1915 and 1929. The actual GFR in figure 1 fell below this trend by the late 1920s and fell even further below in 1933. From 1933 through 1940 the GFR remained around eighty live births per thousand before rising during World War II and the baby boom.

Trends in the national infant mortality rate (IMR), defined as the number of deaths under age one per thousand live births, show long-term declines prior to the 1930s (Cutler & Meara, 2001). The IMR in figure 1 continued downward through 1932 along the 1915–1929 trend line. As the Depression reached bottom in 1933, the IMR rose slightly in 1933 and 1934 and then remained well above predictions from the pre-Depression trend for the rest of the 1930s. Part of the decline in the IMR from 1915 to 1929 may have been associated with a decline in the non-infant death rate (NDR), the number of deaths of people over one year old per 1,000 people in the population. Between 1930 and 1933 the NDR matched predictions based on the earlier trend. As the economy recovered, the NDR rose to a peak near eleven deaths per thousand in 1936 before dropping back to ten in the late 1930s. In a rough sense the NDR displayed the same kind of pro-cyclical behavior that Christopher Ruhm (2000) documented for the United States between 1972 and 1991.

The national averages disguise substantial variation across cities for relief spending and demographic outcomes over the course of the decade. The average IMR for the 1930s, for example, ranged from a low of 30.1 infant deaths per 1,000 live births in Newton, Massachusetts, to a high of 104.5 in El Paso, Texas. More importantly, there was substantial variation across cities in the changes in birth and death rates and in the changes in per capita relief spending across cities between the early 1930s and the New Deal era. Figure 3 shows for each city the average IMR rate in 1933 through 1940 minus the average for 1930 through 1932 plotted against the difference in average per capita relief spending for the two time periods. The differences in average IMRs across cities ranged from 8 per thousand to −30 per thousand, while the differences in average per capita relief spending ranged from \$8 to \$70. In a preview of one of the articles' findings, the differences in relief spending and the differences in infant mortality rates are negatively correlated ($p = -0.22$). A regression of the change in

[3] Inclusion of 1918 in the trend calculation has little effect on the general fertility rate trend, while leading to more negative trends for infant mortality and non-infant mortality, strengthening our claim that the Depression significantly influenced demographic outcomes. Trends for our 114-city sample in the 1930s are similar to the national trends in figures 1 and 2.

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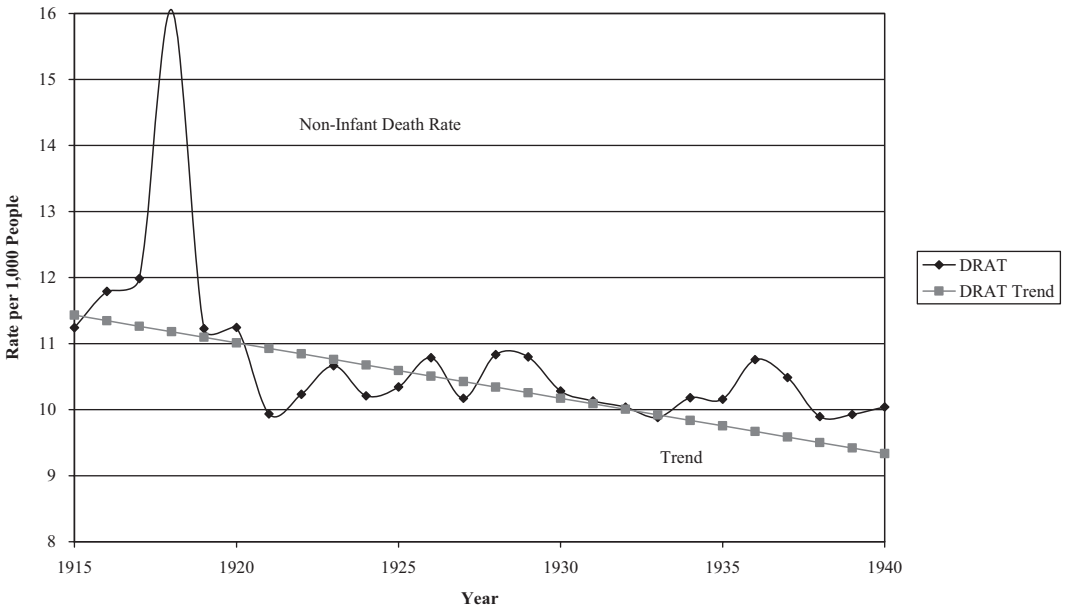
FIGURE 2.—NON-INFANT DEATH RATE 1915–1940, AND TREND BASED ON 1915–1917, 1919–1929



average IMR on the change in average per capita relief led to a statistically significant coefficient of −0.094.[4]

III. Relief Spending during the Great Depression

The relief programs of the 1930s were designed to enhance the material well-being of families in dire straits. It gave them access to such critical items as food, housing, clothing, and healthcare, which likely led to lower infant mortality and non-infant death rates. The presence of a stronger financial safety net might have contributed to families' feeling more secure in returning to their long-range fertility plans.

When the Great Depression struck, provision of welfare and social insurance was the primary responsibility of local governments, with some specific support from state programs. A number of cities provided shelter and food in almshouses, while some cities provided relatively small amounts of cash assistance and in-kind aid to the poor. Private charities often distributed various forms of aid, of which a significant portion was funded by local governments. Nearly all states had established mothers' pensions for widowed women with dependent children. Injured workers were covered under workers compensation laws established during the 1910s. An increasing number of states during the early 1930s instituted limited cash benefits for the elderly poor, and about half the states offered cash benefits to the blind. Some governments tried to provide work for the unemployed through limited public works projects. The aid was administered by social workers, charities, and local officials who tried to assess the recipients' needs and to some extent their "moral worthiness." Prior to 1933 the federal government played almost no role in providing relief spending beyond aid to veterans (Skocpol, 1992, ch. 2).

As the Depression deepened and tax revenues dropped between 1929 and 1933, state and local social welfare resources were overwhelmed. The state and local governments in the sample cities had managed to increase real per capita relief spending from $3.74 in 1930 to $9.06 in 1931 (1967 dollars). With the aid of loans from the newly created Reconstruction Finance Corporation in 1932, the average had risen to $18.06. Faced with national unemployment rates near 25% in 1933, the Roosevelt administration accepted responsibility for relief, arguing that unemployment and poverty had become a national problem. As the federal government raised its share of total relief spending from 2% in 1932 to 79% by 1934, average per capita relief spending from all sources in the sample of 114 cities jumped to $30 in 1933 and then to $48 in 1934, the first full year of the New Deal. The annual average benefit payments to a relief household during this early phase of the New Deal replaced between 31.2% and 33.3% of average annual manufacturing earnings.[5]

[4] The downward trend in infant mortality rates prior to the 1930s is not driving the negative correlation in figure 3. The correlation across cities between the change in average IMR between 1927–1929 and 1930–1932 and the change in average per capita relief from 1930–1932 to 1933–1940 is positive 0.165. See also the discussion of trend effects in the econometric analysis.

[5] The measure of per capita relief spending for the 114 cities in the analysis and in this section combines direct relief, work relief, and private relief funds from all levels of government. The federal relief data includes the CWA, FERA, WPA, and Social Security programs for aid to dependent children, aid to blind, and old-age assistance. The sample average of per capita relief in this section is a weighted average based on population. Average annual manufacturing earnings are from U.S. Bureau of the Census (1975, p. 166) and average relief expenditures per household are

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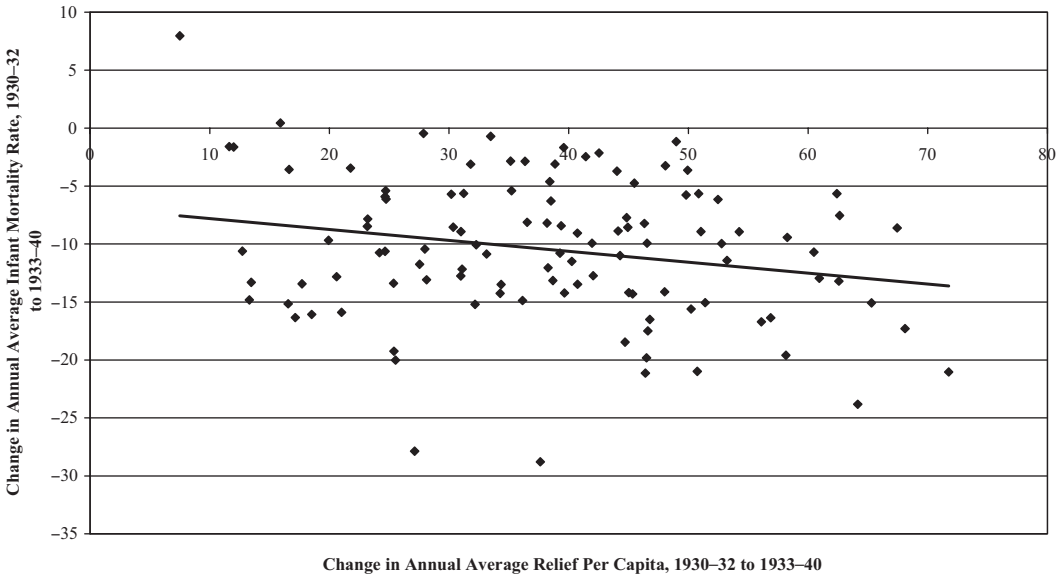
FIGURE 3.—PLOT OF DIFFERENCES IN DIFFERENCES BETWEEN 1930–1932 AND 1933–1940 FOR SAMPLE CITIES



Between July 1933 and June 1935, the primary relief agency was the Federal Emergency Relief Administration (FERA). Federal FERA officials distributed funds to state governments through an opaque process in which officials seem to have paid attention to economic distress in the state, the state's entreaties to FERA administrators, the state's own efforts to fund relief, and likelihood of funds influencing Roosevelt's reelection.[6] State governments then distributed the funds internally to local governments. Once established, the FERA offered both direct relief and work relief. Direct relief included programs that had no specific work requirements, and assistance was provided in cash or in-kind, including subsistence items, such as food, shelter, clothing and household necessities, or medical care and hospitalization. Work relief required labor on a government project. The FERA set a series of broad guidelines for its programs, but relied heavily on state and local officials to administer them and to determine the appropriate amounts of relief that individuals would receive. Applicants for relief applied to local offices, where officials met with them and determined their eligibility for relief based on the "budget deficit" between the family's total income and hypothetical expected spending for a family of that size. This budget deficit was the basis for the family's direct relief benefits or the work relief payment on a FERA project. The actual relief benefit fell short of the budget deficit when FERA local officials faced a large number of cases and lowered benefits per case to aid more households.

In response to a harsh winter and high levels of unemployment, FERA activities were supplemented temporarily by the Civil Works Administration (CWA) work relief program from November 15, 1933, through March 1934. Large numbers on the FERA relief rolls were transferred to CWA employment, where they received wages that were not based on the budget deficit principle. After employing up to four million workers in January 1934, the CWA shut down two months later and many CWA workers were shifted on to new FERA work relief projects.[7]

In mid-1935 the Roosevelt administration redesigned the relief system. The federal government continued to provide work relief for the "employable" unemployed through the Works Progress Administration (WPA) and returned much of the responsibility for direct relief of "unemployables" to state and local governments. Applicants for aid were certified by state and local officials, who still considered a family's budget deficit when assessing its need for relief employment. The federal WPA then hired people from the certified rolls. The WPA, like its FERA predecessor, used no hard and fast set of rules to distribute the funds, but econometric studies have found that local economic distress, the lobbying of state and local governments, presidential politics, and representation on key congressional committees were correlated with greater spending in a county.[8]

The federal government contributed matching funds for aid to "unemployables," as the Social Security Act of 1935 introduced state-federal versions of many states' prior old-age assistance, mothers' pensions (aid to dependent children), and aid to the blind. By the end of 1938, all but eight states were receiving federal grants. The shift in federal

based on data in U.S. National Resources Planning Board (1942, pp. 557–561). The federal share of relief spending is from the U.S. National Resources Planning Board (1942, p. 292, pp. 598–603).

[6] Fishback, Kantor, and Wallis (2003) and Fleck (1999c) analyze the FERA distribution.

[7] Brown (1940, pp. 218–298) and U.S. National Resources Planning Board (1942, pp. 26–97) describe the administration of the FERA and CWA.

[8] See Fishback, Kantor, and Wallis (2003) and the numerous references cited therein.

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relief efforts and the eventual reductions in WPA spending caused the federal government's share of relief spending to decline from 79% to 57.4% between 1935 and 1940. Meanwhile, average per capita relief spending in the 114 cities rose to $62 in 1936, fell to $52 in 1937, spiked to $70 in the downturn of 1938, and declined to $53 by 1940. Relief benefits rose to between 35% and 42% of average annual manufacturing earnings after 1935.

In performing the analysis, we focus on the combined value of all forms of relief rather than examine the impact of particular relief programs. Many households received funds from multiple programs over the course of the 1930s, making it hard to isolate each program's effect. In addition, categorical programs like ADC and state mothers' pensions are too narrowly focused when matched with our measures of demographic outcomes. In many states, mothers' pensions and the later aid-to-dependent children programs were limited to households with children with at least one parent missing. Only 7% of the households with infants under age one met this criterion; therefore, the vast majority of households with infants received relief from the general programs.[9]

IV. The Empirical Model and Infant Mortality Rates

Our goal is to examine the relationship between relief spending and various demographic outcomes—infant mortality rates, non-infant death rates, cause-specific death rates, and the general fertility rate. We use the same basic modeling procedures for each demographic outcome. We establish the template for all the analyses by first working through the estimations for infant mortality rates and then examine the remaining demographic outcomes.

We estimate the following reduced-form infant mortality equation:

$$IMR_{it} = f(R_{it}, Y_{it}, X_{it}, e_{it}), \tag{1}$$

where IMR_{it}, the number of infant deaths per 1,000 live births in city i in year t, is modeled as a function of per capita relief spending in city i in year $t(R_{it})$, economic activity (Y_{it}), a vector of demographic characteristics (X_{it}), and random error (e_{it}). Because income data at the city level do not exist for the 1930s, we use per capita retail sales in the county where the city was located as the measure of economic activity.[10] The X vector contains a series of socioeconomic factors. These include the percent black, percent foreign born, and percent illiterate to capture ethnic and racial differences in income and cultural practices toward raising infants. To control for the differences in health and maturity among the potential childbearing population in the infant mortality and general fertility equations, we include the percentages of women in their prime childbearing years, separated into age categories 15–19, 20–24, 25–29, 30–34, and 35–44. In estimation equations for non-infant deaths and the specific causes of death, we control instead for the age distribution of the entire population. Finally, percent urban is included because for many observations relief information was reported for the entire county, including the city and rural areas. In those situations county-level death and birth rates were used.

A variety of econometric specifications show an inverse relationship between per capita relief spending and the infant mortality rate. Table 1 reports the coefficients and t-statistics of the relief variable under different specifications. It also includes the number of standard deviations by which infant mortality changed in relation to a one-standard-deviation (OSD) increase in per capita relief. Because changes of one standard deviation are fairly common, the OSD effect is a good measure of the historical explanatory power of changes in relief spending. The coefficients and t-statistics for all of the correlates in the OLS specification with and without city and year effects are reported in table 2.

The baseline OLS coefficient suggests that a $1 increase in per capita relief was associated with a 0.32 reduction in infant deaths per thousand life births. The addition of a series of controls for economic activity (retail sales per capita) and demographic differences across cities leads to a coefficient of −0.27. Other unmeasured factors, like public health and sanitation programs, varied across cities in ways that likely cause a negative bias in this coefficient. Cities with better public health and sanitation were also more likely to spend on relief themselves and lobby more effectively for relief spending from the federal government. The combination of the positive correlation between public health and relief programs and the negative correlation between public heath activity and the infant mortality rate would bias the OLS coefficient downward. We therefore include city effects to control for the time-invariant components of public health and sanitation, as well as time-invariant aspects of local customs and attitudes, regional diets, availability of medical care, cost of living, and climate. Year effects are incorporated to control for shocks to the national economy or technological changes in healthcare, like new vaccines, that all cities experienced. The addition of the fixed effects in tables 1 and 2 substantially reduces the magnitude of the coefficient to −0.032, which is statistically significant at the 10% level. Under this specification, an OSD increase in relief spending reduced the infant mortality rate by 0.05 standard deviations.

The long-term downward trend in the national infant-mortality rate raises the possibility that the rise of relief spending in the 1930s simply coincided with a continuation

[9] The 7% calculation is based on households reported in the Integrated Public Use Microdata Series (IPUMS) from 1940.

[10] At the state level, per capita retail sales have correlations with per capita personal income above 0.87 for the 1930s. See Fishback, Horrace, and Kantor (2005).

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TABLE 1.—THE IMPACT OF RELIEF SPENDING ON THE INFANT MORTALITY RATE, 1929–1940

Specification	Relief Coeff.	*t*-stat.	OSD[1] Effect
Specifications without Non-infant Death Rate and General Fertility Rate			
OLS with no other correlates and no fixed effects	−0.3208	−23.91	−0.526
OLS with other correlates and no fixed effects	−0.2698	−19.87	−0.442
OLS with other correlates and fixed effects			
Full sample	−0.0319	−1.78	−0.052
Subsample of 97 cities for comparison with 1921–1928 trend	−0.0368	−2.05	−0.060
Subsample of 97 cities with 1921–1928 trend included	−0.0358	−2.01	−0.059
2SLS with correlates and fixed effects			
Full sample	−0.1628	−1.82	−0.267
Subsample of 97 cities for comparison with 1921–1928 trend	−0.1141	−1.44	−0.187
Subsample of 97 cities with 1921–1928 trend included	−0.1146	−1.45	−0.188
Specifications Including Non-infant Death Rate and General Fertility Rates			
OLS with no correlates and no fixed effects	−0.2827	−24.26	−0.463
OLS with correlates without fixed effects	−0.2481	−20.18	−0.406
OLS with correlates and fixed effects			
Full Sample	−0.0348	−2.18	−0.057
Subsample of 97 cities for comparison with 1921–1928 trend	−0.0438	−2.74	−0.072
Subsample of 97 cities with 1921–1928 trend included	−0.0418	−2.64	−0.069
2SLS with correlates and fixed effects			
Full sample	−0.1699	−2.12	−0.278
Subsample of 97 cities for comparison with 1921–1928 trend	−0.1299	−1.82	−0.213
Subsample of 97 cities with 1921–1928 trend included	−0.1297	−1.82	−0.213

[1]The OSD effect is the estimated change in the infant mortality rate, expressed in terms of a standard deviation change from the sample mean, associated with a one-standard-deviation increase in per capita relief spending.

Notes: The correlates are listed in table 2. The *t*-statistics are based on White-corrected standard errors. In all 2SLS specifications, the identifying instruments in the first stage have coefficients that are statistically significant at the 10% level individually. The *t*-statistics on the swing-voting measure range from 3.30 to 5.20, the House Labor Committee from 1.80 to 2.05, and the Democratic governor dummy from 4.97 to 5.63. The *F*-statistics for the identifying instruments as a group are all 16.8 or higher. The results of Hausman overidentification tests in all cases are consistent with the hypothesis that the identifying instruments have not been inappropriately omitted from the second-stage equation.

Sources: Relief information from 1929 through 1935 was reported in Winslow (1937) and data from 1936 through 1940 is from Baird (1942). Infant mortality and birth rates are from the U.S. Bureau of the Census, *Birth, Stillbirth and Infant Mortality Statistics.* Non-infant deaths are reported in the annual volumes of the U.S. Bureau of the Census, *Mortality Statistics.* Population figures are from Haines and ICPSR (2004), and measures of the county-level age distribution of the population are from Gardner and Cohen (1992). Retail sales from 1929 and 1939 are from Haines and ICPSR (2004), and retail sales in 1933 and 1935 are from the U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1936, 1939). The demographic characteristics of the cities and counties, such as ethnicity, nativity, illiteracy, and the extent of urbanization, are from Haines and ICPSR (2004). The three instrumental variables in the analysis—the volatility of the vote for the Democratic presidential candidate from 1896 to the election prior to the observation year, representation on the House Labor Committee, and a Democratic governor dummy—were drawn from ICPSR (1999), U.S. Congress (various years), and from Congressional Quarterly Inc. (1995, pp. 639–663), respectively. For a more detailed discussion of the sources, see Fishback, Haines, and Kantor (2005, pp. 30–34). The cities eliminated when the 1920s trends are included are Atlanta, Birmingham, Denver, El Paso, Fort Worth, Houston, Kansas City (MO), Knoxville, Memphis, Mobile, Nashville, New Orleans, San Antonio, St. Louis, and Tulsa.

TABLE 2.—RESULTS OF THE INFANT MORTALITY RATE ESTIMATION UNDER ALTERNATIVE SPECIFICATION

	OLS		OLS with Fixed Effects		2SLS with Fixed Effects			
					Infant Mortality Rate		Relief per Capita	
	Coeff.	*t*-stat.	Coeff.	*t*-stat.	Coeff.	*t*-stat.	Coeff.	*t*-stat.
Relief per capita	−0.270	−19.87	−0.034	−1.93	−0.163	−1.82	Dependent	
Instruments								
Swing							1.261	3.34
House Labor							2.560	2.05
Governor democrat							4.893	5.63
Correlates								
Retail sales per capita	−3.990	−2.02	1.696	0.52	−0.242	−0.06	−15.218	−3.05
Percent black	0.017	0.19	−0.534	−0.65	−0.356	−0.44	0.706	0.70
Percent illiterate	3.069	7.75	0.880	1.03	1.634	1.71	5.125	4.68
Percent urban	0.083	2.78	−0.144	−1.10	−0.091	−0.65	0.705	3.10
Percent foreign born	−0.682	−8.12	−0.499	−0.95	−0.828	−1.47	−2.600	−6.24
Percent of women in the following age categories:								
15–19	−0.133	−0.28	0.853	0.79	1.998	1.49	9.874	6.59
20–24	2.256	3.58	1.861	1.75	1.818	1.70	−0.515	−0.32
25–29	−2.432	−3.01	−3.178	−2.56	−2.144	−1.53	8.524	5.26
30–34	1.103	1.06	2.441	1.44	2.781	1.60	3.634	1.45
Constant	41.836	1.60	43.948	0.77	2.319	0.04	−395.603	−4.79
City effects	excluded		included		included		included	
Year effects	excluded		included		included		included	
N. obs.	1,349		1,349		1,349		1,349	
R-squared	0.587		0.862		0.856		0.878	

Note: The *t*-statistics are based on White-corrected standard errors.
Sources: See table 1.

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of the downward trend in the IMR from the 1920s. Following Werner Troesken (2004), we develop city-specific predictions of the trend in the IMR by running linear regressions of the infant mortality rate on the year for each city with four or more observations for the period 1921 through 1928. We then develop trend rate predictions for infant mortality rates in each city from 1929 through 1940. Due to lesser coverage of the death and birth registration areas in the 1920s, the trends could be determined for only 97 cities. Of the 97 cities, 87 displayed negative trends between 1921 and 1928, of which 81 trends were statistically significant at the 10% level. Table 1 shows that the fixed-effects results for 1929 through 1940 for this group of 97 cities are similar when excluding and including the predictions from the prior trend; and both coefficients are similar to the fixed-effect coefficient for the entire sample. The addition of controls for prior time trends had little effect on the relief coefficients and *t*-statistics in all of the various estimation procedures for infant mortality rates, general fertility rates, and non-infant death rates that follow; therefore, we focus on the results without the time-trend controls in the rest of the paper.[11]

Even after controlling for fixed effects and prior trends, there might remain another channel for endogeneity. Although the retail sales measure controls for changes in general economic activity during the 1930s, we still may be inadequately controlling for differential shocks to incomes and other factors affecting the poorest segment of the population. People in the lower tier of the income distribution were more likely to experience health problems in response to negative shocks, given their low starting levels of income. In the modern era, greater income inequality is associated with worse health outcomes and relatively riskier behaviors (Kaplan et. al, 1996; and Kennedy, Kawachi, & Prothrow-Stith, 1996). If the absence of an income inequality measure were to create problems with endogeneity bias in the OLS with fixed-effects analysis, there would need to be variation in poverty across time within cities and/or across cities for a specific year in ways unrelated to the retail sales measure. Such unmeasured shocks for the poor were likely to be associated with greater relief spending and also with higher infant mortality and death rates. The combination of these two positive correlations would lead to a positive bias in the relief coefficient in the fixed-effects equation that would cause the coefficient to understate the salutary impact of relief spending. To address this potential channel for endogeneity, we use an instrumental variables approach.

Unlike the modern federal system with precisely specified allocation rules, New Deal administrators distributed funds through an opaque process. As a result, an extensive econometric literature on the determinants of the geographic distribution of New Deal spending has developed.[12] Nearly every study finds that the Roosevelt administration used New Deal funds to attract swing voters to ensure reelection. Robert Fleck (1999b) and Fishback, Horrace, and Kantor (2005) use a swing-voting measure—the standard deviation of the percent voting for the Democratic candidate in presidential elections between 1896 and 1928—as an instrument for New Deal activity in studies of official unemployment rates and changes in per capita retail sales, respectively. Since New Deal administrators updated relief spending decisions periodically, we update the voting volatility measure after each presidential election in the panel. For observations from years up to and including 1932, we calculate the standard deviation of Democratic voting using the election years from 1896 through 1928; for the city-years 1933 through 1936, we use the 1896 through 1932 elections, and so on.

In addition to the presidential swing-voting measure, we have included measures of the influence of politicians in Congress and at the state level. Fishback, Kantor, and Wallis (2003, p. 299, note 21) find that members of the Labor Committee in the House of Representatives, in particular, influenced the distribution of federal relief monies across counties. The Labor Committee was the primary committee that was devoted to unemployment issues during the New Deal. State-level political influence is captured with a dummy variable for Democratic governors. In lobbying for federal relief funds, governors from Roosevelt's party were likely to have better access to funds, and Democratic governors, controlling for the South, historically were more likely to advocate greater social-insurance spending.

For the instruments to be effective, they must have an impact on the distribution of per capita relief even after controlling for other measurable exogenous variables in the system and they must be uncorrelated with the error term in the second-stage death rate (or fertility rate) equation. Although the political variables might display raw correlations with death rates and fertility rates, the controls for economic activity, ethnic minorities, age distributions, city, and year are already capturing many of the avenues through which election outcomes would be correlated with birth and death rates.

There might be a problem if the measures of political activity are correlated with the unmeasured poverty that may be contributing to endogeneity. The political measures, however, are unlikely to be correlated with unmeasured poverty for the following reasons. First, the poor tend not to have much political clout in elections because either they do not choose to vote or they are implicitly or explicitly disfranchised (see Kousser, 1974; Lijphart, 1997). Thus, unmeasured income shocks to the poor were likely well down the list of the major issues that were decisive in

[11] See Fishback, Haines, and Kantor (2005) for details on the results with trend controls.

[12] See Wright (1974), Anderson and Tollison (1991), Wallis (1998), Couch and Shughart (1998), Fleck (1999a, 1999b), and Fishback, Kantor, and Wallis (2003).

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elections. Second, we chose instruments with temporal, spatial, or institutional distance from the city for each city-year. The presidential swing measure covers a long series of years prior to the observation year to avoid temporal simultaneity bias. The gubernatorial instrument focuses on the state's political climate. House committee representation has institutional distance due to the House committee assignment process. Since we cannot be sure that the unmeasured factors are not correlated with the instruments, we allay concerns further by performing tests described below.

We explored the use of a variety of potential instruments that have been discussed in the New Deal literature, but chose the presidential swing-voting, House Labor Committee, and Democratic governor instruments because they meet the following criteria in estimations of the model with fixed effects. First, as discussed above, we believe that the instruments are exogenous and not themselves influenced by relief spending or infant mortality. Second, as seen in the first-stage results in the 2SLS estimation in table 2, the coefficients of the instruments have the predicted positive signs in the first-stage relief regression. More relief funds were distributed to cities with greater volatility in their support for Democratic presidential candidates, with representation on the House Labor Committee during the New Deal, and located in states with Democratic governors. The coefficients of these variables are statistically significant, and the F-statistic for the joint hypothesis that all three coefficients are zero is 17.17. Third, using a Hausman (1983, p. 433; see also Greene, 2003, pp. 413–414) test we could not reject the hypothesis that the group of identifying instruments were uncorrelated with the 2SLS estimates of the error term in the second-stage infant mortality equation. The test results suggest that the identifying instruments have not been inappropriately omitted from the second-stage infant mortality equation.[13]

The relief spending coefficient in the second-stage 2SLS equation, as reported in table 2, is statistically significant at the 10% level and negative. An additional dollar of per capita relief spending was associated with a reduction of 0.16 infant deaths per 1,000 live births, which means that an OSD increase in relief spending per capita reduced infant mortality by 0.27 standard deviation. The difference between the OLS and 2SLS fixed-effects coefficients is consistent with the predicted positive endogeneity bias in the OLS estimates. Thus, it appears that the OLS fixed-effects estimate of -0.032 is a lower bound of the inverse effect of relief spending on infant mortality rates.

In one sense the results in tables 1 and 2 can be considered reduced-form estimates from a structural demographic model in which the infant mortality rate is also a function of the general fertility rate and the non-infant death rate. Demographers have found positive relationships between the infant mortality rate and both the fertility rate and non-infant death rate. Increases in birth rates can lead to reductions in intervals between births with the consequence of less maternal care available per child and shorter durations of breast feeding, both of which contribute to poorer infant health.[14] Infant and non-infant death rates tend to be positively related to the extent that the two age groups are struck by the same contagious diseases or faced similar environmental hazards.[15] Thus, it is possible that the omission of the non-infant death rate and the birth rate from the specification causes the relief coefficient to capture the effects of changes in these other demographic variables that may have also contributed to declines in infant mortality.

We therefore reestimated the models with the fertility rate and the non-infant death rate included as controls.[16] Comparisons of the results for the same specifications in the top and bottom halves of table 1 show little difference in the impact of relief spending on infant mortality with and without controls for the birth and non-infant death rates. Thus, the finding that relief spending reduced infant mortality is not driven by a secondary influence of relief spending through changes in birth rates or non-infant death rates.

Although the magnitudes vary, each specification suggests that relief programs were successful in alleviating the economic and resulting physical hardship for infants that resulted from the Great Depression. This finding is similar to prior findings that federal relief and public works programs contributed to lower infant mortality in the southern states, particularly in black families.[17]

V. Non-infant Death Rates

To the extent that infants are more vulnerable to changes in economic circumstances than the rest of the population, relief spending will not have as large an effect on the non-infant mortality rate. The rate is measured as the

[13] We have explored using several other instruments that have been proposed in other settings. These include voter turnout in the prior presidential election, the percentage voting for the Democratic presidential candidate in prior elections (both based on Fleck, 1999b), the percentage of Democratic legislators in the upper and lower houses of the state legislature, representation on other congressional committees, and the timing of mayoral and gubernatorial elections (based on Levitt, 1997). These additional instruments failed to meet one or more of our criteria for inclusion. In many cases when we added the extra instrument, its coefficient in the first stage was statistically insignificant or had an unexpected sign, and the estimates of the relief effects were roughly comparable to the results reported here.

[14] Studies of family fertility patterns suggest that births rose about .6 to .8 children for each child death in the United States around 1910, either as families tried to replace a child or avoid having too few surviving children (Haines, 1998, table 7.6).

[15] Preston (1976, p. 106) finds that 80% to 90% of the variance in the death rate of any particular age group can be "explained" by the variation in the death rate among all age groups.

[16] In these analyses the infant mortality rate was positively related to the non-infant death rate, while the general fertility rate is positively related to infant mortality in some specifications and tends to be statistically insignificant and negative in others.

[17] The study of southern counties did not have measures of state and local relief spending. See Fishback, Haines, and Kantor (2001).

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TABLE 3.—IMPACT OF RELIEF SPENDING ON NON-INFANT DEATH RATE AND THE GENERAL FERTILITY RATES, 1929–1940

Rate		OLS with No Correlates or Fixed Effects	OLS with Correlates and without Fixed Effects	OLS with Correlates and Fixed Effects	2SLS with Fixed Effects
Non-infant death rate	OSD effect[1]	−0.116	−0.087	−0.026	−0.099
	t-statistic	(−4.36)	(−3.11)	(−1.42)	(−0.99)
Non-infant death rate controlling for GFR and IMR	OSD effect	0.178	−0.019	−0.001	−0.271
	t-statistic	(6.59)	(−0.79)	(−0.1)	(−1.47)
Homicides	OSD effect	−0.317	−0.119	−0.032)	−0.211
	t-statistic	(−13.73)	(−5.25)	(−1.42)	(−1.58)
Suicides	OSD effect	−0.148	−0.323	−0.123	−0.853
	t-statistic	(−4.9)	(−8.55)	(−2.14)	(−2.51)
Respiratory tuberculosis	OSD effect	−0.273	−0.057	0.010	0.154
	t-statistic	(−9.55)	(−1.75)	(0.41)	(1.01)
Infectious and parasitic diseases	OSD effect	−0.339	−0.037	0.117	−0.576
	t-statistic	(−13.67)	(−1.3)	(2.21)	(−2.24)
Neoplasms	OSD effect	0.369	0.113	0.051	0.265
	t-statistic	(13.2)	(3.4)	(1.51)	(1.47)
Cardiovascular disease	OSD effect	0.364	0.069	0.018	n.a.
	t-statistic	(12.42)	(2.47)	(0.73)	
Influenza, bronchitis, and pneumonia	OSD effect	−0.260	−0.072	−0.134	0.321
	t-statistic	(−8.62)	(−2.08)	(−3.06)	(1.26)
Diarrheal	OSD effect	−0.317	−0.067	−0.085	−0.383
	t-statistic	(−12.66)	(−2.48)	(−2.6)	(−1.91)
Degenerative	OSD effect	−0.079	−0.137	−0.075	0.216
	t-statistic	(−2.69)	(−3.9)	(−2.07)	(1.15)
Maternal mortality	OSD effect	−0.350	−0.051	−0.010	−0.058
	t-statistic	(−12.85)	(−1.49)	(−0.19)	(−0.17)
Motor vehicles	OSD effect	−0.151	−0.042	−0.214	0.191
	t-statistic	(−5.2)	(−1.15)	(−3.62)	(0.61)
General fertility rate	OSD effect	−0.169	−0.307	0.026	0.820
	t-statistic	(−5.51)	(−9.82)	(0.98)	(2.59)
General fertility rate controlling for NDR and IMR	OSD effect	−0.018	−0.243	0.019	0.877
	t-statistic	(−0.52)	(−8.02)	(0.72)	(2.78)

Notes: The *t*-statistics are based on White-corrected standard errors. Correlates included in the death rate analyses are retail sales per capita, percent black, percent illiterate, percent urban, percent foreign born, percentages of population in the age groupings 10–19, 20–29, 30–34, 35–44, 45–54, 55–64, 65–74, and 75 and over. The identifying instruments are the voting volatility measure and the Democratic governor and House Labor Committee dummy variables. We excluded the Democratic governor variable as an instrument in the fertility rate equation because whenever it was included, the hypothesis that the identifiers were not inappropriately excluded was rejected.

[1]The OSD effect is the estimated change in the dependent variable, expressed in terms of a standard deviation change from the sample mean, associated with a one-standard-deviation increase in the independent variable.

Sources: See the sources for table 1. Deaths from specific causes through 1936 were reported in the annual volumes of the U.S. Bureau of the Census, *Mortality Statistics* and for 1937 in *Vital Statistics*. We aggregated the information on specific causes of death into larger categories based on a framework established by Preston, Keyfitz, and Schoen (1972). See Fishback, Haines, and Kantor (2005, p. 33) for an explicit categorization of the causes of death.

number of deaths of people over one year old per thousand people in the city. The results in the top two rows in table 3 show the OSD (one standard deviation) effects and the *t*-statistics associated with the relief coefficients following the same estimation procedures used in the infant mortality analysis. Since the rate is for the entire population, the age controls have been changed to reflect the shares of the entire population at ages 10–19, 20–29, 30–34, 35–44, 45–54, 55–64, and 65 and up.

The baseline OLS result without other correlates shows that an OSD increase in relief spending was associated with a 0.116 standard deviation reduction in the non-infant death rate. As in the case of infant mortality, as more correlates and fixed effects are added, the impact of relief is reduced in magnitude. In the OLS analysis with city and year effects, the relief coefficient is statistically significant in two-tailed tests at only the 15% level, and an OSD increase in relief spending would have reduced the death rate by only 0.026 standard deviations. In the 2SLS analysis the coefficient is negative and larger in absolute value, as the OSD effect becomes −0.099, but the *t*-test cannot reject the hypothesis of no effect.

Some of the impact of relief on the non-infant death rate potentially operated through changes in fertility rates and infant mortality rates. Higher fertility rates meant that more women were at risk of maternal mortality, for example, and there may have been contagion cross-effects from infant mortality rates. When we control for these rates (see rows 3 and 4), the coefficient of relief spending is positive in the OLS specification, and statistically insignificant in the other specifications. In the fixed-effects analyses, the relief coefficient is extremely small. Meanwhile, the 2SLS analysis suggests a very strong but statistically insignificant effect of relief on the non-infant death rate.

VI. The Impact of Relief and General Economic Activity on Specific Causes of Death

The lack of statistically significant effects of relief spending on the non-infant death rate might have arisen because the non-infant death rate includes a wide variety of causes. Some causes of death are reduced little by the better access

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to nutrition, housing, health information, or medical care that relief payments afforded. In modern studies, for example, higher incomes have little impact on death rates due to cardiovascular disease. In fact, increased income can lead to higher death rates if the person is more likely to overeat, eat fattier foods, or drink more alcohol (Ruhm, 2000). Non-infant deaths also included deaths from motor vehicle accidents, on which relief had conflicting effects. Relief would have lowered the death rate from automobile accidents because people were better able to maintain the safety of their vehicles. On the other hand, more resources also allowed people to drive more, raising the risk of a fatal accident during the year.

We therefore examine death rates associated with specific causes of death for the period 1929 through 1937.[18] There is likely to be more measurement error in the estimation of the relief effect because the cause of death data were available only for the cities, while the relief spending information for roughly half the sample was for the county in which the city was located. The causes of death are grouped into the categories listed in table 3.

Suicides and homicides are of particular interest in this list because, unlike nearly all of the other causes of death, they are based directly on an individual's decision, whether rational or irrational. Modern studies around the world suggest that suicide rates are lower in areas with better economic opportunity and less inequality. Some studies, but not all, find the same result for homicides.[19] The funds and work opportunities from New Deal relief probably eased some of the economic tensions and material inequality that contributed to more homicides and suicides. On the other hand, such beneficial effects might have been offset by the stresses imposed by the perceived social stigma of accepting relief.

Table 3 shows the results under various specifications for the various causes of death. When the death rate is estimated as a function only of per capita relief spending, relief has a negative relationship with all the causes of death except for deaths due to neoplasms and cardiovascular disease. Once the city and year controls are included, negative relationships are still present for roughly half of the disease categories, including homicides, suicides, diarrheal diseases, degenerative diseases, motor vehicles, and influenza, bronchitis, and pneumonia. As in the infant mortality section, the potential inability to fully control for shocks to the very poor might lead to a positive bias in the relief coefficients. Modern homicide rates, for example, are associated with greater poverty, and we expect that more poverty was associated with greater relief spending.[20] As expected, the negative impact of relief on homicides and suicides increases when using 2SLS techniques to control for this potential bias. The 2SLS estimates suggest that an OSD increase in relief spending lowered the homicide rate by 0.21 standard deviations, although the relief coefficient in the homicide analysis is only statistically significant at around the 15% level. The OSD effect for suicides was substantially larger at −0.85 standard deviations, and the effect is statistically significant. Given that suicides and homicides are extreme manifestations of the strains associated with economic depressions and that the relief programs were not targeted specifically toward mental health or crime, relief appears to have had a material influence on the poor's well-being.

Only two other causes of death were reduced by greater relief spending in the 2SLS estimation: deaths from infectious and parasitic diseases, and diarrheal deaths. The 2SLS OSD effects of relief spending were −0.58 and −0.38, respectively. Both diseases are considered by demographers to be acute diseases that are responsive to the better nutrition, access to medicine, improved sanitation, and better housing that could result from more relief aid. Meanwhile, many of the death causes not affected by relief spending are degenerative or long-term diseases that are less responsive to short-term changes in income.

Although we have focused on the impact of relief spending, the analysis also shows the impact of retail sales, our measure of economic activity, on the various death rates. Table 4 shows the OSD effects of per capita retail sales for both the OLS fixed effects and the 2SLS analyses above. Some caution should be used in interpreting these results because there are no controls for potential endogeneity between the death causes and retail sales. However, we do not anticipate a strong direction of causation from death rates to per capita retail sales. The impact of retail sales is positive and statistically significant for the non-infant deaths, homicides, respiratory tuberculosis, neoplasms, infectious and parasitic diseases, degenerative diseases, maternal mortality, and motor vehicle accidents. These procyclical findings for most causes of death and similar findings for the milder cycles in the 1970s and 1980s lend credence to the possibility that many death rates may have been procyclical throughout the course of the century. Suicides were the exceptions to the rule, displaying negative and statistically significant relationships with economic activity in the 1930s and in the 1970s and 1980s (Ruhm, 2000).[21]

The different signs of the effects for retail sales and for relief for many of the causes of death suggest that the

[18] We could not extend the data set to 1940 because the census stopped reporting the cause of death data for individual cities when it revamped its publications in 1938.

[19] See Brainerd (2001), Ruhm (2000), and Cutler, Glaeser, and Norberg (2000) on suicides. Fajnzylber, Lederman, and Loayza (2002a, 2002b) find that more economic growth reduces homicides, but Ruhm (2000) finds that homicides are procyclical.

[20] Fajnzylber, Lederman, and Loayza (2002a, 2002b), Kennedy, et al. (1996), Kaplan et al. (1996), and Potter (1991) find that reductions in poverty reduce homicides.

[21] Table 4 shows no strong positive relationship between infant mortality and retail sales, in contrast to Dehejia and Lleras-Muney's (2004) findings of a selection effect with modern data that leads to better infant health when unemployment is high.

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TABLE 4.—ONE-STANDARD-DEVIATION (OSD) EFFECTS OF PER CAPITA RETAIL SALES ON DEMOGRAPHIC VARIABLES

Demographic Variables	OLS Fixed Effects		2SLS with Fixed Effects	
	OSD Effect	*t*-stat.	OSD Effect	*t*-stat.
Infant mortality	0.022	0.52	−0.003	−0.06
Non-infant deaths	0.068	1.91	0.061	1.66
Homicides	0.171	3.96	0.154	3.52
Suicides	−0.168	−1.98	−0.235	−2.55
Respiratory tuberculosis	0.063	1.57	0.076	1.74
Infectious and parasitic diseases	0.216	2.74	0.152	1.77
Neoplasms	0.102	1.51	0.122	2.02
Cardiovascular disease	0.004	0.11	0.042	0.90
Influenza, bronchitis, and pneumonia	−0.128	−1.97	−0.087	−1.23
Diarrheal	0.010	0.21	−0.017	−0.32
Degenerative	0.133	2.69	0.160	3.00
Maternal mortality	0.150	1.62	0.145	1.50
Motor vehicles	0.252	3.03	0.289	3.18
General fertility rate	0.357	6.97	0.410	6.63

Notes: The OSD effect is the estimated change in the specified demographic rate, expressed in terms of standard deviation changes from the sample mean of the variable, associated with a one-standard-deviation change in per capita retail sales. The *t*-statistics pertain to the underlying regression coefficient and are based on White-corrected standard errors. 2SLS refers to the use of instruments for the relief variable.

mechanism by which relief spending reduced death rates differed from simply increasing general income. Infant mortality rates were not influenced by general economic activity but were reduced by relief spending. Similarly, the non-infant death rate and homicide rates increased in response to general economic activity while declining to some extent in response to relief spending. Only suicide rates responded in the same direction to increases in general economic activity and relief spending.

VII. Relief Costs per Life Saved

One way to show the effectiveness of relief in reducing death rates is to compare the relief spending associated with saving a life to other measures of the value of life. Table 5 contains estimates of the relief cost per life saved for the various death rate measures where the OLS fixed effects and/or the 2SLS fixed effects estimates are negative and statistically significant at the 20% level.

We illustrate the derivation of the measure using the infant mortality rate, which requires more complicated calculations than the non-infant death rates or the death rates from specific causes. Consider the fixed-effects 2SLS coefficient from table 1, which suggests that an additional per capita dollar of relief spending would have lowered the number of infant deaths by 0.163 per 1,000 live births.

The number of infant deaths (I) can be written as

$$I(r) = IMR(r) \times GFR \times W1544 \times P, \tag{2}$$

where IMR is infant deaths per live birth, GFR is births per women aged 15 to 44, $W1544$ is the share of the total population composed of women aged 15 to 44, P is the total population, and r is per capita relief spending. Differentiating with respect to r,

$$dI/dr = dIMR(r)/dr \times GFR \times W1544 \times P \tag{3}$$

gives a measure of the direct effect of added relief spending on infant deaths. After rearranging terms and setting dI equal to 1, we can determine the total amount of relief spending (dR) associated with a reduction of one infant death, which is the change in per capita relief spending (dr) times population (P),

$$dR = drP = 1/(dIMR(r)/dr) \times GFR \times W1544). \tag{4}$$

Using the 2SLS with fixed-effects coefficient for $dIMR/dr$ of −0.163 and sample means for the other variables in

TABLE 5.—RELIEF COST PER DEATH PREVENTED (YEAR 2000 DOLLARS)

	Estimate Based on Coefficient from Specification	
	OLS with Correlates and Fixed Effects	2SLS with Fixed Effects
Infant mortality	$8,614,412	$1,947,976
Non-infant death	$2,482,176	$659,819
Homicide	$29,714,799	$4,547,812
Suicide	$13,389,218	$1,935,784
Death from infectious and parasitic disease	n.a.	$840,014
Deaths from diarrhea	$7,944,865	$1,760,966

Notes: Values for all but the 2SLS estimate for non-infant deaths are statistically significant at the 20% level in two-tailed tests. See tables 1 and 3 for the *t*-statistics of the coefficients on which the estimates are based. The estimates are based on coefficients from specifications that include correlates and fixed effects. See the text for a discussion of the methods underlying the calculations. The relief costs are insensitive to changes in the population size. The relief costs for infant deaths are sensitive to changes in the ratio of women aged 15 to 44 to the population and the general fertility rate. The coefficients were estimated with monetary values based on the CPI with 1967 as the base year. Dollar values were converted to 2000 values by multiplying by 5.213. The 2000 values can be converted to 1935 values by dividing by 12.68.

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equation (4), an additional $153,045 of relief spending in 1935—roughly $1.95 million in year 2000 dollars—would have been associated with saving an infant's life. The estimate of the relief cost per life saved is much higher ($8.61 million) using the coefficient from OLS with fixed effects.

The relief costs per life saved based on the 2SLS coefficients for the other types of deaths are in the same range as the costs per infant life saved. The relief cost per life saved is $4.5 million for homicides, $1.9 million for suicides, $840,000 for infectious and parasitic diseases, and $1.76 million for diarrheal diseases. As was the case for the infant mortality rates, the OLS fixed-effects coefficients imply much higher relief costs per life saved.

The estimates based on the 2SLS coefficients are roughly comparable to modern estimates of the market valuations of life for working adults, which range from about $1 million to $9.5 million.[22] The estimates for infant mortality are also similar to the costs associated with saving a child's life with modern social welfare spending that more directly target children. Currie and Gruber (1996) estimated that changes in the modern Medicaid eligibility requirements that targeted young children spent $840,000 for each infant life saved. When they examined the impact of broader changes in the Medicaid program not specifically targeted at high-risk groups, the cost per life saved was about $4.2 million. These comparisons suggest that during the economic crisis of the Great Depression, the flood of relief spending was roughly as cost effective in saving infant lives as modern Medicaid spending.

One way to make comparisons with the targeted programs is to consider what share of the relief population was at risk of an infant death, a homicide, or a suicide. Consider the infant population, which is much easier to measure than the population at risk of homicides or suicides. Given that people of all ages were experiencing significant problems during the Depression, a relatively small percentage of the relief spending was likely to have been devoted to infants. In an October 1933 census of relief families conducted by the FERA (1934, pp. 122–143) and in the overall 1930 Census, children under age one accounted for 1.5% of the sampled populations in cities with more than 100,000 people. Since relief administrators based the amount of relief on deficits in household budgets, we can reasonably assume that infants were allocated roughly 1.5% of the relief dollars. Multiplying this 1.5% share by relief dollars spent per infant life saved suggests that of the $1 million to $9 million (in year 2000 dollars) in relief associated with saving an infant life, only $15,000 to $135,000 actually was used for infants.

[22] Market values of life are from Moore and Viscusi (1990, p. 14) with adjustments to year 2000 values using the GDP Deflator (Council of Economic Advisors, 2001, p. 282).

VIII. General Fertility Rates

The drastic declines in economic activity during the early 1930s coincided with a drop below trend in the general fertility rate (GFR) both in figure 1 and in the averages for the cities in the sample. Because the drop in fertility in the early 1930s coincided with a sharp drop in marriage rates, we anticipate that the effect of relief spending in this environment was more likely to encourage couples to return to their normal fertility decisions than it was to give incentives to single women to have children.[23]

The bottom two rows of table 3 show the results from the series of estimation procedures for the general fertility rate.[24] Although the rise in relief spending coincided with an increase in the general fertility rate at the national level, the baseline OLS coefficient with no correlates suggests a negative relationship. A negative cross-sectional relationship between relief spending and the general fertility rate appears to be causing this negative relationship. Once we control for city and year fixed effects, the relief effect is positive and statistically significant at the 10% level but has little explanatory power. An OSD change in per capita relief only raises the general fertility rate by 0.05 standard deviations. The 2SLS coefficients are statistically significant and much larger. An OSD increase in per capita relief was associated with a 0.82 standard deviation increase in the general fertility rate. The impact of more relief spending on the general fertility rate is likely operating through the same channel as an improvement in general economic activity. OSD changes in per capita retail sales shown at the bottom of table 4 also reveal a large and positive effect on the general fertility rate, ranging between 0.35 and 0.41 standard deviations. Thus, the combination of an improving economy and a more substantial social safety net contributed significantly to the leveling off of the fertility rate in the late 1930s.

The sharp increase in the relief coefficient in moving from fixed effects to 2SLS is likely driven in part by two negative endogeneity biases in the OLS fixed-effects estimates. First, as was the case for the death rates, the absence of good measures for the impact of the Depression on the poor might lead to negative endogeneity bias in our estimates of the impact of relief on the general fertility rate. Unmeasured negative shocks to the poor were likely to lead to lower birth rates at the same time as they would lead to

[23] The GFR was lower for the sample cities than for the nation, but the trend in the 1930s was similar. The average city GFR fell from 69 in 1930 to 57 in 1933, rose through 1937 and then fluctuated between 58 and 63 through 1940. The decline in the GFR between 1930 and 1932 coincided with a 19% drop in marriages (U.S. Bureau of the Census, 1975, Series B 1–4).

[24] The one difference in the 2SLS specifications between the infant mortality and general fertility equations was the exclusion of the Democratic governor from the instrument set. In all cases when the Democratic governor was included, the Hausman test rejected the hypothesis that the error in the second-stage general fertility equation was uncorrelated with the instruments. When the governor variable was excluded, the Hausman test no longer rejected the hypothesis.

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more relief spending. Second, the 1930s saw rapid growth in the family planning movement. Many of the obstacles to birth control and family planning were eliminated by the end of the 1920s, and the number of family planning clinics nationwide rose from 28 in 1929 to 374 by 1938 (McCann, 1994, pp. 215–217 and ch. 6). Given that many local New Deal relief administrators were interested in family planning, we anticipate a positive correlation between the clinics and relief spending and a negative relationship between the clinics and the general fertility rate that combined might lead to the negative bias that was eliminated by the use of the 2SLS procedure.

IX. Concluding Remarks

The Great Depression of the 1930s, with its unusually high unemployment rates, might well have become a demographic disaster with rising infant mortality and non-infant death rates and declining fertility throughout the decade. The national aggregates show, however, that the infant mortality rate stopped falling only temporarily in the mid-1930s before continuing on a downward trend. The non-infant death rate stayed on trend through the early 1930s and then rose above trend in the late 1930s, while the general fertility rate fell below trend in the early 1930s before leveling out in the late 1930s. What can explain these puzzling patterns?

A key factor in the explanation of all three patterns is the sharp increase in relief spending during the 1930s when the federal government stepped in to combat the problems of the unemployed and the poor. In essence, federal relief spending provided a safety net for the unemployed and the poor that contributed to a continuation of the long-term decline in mortality rates for infants under age one, the population most vulnerable to the effects of economic downturns. Increased relief spending had little effect on the overall non-infant death rate but contributed to reductions in suicides, deaths due to infectious and parasitic diseases, deaths from diarrheal diseases, and possibly homicides. The relief costs associated with saving a life were similar to modern estimates of the value of life in labor markets and the cost of saving lives through Medicaid.

The effect of fluctuations in economic activity during the 1930s mimicked a pattern found by Ruhm (2000) for the modern U.S. economy. The overall non-infant death rate and the fatality rates for homicide rates, infectious and parasitic diseases, cancers, degenerative diseases, and motor vehicles all displayed procyclical patterns, falling when the economy plunged and rising during the recoveries. A key exception during the 1930s and the modern era was the suicide rate, which tended to be countercyclical. The differences in how most of the specific death rates responded to increases in relief and in general activity are indicative of the different channels through which relief and general improvements in the economy influenced death rates. This finding should not be surprising given that relief was targeted at lower-income and unemployed households while improvements in general economic activity had widespread effects.

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