NBER WORKING PAPER SERIES

THE DYNAMICS OF RELIEF SPENDING AND THE PRIVATE URBAN LABOR MARKET DURING THE NEW DEAL

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Working Paper 13692
http://www.nber.org/papers/w13692

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2007

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JEL No. N0

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Abstract

During the New Deal the Roosevelt Administration dramatically expanded relief spending to combat extraordinarily high rates of unemployment. We examine the dynamic relationships between relief spending and local private labor markets using a new panel data set of monthly relief, private employment and private earnings for major U.S. cities in the 1930s. Impulse response functions derived from a panel VAR model that controls for time and city fixed effects show that a work relief shock in period t-1 led to a decline in private employment and a rise in private monthly earnings. The finding offers evidence consistent with contemporary employers' complaints that work relief made it more difficult to hire, even though work relief officials followed their stated policies to avoid affecting private labor markets directly. Meanwhile, negative shocks to private employment led to increases in work relief, consistent with Roosevelt's stated goal of using relief to promote relief and recovery.

I. Introduction

The Great Depression of the 1930s remains the most serious economic disruption in U.S. history, as unemployment rates ranged from 10 to 25 percent throughout the decade (Darby 1976). After Roosevelt's resounding electoral victory in 1932, the federal government for the first time took major responsibility for providing relief assistance. The federal government offered billions of dollars in relief grants. The income assistance took two primary forms – work relief in which recipients were required to provide labor on public works projects, and direct relief that required no work obligation. The Roosevelt administration's stated goals were to provide income relief to the poor and the unemployed and to promote the recovery of the economy through investment in civil infrastructure and by stimulating consumer spending as a result of the relief grants. As relief projects focused on building public works that had traditionally been the role of government, it was hoped

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that private industry would have been minimally impacted. Moreover, the payments to work relief recipients per hour worked were typically well below the wages of those working in private jobs, and relief officials encouraged workers to accept private employment. Despite these practices, private employers complained that work relief made it more difficult for them to hire workers. They claimed that they had to raise wages to entice workers, thus reducing the number of workers they were willing to hire.[1]

The research to date on the New Deal relief programs and their relationship with labor markets during the 1930s has come in three forms: narratives that describe how the programs worked, a large literature on the extent to which the geographic distribution of New Deal spending was used to offset prior unemployment and downturns, and attempts to assess the extent to which relief jobs crowded out private jobs. The narratives include Donald Howard's (1943) encyclopedic description of the workings of the WPA, Jonathan Kesselman's (1978) description of work relief programs in the context of a macroeconomic model, and Robert Margo's depictions of work relief using the 1940 Census with other scattered cross-sections.[2] Cross-sectional studies by Wright (1974), Fleck (1999a, 1999b), and Fishback, Kantor and Wallis (2003) show that more New Deal relief funds were distributed to areas with higher unemployment and deeper downturns in earlier periods. The published crowding-out studies include cross-sectional analyses by Robert Fleck (1999a) of 1937 and 1940 unemployment statistics and Wallis and Benjamin's (1981) analysis of employment at the city level in 1934/1935. Both studies find no effect of relief assistance on private employment. Bernanke's (1986) time series study of eight industries during the Depression finds that the presence of federal relief had no impact on labor markets. On the other hand, Wallis and Benjamin's (1989) unpublished study of an annual panel of states suggests that an additional relief job contributed to a reduction of about half of a private job.

Most of the research to date has relied on cross-sectional data and thus can not capture the dynamic interactions between relief and private labor markets. In this paper we develop a new monthly panel dataset containing private employment, private earnings, and complete measures of both direct and work relief spending for 44 cities during the 1930s. There were dramatic changes both across years and within years in private labor markets and in relief spending that have not been captured with the prior data sets. Further, we anticipate that the interactions between private labor markets and relief spending were different for work and direct relief. Our goal

[1]See Howard (1940, 486-496), Coyle (1939), Brimhall (1937), Petree (8 November 1934, 15 April 1935, 12 November 1935, 27 April 1936, 17 August 1936, 15 September 1936, 9 October 1936, 12 July 1937; and 13 September 1937), Larned (January 1935, 18 April 1935, 17 June 1935, 29 July 1935, 12 August 1935, 19 August 1935), Wood (12 April, 1937), and Works Progress Administration (1937).

[2] For other studies, see Bakke (1969 [1940]), Blumberg (1979), Brown (1940), McKean and Taylor (1955), Millett (1978), Schwartz (1984), Smith (2006), Walker (1979), Williams (1939).

is to show the multi-month dynamics of the private labor market responses to changes in both direct and work relief spending, while also showing the dynamic responses of relief spending to increases in monthly earnings and/or greater levels of private employment. We focus on private employment rather than measures of unemployment for two important reasons. First, defining unemployment for the 1930s is complicated by the issue of whether people on work relief projects should be treated as employed or not (Darby 1976 and Fleck 1999a). Second, unemployment surveys were conducted only in 1930, 1937, and 1940, while we are able to use consistent monthly series for private employment from 1932 through 1940.

We employ a panel vector autoregressive (VAR) model with fixed effects to estimate the dynamic influences between the endogenous employment and relief variables. The panel VAR method is a useful approach in that it fits the high frequency nature of these data and takes into account the persistence of employment, earnings, and relief spending across months. Given the monthly frequency of the data, we expect that the effects of changes in relief and the private labor market in a city could take several time periods to manifest themselves. For example, scholars using cross-sectional data at the county level have found that government officials responded to declines in private labor markets with increased relief spending (Fishback, Kantor and Wallis 2003; Fleck 1999). It is likely that these responses were spread over more than one month, which we can test explicitly using the VAR framework.

The VAR structure allows for flexibility in measuring the dynamic relationships between relief and private labor markets by allowing measurement of the extent to which changes in relief spending might have led and lagged changes in private labor markets. Meanwhile, the panel nature of our data allows us to control for unobserved time-invariant features of cities and national month-specific shocks. The result is an empirical model that allows us to estimate a set of reduced-form coefficients that can be used to graph impulse response functions (IRF) that visually illustrate how a one-month shock in work relief spending, for example, affected private employment, monthly earnings, and direct relief in the months that followed. We also examine how one-period shocks in the labor market variables influenced spending on relief programs in the months that followed. Finally, Granger causality tests identify the statistical significance of these relationships.

Our results indicate statistically significant bi-directional Granger-causal relationships between work relief spending and private urban labor markets outcomes. The impulse response functions show that private employment decreased and private earnings increased in the months following a positive shock in work relief spending. This result is consistent with employers' complaints during the 1930s that increases in the availability of

work relief were associated with greater difficulty in hiring workers. WPA investigations of the complaints showed that local WPA officials were encouraging workers to accept private employment and could find little evidence that workers were refusing private jobs (Howard 1940, 486-496; Coyle 1939, Brimhall 1937, and Works Progress Administration 1937). There was a tendency at the time to believe that the WPA investigations had rebutted the employers' complaints. But the WPA investigations primarily focused on their own administrative procedures and were not addressing the systemic changes in local labor markets that employers had observed.

The impulse response functions also provide new evidence in the debates regarding the purported relief and recovery motivations of the New Deal. Work relief reacted to offset negative shocks in private employment in ways consistent with the Roosevelt administration's proclaimed goals to promote relief and recovery. The focus was on employment itself, however, as changes in private monthly earnings had little impact on work relief.

Unemployment and New Deal Relief Institutions

The 1930s recorded the highest level of unemployment in United States history. The official unemployment rate increased from 3.2 percent in 1929 to 15.9 percent in 1931 and then peaked at nearly one quarter of the work force in 1933. The rate remained above 20 percent through 1935 and above 14 percent through 1939 (Darby 1976). In response to this massive unemployment as well as other economic and financial panics, Roosevelt's New Deal in 1933 introduced the first targeted federal relief programs for the poor and unemployed.

Several different agencies contributed to the relief effort during the Depression. Between summer 1933 and June 1935 the Federal Emergency Relief Administration (FERA) provided grants to the states, which, in turn, distributed funds to local governments to make need-based payments to the unemployed either through direct or work relief programs. The payments that individual households received were based on the deficit between the household's income and a standard budget for a household of the same composition. Work relief required a recipient to perform work on a government project, while direct relief imposed no such requirements. Local relief agencies had the discretion to set their own FERA wages, which sometimes meant that limited relief budgets were spread relatively thinly so that more unemployed workers could be hired on relief projects. Worried about high unemployment and an anticipated harsh winter, the administration created the Civil Works Administration (CWA) in mid-November 1933. The agency employed up to four million people per week before it was terminated and many of the workers were then transferred to FERA projects in March 1934. Eligibility for the CWA was also based

on the household budget deficit principle, although the CWA paid hourly wages similar to the wages paid by private contractors who were hired by the government to build public works. As a result, hourly CWA wages were substantially higher than the hourly relief wages on the FERA projects.

As part of a compromise associated with the enactment of the Social Security Act in 1935, there were several major shifts in the provision of relief. Responsibility for relief of "unemployables" was returned to state and local governments in the summer of 1935. Federal matching grants were offered to the states to help them run direct relief programs for old-age assistance, aid to the blind, and aid to dependent children. Local governments also offered direct relief in the form of "general relief" For those who were unemployable. The federal government provided work relief directly through the Works Progress Administration for those considered "employable," whose eligibility for WPA jobs was determined by local governments using the budget-deficit principle. WPA workers received hourly earnings that were roughly half those paid for similar work on government construction projects that used private contractors who hired their workers in the construction labor market.[3] Employables without jobs and some WPA workers also were eligible to receive direct relief (Kesselman 1978, Howard 1943, 200-207).

The Interactions between Relief and Private Labor Markets

The possible dynamic interactions between relief and private labor markets move in both directions and involve a complex set of different effects. Faced with massive unemployment and a moribund economy, Roosevelt's stated goals for his relief programs were to promote relief by providing basic aid to the unemployed and the poor and, in turn, to stimulate recovery as relief recipients spent the funds they received. The Roosevelt administration anticipated that the work relief jobs would lead to a rise in consumer spending and thus a rise in the derived demand for labor, which would raise private employment and earnings. Fishback, Horrace, and Kantor (2005) find evidence that New Deal public works and relief spending stimulated consumer spending. Their point estimate implies that spending an additional per capita dollar on New Deal relief and public works grants in a county during the period from 1933 through 1939 raised per capita incomes in 1939 by roughly 80 to 85 cents.

[3] Wallis, Fishback, and Kantor 2006; Fleck 1999; Wallis 1989; Fishback, Haines, & Kantor forthcoming. The Civilian Conservation Corps (CCC) and National Youth Administration (NYA) also provided work relief during the 1930s, but operated on a smaller scale than the WPA and had a much different focus. The Social Security Act also established Unemployment Insurance programs, but their impact was delayed in most states until the last two years of the decade to allow the build up of insurance funds before payments were made.

Even though relief and public works spending stimulated consumption, there still remain questions about whether they stimulated private labor markets or even crowded out private employment. Wallis and Benjamin (1981, 1989), Bernanke (1986), and Fleck (1999a), for example, have performed studies of the extent to which work relief jobs contributed to a reduction in private employment. The crowding-out argument suggests that the presence of work relief gave workers an extra outside option that allowed them to search less and to seek higher pay in the private sector before accepting private employment. Higher pay would have induced private employers to reduce their demand for workers.

The Roosevelt administration explicitly stated that they were trying to avoid adversely influencing private employment by focusing on public works projects typically built with government funds.[4] Work relief projects did not provide job opportunities for all of the unemployed. Darby's (1976) estimates of the shares of the labor force on work relief or fully unemployed suggest that roughly 4 to 7 percent of the labor force had work relief jobs, but the unemployment rate after subtracting out relief workers remained above 9 percent each year between 1931 and 1939.

It may seem surprising that work relief jobs would have been considered more attractive than private employment because the WPA paid substantially less per hour than most regular jobs (Sundstrom 1992, 1995). However, a worker facing a depressed economy also worried about job security, so the expected earnings for the year or month certainly turned on a worker's assessment of his unemployment risk (Kesselman 1978, 196-7). The risk of job loss in the private labor market during the Depression was high, while work relief jobs were viewed as relatively secure. Fifty-seven percent of relief job holders in September 1937 held their job continuously until February 1939 (Margo 1991, p. 337). A WPA mandate that workers continuously employed on projects for over 18 months be released from work relief was clearly not enforced because 16 percent of those on work relief in February 1936 remained employed continuously for 36 months through February 1939. In August 1939 the WPA dismissed over 750,000 persons who had exceeded the 18-month limit, only to rehire 57 percent of them within the next year (Margo 1991, pp. 337-9; FWAWPA 1940).

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[4] See FWAWPA 1940, Kesselman 1978,, and Howard 1940, 124-126. In fact, the WPA occasionally was criticized for creating work and performing tasks for which there was no and never would be any demand such as "leaf-raking" in the forest (Congressional Record, June 16, 1939, p. 7294). Even though there was a focus on public projects, the WPA employment would have had effects on the labor markets for construction workers. To distribute aid to a larger relief population more quickly, the Roosevelt administration chose to pay relief workers low hourly earnings on the FERA and WPA public works projects during the 1930s (Clarke 1996, 62-68; Schlesinger 1958, 263-96). Generally, the workers were given a monthly stipend and their hourly earnings were determined by the number of hours they were required to work. This contrasts with the long-term practice, followed by the Public Works Administration (PWA), of hiring construction contractors who then hired their workers in the construction labor market.

After several years of high unemployment, risk-averse workers may well have seen the WPA jobs as viable alternatives, despite the low hourly earnings. One WPA worker remarked, "Why do we want to keep these jobs? Well… we know all the time about persons on direct relief… just managing to scrape along… My advice, buddy, is better not take too much of a chance. Know a good thing when you got it."[5]

In addition to the uncertainties of job loss in the private sector, a number of workers were "sharing" jobs in the sense that firms spread their limited amount of work to more workers who all worked relatively fewer hours. Such practices made the gap between private and work relief employment even smaller. New Deal administrators were conscious of the worker's willingness to stay on relief. Relief regulations called for relief workers to actively seek and accept any "bona fide" offers of private employment. The WPA also assured workers of "immediate" rehire if they lost such private employment through no fault of their own. In fact, the WPA in some areas allowed workers to retain contacts with private employers by allowing them to supplement their WPA earnings with private earnings (Howard 1941, 207-9; Kesselman 1978, 195-6). Many workers stayed on WPA projects for extended periods of time. Margo (1993, p. 54) concluded "the continuous nature of WPA employment makes it difficult to believe that the WPA did not reduce, in the aggregate, the amount of job search done by unemployed workers in the late 1930's." Thus, in a time of large-scale unemployment, increases in relief spending may have made it more costly for private employers to attract workers.

The WPA's offer to relief workers of immediate rehire if they lost private jobs also may have encouraged employers to reduce the extent of job sharing at their firms. When demand for their product waned early in the Depression, a number of employers tried to retain a larger group of workers by reducing the number of hours each employee worked each month. Contemporaries ascribed some of the employers' motives to compassion, while others recognized that there were benefits to retaining workers with employer-specific skills in anticipation of a future upturn.[6] The presence of more work relief jobs with rehire options gave employers an alternative to job sharing. They could off-load the less productive of the job-sharing workers on to the relief rolls, hire them temporarily when business improved (in some cases while the worker still received WPA earnings), and then fully release them back to the WPA rolls when business worsened. Workers moving back and forth between private employment and public relief may have been no worse off than under the prior job sharing arrangement. As a result, expansions in relief availability allowed job sharing employers to reduce the number of people employed.

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[5] Quoted in E. W. Bakke, *The Unemployed Worker* (New Haven, 1940), pp. 421-22 and cited in Margo 1991 p.340.

[6] For contemporary policy statements on job sharing, see Walker (1933) and Moulton (1936).

Further, the measures of monthly earnings of the workers still employed would have risen for two reasons. The employed workers worked more hours in the absence of job sharing, and the still-employed workers were likely the more productive group and thus recipients of higher hourly wages.[7]

By applying the panel VAR with fixed effects to the monthly data for cities, we can examine the response of both work relief and direct relief to changes in labor markets, and vice versa, within and across years. The richness of the data also enables us to explore the political economy of the relief spending. Contemporary critics claimed that Roosevelt had been trying to "buy" votes by timing increases in relief to coincide with impending elections. Such claims appear consistent with large spikes in WPA employment around the 1936 and 1940 elections. Howard (1943, 534-39) claimed that the relief peaks coincided with economic downturns as well as with the election cycle. Gavin Wright's (1974, 35) cross-sectional regressions show that the number of per capita WPA jobs in December 1936 was higher in states that had a relatively higher number of jobs in January 1935, but also higher in states where there were more swing voters. He found similar results for a separate cross-sectional analysis of November 1940 WPA jobs as a function of 1937 unemployment rates.

The monthly information allows us to refine the analysis of the debate in ways that offer a more accurate picture of the situation. Critics of the New Deal focused on WPA employment in August, September, and October. The November and December job totals that Wright used were not as relevant for testing the political economy of relief spending because the elections occurred the first Tuesday of November. Further, Wright was forced to rely on economic distress evidence from 15 months or more prior to the election. The panel VAR applied to monthly data allows us to investigate the timing more precisely. We can examine the extent to which employment declines preceded increases in spending. Further, the use of time effects for the key months prior to elections, while controlling for employment changes, offer relatively flexible ways of examining the extent to which the months before elections may have contributed to rising relief spending.

II. Data

We have compiled a new monthly panel data set of relief spending at all levels of government in each of 44 cities. The original relief data contain monthly spending figures for work relief and direct relief. The work relief and direct relief expenditures are converted to per capita estimates using linear interpolations of population

[7] Kesselman (1978, 191) outlines a model and information from the 1930s that shows how heterogeneity of workers could contribute to this pattern. When the WPA investigated whether people on work relief were refusing private work, they often suggested that the people on work relief did not have the skills or ability to accept the work (Brimhall 1937, Coyle 1939, WPA 1937.

between 1930 and 1940 and then deflating by the national consumer price index. A detailed description of the sources and procedures used to construct the relief variables is found in Data Appendix A.

The relief spending information is merged with labor market, business composition, demographic, and price data to create an unbalanced panel of monthly observations from 44 cities between 1932 and 1940. The private labor market data for each city are based on linked-relative indices collected from surveys of a changing set of employers. The indices, with January 1932 as the base, capture changes from one month to the next in the number of workers and the monthly payroll for the same set of private employers.[8] The earnings index is a measure of average monthly earnings of workers and is constructed by dividing the payroll index by the employment index.[9] The average monthly earnings are a combination of the average hourly wage and the number of hours worked during the month.

To develop a measure of exogenous changes in industrial conditions in each city, we gathered information on each industry's share of employment within the city in 1929 and on monthly changes in the national production of each industry. We then created a National Industrial Production Shock (NIPS) index for each city by using the 1929 industry shares in that city as weights applied to the national monthly industrial production indexes for each industry. The index measures the time path of nation-wide industry shocks that were likely felt in each city based on the distribution of industries in that city. Since nearly all of the industries were geographically dispersed, the index is largely unaffected by local labor market conditions.[10] The 1929 employment weights come from the 1929 Census of Manufacturing and the national industrial production indices are from the Federal Reserve Board (August 1940, 826-82, November 1941, 1174).

Correlations between Private Employment and Relief

The Depression hit the entire country hard, but the extent of the downturn varied across locations. Similarly, the size of New Deal expenditures was unprecedented but also distributed unequally. Table 1 illustrates the cross-sectional variation by showing the means across the 44 cities and the standard deviation, minimum, and maximum for the sample. For example, between 1932 and 1940, Brockton, Massachusetts received the highest

[8] For further information on linked-relative indices for 1930s employment, see Wallis (1989)

[9] Further information on the construction of the variables appears in Data Appendix A. The linked-relative indices do not capture increases in employment when new firms start hiring or declines in employment when a firm closes.

[10] The local IP index variation over time is driven by the national variation in the subcomponents of IP like mining, automobile industry, etc. The variation between cities in our sample is then determined by each components share in the local economy in 1929.

average monthly per capita work relief spending of $2.80.[11] Baltimore received the least at $0.34. For the entire sample of 44 cities, the average monthly expenditure on work relief programs was $1.12 per person. The standard deviation of these means was $0.44.

Table 1

Cross Sectional Summary Statistics

	N	Mean	Std	Minimum	Maximum
Employment Index	44	1.14	0.17	0.72	1.52
Earnings Index	44	1.10	0.17	0.76	1.56
Direct Relief per capita	44	$0.58	$0.22	$0.16	$0.99
Work Relief per capita	44	$1.12	$0.44	$0.34	$2.80
Industrial Production Index	44	1.11	0.04	1.03	1.22

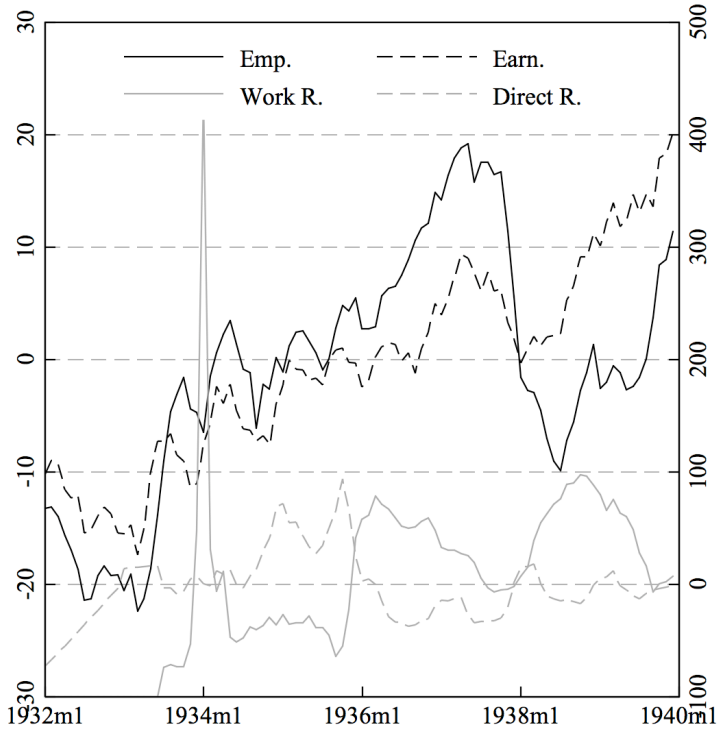
The average monthly per capita direct relief expenditure was $0.58, with a standard deviation of $0.22. Kansas City, Kansas received the least, on average, while Philadelphia received the most. Recall that employment and earnings are measured as indices, consequently they have no units associated with them. But the average of the employment index over the sample period was 1.14, or 14 percent above the level of employment in January 1932. Jersey City. New Jersey had the lowest average employment relative to its January 1932 level at 0.72. Toledo, Ohio had the highest at 1.52. The monthly earnings index averaged over the sample period is 1.10, or 10 percent above the level of earnings in January 1932. Kenosha, Wisconsin had the lowest average monthly earnings relative to its January 1932 level (0.76), while Lynn, Massachusetts had the highest (1.56). The means for the National Industrial Production Shock (NIPS) Index had the least cross-sectional variability with a range of 1.03 to 1.22.

One advantage of the panel VAR approach is that we can follow the common practice of "demeaning" the variables for each city. Demeaning effectively eliminates any city-specific unobservables that are fixed over time. We transform each city's monthly observation into that month's percent deviation from the city mean between 1932 and 1940. Figure 1 shows on one graph the deviations from the mean, averaged across cities, for the labor market and relief variables. Labor market variables are indexed on the left hand axis and relief variables are indexed on the right.

[11] January 1935 dollars.

Figure 1

Trends in Deviations from Mean Levels of Relief and Private Labor Market Variables (averaged across cities)



The gigantic nationwide spike in per capita work relief spending during the winter of 1933/1934 is associated with the CWA, which employed over 4 million people at its peak while paying prevailing wages for up to thirty hours per week (Schwartz 1984, p. 117). The per capita spending in the sample cities exhibits an even more pronounced spike than what one would see nationally because they received a disproportionately large portion of CWA spending compared with the national average. After the Second New Deal policies were established in mid-1935, there were two peaks in mid-1936 and late 1938. The peaks in WPA spending fell well short of the CWA peak in the winter of 1933-34 because the WPA never employed more than 3.2 million people and offered payments per hour worked that were roughly half of the CWA level (National Resources Planning Board 1942, 562-563). Per capita direct relief was less volatile. It peaked in late 1935, soon after the federal government returned responsibility for "unemployables" to the state and local governments.

It is difficult to see any consistent correlation between the time paths of the cross-city average deviations of the relief spending and the labor market variables. There is one striking rise in per capita work relief in 1938/1939 that occurred at about the same time as a drop in employment. The relationship between the time paths of the average deviations across cities in private employment and earnings is also not clear. Over some

periods it appears that increases in monthly earnings precede increases in employment. Since monthly earnings combine hours worked and wages, the pattern is consistent with employers' following a practice of raising monthly hours before adding new employees.

Figure 1 shows only the pattern of the cross-city average deviation from the 1932-1940 mean for the 44-city sample. To show the full variation, Figures 2 and 3 plot each city's monthly deviation from its 1932-1940 mean for private employment and earnings. The solid line is the mean value across the cities in each month, while each dot represents an observation for an individual city. The dispersion of dots for each month highlights the vastly different experiences in each city relative to its own mean, while the solid line shows the average experience for all cities across time. Figure 3 plots the National Industrial Production Shock (NIPS) index in deviation form for each city (the monthly mean across the cities is represented by the solid line). Similarly, Figures 4 and 5 plot the deviations in work and direct relief spending, respectively.

Figure 2

Deviations from Mean Levels of Private Employment in Each City

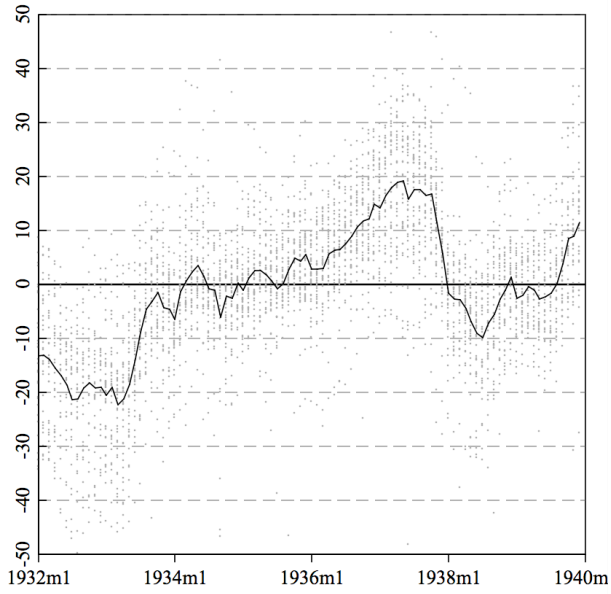


Figure 3

Deviations from Mean Levels of Private Earnings in Each City

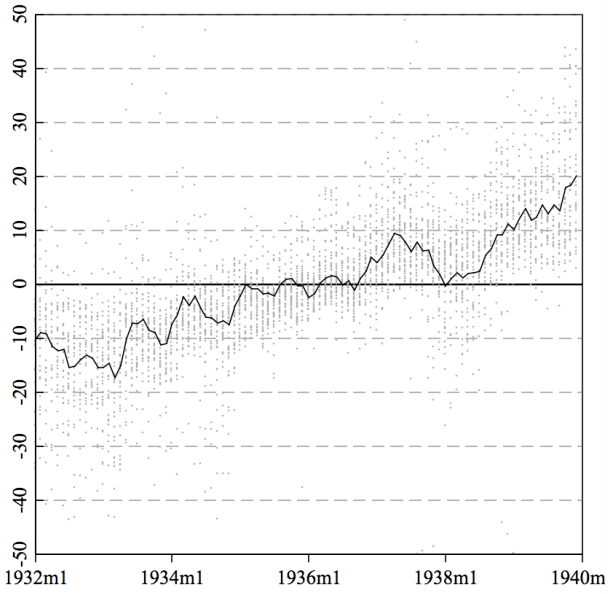


Figure 3

Deviations from Mean Levels of
National Industrial Production in Each City

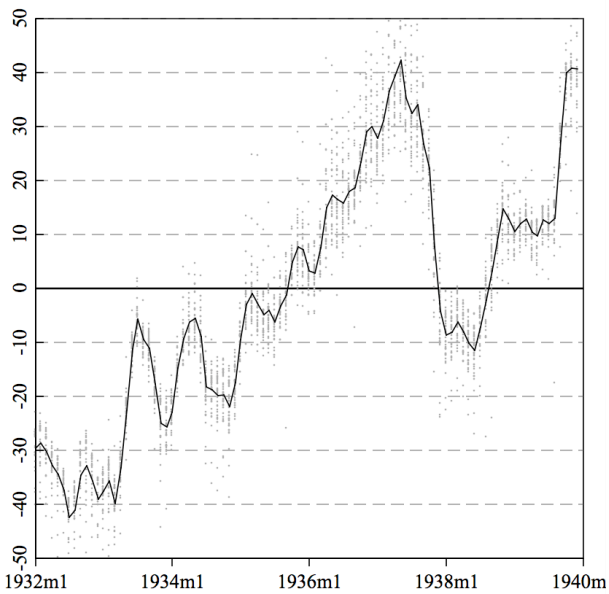


Figure 4

Deviations from Mean Levels of
Work Relief in Each City

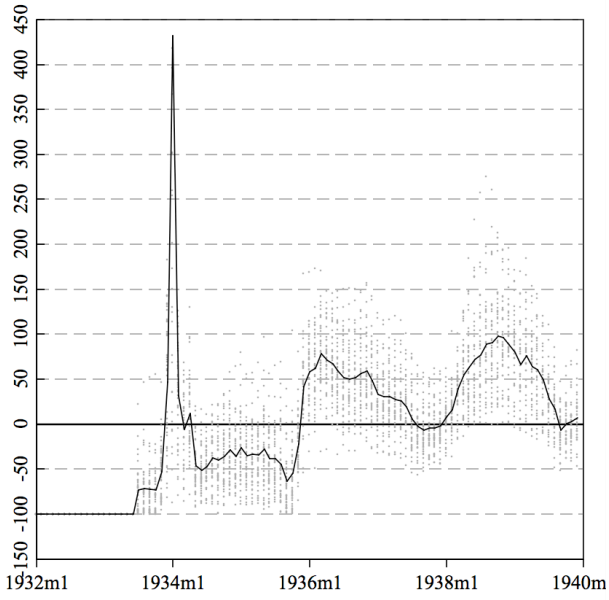
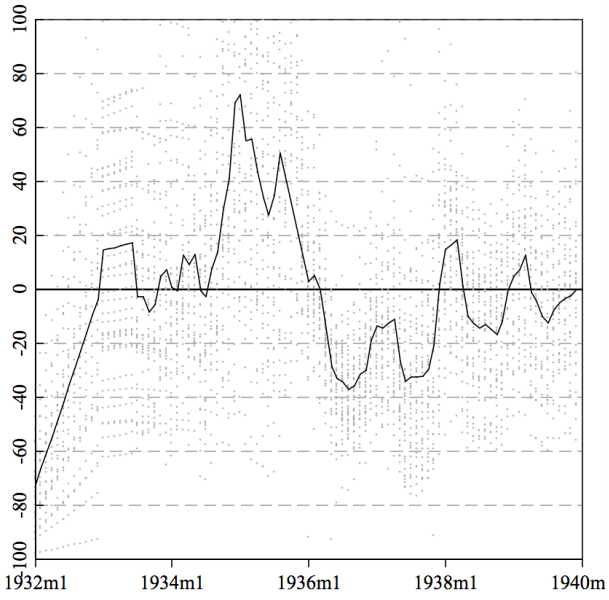


Figure 5

Deviations from Mean Levels of
Direct Relief in Each City



A time-series VAR model based on the national averages would use the time series variation of average deviations across cities like those graphed in Figure 1 to determine the relationship between the variables in the model. But such a VAR would only have one observation per time period. The panel VAR we employ is more

powerful in capturing the average relationships between relief and labor markets because we have as many as 44 city observations for each period. We can therefore use time effects to control for national shocks that might have struck all cities during the same month. The time effects effectively eliminate the time patterns illustrated by the solid lines in Figures 2 through 5.

Before describing the VAR model, it is necessary to check that the data meet the requirements for this methodology. Data used in a panel VAR setting must be stationary and comparable between different cross sections in order to consistently estimate the reduced form coefficients. The use of per capita expenditures makes relief spending comparable across location, while taking the percent deviation relative to the mean in each variable makes the employment, earnings, and industrial production indices comparable. The stationarity condition is tested using the augmented Dickey-Fuller unit root test. For each variable, the test rejects the null hypothesis of a unit root, confirming the stationarity of our deviation-adjusted data.

III. Empirical Model

Consider an empirical model of the relationships between employment, monthly earnings, direct relief, and work relief like equation system (1) below.

$$
\begin{aligned}
Emp_{it} &= \sum_{j=1}^{L} \alpha_j^{Emp} Emp_{it-j} + \sum_{j=0}^{L} \alpha_j^{Earn} Earn_{it-j} + \sum_{j=0}^{L} \alpha_j^{Work} Work_{it-j} + \sum_{j=0}^{L} \alpha_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} \alpha_j^{IP} NIPS_{it-j} + \varepsilon_{it}^{Emp} \\
Earn_{it} &= \sum_{j=0}^{L} \beta_j^{Emp} Emp_{it-j} + \sum_{j=1}^{L} \beta_j^{Earn} Earn_{it-j} + \sum_{j=0}^{L} \beta_j^{Work} Work_{it-j} + \sum_{j=0}^{L} \beta_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} \beta_j^{IP} NIPS_{it-j} + \varepsilon_{it}^{Earn} \\
Work_{it} &= \sum_{j=0}^{L} \gamma_j^{Emp} Emp_{it-j} + \sum_{j=0}^{L} \gamma_j^{Earn} Earn_{it-j} + \sum_{j=1}^{L} \gamma_j^{Work} Work_{it-j} + \sum_{j=0}^{L} \gamma_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} \gamma_j^{IP} NIPS_{it-j} + \varepsilon_{it}^{Work} \\
Direct_{it} &= \sum_{j=0}^{L} \delta_j^{Emp} Emp_{it-j} + \sum_{j=0}^{L} \delta_j^{Earn} Earn_{it-j} + \sum_{j=0}^{L} \delta_j^{Work} Work_{it-j} + \sum_{j=1}^{L} \delta_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} \delta_j^{IP} NIPS_{it-j} + \varepsilon_{it}^{Direct}
\end{aligned}
\tag{1}
$$

where *i* indexes cities and *t* indexes months. The endogenous variables are period *t* private employment, private earnings, per capita work relief spending, and per capita direct relief. The model incorporates contemporaneous endogenous variables on the right hand side as well as several lags (up to L) for each variable in each equation. The lag dependent variables account for the fact that in monthly data there is likely to be persistence of a variable like employment as employers often have a core group of workers that they retain even when laying off workers. Further, the relationship between employment and another variable like work relief might have lasted for several periods, as it took time for relief officials to recognize a drop in employment and additional time to take actions to

expand relief. These same patterns were true for all of the variables. The appropriate number of lags is an empirical issue.

Estimation of the parameters of this empirical structural equation is made difficult because employment, earnings, work relief, and direct relief were likely determined simultaneously and endogenously. In a much simpler system with no lagged effects, estimating the coefficients effectively would require the development of instruments for each of the endogenous variables. Given the paucity of monthly data at the city level during the 1930s, finding such instruments is a daunting if not impossible task. It is likely that any successful instrument strategy would not allow the estimation of the full lag structure in the system.[12]

Vector Autoregressive Equations

In this paper we follow an alternative strategy that employs a four variable panel VAR model to explore the dynamic relationships between relief spending and the private labor market during the Depression. Although we can not directly estimate the underlying structural parameters in equation system (1), the VAR methodology has several advantages when there are multiple lags. It does not require any a priori assumptions on the direction of feedbacks between the variables in the model. Instead, we allow all current period measures of relief and the private labor market to be a function of the past values of each other and an exogenous industrial production index.[13] This allows us to estimate, for example, the total reduced-form effect that a past increase in work relief had on each of the other dependent variables and how those changes moved through the local economy over time. In this way, as documented by Zellner and Palm (1974), the VAR is a reduced-form representation of a structural econometric model.[14]

The structure of the panel VAR is found in Equation System (2).

[12] We considered the use of an Arellano-Bond strategy of using lagged levels of the dependent variable as instruments for differenced dependent variables. However, Stephen Bond informed us in correspondence that he considered 44 cross-sectional observations to be too small a sample for effective use of the technique.
[13] The exogenous nature of the IP index was tested by running a 5 variable VAR model with IP as one of the dependent variables. Coefficient estimates and IRF were largely unchanged. Granger causality tests showed that the IP index was not Granger caused by direct relief, work relief, or private employment. [What about monthly earnings?????]
[14] Nelson and William (1982) show that tests for Granger causality in a multi-equation model are more powerful than any tests that might be performed in a single equation model.

$$
\begin{aligned}
Emp_{it} &= \sum_{j=1}^{L} a_j^{Emp} Emp_{it-j} \sum_{j=1}^{L} a_j^{Earn} Earn_{it-j} + \sum_{j=1}^{L} a_j^{Work} Work_{it-j} + \sum_{j=1}^{L} a_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} a_j^{IP} NIPS_{it-j} + e_{it}^{Emp} \\
Earn_{it} &= \sum_{j=1}^{L} b_j^{Emp} Emp_{it-j} + \sum_{j=1}^{L} b_j^{Earn} Earn_{it-j} + \sum_{j=1}^{L} b_j^{Work} Work_{it-j} + \sum_{j=1}^{L} b_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} b_j^{IP} NIPS_{it-j} + e_{it}^{Earn} \\
Work_{it} &= \sum_{j=1}^{L} c_j^{Emp} Emp_{it-j} + \sum_{j=1}^{L} c_j^{Earn} Earn_{it-j} + \sum_{j=1}^{L} c_j^{Work} Work_{it-j} + \sum_{j=1}^{L} c_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} c_j^{IP} NIPS_{it-j} + e_{it}^{Work} \\
Direct_{it} &= \sum_{j=1}^{L} d_j^{Emp} Emp_{it-j} + \sum_{j=1}^{L} d_j^{Earn} Earn_{it-j} + \sum_{j=1}^{L} d_j^{Work} Work_{it-j} + \sum_{j=1}^{L} d_j^{Direct} Direct_{it-j} + \sum_{j=1}^{L} d_j^{IP} NIPS_{it-j} + e_{it}^{Direct}
\end{aligned}
\tag{2}
$$

There are two key differences between systems (1) and (2). First, in system (2) there are no contemporaneous interactions between the endogenous variable at time t on the left side of the equation with the other variables at time t on the right side of the equation. In other words, all right hand side variables are lags. Second, the coefficients in system (2) are complex functions of the coefficients in system (1). The NIPS index (IP) is included as a further control for national level industrial shocks in each time period that had a differential impact on local labor markets based on the disparate industrial employment in each city. All variables are represented in the form of the percent deviation from the 1932-1940 city mean; therefore, the coefficients are all elasticities. We assume that the vector of the error terms from each equation is identically and independently distributed across cities and time.

In estimating the elasticities in equation system (2), the panel VAR allows us to treat the past values of the variables as exogenous for several reasons. Since the equations are current values as functions of past values, the simultaneity of the system is eliminated. Potential problems with serial correlation in the errors of each equation are eliminated by incorporating an appropriate number of lags for each variable. By demeaning the data in the panel VAR, we are able to control for time-invariant factors for each city that might have been correlated with both relief spending and the private labor market. These might include structural features of the city's economy and political environment. By controlling for national shocks for each month between February 1932 and December 1940 we can control for multiple factors that were the same across the country, but varied from month to month. These would include seasonal factors that influenced employment patterns, national economy-wide shocks like the move away from the gold standard or changes in federal tax rates, the timing of elections, and shocks to relief associated with the timing of Congress's approval of new federal budgets for relief, or changes in the federal relief program's rules.

VAR Specification

To insure the consistency of the reduced-form VAR coefficient estimates, we use several criteria to determine the appropriate lag structure. The first criterion selects the lag length that minimizes one or more of three goodness of fit measures, which trade off the complexity of an estimated model against how well the model fits the data. As shown in Table 2, a lag of three months minimizes the value of three such statistics: the Akaike information criterion (AIC), the Hannan and Quinn information criterion (HQIC), and Schwarz's Bayesian information criterion (SBIC).

Table 2

Lag Length Specification

Lags	AIC	HQIC	SBIC	N
1	36.55	36.87	37.44	3966
2	36.35	36.67	37.27	3922
3	**36.27**	**36.61**	**37.24**	**3878**
4	36.30	36.66	37.31	3834
5	36.30	36.67	37.35	3790
6	36.33	36.72	37.42	3746
7	36.35	36.75	37.49	3702
8	36.37	36.79	37.56	3658
9	36.34	36.78	37.58	3614
10	36.32	36.77	37.60	3570
11	36.33	36.80	37.66	3526
12	36.31	36.80	37.69	3482

The second criterion is to examine cross-correlations to ensure that there are enough lags to satisfy the assumption that the vector of error terms is independently and identically distributed. Table 3 reports the number of statistically significant cross-correlations for lags of up to 12 periods under 4 specifications. The first two use a maximum of three lags with and without accounting for time effects. The second two use 6 lags with and without time effects. The use of 6 lags and time effects greatly reduces the chance of residual cross-correlation.

Table 3

Lag Length Specification Tests

	VAR Specification			
	3 lags no time effects	3 lags with time effects	6 lags no time effects	6 lags with time effects
Significant Correlations	232	155	161	55
Possible Bad Pairs	2574	2574	2574	2574
Percent	9.01%	6.02%	6.25%	2.14%

A model with 6 lags and time effects also meets the third commonly-used criterion to not include unnecessary lags that reduce the precision of the estimates. Given that the selection statistics in Table 2 rise as the

maximum lag length is increased above 6 and that the number of statistically significant residual cross-correlations is substantially smaller with 6 lags including time effects, the use of 6 lags with time effects seems appropriate.[15] Appendix B contains the coefficient estimates and impulse response graphs for the other specifications in Table 3.

The contemporaneous cross correlations of the residuals from the different equations in the model suggest that the panel VAR is a reasonable model because the correlations are generally low and for the most part not statistically different from zero. As shown in Table 4, the residuals from the employment and earnings equations are positive and statistically significantly correlated as one might expect, although the correlation is small. There is also a small negative and statistically significant correlation between work relief and direct relief.

Table 4

Contemporaneous Residual Correlation Matrix

	Private Employment	Private Earnings	Work Relief	Direct Relief
Private Employment	1.00			
Private Earnings	0.08 (0.00)	1.00		
Work Relief	-0.01 (0.48)	0.02 (0.23)	1.00	
Direct Relief	-0.05 (0.01)	0.01 (0.49)	-0.06 (0.00)	1.00

Note. The p-values of hypothesis test with the null that the correlation is zero are in parentheses.

IV. Results

Coefficient Results

After demeaning the data and accounting for month specific shocks, we estimate the model specified in equation system (2) with 3746 observations (44 cities for the period January 1932 through June 1940 with some missing observations). Table 5 presents the coefficient results estimated using GMM. The coefficients, which are elasticities, suggest some interesting contrasts in the extent of persistence in the impact of past values of a variable on its own contemporary level. The private employment, private monthly earnings, and direct relief variables all display strong persistence from the previous period but little persistence beyond that point. Each variable has an elasticity with respect to its own first-period lag that exceeds .785 and is statistically significant. The own-lag elasticities at longer lengths are several orders of magnitude smaller. The strong one-period persistence makes

[15] All specifications were tested to assure they met eigenvalue stability conditions.

sense in the earnings and employment variables because many firms retained a stable core of workers from month to month. The even stronger persistence of direct relief also makes sense given that a significant share of direct relief was commonly given to a core group of "unemployables" who were likely in dire straits for reasons that were not driven by the volatility of the labor market.

Table 5

VAR Coefficient Results—6 lags with Time Fixed Effects

		Dependent Variable							
		Employment		Earnings		Work Relief		Direct Relief	
order	Independent Variable	beta	t-stat	beta	t-stat	beta	t-stat	beta	t-stat
1	Employment Lag 1	0.758	9.275	0.051	1.386	-0.486	-3.747	-0.045	-0.860
1	Employment Lag 2	0.039	0.630	-0.074	-2.275	0.155	0.846	-0.058	-0.895
1	Employment Lag 3	0.067	2.058	0.009	0.267	0.152	0.943	0.076	1.173
1	Employment Lag 4	0.003	0.116	-0.004	-0.183	-0.462	-2.155	0.092	1.478
1	Employment Lag 5	-0.002	-0.071	0.007	0.221	0.214	0.905	-0.092	-1.436
1	Employment Lag 6	0.006	0.281	0.024	0.972	0.146	0.964	-0.009	-0.175
2	Earnings Lag 1	0.168	3.817	0.849	15.665	-0.033	-0.206	-0.172	-2.839
2	Earnings Lag 2	-0.116	-2.459	0.068	1.384	-0.241	-0.937	0.168	1.884
2	Earnings Lag 3	-0.012	-0.337	-0.030	-0.819	0.249	1.350	-0.030	-0.397
2	Earnings Lag 4	-0.019	-0.650	-0.014	-0.332	0.019	0.098	0.040	0.503
2	Earnings Lag 5	0.037	1.174	0.054	1.164	-0.015	-0.081	-0.043	-0.549
2	Earnings Lag 6	-0.047	-2.115	0.006	0.158	0.056	0.381	0.035	0.594
3	Work Relief Lag 1	-0.001	-0.949	0.002	1.517	0.218	4.656	-0.019	-2.321
3	Work Relief Lag 2	-0.004	-1.964	0.003	0.941	0.132	4.564	0.007	1.181
3	Work Relief Lag 3	-0.002	-0.650	-0.004	-1.183	0.086	4.083	-0.003	-0.368
3	Work Relief Lag 4	0.002	1.363	0.003	1.219	0.069	3.704	-0.001	-0.084
3	Work Relief Lag 5	-0.002	-1.076	0.001	0.911	0.048	3.229	0.011	1.311
3	Work Relief Lag 6	0.002	1.505	0.001	0.370	0.041	3.457	-0.010	-1.781
4	Direct Relief Lag 1	-0.008	-2.124	0.002	0.532	-0.206	-3.845	0.909	15.793
4	Direct Relief Lag 2	0.010	1.781	0.001	0.157	0.121	1.783	-0.155	-2.158
4	Direct Relief Lag 3	-0.009	-1.240	-0.006	-0.929	-0.057	-1.064	0.075	1.214
4	Direct Relief Lag 4	0.004	0.453	0.014	2.098	0.132	2.211	0.035	0.855
4	Direct Relief Lag 5	0.007	0.920	-0.009	-1.661	-0.096	-1.648	-0.029	-1.077
4	Direct Relief Lag 6	0.001	0.339	0.003	0.987	0.012	0.445	0.009	0.493
5	NIPS Lag 1	0.131	2.075	0.002	0.035	-0.419	-1.597	-0.616	-3.791
5	NIPS Lag 2	-0.172	-2.117	-0.106	-1.348	0.199	0.325	0.220	0.942
5	NIPS Lag 3	-0.002	-0.019	0.029	0.244	-0.046	-0.089	0.173	0.727
5	NIPS Lag 4	-0.025	-0.259	0.064	0.711	-0.026	-0.053	-0.540	-1.948
5	NIPS Lag 5	0.059	0.608	0.041	0.600	0.010	0.027	0.383	1.386
5	NIPS Lag 6	0.049	0.865	-0.080	-1.787	-0.308	-1.067	-0.140	-0.883
	observations	3746		3746		3746		3746	

In contrast, the elasticity of current work relief with respect to the prior period's work relief was much lower at 0.218, but was sustained longer with a statistically significant second lag elasticity of 0.138. The presence of some persistence that lasted a couple of months likely arose because work relief distributions were typically tied

to specific projects that might last for a couple of months or more. It appears, however, that the lumpy nature of the fund allocations was relatively easily offset because most cities had a portfolio of projects and they appeared to have the power to adjust work relief spending by changing the numbers employed.

Granger Causality Tests

A common technique used to evaluate the results from a VAR model is a test for Granger causality. The Granger causality test compares the fit of the model using a likelihood ratio test before and after restricting the coefficients on a given variable's lags to zero. For example, in testing the null hypothesis that a group of lagged values of work relief spending did not influence current private employment, the VAR model is re-estimated restricting the coefficients on all lagged values of work relief in the employment equation to zero. The likelihood ratio test statistic is distributed χ^2 with degrees of freedom equal to the number of restricted coefficients. If the null is rejected, work relief spending in earlier months can be said to influence or "Granger-cause" private employment.

Table 6

Granger Causality Tests

Null Hypothsis		Direction	Chi-Squared	Degrees of Freedom	p-value
Employment is not Granger Caused by	Earnings	+	101.64	6	0.000
	Work	—	11.81	6	0.066
	Direct	+	17.09	6	0.009
	NIPS	+	35.23	6	0.000
	All	na	179.88	24	0.000
Earnings is not Granger Caused by	Employment	+	20.65	6	0.002
	Work	+	19.38	6	0.004
	Direct	+	11.68	6	0.070
	NIPS	—	24.63	6	0.000
	All	na	81.65	24	0.000
Work Relief is not Granger Caused by	Employment	—	20.83	6	0.002
	Earnings	0	2.87	6	0.825
	Direct	—	36.22	6	0.000
	NIPS	0	4.98	6	0.546
	All	na	66.61	24	0.000
Direct Relief is not Granger Caused by	Employment	0	4.83	6	0.566
	Earnings	0	6.79	6	0.340
	Work	—	13.86	6	0.031
	NIPS	—	38.96	6	0.000
	All	na	75.05	24	0.000

The tests, summarized in Table 6, show that work relief spending Granger-caused private earnings at a 99% confidence level and private employment at a 90% confidence level. Direct relief Granger-caused private

employment at the 99% level and private earnings at the 90% level. These results show that the prior levels of New Deal relief spending influenced the current labor market during the 1930s. Based on these statistical tests and the cumulative signs derived from the impulse response functions (discussed below), a shock to direct relief spending led to later increases in both private employment and monthly earnings. On the other hand, prior work relief spending served to reduce the number of people working while increasing the monthly earnings of the employed.

Conversely, New Deal work relief was Granger-caused by prior private employment but not by private monthly earnings. The finding provides evidence that New Deal administrators sought to provide economic relief and recovery, although they appear to have been worried more about lost employment than about drops in monthly earnings that could have been caused by lower wages or reduced working time. The number of jobs seemed to have mattered more than the actual earnings that workers received.

Direct relief and work relief Granger-caused each other, suggesting that the two types of work relief may have served as substitutes. Unlike work relief, however, direct relief was not Granger-caused by either private employment or earnings. This finding is not surprising, particularly after 1935, because a large component of direct relief was generally provided to people who were considered "unemployable."[16]

As expected, private earnings and employment Granger-caused one another at the 99% confidence level. Both were Granger-caused by changes in the National Industrial Production Shock (NIPS) index.

Impulse Response Functions

The dynamic relationships estimated in the panel VAR are best illustrated by graphing Impulse Response Functions (IRFs) calculated from the estimated elasticities. Figure 6a charts the net response to a one-standard-deviation one-month shock in work relief at time zero for each of the endogenous variables over the following two-year period. Figures 9b, 9c, and 9d show the time paths of the responses following shocks in month zero in direct relief, private employment, and private earnings, respectively. The units on the vertical axis show the dependent variable's percent deviation from its mean in each month over the 24-month period shown on the horizontal axis. The dark line is the point estimate of the IRF, and the dotted lines show the 95% confidence interval around that estimate, created using bootstrap standard errors with one thousand repetitions.

[16] We have estimated the model for the periods 1932 through June 1935 and June 1935 through December 1940 to examine whether the administrative shift from the FERA to the WPA forms of emergency relief had substantial effects on the analysis and found little difference between the time periods.

Work Relief Shocks and Private Labor Markets

At several points during the New Deal, there were positive shocks to a city's work relief budget when the federal relief administration approved a new works project.[17] A likely size of the shock at the city level was one standard deviation of the work relief variable, or 51.7 percent of the 1932-1940 mean. The first row of Figure 6a shows the net response of private employment and private earnings to the shock. Although New Deal administrators tried to prevent work relief from having a negative impact on private employment, the impulse response function in column 1 row 1 shows that an increase in work relief spending at time zero was associated with a statistically significant decrease in private employment of over 0.2 percentage points by month 6. The negative effect then slowly trended back toward zero over the next 12 months.

Using the point estimates from the impulse response functions, we can construct the cumulative rise in work relief spending and the cumulative fall in employment associated with a one-standard deviation shock in work relief spending in period zero by summing the monthly deviations in column 1 row 2 and column 1 row 1 of Figure 6a, respectively. These cumulative effects are also equivalent to the area between the impulse response function line and zero in column 1 row 2 and column 1 row 2 of Figure 6a.

As seen in column 1 and row 2 of Figure 6a, the 51.7 percent shock to work relief spending in period zero resulted in additional increases in work relief of 11.5 percent above the mean in period one, 9.6 percent in period 2. When we sum up all of the deviations over the entire 24-month period, they total a 132 percent deviation. If we assume that all of this increase went to an increase in the number of work relief job months rather than higher work relief pay, it would have meant an 132 percent increase relative to the mean number of relief job months. In a city such as Cleveland, Ohio, which averaged about 30,250 relief jobs per month, the initial one-standard deviation shock would have led to an additional 39,930 relief job months over the 24-month period after all feedback effects were considered.

Meanwhile, the cumulative effect of the work relief shock was to reduce the number of job months of private employment over the following 24 months by a total of negative 2.9 percent of the mean. In Cleveland, where a rough estimate of the mean employment was around 450,000 workers, this translates into a cumulative

[17] Howard (1941, notes on pp. 106-7) describes the WPA as having a hand-to-mouth existence because the funding was enacted piece-meal over time. In 1939, for example, the Emergency Relief Appropriation Act was not passed until 11:30 on June 30, a half-hour before the start of the new fiscal year. The WPA had already laid off workers in June because of the uncertainty. Some new WPA appropriations were enacted as supplementary measures in other parts of the year. Even though the bills were designed to fund the entire WPA, the timing of their impact might have varied from city to city based on the timing of project approvals related to the total WPA budget.

decrease in private job months of nearly 13,000 over the 24-month period.[18] The comparison suggests that for every 3.5 relief job months added over the 24-month period, private sector employment declined by one job month.

At the same time, the one standard deviation shock to work relief spending was also associated with a rise in private monthly earnings (see column 2 row 1 of Figure 6a). At its monthly peak the work relief shock raised private earnings nearly one-half percentage point from its mean. After adding up the deviations from the mean over the 24-month period, the cumulative effect is 7.4 percent. Since monthly earnings are the product of average hourly earnings and hours worked per month, monthly earnings may have increased either because hours rose or hourly wage rates increased.

Figure 6a

VAR Impulse Responses Following a Shock to Work Relief

The combination of effects of a work relief shock to lower private employment and to raise monthly earnings is consistent with a model of crowding out. It is important to note, however, that the impulse response

[18]Cleveland had a population of 1.2 million. Assuming a labor force participation rate of 46.6% and an average unemployment rate of 20%, the employed population would have been 448,276.

functions are not providing a pure estimate of crowding out in the sense that a work relief job in time t led to the elimination of a private job in time t.

Alternatively, it is not unreasonable to believe that it might have taken private employers and work relief managers a month or longer to respond to changes in the labor force or relief spending. If it took a month or more for each of the endogenous variables in the system to respond to a shock in another variable, all of the coefficients of the time t variables in the structural system of equations would be zero. In that case, the panel VAR model in equation system 2 is exactly the same as the structural model in equation system (1). The coefficient on the first lag of work relief then would be an estimate of the initial impact of work relief on employment before feedback effects came into play. That initial elasticity is negative but very small at -0.001 and not statistically significant. This suggests one of two possibilities, 1) relief spending did not have strong initial crowding out effects and work relief provided no stimulus to consumption, or 2) any positive stimulus from work relief to consumption that funneled into higher labor demand would have been offset by a crowding out effect that reduced private labor demand.

Instead of a pure crowding out effect, the IRFs show that the complex feedbacks between relief and the private labor market led to a negative relationship between work relief and private employment and a positive relationship between work relief and private earnings over an extended period of time. These relationships can help explain a common occurrence during the New Deal. People might have anticipated better times as new work relief projects came on line in a city from a new stimulus to the local economy. The IRFs suggest, however, that over the next few months workers found it harder to find private jobs in the city as private employment declined. Private employers complained that the WPA earnings were too high and that they were having trouble hiring workers at wages they had been paying before. Among the numerous reports, a June 1937 survey of State Administrators of the New Deal by the Office of Government Reports found that private industry demanded WPA workers in 35 states, but that there were workers reluctant to accept private work or actually refusing it in 19 of the states (Petree, 15 June 1937). The type of work refused ranged from domestic labor to farm labor to factory work to skilled labor (Howard 1943, 487-88).[19]

Worried about this issue, the WPA investigated their procedures and typically found that the local project managers were encouraging workers to accept private jobs when the jobs became available. In general, the WPA

[19] For other examples of complaints, see Coyle (1939), Brimhall (1937), Petree (8 November, 1934, 15 April 1935, 27 April 1936, 17 August 1936, 15 September 1936, 9 October 1936, 15 June 1937, 12 July 1937; 13 September 1937), Larned (January 1935, 18 April 1935, 29 July 1935, 12 August 1935, 19 August 1935), Wood (13 July 1937), and Works Progress Administration (1937).

would not require workers to accept jobs paying less than the prevailing wage. In some cases, the workers were even allowed to supplement WPA earnings with private earnings. At various times, the WPA promised workers an opportunity to return to work relief if the private job fell through. When investigating the pay per hour worked on WPA projects, the officials noted that the pay was often well below the prevailing earnings. The results of the WPA investigations often turned up relatively few cases where workers had turned down private work at prevailing wages, and those who had were immediately fired. When the WPA officials reported the findings of their investigations they sounded a triumphant note that they had proved the complaining employers wrong. H.O. Hunter of the WPA in a 1939 radio address stated: "Now we have found one single overwhelming fact in all of these cases—as soon as we ask for concrete details, with names and dates, the stories melt away into nothing but idle rumor."[20]

The impulse response functions show, however, that the complaining employers and the WPA officials both were right but in some sense were talking past each other. The employers were describing real patterns and experiences that they were witnessing in their cities. They might not have been able to pin down precise names and dates, but they were seeing upward pressure on wages they had to pay and, thus, their incentives to hire diminished. Meanwhile, WPA officials could document that they were following the procedures that they had designed to avoid harming private labor markets. WPA wages that were "too high" in the employers' view did not have to exceed wages on private jobs. It is very possible that the security of the WPA job made the relief job attractive enough that employers were discovering that they were raising monthly wages and hiring fewer workers in a time of extraordinarily high unemployment rates. The complex feedback effects from a work relief shock over time led them to see these patterns.

The higher monthly earnings and lower employment shown by the impulse response functions are also consistent with the idea that an increase in work relief jobs allowed employers to cut back on job sharing.[21] When more work relief jobs were available, employers saw an opportunity to lay off less productive workers, who could then join the work relief rolls until they could be rehired if the economy improved. The measure of monthly earnings rose because the remaining workers could work more hours and were likely paid higher hourly wages because they were more productive than the laid off workers.

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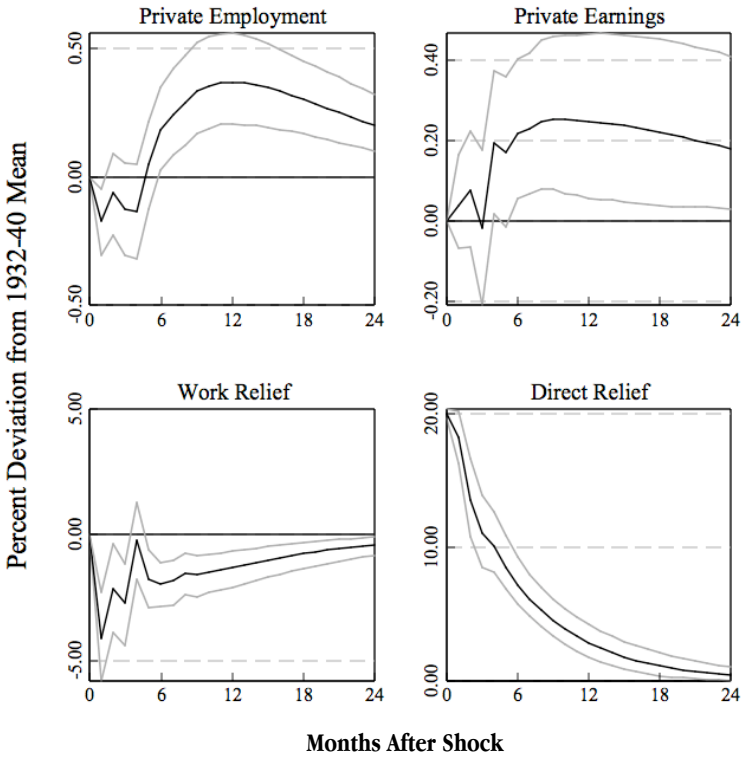
[20] Quoted in Howard (1940, 489). For other discussions of the investigations, see Coyle (1939), Brimhall (1937), Howard, (1940, 488-492, WPA (1937).
[21] For contemporary policy statements on job sharing, see Walker (1933) and Moulton (1936).

Direct Relief Shocks and Private Labor Markets

As was the case with work relief, the employment elasticities with respect to direct relief in Table 4 tended to be quite small, as were the elasticities of monthly earnings with respect to work relief. As a result, the initial effects of direct relief are relatively small and then become magnified after about four months. Private earnings rose slightly with one dip over the first four months, but the confidence intervals show we cannot reject the hypothesis of no effect. At that point the effect of direct relief jumped to about 0.20 percent and remained in that range over the rest of the two-year period.

Figure 6b

VAR Impulse Responses Following a Shock to Direct Relief



The response of private employment was not statistically different from zero over the first 5 months and then jumped sharply into the positive range for the remainder of the two-year period. Over the 24-month period the cumulative effect of an increase in direct relief spending on private job months was 5.2 percent. In Cleveland, a city with about 450,000 workers, this cumulative impact translates into about 23,000 private job months over the two-year period.

Interactions of Work Relief and Direct Relief

The VAR estimates show that work relief and direct relief tended to move in opposite directions, as each Granger-caused the other in a negative direction. When work relief is shocked by one standard deviation, direct relief spending drops 2 percent by the first month (Figure 6a). The 24-month cumulative effect is negative 20 percent. Recall that the 24-month cumulative response of work relief spending to a one-time one-standard-deviation increase in work relief was 132 percent. Based on the 1932-1940 mean values for work relief and direct relief spending, the 24-month changes suggest that a 5-dollar increase in work relief spending was associated with a one-dollar decrease in direct relief.

When direct relief is increased by one standard deviation, there is a decrease in work relief spending of over 4 percent in the first period and the cumulative sum of the effects over 24 months is 31 percent (Figure 6b). The cumulative increase in direct relief after a shock to itself is 129 percent. When translated into dollar-for-dollar measures, every 5.50-dollar increase in direct relief spending was associated with a one-dollar decrease in work relief.

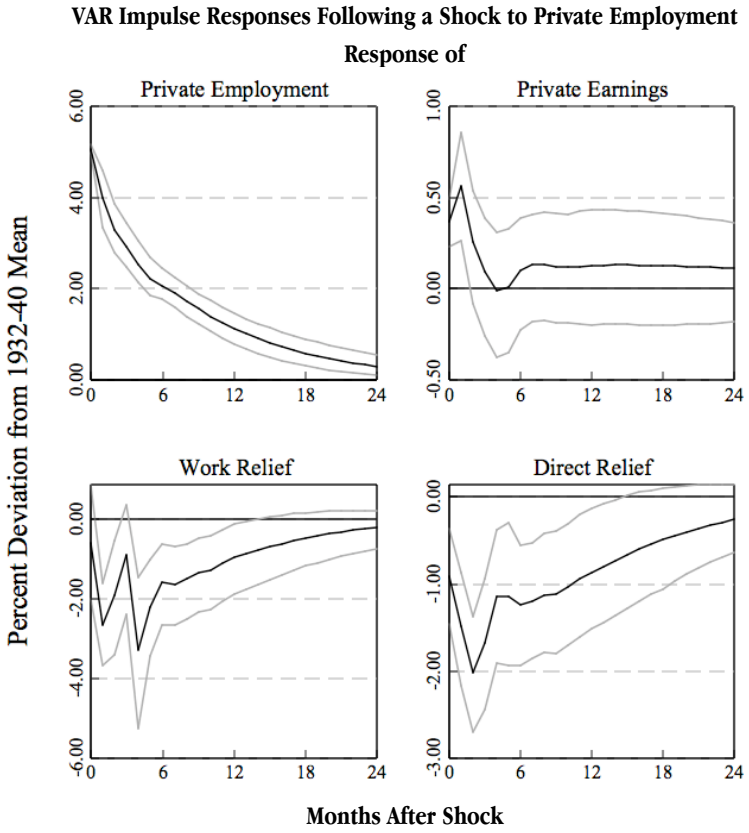
Did Relief Respond to Economic Distress?

In the extensive debate over the political economy of New Deal spending, the cross-sectional studies that focus on relief typically used measures of economic activity from earlier periods to avoid simultaneity bias. The studies show that areas that had higher unemployment and/or experienced downturns from one to six years earlier tended to receive more federal money for relief. The monthly data and panel VAR offer the first opportunity to examine how the timing of relief funds responded to economic upturns and downturns within the same city.

Under the assumption that it took a month for each of the endogenous variables at time t to respond to each other the elasticity on the first lag from the panel VAR in Table 4 shows the direct causal responses before feedback effects developed. Work relief spending responded strongly to the first lag of private employment with a statistically significant elasticity of -0.486. The fourth-period elasticity also displayed a strong elasticity of -0.462. These are the largest elasticities in the table aside from the persistence elasticities for each variable with respect to its own first lag. It appears that work relief was responsive primarily to loss of jobs and not to declines in earnings. The elasticities of work relief with respect to all the lagged earnings variables were statistically insignificant. In contrast, the direct elasticities of lagged employment changes on direct relief were small and statistically

insignificant. Meanwhile the direct relief elasticities for the first and second lag of monthly earnings were statistically significant in nearly equal and opposite in direction.

Figure 6c



To the extent that there were contemporary interactions between the private labor market and relief, we can not draw such strong conclusions from the Table 4 elasticities. After incorporating all of the feedback effects over the two year period, the impulse response function in column 1 row 2 of Figure 6c shows that a one-period positive one standard deviation shock to private employment of 5 percent of the 1932-40 mean was associated with a work relief decline each month through the two-year period. The largest decline was in month four and the declines were statistically different from zero for all but one month of the first year. The one-month shock to private employment was associated with a cumulative increase in private job months of 38 percent over the 24-month period. This same shock was associated with a peak period decrease in work relief spending of 3.3 percent in period 4 and a cumulative decrease of 27 percent.

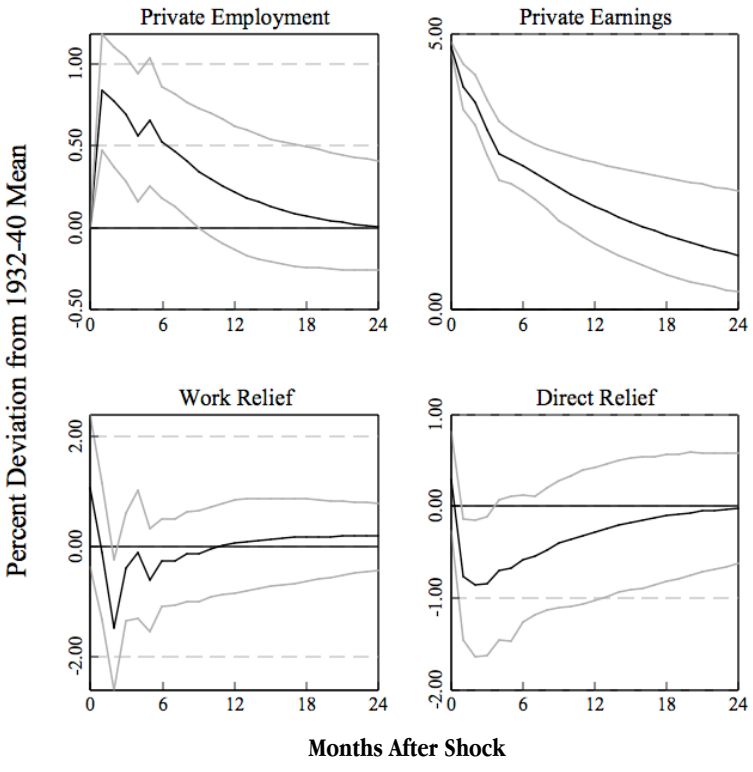
Using Cleveland as an example again, the one month shock to private employment ultimately was associated with a cumulative increase of 170,000 private job months and a decrease of 8,200 relief job months over a two-year period. This translates into a relatively small decrease of one work relief job month for every 21 private

job months added over the two year period. This ratio of relief job months eliminated for each private job month added is dramatically smaller than the ratio of 1 private job month eliminated for each 3 work relief job months added when work relief was shocked. One of the reasons is that the mean number of relief workers each month was roughly 10 percent of the number of private workers. Even after adjusting for the relative size of the workforce on relief and in the private sector, the difference in the ratios still seems quite large. One likely reason for the relatively limited response to the increase in private employment is that the average share of the labor force unemployed and not on relief still exceeded 9 percent nationwide. Relief officials, therefore, would have kept spending to extend work relief to the unemployed without work.

A private employment shock was associated with the negative direct relief changes throughout the two-year periods, as seen in the impulse response function in column 2 and row 2 of Figure 6c. The effect is statistically significant for about 14 months but the cumulative effect was a relatively small 22 percent over the period. This small effect fits with the fact that for most of the time period in our study there were a large group of "unemployables" included in the direct relief population. Increases in the employment prospects for those working in a city therefore were likely to have led to much smaller percentage changes in direct relief.

Figure 6d

VAR Impulse Responses Following a Shock to Private Earnings



Positive shocks to private earnings, like shocks to private employment, represent improvements in the local economy. Consequently, it is not surprising that like employment, increases in earnings led to subsequent decreases in relief expenditures. The effects are neither large nor precisely estimated, however. The cumulative response in work relief spending to the shock in earnings over the 24-month period is only negative 0.5 percent. In Cleveland that would represent a decrease of only 150 relief job months. The impulse response function is consistent with the idea that New Deal administrators were most sensitive to the number of people in a city with a job, not how many hours per month they worked or what their hourly wage was. The cumulative impulse response in direct relief to this shock would have been negative 7.8 percent. This small and statistically insignificant relationship further fits with the fact that for most of the time period, direct relief was targeted at "unemployables." Increases in the pay of "employables" working in a city would not necessarily have reduced the need for direct relief in that area.

Interactions of Private Employment and Earnings

Given the high levels of unemployment during the period, it seems likely that a positive shock to either private employment or monthly earnings would have occurred on the demand side and that the two labor market variables would have moved together. The small positive contemporaneous correlation of the residuals of private employment and monthly earnings in Table 4 suggest that unmeasured contemporaneous shocks caused them to move together. The impulse response functions in row 1 column 2 of Figure 6c and row 1 column 1 of Figure 6d show initial positive and statistically significant responses in both directions. A positive shock to employment has a statistically significant positive elasticity in only the first month. Given the unprecedented unemployment, it is not a surprise that employment could have risen without putting sustained upward pressure on wages. This result also lends further support to the idea that employers increased hours first, not employment. Moving in the other direction, the employment response to a shock in monthly earnings is positive and statistically significant for 8 months. A shock to monthly earnings is likely measuring an increase in average hours worked, which often preceded future increases in employment.

Was Relief Spending Timed to Match National Elections

The panel VAR approach also offers an opportunity to examine whether the Roosevelt administration may have adjusted national relief expenditures in response to the timing of elections. There was considerable criticism

of the Roosevelt administration related to the timing of increases in WPA employment. In both 1936 and 1938, the national WPA numbers showed all-time highs in WPA employment in October and November, followed by December declines. Critics also sought to make political hay of autumn increases in 1940 (Howard 1943, 586-594). The controversy was particularly heated during the 1938 election. In late summer 1938 WPA director Harry Hopkins was alleged to have told his friends at a New York racetrack that the New Deal followed the following formula for success: "We shall tax and tax, spend and spend, and elect and elect." The statement was widely quoted, as newspapers disparaged the motives of Hopkins and the Roosevelt administration. Although Hopkins claimed that he never made such a statement, Smith (2007, 175-180) offers extensive evidence that he did (see also, McJimsey 1987, 124). Nonetheless, critics of the New Deal considered it an accurate assessment of New Deal policy.

The WPA's defense of the timing was that they were striving to offset unemployment caused by seasonality, droughts, and unusual periods of unemployment around the country. WPA officials argued that comparisons of WPA employment across states did not show higher than normal WPA employment in states where elections were tight (Howard 1943, 586-594). The month-year fixed effects in the panel VAR offer an opportunity to systematically examine whether the Roosevelt administration increased work relief employment around election times, while controlling for the changes in the local economies and relief over the prior six months. The timing is assessed using the month-year fixed effects that are estimated in a regression of each relief measure on the lag structure specified in equation system 2.

Because the dependent variable in our regressions is measured as a deviation from the city's mean, the month-year fixed effect measures the average for the entire sample of how much relief spending exceeded or fell below the city mean relief in that month and year while controlling for shocks to the NIPS index and fluctuations in local employment and earnings.. In Figures 7a through 7d we examine the percentage difference in that average between the month-year fixed effect in a given month and the same month in the previous year. For example, the October observation in Figure 7a is the percentage change in the October 1934 fixed effect from the October 1933 fixed effect. The charges of electioneering focused on October and November as the key months for increases, but a case can be made that August and September jobs likely would have influenced the election, although to a lesser degree.[22] Thus, if electioneering were going on, the percentage change in the fixed effect from the same month in

[22]In the south the key election was likely the Democratic primary earlier in the year. We do not focus on that period because the only remotely southern city in the sample is Baltimore.

the previous year would be systematically positive, and also larger in those months than the rest of the year. The figures also show the difference in percentage points in the national unemployment rate from the same month in the prior year.

Figure 7a

Percent Change in Year-Month Fixed Effect in 1934 from Same Month in 1933

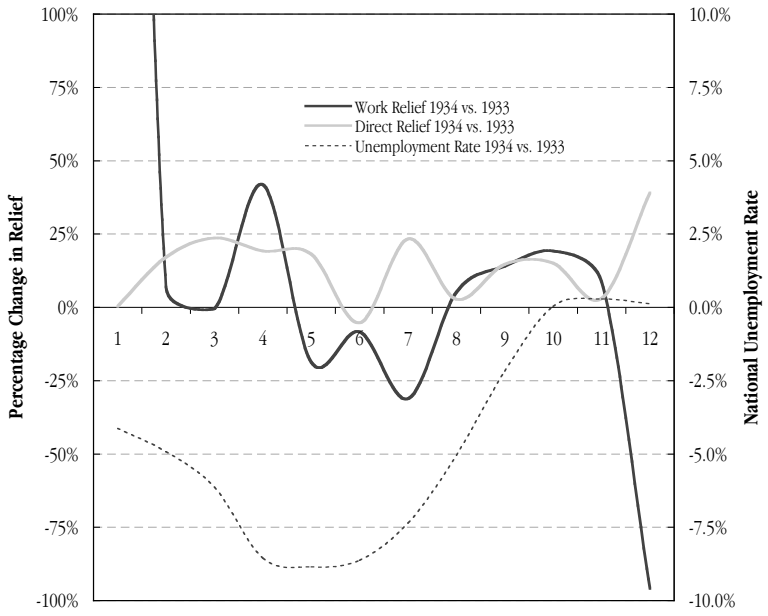


Figure 7b

Percent Change in Year-Month Fixed Effect in 1936 from Same Month in 1935

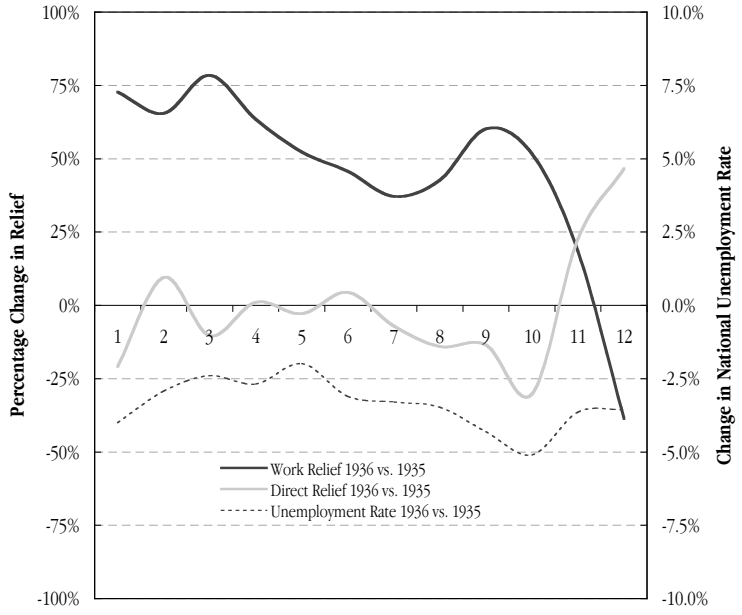


Figure 7c

Percent Change in Year-Month Fixed Effect in 1938 from Same Month in 1937

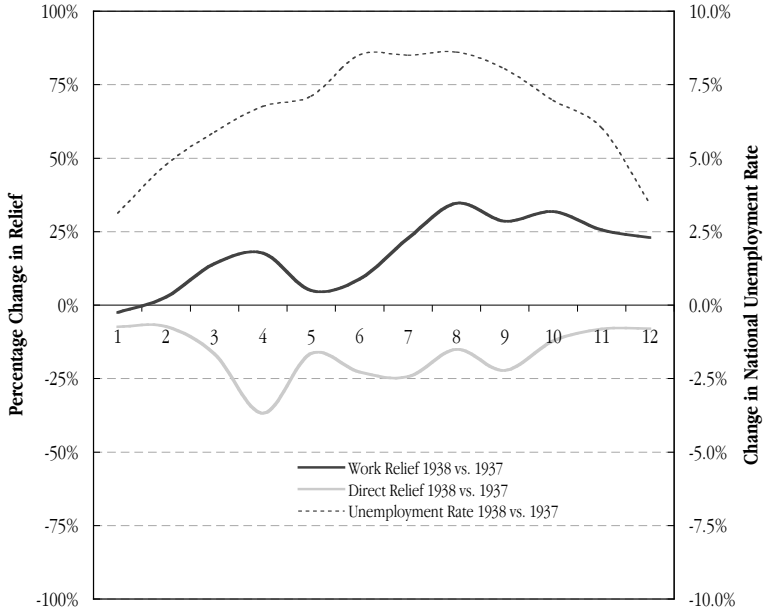
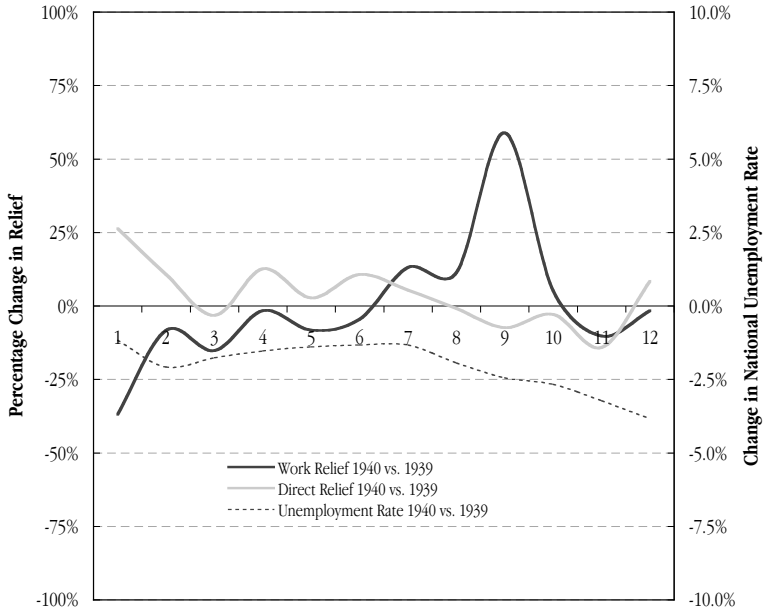


Figure 7d

Percent Change in Year-Month Fixed Effect in 1940 from Same Month in 1939



The patterns in Figures 7a through 10c for the election years of 1934, 1936, and 1938 are visually consistent with a pattern in which the Roosevelt administration spent more during August, September, October, and November of the national election year than in the prior year even after controlling for the prior six months of local labor market and relief fluctuations.

The 1934 Congressional election year was the only one in which the federal government was heavily involved in both work and direct relief. We will focus on the months of July through December because the federal government did not become heavily involved in distributing relief grants until FERA was started in June 1933. The percentage changes in the work relief month-year fixed effects from 1933 to 1934 show a rising pattern from roughly 10 percent in August to 20 percent in October. The November percentage difference was somewhat lower before the large negative percentage in December. Although the fixed effects are estimated after controlling for the prior local employment shocks, the fixed effects could have been buffeted by national shocks to unemployment. However, the Roosevelt administration could not claim that the percentage jumps were matching sharp jumps in the national unemployment rate (including relief workers as unemployed) because the national unemployment rate was 2.2 percentage points lower in August 1934 than in August 1933 and the rates in the two years for September, October, and November were virtually identical.

There is also some evidence of electioneering using direct relief funds. From July through the end of the year, the percentage difference from 1933 to 1934 in the month-year fixed effects was positive with percentage

changes in the high teens in September and October. This pattern is quite different from the direct relief patterns in the later election years, when the federal government had little control of direct relief expenditures. In the case of direct relief, it appears that the administration did not forget their constituents at Christmas as there was a sharp increase in the direct relief fixed effect growth rate from December 1933 to December 1934.

Critics of the administration had identified 1936 and 1938 as years of the most electioneering by the WPA. In both years the fixed effects in the election year were higher than in the prior non-election year. In the 1936 presidential election year, when Roosevelt's own re-election was at stake, the work relief month-year fixed effects in 1936 ranged from 80 percent higher to 45 percent higher in October. The Roosevelt administration could not effectively have claimed that they were responding to a national shock that raised unemployment because national unemployment rates (including work relief workers) were lower in every month in 1936 than in the same month in 1935. The November 1936 work relief fixed effect was only 20 percent higher than its 1935 counterpart, while the December 1936 fixed effect was 40 percent below the December 1935 one. The declines are consistent with a pattern of reducing spending once the election had ended. The one within-year anomaly for the electioneering story in 1936 is that the 1936 month fixed effects were at their peak earlier in the year with a smaller peak around September.

In the 1938 Congressional election year, when the Hopkins tax-spend-elect quotation generated such controversy, the work relief month-year fixed effects were higher from February through December of 1938 than in 1937. The differences between 1938 and 1937 were much smaller than the ones for 1936 and 1935. However, the August, September, November, and December percentage differences between 1938 and 1937 were the largest of the year, ranging from 20 to 35 percent. Although the fixed effects are estimated after controlling for the prior local employment shocks, the fixed effects could have been buffeted by national shocks. Nationwide unemployment rates were four to 8.6 percent higher in 1938 than in 1937, but the peaks happened in May, June, and July.

The presidential election year 1940 was the period least consistent with charges of using work relief for electioneering. Throughout most of the year, the work relief fixed effects for 1940 were lower than in 1939. The spectacular exception was a September fixed effect that was 60 percent higher in 1940 than in 1939.

To test the statistical significance of these visual differences, we perform a series of regression analyses with the month-year fixed effects as the dependent variable. Table 7 presents a summary of these results. First, we estimate regression equation (3) below to test if the fixed effects in election years are systematically different from

non-election years by regressing the fixed effects on a quadratic time trend, dummies for the months surrounding the CWA buildup and build down, and a dummy that equals 1 in election years. The election year coefficient in the first column of Table 7 shows that work relief was 12.31 percentage points larger in election years, and the effect is statistically significant. Using regression equation (4), we test if the fixed effects on the months of August through November are systematically larger than the fixed effects for other months during the election year. The coefficient on the interaction between the August through November and election year dummies shows that work relief was on average 16.03 percentage points higher in August through November than during the remaining months during the election years. The effect is statistically significant at the 5 percent level. The fixed effects for work relief for August through November in election years are on average 35.84 percentage points higher than August through November in non-election years (16.03 + 7.00 – (-11.81)).

$$(3)\ FE = \alpha + \beta^{t} t + \beta^{t^2} t^2 + \sum_{t=1}^{t} \beta CWA^{t} + \beta^{e} elect + e$$

$$(4)\ FE = \alpha + \beta^{t} t + \beta^{t^2} t^2 + \sum_{t=1}^{t} \beta CWA^{t} + \beta^{e} elect + \beta^{an} an + \beta^{ane} an * elect + e$$

The comparisons based on the fixed effects capture nation-wide changes around election years in relief spending. The Roosevelt administration may well have targeted specific areas for special treatment. Nearly all of the cross-sectional studies of the geography of New Deal spending find that measures of swing voting were key influences on the distribution of funds across areas. As an additional step, we ran the system where we included measures of swing voting interacted with a dummy for August, September, October, and November of the national election years. We tried three measures of swing voting – the standard deviation of the presidential Democratic vote from 1896 to 1928, the same standard deviation for 1896 to 1932, and the difference between the percent voting for Roosevelt in 1932 and the mean percentage voting for Democrats between 1896 and 1928. All three measures led to the same qualitative results, so we report only the coefficients on the interactions with the standard deviation measure from 1896 to 1928.

Table 7

Tests for Systematic Differences in Work and Direct Relief

	Specification A	Specification B
time	3.66	3.72
	(0.00)	(0.00)
time-squared	-0.03	-0.03
	(0.00)	(0.00)
CWA Month 1	11.57	20.53
	(0.55)	(0.30)
CWA Month 2	31.54	40.45
	(0.11)	(0.04)
CWA Month 3	122.38	119.44
	(0.00)	(0.00)
CWA Month 4	468.97	471.30
	(0.00)	(0.00)
CWA Month 5	-36.08	-33.80
	(0.06)	(0.08)
CWA Month 6	-44.83	-42.59
	(0.02)	(0.03)
Election Year	12.31	7.00
	(0.00)	(0.15)
Aug.-Nov.		-11.81
		(0.05)
Aug.-Nov.*Election Year		16.03
		(0.05)
Aug.-Nov. 1936		
Aug.-Nov. 1938		
Aug.-Nov. 1940		
Constant	-100.66	-98.67
	(0.00)	(0.00)
N	102	102
R-Squared	0.91	0.91

Note. (p-values of t-tests in parentheses below coefficients.)

The results in Table 8 show statistically significant effects of the swing interaction with the key election months in the election years 1934, 1936, and 1938. The 1934 and 1938 election/swing coefficients show that the work relief fixed effects were higher in more swing voting states in the months of August, September, October, and November. The swing voting interaction coefficient for 1936 is unexpectedly negative.

Table 8

Swing Vote Effect on Pre-Election Fixed Effects

		1934	1936	1938	1940
Work Relief	Beta	3.08	-2.56	2.78	0.47
	p-value	(0.04)	(0.07)	(0.07)	(0.92)
Direct Relief	Beta	-0.47	0.29	-0.75	0.9
	p-value	(0.42)	(0.59)	(0.20)	(0.60)

V. Summary

Although more than six decades have passed since the end of the Great Depression, economists are only beginning to develop a good understanding of the interactions of labor markets and relief programs during the 1930s. This paper takes advantage of monthly data for 44 cities to portray the dynamic reduced-form relationships between private labor market activity and relief spending during the 1930s. Since a month is a relatively short time period and it took time for decision makers to learn of changes in relief and labor markets and then to respond, it might have been the case that the responses to shocks did not occur for at least a month. If that were true, our panel VAR analysis estimates the structural relationships between variables and documents how a shock to one variable caused changes throughout the system. If, in fact, there were interactions between the endogenous labor market and relief variables at time t, the coefficients in the panel VAR are reduced form coefficients that embed the structural relationships. The panel VAR then can show how a shock in one period in one variable led to a variety of interactions and feedbacks over time.

The analysis shows that there was strong persistence from month to month such that over 75 percent of the deviation in private employment in the previous month was sustained into the current month. Private monthly earnings and direct relief showed even stronger persistence, while the persistence for work relief deviations was much smaller at about 0.218. The very strong persistence of direct relief is consistent with the emphasis on the use of direct relief to provide aid to a large group of people who were considered "unemployable." The

substantially lower persistence in work relief suggests more flexibility than might have been anticipated based on the lumpy, project-oriented nature of FERA and WPA work relief spending.

By focusing primarily on building projects traditionally under the purview of local governments, the Roosevelt administration tried to ensure that work relief did not lead to reductions in private employment. Work to date offers varying estimates of their success in achieving this goal, ranging from no effect of work relief on private employment to the loss of one private job for every two work relief jobs created. The impulse response functions show that after all feedback effects are taken into account, a positive work relief shock in one month would have contributed over a two-year period to a proportional decline in private employment of one private job month for every 3.5 relief job months. In addition, increases in work relief were associated with increases in private monthly earnings.

While the decline in private employment following a positive shock to work relief spending is not a pure measure of crowding out, our estimates do fit with the WPA worker's advice about private versus relief employment when he cautioned "…better not take too much of a chance. Know a good thing when you got it."[23] The impulse response functions also help explain a wide range of complaints filed by employers against the WPA despite the WPA's administrative efforts to get workers to accept jobs. Following an expansion in work relief, employers complained that they had trouble hiring workers, had to pay higher wages, and thus they could not hire as many workers. The WPA officials documented that they were actively encouraging workers to take private jobs and that they could find few cases where workers had turned down private jobs. The impulse response functions show that employers were seeing patterns that existed in the economy that may have been missed by the WPA investigations into specific allegations.

The dynamics we document also contribute to the large literature on the extent to which the distribution of New Deal funds reflected Roosevelt's declared goal of promoting relief and recovery. While nearly all of the prior work has focused on cross-sectional analysis, we show the short run dynamics of relief spending within cities over time. Month to month adjustments in work relief served to offset drops in private employment but were not responsive to changes in monthly earnings, suggesting an emphasis on replacing jobs and not income. After all feedback effects are considered, it took a relatively large shock to private employment to be associated with the elimination of one relief job. Partly this was due to the fact that the numbers on WPA projects were small relative to the number of private workers.

[23] Quoted in E. W. Bakke, *The Unemployed Worker* (New Haven, 1940), pp. 421-22 and cited in Margo 1991 p.340.

Finally, our estimates allow us to examine the long contentious claim that the Roosevelt administration was timing rises in work relief spending to peak during the national election seasons. Unlike many cross sectional or national studies of this issue, our panel data allows us to control for both unobserved city characteristics as well as seasonal and local economic shocks by examined the month-year fixed effects from the VAR panel to see whether charges of electioneering hold true after controlling for the prior six months of changes in the local economies. Graphical comparisons of the month fixed effects between election years and non-election years suggest that the Roosevelt administration raised work-relief spending in August, September, October, and November during the election years of 1934, 1936, and 1938 relative to the prior non-election years. On average over the period, after controlling for local changes in wages and employment over a six-month period, the Roosevelt administration spent substantially more during election years than in non-election years, and spent still more during the key electoral months of August through November of election years. Thus, the timing of spending is consistent with the view that the Roosevelt administration sought to use the timing of the spending to influence elections.

Appendix A

Variable Construction

Both data samples consist of observations of various lengths on 44 cities between 1932 and 1940. The sample constituted these specific 44 cities[24] because relief, employment, demographic, and economic control variables were available. Relief measures come from the Records of the Work Progress Administration found on microfiche at the National Archives in Washington and the Federal Emergency Relief Administration Annual Report. This data is augmented with material from the Final Statistical Report of the Federal Emergency Relief Agency and the Federal Works Agency Works Progress Administration's Report on Progress of the WPA Program. The required work and direct relief measures are combinations of spending and cases from 4 different programs during the decade: the Civil Works Administration (CWA), the Federal Emergency Relief Administration (FERA), the Works Progress Administration (WPA), as well as miscellaneous state and local programs reported in the Final Statistical Report of the Federal Emergency Relief Agency. Because these programs operated at different intervals during the decade care was taken to accurately compile our annual and monthly measures of relief. Tables A.1 & A.2 illustrate the years in our sample, which different programs operated.

Table A.1

Work Relief Timeline

1932	1933	1934	1935	1936	1937	1938	1939	1940
	June: FERA begins work relief programs	FERA	December: FERA closes or transfers all work relief programs					
	November: CWA begins operations	March: all CWA operations are closed						
			June: WPA begins operations	WPA	WPA	WPA	WPA	WPA

Table A.2

Direct Relief Timeline

1932	1933	1934	1935	1936	1937	1938	1939	1940
State & Local	State & Local	State & Local	State & Local	State & Local	State & Local	State & Local	State & Local	State & Local
	June: FERA begins providing direct relief	FERA	FERA	June: FERA begins closing programs	FERA	March: last FERA programs closed		

$G is general relief spending from the Final Statistical Report of the FERA (FSRFERA). At different points in time it includes FERA direct relief, FERA work relief, state relief, and local relief. *Pop*. represents population

[24] San Francisco, Los Angeles, Canton-OH, Cleveland, Cincinnati, Dayton, Columbus, Toledo, Youngstown, Akron, Syracuse, Rochester, Albany, Buffalo, New York City, Utica, Detroit, Baltimore, Lynn-MA, Lowell, Boston, Springfield-MA, Brockton, Cambridge, Fall River, Lawrence, Worchester-MA, Minneapolis, St. Paul, Duluth, Chicago, Springfield-IL, Kansas City-KS, Topeka, Wichita, Milwaukee, Kenosha, Racine, Philadelphia, Pittsburg, St. Louis, Jersey City, Newark, Trenton

computed by a simple linear interpolation between 1930 & 1940 census numbers. *$GW* is FERA work relief spending from the FSRFERA.

The monthly relief figures were converted into per capita estimates by using a monthly linear time trend for population between the populations in March 1930 and March 1940 censuses.

I. Monthly Data

a. Direct Relief

i. January 1932 – December 1932: This is computed by dividing the 1932 annual observation of direct relief payments from state and local sources in the city into 12 payments that follow a linear time trend dividing an annual observation of direct relief in city i by 12 and then weighting that follow a linear trend between January 1932 and the January 1933 figure. $Trend\left[\frac{\$G_{iy}}{12}\right]$

ii. January 1933 – June 1933: This is computed by removing the known monthly observations on direct relief over the second half of the year from an annual observation, then evenly dividing the half year spending over 6 months. $\frac{\$G_{iy} - \sum_{m=1}^{6} \$G_{im}}{6}$

iii. June 1933 – June 1935: The value is computed by taking monthly observations on total relief spending for the city and calculating city direct relief spending by multiplying city total relief by the direct relief share of total relief for the state in that month. $\frac{\$G_{im}(\$G_{sm} - \$GW_{sm})}{\$G_{sm}}$

iv. July 1935 – March 1937: This is simply total direct relief. $\$G_{im}$

v. April 1937 – December 1940: This is computed by multiplying an annual observation on direct relief in the city by that month's share of annual spending in the state where the city was located. $\frac{\$G_{iy} \cdot \$G_{sm}}{\$G_{sy}}$

b. Work Relief Spending per capita.

i. June 1933 – June 1935: There were two forms of work relief in this period, FERA for the entire period and CWA for the months from November 1933 through March 1934. FERA monthly work relief spending in city i is calculated by multiplying that month's total FERA spending by the share of total FERA spending devoted to work relief in that month at the state level. in the state where the city was located. To obtain CWA spending for each month, the city total CWA spending for the November through March period was allocated to each month based on national information on that month's share of total CWA spending while it existed. $\frac{\$G_{im} \cdot \$GW_{sm}}{\$G_{sm}} + \frac{\$CWA_{iy} \cdot \$CWA_{nm}}{\$CWA_{ny}}$

ii. June 1935 – December 1935 [25]: Work relief included both FERA spending and WPA spending. The monthly FERA total was calculated as the city's month total relief spending multiplied by the share of relief spending devoted to work relief at the state level for that month. The WPA spending for the city in each month was reported directly. $\frac{\$G_{i\,m} \cdot \$GW_{s\,m}}{\$G_{s\,m}} + \$WPA_{i\,m}$

iii. January 1936 – End: Monthly WPA work relief spending was reported directly. $\$WPA_{i\,m}$

c. National Industrial Production Shock (NIPS) Index

i. Entire time period: Using information from the 1929 manufacturing census for each city, the share of value added in each of 11 industries out of the total value added in that city was calculated. The 11 industries included Electrical Machinery, Printing, Boots and Shoes, Iron and Steel, Leather tanning, Lumber and Millwork, Meat Packing, Motor vehicles, Paper and Pulp, Wool Textiles, and an "all else" category. Monthly industrial production figures at the national level were collected for each of the eleven industries. The NIPS index was created by calculated a weighted industrial production index for each city using monthly national industrial production indices weighted by the 1929 industry shares in that city. The NIPS index for a given city i in month t is $NIPS_{i\,t} = \sum_{n=1}^{11} \frac{IndustryValueAdded_{n\,i1929}}{TotalValueAdded_{i\,1929}} \cdot IndustryIP_{n\,t}$.

These relief data are matched with monthly employment and payroll measures. The raw employment data vary by city. Some states observe manufacturing employment while others record non-manufacturing or both. Some states record raw numbers while others record indices. In order to make this data comparable, all raw employment and payroll data are converted into chain indices with January 1932 as the base year.

Relief and employment data are next matched with population and price data. Population measures come from the U.S. Bureau of Census' 1930 and 1940 Census. Monthly population figures are based on a straight-line interpolation between the two dates. Price adjustments are made using the city level Consumer Price Index for all items from the United States Bureau of Labor Services. CPI observations are bi-annual between 1932 thru 1934 and quarterly thru 1940. Monthly observations are based on a straight-line interpolation. A select number of cities lacked price data in which case the closest neighbor's data was used.

[25] $WPA is WPA spending from National Archives in Washington.

Appendix B

Robustness of VAR Results

Table A.3

VAR Coefficient Results—3 Lags no Time Effects

	Dependent Variable							
	Employment		Earnings		Work Relief		Direct Relief	
Independent Variable	beta	t-stat	beta	t-stat	beta	t-stat	beta	t-stat
Employment Lag 1	0.777	9.640	0.018	0.533	-0.498	-2.886	0.038	0.729
Employment Lag 2	0.060	0.892	-0.057	-1.744	0.218	0.759	-0.097	-1.388
Employment Lag 3	0.045	1.369	0.024	0.981	0.069	0.283	0.092	1.583
Earnings Lag 1	0.146	3.538	0.846	15.721	-0.230	-0.879	-0.102	-1.573
Earnings Lag 2	-0.097	-2.100	0.084	1.743	-0.237	-0.628	0.131	1.428
Earnings Lag 3	-0.047	-1.449	-0.003	-0.082	0.503	1.937	-0.035	-0.468
Work Relief Lag 1	0.002	1.752	0.004	2.567	0.383	5.196	-0.020	-3.132
Work Relief Lag 2	-0.001	-0.804	0.005	1.865	0.127	3.219	0.021	4.482
Work Relief Lag 3	-0.001	-0.318	-0.002	-0.907	0.138	4.546	-0.002	-0.390
Direct Relief Lag 1	-0.003	-0.836	0.006	1.635	-0.238	-3.457	1.007	17.699
Direct Relief Lag 2	0.008	1.393	0.002	0.264	0.286	3.122	-0.198	-2.662
Direct Relief Lag 3	-0.001	-0.335	-0.003	-0.715	-0.016	-0.284	0.062	1.135
IP Lag 1	0.220	7.537	0.165	6.280	0.708	3.856	-0.454	-5.609
IP Lag 2	-0.124	-2.827	-0.236	-6.257	-2.050	-4.398	0.295	2.231
IP Lag 3	-0.049	-1.847	0.088	3.190	1.806	5.778	0.041	0.536
observations	3878		3878		3878		3878	

Table A.4

VAR Coefficient Results—3 Lags with Time Effects

	Dependent Variable							
	Employment		Earnings		Work Relief		Direct Relief	
Independent Variable	beta	t-stat	beta	t-stat	beta	t-stat	beta	t-stat
Employment Lag 1	0.763	9.615	0.049	1.365	-0.523	-4.043	-0.043	-0.858
Employment Lag 2	0.042	0.668	-0.072	-2.292	0.156	0.884	-0.067	-1.059
Employment Lag 3	0.064	1.965	0.027	1.160	-0.001	-0.003	0.091	1.748
Earnings Lag 1	0.170	3.937	0.859	16.793	-0.053	-0.328	-0.173	-2.962
Earnings Lag 2	-0.116	-2.511	0.058	1.195	-0.173	-0.704	0.157	1.849
Earnings Lag 3	-0.033	-1.049	0.003	0.079	0.278	1.644	0.004	0.062
Work Relief Lag 1	-0.001	-0.856	0.003	1.734	0.236	4.811	-0.018	-2.289
Work Relief Lag 2	-0.003	-1.784	0.004	1.254	0.154	4.696	0.008	1.411
Work Relief Lag 3	-0.001	-0.532	-0.003	-1.010	0.118	4.576	-0.002	-0.295
Direct Relief Lag 1	-0.007	-1.898	0.002	0.626	-0.199	-3.775	0.915	15.899
Direct Relief Lag 2	0.010	1.651	0.001	0.115	0.118	1.756	-0.158	-2.179
Direct Relief Lag 3	-0.001	-0.257	0.000	0.110	-0.015	-0.349	0.095	1.807
IP Lag 1	0.143	2.276	0.032	0.560	-0.361	-1.452	-0.630	-4.034
IP Lag 2	-0.196	-2.659	-0.188	-2.713	0.330	0.608	0.375	1.753
IP Lag 3	0.044	0.784	0.126	2.092	-0.325	-1.075	-0.202	-1.578
observations	3878		3878		3878		3878	

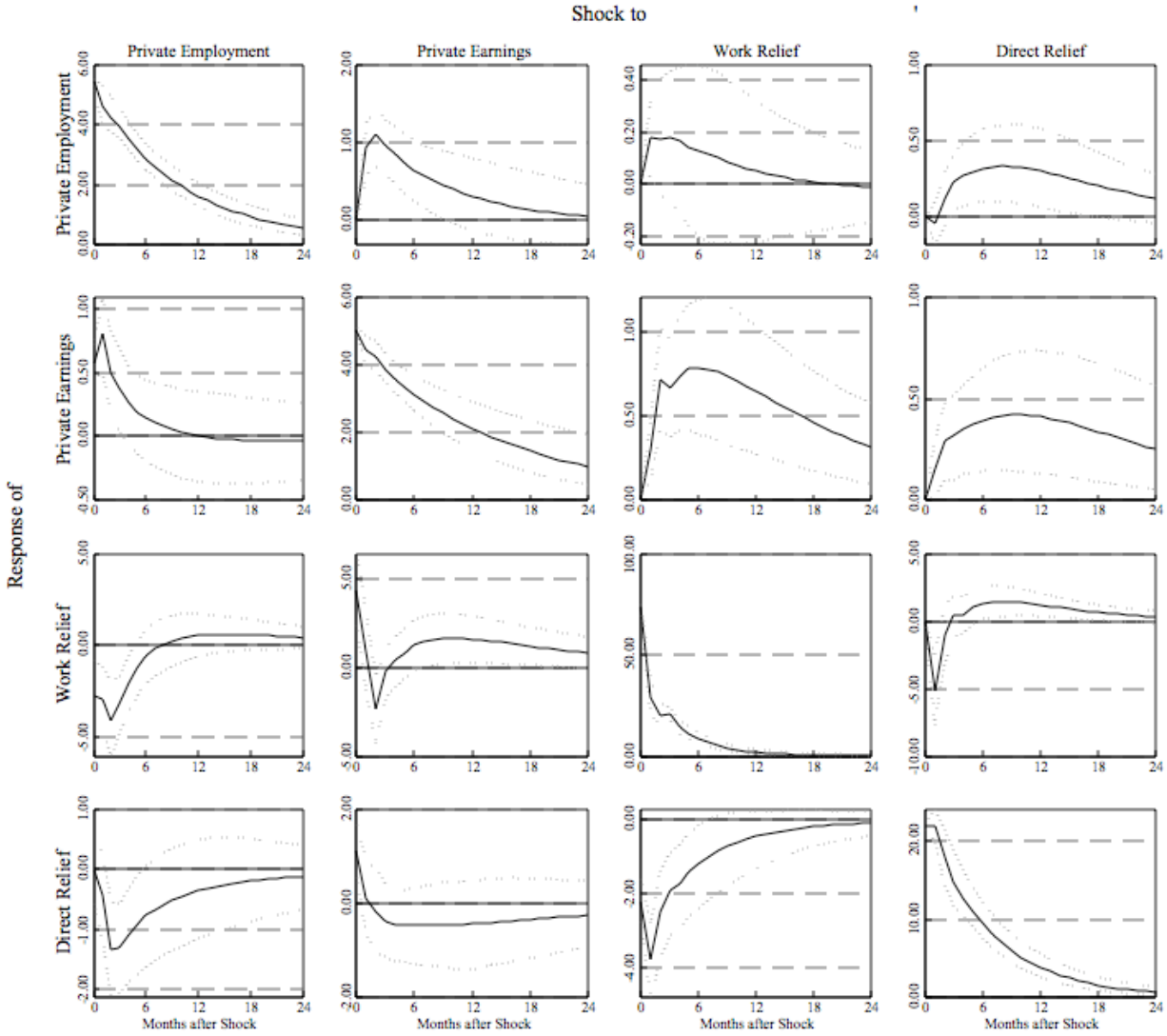
Table A.5

VAR Coefficient Results—6 Lags no Time Effects

	Dependent Variable							
	Employment		Earnings		Work Relief		Direct Relief	
Independent Variable	beta	t-stat	beta	t-stat	beta	t-stat	beta	t-stat
Employment Lag 1	0.769	9.245	0.024	0.685	-0.433	-2.429	0.063	1.158
Employment Lag 2	0.053	0.799	-0.058	-1.779	0.189	0.621	-0.129	-1.776
Employment Lag 3	0.043	1.241	0.005	0.143	0.854	2.903	0.147	1.805
Employment Lag 4	0.012	0.462	-0.007	-0.318	-0.910	-1.807	0.024	0.333
Employment Lag 5	0.009	0.312	0.019	0.660	0.614	1.247	-0.014	-0.206
Employment Lag 6	-0.005	-0.230	0.008	0.353	-0.781	-2.244	-0.092	-1.601
Earnings Lag 1	0.151	3.565	0.841	14.969	-0.184	-0.683	-0.092	-1.363
Earnings Lag 2	-0.098	-2.135	0.089	1.819	-0.380	-0.956	0.111	1.156
Earnings Lag 3	-0.035	-0.974	-0.050	-1.393	0.640	2.056	-0.035	-0.373
Earnings Lag 4	-0.005	-0.177	0.001	0.034	-0.442	-1.354	-0.029	-0.342
Earnings Lag 5	0.025	0.773	0.058	1.222	0.545	1.588	-0.007	-0.079
Earnings Lag 6	-0.040	-1.772	0.003	0.094	-0.192	-0.786	0.060	0.843
Work Relief Lag 1	0.002	1.713	0.004	2.623	0.365	5.054	-0.024	-3.326
Work Relief Lag 2	-0.001	-0.821	0.005	1.748	0.105	3.017	0.019	4.055
Work Relief Lag 3	-0.001	-0.520	-0.003	-1.237	0.112	4.509	-0.006	-0.979
Work Relief Lag 4	0.003	1.732	0.004	1.590	0.031	1.575	0.010	1.252
Work Relief Lag 5	-0.004	-2.240	0.000	-0.287	0.028	1.986	-0.002	-0.364
Work Relief Lag 6	0.000	-0.394	-0.001	-0.377	0.022	1.946	-0.001	-0.100
Direct Relief Lag 1	-0.004	-1.084	0.006	1.641	-0.258	-3.690	1.002	17.572
Direct Relief Lag 2	0.010	1.643	0.002	0.222	0.262	2.807	-0.205	-2.764
Direct Relief Lag 3	-0.015	-2.008	-0.010	-1.506	-0.084	-1.102	0.065	0.966
Direct Relief Lag 4	0.003	0.389	0.013	2.036	0.114	1.810	0.012	0.276
Direct Relief Lag 5	0.002	0.294	-0.006	-1.204	-0.058	-0.953	-0.033	-1.155
Direct Relief Lag 6	0.009	2.549	0.001	0.295	0.076	2.010	0.027	1.569
IP Lag 1	0.216	6.861	0.147	5.548	0.495	2.601	-0.537	-6.398
IP Lag 2	-0.096	-1.801	-0.192	-4.030	-1.570	-3.372	0.541	3.612
IP Lag 3	-0.098	-1.701	0.029	0.462	0.478	1.274	-0.456	-2.674
IP Lag 4	0.045	0.804	0.085	1.653	1.238	2.213	0.509	2.677
IP Lag 5	-0.011	-0.216	-0.074	-1.801	-1.744	-2.800	-0.357	-1.988
IP Lag 6	-0.009	-0.339	0.014	0.619	1.640	4.183	0.190	2.026
observations	3746		3746		3746		3746	

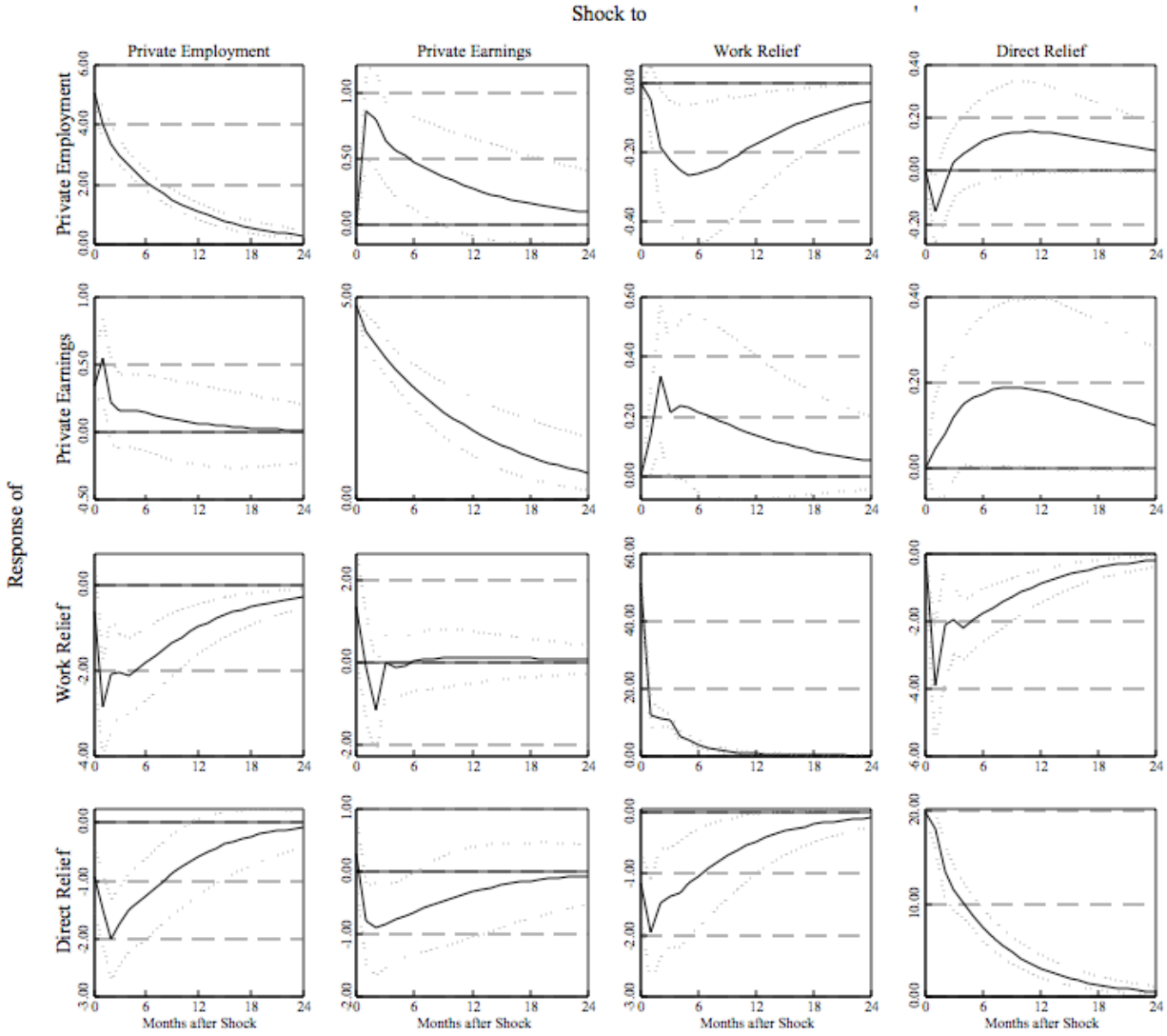
Figures A.1

VAR Impulse Responses—3 Lags with no Time Effects



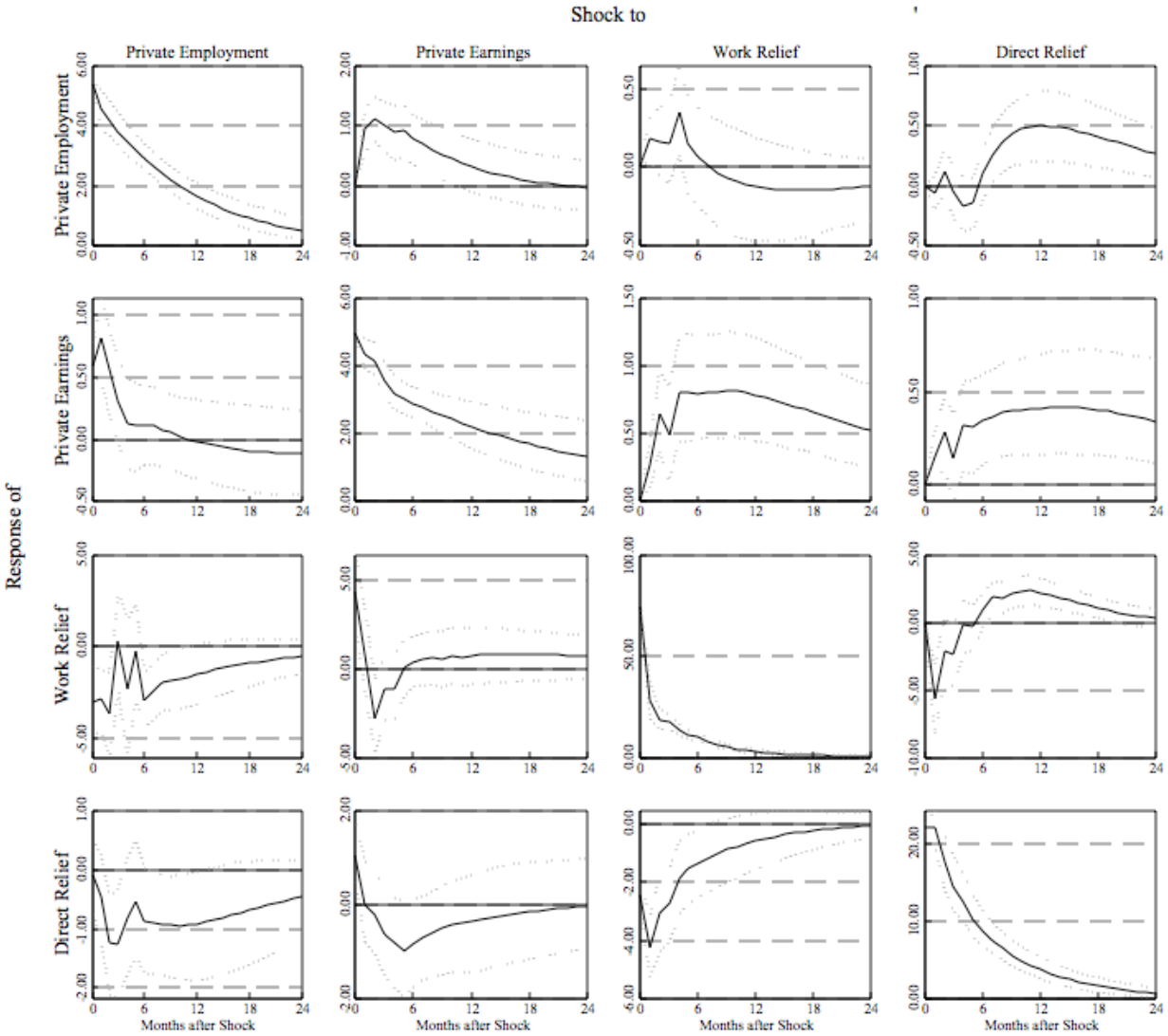
Figures A.2

VAR Impulse Responses—3 Lags with Time Effects



Figures A.3

VAR Impulse Responses—6 Lags with no Time Effects



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