New Deal Work Relief and Private Wages

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Abstract

The Great Depression and the New Deal are together one of the most significant economic events of the 20th century. Recent research has questioned the New Deal's role in the recovery leading up to World War II. Using a recently compiled data set that describes monthly relief spending in U.S. cities along with business indicators from this period, this paper examines the relationship between relief spending and the private labor market in the framework of a search model. We find evidence that relief spending increased the private wage, but that this relationship was complex. Work relief's effect on private wages depended not simply on the magnitude of spending in a particular city, but on the volatility of the private labor market, the likelihood a job seeker might be able to find work relief, and how much a potential relief job paid.

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I. Introduction

The Great Depression remains the most serious economic disruption in the history of the United States. During the worst days of the Depression, the unemployment rate peaked at over 25 percent (Darby 1976). Upon Roosevelt's resounding electoral victory in 1932, the new administration instituted hundreds of reforms and programs known in general as the New Deal. As part of this New Deal, billions of dollars were spent throughout the 1930s on income assistance and work programs. Economists and historians have studied this relief in the past, however, disagreement remains over the impact New Deal spending had on private employment and wages.

Two obstacles arise when investigating the relationship between private employment and New Deal relief spending. The first is the fundamental endogeneity between relief in an area and the number of people employed in that same place. Relief spending may have affected the private labor market in a city or county, but no doubt the level of employment (or unemployment) influenced how much relief was spent in that same area. The second issue is one of measurement. Most past studies of employment and relief have used unemployment figures; however, relief workers in programs such as the Works Progress Administration and its predecessor the Civilian Works Administration were counted as unemployed by the Bureau of Labor Statistics (Darby 1976). Using these data can mask or confuse the true nature of the private labor market and how it was affected by relief spending.

Answering this question is not simply important for historical curiosity. How a government should best respond to large and persistent economic downturns remains an important policy issue not only for the United States but also for developing nations and many European countries that have relatively large and persistent unemployment. Few economists would argue that beyond a limited set of public goods the private sector is usually best left to allocate resources and employment. However, when an economy is as depressed as the United States' was during the 1930s one might expect that large-scale employment programs could transfer people from unemployment to employment without some of the detrimental crowding out effects that might occur when an economy is near full employment. Despite this intuition past work by Wallis & Benjamin (1989) found some evidence that New Deal relief did reduce private employment and may consequently have slowed the recovery of the private sector. Meanwhile other work by Wallis & Benjamin (1981) and Fleck (1999) found no relationship between private employment and relief. Determining the true relationship can be important for not only understanding history but also understanding how governments should react to future economic crisis.

This paper utilizes new annual and monthly panel data sets of private employment and relief spending for over 40 cities to re-examine the relationship between the labor market and federal relief programs. The nature of the data will allow us to address the problems of endogeneity and unemployment measures that have hampered previous work.

We proceed by looking at patterns of employment, earnings and relief during the Great Depression and what insight previous research has shed on the relationship between the two. We then examine three models, a simple supply and demand model, a wage distribution model, and a job search model, to determine what each predicts about statistical and or theoretical economic relationships between relief & private earnings. Finally, we will use our panel data to examine the predictions of these 3 models and show that there was a significant and positive relationship between New Deal Relief and private earnings.

The Depression and New Deal

The 1930s recorded the highest level of unemployment in United States history. In less than 2 years the official unemployment rate increased 5 fold from 3.2 percent in 1929 to 15.9 percent in 1931. The rate peaked at nearly one quarter of the work force in 1933, remained above 20 percent through 1935 and above 14 percent the last half (Darby 1976). In response to this massive unemployment as well as other economic and financial panics, the relationship between the federal government and social relief changed dramatically. The election of Roosevelt in 1932 and his New Deal resulted in a fundamental shift in relief from the local to federal level. Large federally funded cash relief programs were begun as well as federal work relief programs.

Several different agencies played a role in relief during the Depression. The Federal Emergency Relief Administration (FERA) was the principal agency devoted to providing relief early in the New Deal. FERA provided direct relief and work relief to the unemployed. Direct relief refers to money spent based on strictly on need with no other requirement. Work relief was also granted based on need, but required an exchange of labor in order to receive assistance. In addition to FERA, the Civil Works Administration (CWA) operated between November 1933 and March 1934 to provide federal work relief. FERA work relief programs were replaced in 1935 by the Works Progress Administration (WPA), which became the major provider of work relief. The Civilian Conservation Corps (CCC) and National Youth Administration (NYA) also provided work relief during the second half of the '30s, but operated on a much smaller scale than the WPA and had a much different focus (Fleck 1999, Wallis 1989, Fishback, Haines, & Kantor 2002).

Given the massive unemployment that persisted throughout the decade, the labor market effects of relief spending, particularly work relief, might seem obvious. While measured unemployment remained above 14 percent throughout the decade, between one-fourth to one-third of workers classified as unemployed held relief jobs once the large-scale New Deal work programs were established (Margo 1988). The WPA put a great many people to work, over 3.1 million in December of 1938 (FWAWPA 1940), yet there remained a large pool of available labor. Darby (1976) adjusts unemployment figures to remove those who were employed by the WPA or other federal programs. He finds that even during the peak of WPA employment in 1938 an unemployment rate was still over 12.5 percent. The lowest adjusted unemployment rate between 1932 and 1940 was 9.1 percent in 1937.

Table 1

One might expect that with such a large pool of available workers there would be no competition between private and federal work programs for labor. Supporting this hypothesis is the fact that work relief generally paid less than the existing private wage. Sundstrom (1992) finds that for most workers "the WPA paid substantially less than what they could have expected to make in the regular sector," concluding that few relief workers would have turned down private sector job offers. This suggests that WPA work relief would not have caused significant crowding-out effects in the private labor market.

Crowding-out in this paper refers to a reduction in private employment caused when competition for labor between work relief projects and private employers bids up wages. This is meant to distinguish from any possible substitution effects caused by federal work projects. The WPA may have undertaken projects that were otherwise being conducted by private employers. For example, the WPA built a number of municipal buildings that may have otherwise been funded by the government but built by private contractors. This effect we call the "substitution effect". The size of this substitution effect is unclear. It was WPA policy not to engage in any activity that directly competed with private enterprise (FWAWPA 1940). With respect to normal government projects, someone can make the case that many would have been built better and/or more efficiently had the government contracted with private firms for the building. However, the federal government likely would not have provided the funds for these projects in the

absence of the Great Depression. A primary goal of the work relief projects was to provide incomes for the unemployed, and administrators argued that most of the unemployed preferred work relief to direct relief. Thus, the "leaf-raking" jobs and more worthy projects might not have led to a large substitution effect.

This paper will not attempt to directly connect changes in employment with relief. No doubt there was some substitution effect, however, who manages a project is less important for the macro-economy than if government participation in the labor force hampered private employment in activities unrelated to work relief projects. Consequently, the bulk of the paper is devoted to determining if government participation in the labor force crowded out labor from the private market by increasing private wages and thereby reduced the ability of private employers to expand private employment.

Previous Empirical Findings

Deducing the relationship between relief spending and the private labor market is difficult for a number of reasons. Private employment fluctuated dramatically between 1932 and 1940. These changes may have been the result of macroeconomic forces and/or federal relief programs. Fishback, Horrace, and Kantor (2001) find that greater relief spending at the county level increased economic activity as measured by retail sales in a county, suggesting an increase in demand at the local level occurred with greater relief spending. This stimulus may have served to increase, not decrease, private employment. Wallis and Benjamin (1981) develop a model that enables them to estimate the effects of direct relief programs on private employment. Analysis of cross-sectional city level data on FERA projects over 1934/35 reject the hypothesis that relief programs reduced private employment. They also find no effect on private wages but instead conclude, "it would appear that relief served only to redistribute wealth towards people who would have otherwise been unemployed." Robert Fleck (1999) studied a cross section of county level unemployment statistics in 1937 and 1940 and found, at the margin, relief spending significantly affected official county unemployment statistics. His results suggest that an additional relief job in a county would have increased the official measure of unemployment by nearly one worker. However, he cautions that the source of this change is ambiguous. Previously discouraged or out of the labor force workers are

immediately included as unemployed when they obtain a relief job. In order to qualify for such work, one must also be registered as unemployed, which would further increase reported unemployment in an area with relief projects. Fleck argues that an increase of less than or approximately one suggests that private employment was not reduced. Wallis and Benjamin (1989) reexamine the issue using an annual panel of state data and conclude that private employment was reduced. Their results suggest that an additional relief job reduced private employment by a half a job, yet the source of this decrease is unknown. Was it a substitution effect caused by WPA projects taking on some previously privately managed work or was the decrease crowding out caused by increases in private wages?

Data Trends in Employment & Relief

The lack of detailed data on private employment and relief has hampered previous work in this area. This paper uses two newly compiled data sets of monthly and annual private employment, private payroll, New Deal relief spending, building construction, and price index statistics on 44 cities from 13 states across the country[1]. The data span 10 years from 1931 through 1940. While the data offer new opportunities for regression analysis, they also provide an interesting look at the nature of the depression and employment across the U.S.

The depression hit the entire country hard but not equally. Employment fluctuated to various degrees over the entire sample of cities during the 1930s. The most volatile city in our sample had a standard deviation in employment of over 23 percent of its mean between 1932 and 1939, while another city's standard deviation was under 7 percent during the same period. The standard deviation of employment over the entire sample of 44 cities was approximately 16 percent of the city's mean.

Figures 1 and 2 illustrate the path of employment and average monthly earnings across the decade. Each dot represents a city's employment or earnings index in a given month. The employment indices are a construction of chain-weighted employment levels with January 1935 as the base month. The earnings indices are constructed by dividing a city level chain-weighted payroll index by the employment index with January 1935 as the base month. The dark line traces the median of the 44 cities in any given month. One

[1] Together these 44 cities represent over 25 percent of the U.S. population.

first notices the vast difference in employment levels and private earnings across cities. The median monthly private earnings has a slight trend upward over this time period, while employment seems to peak in late 1937 before the recession of 1938.

Figure 1 **Figure 2**

The amount of relief offered a community differed greatly across time and location as well. Maximum per capita work relief spending in a year ranged from over $47 for Jersey City, New Jersey to under $9 for Baltimore, Maryland. Meanwhile maximum per capita direct relief spending ranged from over $22 for Pittsburg, Pennsylvania to under $4 dollars for Topeka, Kansas. Figure 3 plots each city's per capita direct relief receipts in a given month along with the median across all cities. Direct relief peaks in late 1934 and drops off from there as many of FERA's programs were closed and direct relief began to shift back to the responsibility of state and local governments.

Figure 4 shows each city's per capita work relief as well as the median. The large peak in work relief spending during the winter of 1933/1934 represents the brief CWA program. The CWA was a short lived but large program. For example, in January 1935 total work relief spending in the continental United States was approximately 70 million dollars. In January 1934 CWA spending alone was over 218 million dollars. Together with FERA work relief programs this represents an over 300 percent difference in work relief between the two periods. Our data show a more pronounced spike because our sample contains cities that received a disproportionately large portion of CWA spending compared with the national average. The second two humps in Figure 4 represent WPA spending which peaked in early 1936 and late 1938.

Figure 3 **Figure 4**

Generally these four patterns above hold for most cities in our sample, but as one might expect the variation was often more pronounced when looking at individual cities. Figures 5 & 6 illustrate the movement in direct relief, work relief, monthly private earnings, and employment for two cities in the sample- Baltimore, Maryland and Springfield, Massachusetts. In our sample Baltimore had the smallest

average per capita work relief spending between 1932 and 1940, while Springfield, Massachusetts had the greatest.

Figure 5 **Figure 6**

During a decade of economic depression one might expect private earnings to have fallen. It remains a mystery why this did not occur in the United States during the Great Depression. This paper will attempt to determine if relief had any role. Specifically, we ask: Can differences in relief across time and location explain any of the variation in real private earnings? Before proceeding with multivariate regression analysis, we try to better understand the nature of the depression labor market. The next section explores three labor market models that might explain an increase in private wages during this time and the impact the New Deal might have had.

II. The Depression Era Labor Market

Supply & Demand

Figure 7 illustrates a simple demand and supply model of the labor market during the Depression. The prevailing real wage W* is sticky at some point above equilibrium. The quantity supplied of labor at this wage exceeds the quantity demand, resulting in unemployment. Classic sticky wage theory or union contracts could explain why this wage was sticky and the market not in equilibrium, though over time as unemployment persisted, classical economic theory would predict the market wage would fall as time passes and contracts expire until equilibrium was reached. As indicated in Figure 1 and by data from Darby (1976), earnings did not fall during the 1930s. Could the impact of relief spending explain that in the context of a simple supply and demand model?

If there were no substitution effects, work relief spending would tend to decrease the supply of labor to the private market (from Supply to Supply' in Figure 7). Workers were removed from the private labor market as they took up work in the relief sector. As long as there remained a pool of workers not employed in either relief work or the private sector, there should have been no effect on the private wage or private employment levels. Also note that measured unemployment should not change since the Bureau

of Labor Statistics continued to count relief workers as unemployed. If anything measured unemployment would increase, since as Fleck (1999) noted, in order to get a relief job one had to register as unemployed. This would tend to increase measured unemployment as people who had been previously counted as out of the labor force registered as unemployed.

Figure 7 **Figure 8**

Figure 8 shows the private labor market if substitution effects exist. Demand for private labor will decrease (from Demand to Demand' in Figure 8) as projects move from the private sector to the public. In this case private employment would fall and measured unemployment increase. However, just as in figure 6 this should not cause any upward pressure on the private wage so long as there existed a large pool of unemployed workers as was the case during the entire Depression.

Figures 7and 8 illustrate two important points. The first is simply that a demand and supply framework cannot explain why private wages did not trend downward during the Depression or how relief could have affected private wages. The second point is the difficulty of analyzing the market by looking at unemployment figures. Fleck (1999) found that relief did cause a measurable increase in unemployment, however, he could not determine if this increase was due to substitution effects or an increase in the labor force. Relief work and/or the lasting depression could have caused changes in the supply of labor. As workers became discouraged the supply would have decreased along with unemployment. However, requiring people to register as unemployed in order to obtain relief work could have increased the measured supply of labor and consequently increased measured unemployment. Project substitution would also have increased measured unemployment as workers go from private jobs to work jobs. Together this illustrates the importance of using employment (instead of unemployment) data to study the relationship between relief and the private labor market.

Composition Effects and Labor Dumping

As observed in Figure 1, real private wages trended upward during the Depression. A major criticism of aggregated data such as those used in this paper is that such data are subject to "composition

effects." Imagine an employer who laid off workers using a last-in first-out method- in other words, one based on seniority. Under this method as the economy soured, employers would let go the most recently hired workers, those most likely at the bottom of the wage distribution. If we were to observe aggregate data from the employer, then it would appear that the average wage or monthly earnings of workers had risen. This composition effect would be observed also in firms that laid off based on skill. If a firm laid off lower-productivity workers during an economic downturn, this too would alter the wage distribution as low skill workers should have lower wages. Lebergott (1989) cites evidence of this effect and uses these issues to criticize the reliability of BLS chain-weighted private wage indices. Lebergott notes that this effect could play out on at the firm level. Low-productivity and low-wage firms would be the first to fail during the Depression. This would be manifested in chain-weighted measures as a drop in the employment index and an increase in the wage index.

Composition effects offer an explanation for why measured wages did not fall during the Depression. Observed increases may be nothing more than a truncation of the lower end of the wage distribution. However, is there any role for relief in these models? One might argue that relief caused employers to dump workers, either because they felt less moral responsibility with safety nets in place or because they viewed work relief as an alternative means to hoard labor until the economy picked up. There is evidence today (Aizcorbe 1992) that unemployment insurance provides the automobile and construction industries with "hoarding" options. Under this condition relief would have encouraged firms to dump their work force, consequently contributing to any composition effect in the aggregated wage data. We will control for these composition and dumping effects in our empirical analysis in order to distinguish this statistical relationship from predictions of the other models.

Search Model

The supply and demand model with sticky wages cannot explain the increases in wages during the depression or any relationship between relief spending and wages. The various composition effects may explain why wages *appear* to have increased in the aggregated data and indirectly how relief might have induced firms to push desirable, though marginal, workers onto relief rolls. In this section we develop a

search model that provides insights into the direct relationship between relief work and higher private wages. First, it is important to understand the nature of relief employment.

Work relief projects employed people across a wide variety of occupations and educational levels. WPA workers received wages in accordance with these different skill levels. Wage schedules also varied across location with cost of living differences. In most cases, though, WPA wages were less than wages in an equivalent private sector job (Howard 1943). Mindful of its effect on the private sector, regulations called for relief workers to actively seek and accept any "bona fide" offers of private employment. The WPA assured workers of "immediate" rehire if they lost such private employment through no fault of their own. The WPA also mandated separation of workers who had been continuously employed on projects for over 18 months (FWAWPA 1940). Yet Margo (1991) notes that WPA statistics showing similar turnover rates for work relief and private markets are misleading because standard WPA turnover statistics referred to projects and not employment. Using 1940 individual level census data, Margo found that 57 percent of those holding relief jobs in September 1937 held them continuously until February 1939. Meanwhile 16 percent of those on work relief in February 1936 remained employed continuously until February 1939. Clearly the WPA did not strictly enforce the 18-month requirement. In August 1939 the WPA dismissed over 750,000 persons who had exceeded the 18-month limit, only to rehire 57 percent of them a year later. Consequently, relief work could be viewed as relatively secure once obtained. This security made WPA jobs desirable despite a usually lower wage than the private sector. Margo (1993, p. 54) notes that "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."

One can imagine an unemployed worker faced with searching for either private sector work at a high wage or relief work at a low wage. All else the same, the worker would choose the private sector and any additional labor the private market would demand could be filled at that wage so long as a pool of unemployed workers existed. However all else was not the same. Relief jobs and private jobs were not perfect substitutes. A worker had to choose between more certain future relief work at a low wage and, at the time, very uncertain private work at a higher wage. The attraction to relief work is best summarized by a quotation from a WPA relief worker (Margo 1991 p.340).

"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[2]."

Consider the following infinite horizon search model[3]. At the beginning of any time period t, a risk neutral individual will either be unemployed, employed as a relief worker, or employed in the private sector. Unemployed workers choose to search for either a relief or private job, incurring search cost *C*, and receive direct relief *D* with a probability θ^d. Unemployed workers find work the next period with probability θ^p or θ^w, depending on whether they search for a private or relief job. Relief workers earn *W* and have no probability of job loss in future periods if they remain in relief work. Relief workers may, however, search for work in the private market and incur search cost *C*. They find private employment with probability θ^p. Privately employed workers earn *P* but have probability δ that they will lose their current job and become unemployed in the next period. All workers discount every period with a discount rate of $\beta<1$. V_u, V_w, and V_p in equations *2.1*, *2.2*, and *2.3* respectively, represent the Bellman value functions for unemployed, relief employed, and privately employed individuals.

2.1) $$V_u = max\{A + \beta\theta^p V_p + \beta(1-\theta^p)V_u, A + \beta\theta^w V_w + \beta(1-\theta^w)V_u\}$$

2.2) $$V_w = max\{W^* + \beta\theta^p V_p + \beta(1-\theta^w)V_w, W + \beta V_w\}$$

2.3) $$V_p = \{P + \delta\beta V_u + \beta(1-\delta)V_p\}$$

Where:

A	$= \theta^d D - C$	P	= Earnings from private job
C	= Search cost	θ^d	= Probability of getting direct relief
D	= Direct relief payment	θ^p	= Probability of finding private work
β	= Discount factor	θ^w	= Probability of finding relief work
W	= Income from relief job	δ	= Probability of losing private job
W^*	$= W - C$		

[2] Quoted in E. W. Blake, *The Unemployed Worker* (New Haven, 1940), pp. 421-22.

[3] Such a model precludes rational agents from ever returning to a previous wage/job offer meaning what is optimal in one period is optimal for all periods. Changing the model to a finite horizon setting does not change the comparative static predictions to follow.

Solving for V_p, V_u, and V_w where $a = (1 - \beta + \beta\delta)$; $b = (1 - \beta + \beta\theta^p)$; and $d = (1 - \beta + \beta\theta^w)$

$$2.4) \qquad V_u = max\left\{\frac{(A+\beta\theta^p V_p)}{b}, \frac{(A+\beta\theta^w V_w)}{d}\right\}$$

$$2.5) \qquad V_w = max\left\{\frac{(W^* + \beta\theta^p V_p)}{b}, \frac{W}{(1-\beta)}\right\}$$

$$2.6) \qquad V_p = \frac{(P+\beta\delta V_u)}{a}$$

Case 1

Consider the first case where firms hire only from the unemployed. What private wage P, must firms set in order to induce unemployed workers to search in the private labor market? In this case:

$$2.7) \qquad V_u = \frac{(A+\beta\theta^p V_p)}{b}$$

Substituting *2.7* into *2.6* and solving for V_p, where $c = (ab - \delta\beta^2\theta^p)$:

$$2.8) \qquad V_p = \frac{(Pb+\beta\delta A)}{c}$$

To attract unemployed workers, where $V_w = \frac{W}{(1-\beta)}$

$$2.9) \qquad \frac{A+\beta\theta^p V_p}{b} \geq \frac{A+\beta\theta^w V_w}{d}$$

Substituting *2.8* into *2.9* and solving for P:

$$2.10) \qquad P \geq \frac{cA(\theta^p - \theta^w) + bc\theta^w V_w - \delta\beta\theta^p Ad}{db\theta^p}$$

Case 2

Now consider the case where firms only hire current relief workers. What private wage P, must firms set in order to induce relief workers to search in the private labor market? In this case:

$$2.11) \qquad V_u = \frac{(A+\beta\theta^w V_w)}{d}$$

$$2.12) \quad V_w = \frac{(W^* + \beta\theta^p V_p)}{b}$$

Substituting *2.11* into *2.6* and solving for V_p:

$$2.13) \quad V_p = \left(\frac{P}{a} + \frac{\beta\delta A + \beta^2\delta\theta^w V_w}{ad} \right)$$

To attract relief workers:

$$2.14) \quad \frac{W^* + \beta\theta^p V_p}{b} \geq \frac{W}{1-\beta}$$

Substituting *2.13* into *2.14* and solving for *P*:

$$2.15) \quad P \geq \frac{Wba - W^* a(1-\beta)}{\beta(1-\beta)\theta^p} + \frac{A\delta\beta + \beta^2\delta\theta^w \left[\frac{W}{1-\beta} \right]}{d}$$

The *P* in equations *2.15* and *2.10* represent the private market monthly reservation wages for an unemployed and relief employed worker respectively. Implicitly this model assumes that relief income *W*, is exogenous to the model and always high enough such that no unemployed worker would ever favor leisure over working in a relief job. Given the gender composition of the labor force at the time and the extremely depressed nature of the economy, this seems like a reasonable assumption.

Now consider a representative firm's decision with profit, $\Pi = qP_q - lP_l - kP_k$ and without loss of generality a simple Cobb-Douglas production technology such that $q = l^\alpha k^{1-\alpha}$. In the absence of work relief and assuming workers have no preference between employers, a price taking firm hires labor such that, $P_l = \alpha P_q (k/l)^{1-\alpha}$.

With the introduction of work programs, a private firm must compete with relief projects for labor. They must offer a monthly wage P_l high enough to induce search in the private sector from either the unemployed or current relief workers. Assuming all workers are alike, a firm will maximize profits by setting their monthly wage P_l equal to the minimum of equations *2.10* and *2.15*. If this minimum is greater than the prevailing private wage, the result will be a fall in the existing equilibrium employment, *l*.

Equations *2.10* and *2.15* provide comparative static predictions for how changes in direct relief, work relief income, probabilities for finding work, and other exogenous variables influence the private

monthly wage P. Predictions of the effects for private employment follow directly from the price relationships. These first derivatives are summarized below.

- $\frac{\delta P}{\delta W} > 0$
- $\frac{\delta P}{\delta \beta} > 0$
- $\frac{\delta P}{\delta \delta} > 0$
- $\frac{\delta P}{\delta D} < 0$
- $\frac{\delta P}{\delta \theta^d} > 0$
- $\frac{\delta P}{\delta C} > 0$
- $\frac{\delta P}{\delta \theta^p} > 0$

If firms hire from the unemployed:

- $\frac{\delta P}{\delta \theta^w} > 0$

If firms hire relief workers:

- $\frac{\delta P}{\delta \theta^w} < 0$

- $\frac{\delta E}{\delta W} < 0$
- $\frac{\delta E}{\delta \beta} < 0$
- $\frac{\delta E}{\delta \delta} < 0$
- $\frac{\delta E}{\delta D} > 0$
- $\frac{\delta E}{\delta \theta^d} < 0$
- $\frac{\delta E}{\delta C} < 0$
- $\frac{\delta E}{\delta \theta^p} < 0$

If firms hire from the unemployed:

- $\frac{\delta E}{\delta \theta^w} < 0$

If firms hire relief workers:

- $\frac{\delta E}{\delta \theta^w} > 0$

Intuitively, as the income from work relief increases, private employers must raise their pay to attract workers. Larger β (less discounting) and higher costs to search also increase the private income required to attract workers. Kahn and Low (1984) note that a rise in layoff probability has similar effects to a fall in wages. The above model bears this out; a greater probability of being laid off from private employment requires employers to raise the private wage to compensate. Interestingly, larger direct relief payments and a higher probability of receiving them decrease the required private income in our model. The larger expected direct relief payments act to reduce the loss when unemployment does occur, making workers more willing to take the risks associated with private employment. This is similar to Mortensen's

(1977) result that larger unemployment benefits make current employment more attractive thus reducing an employee's reservation wage. A higher probability of finding private work also acts to reduce the required private wage by increasing the expected gain from searching for private employment.

Most interesting is the switching sign on the first derivative of private earnings with respect to the probability of finding work relief. When firms hire the unemployed, a larger θ^w requires an increase in P as the expected value of searching in relief work rises with a larger θ^w. However, when firms hire only relief workers, this effect does not come into play. Instead a greater probability of finding relief work when unemployed increases the value of being unemployed and consequently increases the value of working in the private market where there is a risk of becoming unemployed. This allows firms to reduce the P required to induce relief workers to search.

In general whether firms hire from the unemployed or relief workers depends on the relationship between the probabilities of finding relief work versus that of finding private work. If there are no search costs, its optimal for firms to hire from the unemployed when $\theta^p > \theta^w$. When $\theta^w > \theta^p$ it is optimal to hire from the pool of relief workers. In the case where $\theta^p = \theta^w$ the private wage required to hire unemployed or relief workers is the same. Search costs drive a wedge between this relationship. Because relief workers incur a search cost when looking for private work that they do not incur when not searching, the required private wage to induce search must be larger ceteris paribus. This means that with positive search costs firms will still hire from the unemployed when $\theta^w > \theta^p$. In reality not every firm across the country at a given time will either be hiring from the unemployed or relief ranks. Instead different cities will have different θ^p and θ^w, which will no doubt change with time. For given industries within a city the probability of finding private work will also differ.

This simple search model suggests a method for proceeding with regression analysis and highlights the importance of using better data. Simply greater work relief spending in an area may not capture the important decision variables with which a worker is faced. The worker will compare the total income they will receive during a time period on work relief against income from working in private employment. Greater work relief spending in a city or state may reflect a change in θ^w, the probability of finding work relief as more people are hired. Conversely, greater spending may reflect increased wages or

hours for current relief workers. This would work to increase the expected relief work income W. Each of these could have an opposite effect in the model described above. Just looking at the private labor market level of unemployment is also not enough information to test this model. As Fleck suggested, the enrollment requirements of direct and work relief may change measured unemployment without any change in actual employment or wages. The variability of private employment is key in this setting. Finally the model highlights the importance of direct relief as well as the sometimes unexpected impact that relief might have had.

III. Econometric Estimation

Data and Variables

While the data used for this paper are perhaps more extensive than those used in previous research, the relief data are still aggregated by city and the labor market data consist of chain-weighted indices. Consequently, we cannot directly measure or reject the possibility of compositional effects. Instead, in our econometric analysis we will control for any composition effects caused by labor dumping or hordeing and determine if there remains some relationship between relief and private wages. The potential endogeneity between relief spending and employment must also be addressed. Past studies using cross-sectional data have had to rely on instruments for relief spending to fix this problem. The typical choice of instruments is voting behavior (Fleck, Wallis 1987). As with most instruments this method is subject to criticism based on the relevance and strength of the instruments. We sidestep this issue by exploiting the high frequency and time component of our panel data.

We have compiled two new data sets of relief spending across various cities. Both contain extensive observations on expenditures for nearly all federal, state, and local relief programs. This is merged with business indicator data and construction figures, resulting in two unbalanced panels of annual and monthly observations from 44 cities between 1932 and 1940. The relief data contain monthly raw spending figures for each relief program. Real per capita direct relief spending is computed for each city in the sample using population and price measures with FERA, state, and local direct relief spending. This measure can be interpreted as the expected direct relief benefit, $\theta^l D$ from the search model. The data on

FERA work relief, CWA, and WPA spending is augmented with data on case and job counts to compute the average income per relief job and the per capita number of work relief jobs. In terms of the search model these can be thought of as proxies for W and θ^w respectively.

The business indicator data contain monthly information on total private employment and payroll for each city in the sample. Some of these data are reported in raw form in which case they are converted to monthly chain employment and real payroll indices with January 1935 as the base period. For other cities the original data are reported as a chain-weighted index.[4] Dividing the payroll index by the employment index creates P, our measure of monthly private earning.

The probability of a private employee becoming unemployed, δ, and the probability of an unemployed or relief worker finding private work θ^p, both represent to the worker uncertainty about private work in the context of our search model. Finding a proxy for these variables is challenging. If employment was growing recently this would tend to increase θ^p and decrease δ. The search model predicts both should have a negative effect on private earnings. If employment falls, θ^p would decrease and δ increase with a predicted positive effect on private wages. Unfortunately, simply using the change in employment as a proxy for labor market uncertainty could be interpreted in two ways. It could be labor market uncertainty effects from the search model or it could pick up composition effects. Any composition effect will also tend to raise the earnings index when employment decreases. To address these issues, we use the average monthly percentage change in employment over the previous six months to control for any composition and labor dumping effects. In order to proxy for labor market uncertainty, we use the variance of the monthly percentage change in employment over the previous six months. If employment was growing but with relatively high volatility this would tend to increase workers' uncertainty and mitigate the beneficial effects of increasing employment, thus putting upward pressure on private earnings. If employment was falling and volatile this would put greater upward pressure on private earnings. Consequently, the search model predicts a positive sign on the variance of employment changes. Finally, since the dependent variable itself is an index, all variables are indexed to the first observation of 1935.

[4] For further information on chain weighted indices see Wallis (1989)

Results

We estimate a reduced form econometric model to test the relationship between New Deal Relief spending and private earnings, controlling for any compositional effects. We first estimate equation *3.1* using the annual data.

$$3.1) \qquad P_{it} = \alpha_i + \rho_1 P_{it-1} + \beta_1 D_{it-1} + \beta_2 W_{it-1} + \beta_3 \theta^w_{it-1} + \beta_4 de6_{it-1} + \beta_5 var\,de6_{it-1} + \beta_2 C_{it-1} + \varepsilon_{it}$$

P_{it} represents the real private earnings index for city *i* at time *t*. *W*, θ^w, and *D* represent, respectively, the average monthly work relief income per job, work relief jobs per capita (proxy for the probability of finding a work relief job), and direct relief spending per capita (proxy for the expected direct relief payment). The 6-month average monthly percent change in employment, *de6* controls for composition effects and the 6-month variance of monthly employment changes, *varde6* attempts to capture any labor market uncertainty effects predicted by the search model. We also included the real per capita value of construction, *C*, to control for unobserved economic activity. Because the dependent variable is an index with 1935 as the base year, all regressors are normalized to indices with 1935 as the base year and all variables that may be endogenous to private earnings are lagged one year. That is, today's earnings cannot cause or influence decisions made about relief in the past. Finally a lagged dependent variable is added as a regressor to prevent problems with autocorrelation. This necessitates a fixed effect α_i to assure city *i*'s private earnings index is regressed only on its own lagged earnings. The results from estimating equation *3.1* are summarized in Table 2.

Table 2

Despite the relatively small number of observations and aggregation, the annual data find some significant relationships between relief and private earnings. Likelihood ratio tests indicate that the full model with relief variables is superior to the simple autoregressive model in explaining variations in private earnings[5]. From the search model perspective more relief jobs per capita would suggest a higher probability of finding work relief. The regression results indicate that work relief jobs per capita had a

[5] $LR = 2[\ln L_u - \ln L_r] \sim \chi^2$

positive and statistically significant effect on earnings. It seems that despite the extremely large pool of available unemployed workers throughout the Depression, cities with increasing numbers of relief jobs experienced increases in real private earnings.

While the signs of the work relief variables fit with the predictions of the search model, the sign of the direct relief variable does not. The search model predicts greater direct relief would encourage workers to take the risk of private employment, thus allowing employers to reduce the private wage. The data, however, indicate that cities with greater direct relief also had higher private earnings. These estimated relief effects should represent an actual adjustment in private earnings in response to relief. An alternative hypothesis is that relief may have caused a reduction in employment which would have induced a shift in the workforce composition. We cannot directly test this idea. However, should any dumping have occurred the resulting drop in employment and its effect on private earnings would be picked up directly in the average 6-month percentage change in employment. This variable has the expected negative sign, suggesting such a composition effect, yet it is not statistically significant. The annual data also do not find any significant labor uncertainty effects as the variance of changes in private employment had no effect on private earnings. Finally it seems per capita construction is a good proxy for any unobserved changes in the economy. More real activity in a city led to higher earnings generally.

While the annual data offer a comprehensive set of direct and work relief programs, they do not take full advantage of the variation in private earnings and relief over this time. By using a panel of monthly data we are able to capture the turning points and changes that occurred within years. This econometric model is found in equation *3.2*.

$$3.2) \qquad P_{it} = \alpha_i + m_t + \rho_1 P_{it-1} + \rho_2 P_{it-2} + \beta_1 D_{it-1} + \beta_2 W_{it-1} + \beta_3 \theta^w_{it-1} + \beta_4 de6_{it-1} + \beta_5 var\,de6_{it-1} + \beta_2 C_{it-1} + \varepsilon_{it}$$

As before P_{it} represents the monthly real private earnings index for city i at time t. W, θ^w, and D are respectively the variables monthly work relief income per job, work relief jobs per capita (proxy for the probability of finding a work relief job), and direct relief spending per capita (proxy for the expected direct relief payment). The 6-month average monthly percent change in employment, de^6 controls for composition effects and the 6-month variance of monthly employment changes, $varde^6$ attempts to capture

any labor market uncertainty effects predicted by the search model. In order to control for unobserved economic variation and seasonality the per capita value of construction, C is added, along with month dummies, m. Because the dependent variable is an index with January 1935 as the base year, all regressors are normalized to indices with this month as the base year and all variables that may be endogenous with private earnings are lagged one month. Today's earnings cannot cause or influence decisions made about relief in the past. Finally two lagged dependent variables are added to prevent problems with autocorrelation in the error term. This necessitates a fixed effect α_i to assure city *i*'s private monthly earnings is regressed only on its own lagged earnings. The results from estimating equation *3.2* are summarized in Table 3.

These results offer even more evidence of work relief's positive effect on real private earnings. Likelihood ratio tests indicate that the full model with relief variables is superior to the simple autoregressive model in explaining variations in private earnings[6]. Again, per capita relief jobs seem to raise private earnings despite a persistent pool of otherwise available workers. Recall that according to the predictions of the search model a negative sign on the probability of finding work relief suggests most firms are hiring workers from the relief rolls while a positive sign implies that most firms hired employees from the pool of truly unemployed. Anecdotally this could explain why relief workers were "unemployed" longer than non-relief workers.[7] Some have suggested this was because relief workers were stigmatized or lost human capital while on work relief.[8] Perhaps it was simply cheaper for firms to hire those not on relief, rather than entice relief workers to leave their secure WPA job.

The monthly data also find a positive and statistically significant relationship between work relief income per job and private earnings. This also coincides with predictions from the search model. Higher wages in work relief will make the private sector all the more unattractive, also forcing private employers to raise their wages to attract labor.

Table 3

[6] $LR = 2[\ln L_u - \ln L_r] \sim \chi^2$
[7] Margo 1991
[8] Jensen 1989

Despite this evidence in support of the search model, once again the sign on direct relief is opposite from what the model predicts though it is not statistically significant. One possible explanation is that the chance of finding work relief or private employment was not independent of being on direct relief as our model specifies. There is some anecdotal evidence that direct relief may have been a gateway to work relief. If this was the case, direct relief may have served to further increase the chance of finding a relief job and in turn further reduced search for private employment.

Just as in the annual regression, the results on relief variables should represent a true labor market reaction to relief rather than any composition or dumping effects. These effects would be picked up in the average percent change in employment variable. The coefficient on this variable is negative and statistically significant, lending some evidence to the presence of composition effects. While the change in employment is significant the variance of monthly employment changes is not. This variable was meant to proxy labor market uncertainty. The results suggest workers were not sensitive to the volatility of private employment or their primary gauge for labor market uncertainty was simply the employment level. In the later case labor market uncertainty effects could be tied up with the composition effects in the coefficient on the change in employment variable.

Magnitude

The size and importance of all the above point estimates are difficult to interpret due to the autoregressive nature of the final econometric models. In order to assess the overall magnitude of these coefficients, annual private wages are assumed to follow a first-order autoregressive process as illustrated in equation *3.3* and monthly private wages are assumed to follow a second order process as illustrated in equation *3.4*.[9]

3.3) $$P_{it} - \rho_1 P_{it-1} = X_{it}\beta + \varepsilon_{it}$$

3.4) $$P_{it} - \rho_1 P_{it-1} - \rho_2 P_{it-2} = X_{it}\beta + \varepsilon_{it}$$

[9] Spectral analysis of ACF and PACF support the assumption that annual real private wages follow an AR(1) process. and monthly real private wages follow an AR(2) process.

ρ is the estimated coefficient on the lagged dependent variable from the respective regressions and $X_{it}\beta$ is the exogenous explanatory variables and their coefficients in the respective econometric models. Given equations *3.3* and *3.4* the total effect of any individual variable can be solved with equation *3.5*.

3.5) $$B = \frac{\beta}{1 - \rho_1 - \rho_2}$$

These results are summarized in Table 4. In addition to the total effect for the relief variables the standard deviation of each variable is listed as well as the percent change in private earnings that would result from a one standard deviation increase.

Table 4

The total effects estimated in both the annual and monthly regressions are not small. Based on the monthly-data estimation, a one standard deviation increase in per capita work relief jobs would have caused a 6.9 percent increase in the real private earnings. A standard deviation increase in per capita relief jobs in the annual-data estimation would have increased earnings 8.7 percent. A similar increase in work relief income per job would have raised earnings 1.2 percent and 5.6 percent in the annual and monthly estimations respectively. An increase in direct relief per capita would have raised private wages 3.1 percent in the monthly panel and 4.3 percent in the annual panel, though the coefficient on direct relief in the monthly regressions is statistically insignificant. Finally, the average 6-month change in private employment has a substantial effect on earnings as well. A one standard deviation decrease in the mean change in private employment, would have increased wages over 4 percent based in the monthly panel.

Conclusions

Given the large and persistent pool of unemployed workers throughout the 1930s, one might expect that work relief would simply transfer people from unemployment to relief employment with no effect on the private market. The results from both our annual and monthly panels suggest otherwise. Greater relief spending was associated with increases in the real private wage.

We began the paper by illustrating how a simple supply and demand model with sticky wages could not explain these results. Two hypotheses remained. Relief spending may have encouraged firms to dump or store workers on relief rolls, expecting to be able to hire them back when the economy improved. If this occurred, employers would have truncated the lower part of their wage distribution. This could create in earnings indices a composition effect where total payroll falls by less than total employment. In the data it would appear to be an increase in earnings. By using panel data and changes in private employment levels we are able to control for this effect. We do find evidence of a statistically significant negative relationship between changes in employment and private earnings. This coefficient may be the composition effect induced by dumping or some expression of labor market uncertainty. However, in either case, having controlled for it the remaining significant coefficient on relief measures must be picking up a true reaction in the private labor market to New Deal relief.

We propose a behavioral model that could explain these results. Our search model establishes a framework within which to think about the private labor market and the decisions facing workers during the Depression. It fits well with the previous quotation from the WPA worker that they "knew a good thing when they got it." If workers behaved as our model suggests, the greater certainty of relief work could more than compensate for the lower wages paid in relief jobs. Consequently, in order to attract labor, private employers would have had to raise their wage. The model also predicts that private earnings should fall with greater direct relief and increase with greater labor market uncertainty.

The data over this period seem to support the model to different degrees. More direct relief raised earnings, contrary to what the model predicts. Perhaps workers simply did not find that the safety net of direct relief made taking private employment any more attractive against the security of a relief job or perhaps direct relief increased one's chance of securing work relief. The results for labor market uncertainty are clouded. Due to the confounding presence of composition effects it is difficult to determine how important private labor market uncertainty was to workers. However, after controlling for these composition and dumping effects, our estimations show that greater numbers of relief jobs and higher work relief income had a statistically significant and positive effect on private monthly earnings.

We cannot refute that some of the measured increase in private earnings during the depression may have been a result of simple composition effects. We can also not determine if relief induced firms to lay off workers, contributing to this composition effect. However, by controlling for these effects we have shown that work and direct relief did put upward pressure on private earnings. These results have implications for policy makers in the U.S. and abroad. Despite large unemployment, government sponsored work relief programs that offer not just a job but also job security can cause upward pressure on private wages. This pressure, in turn, could reduce the ability of the private sector to recover. Direct unemployment transfers may better meet the humanitarian need faced in times of economic distress, without altering the wage structure of the private labor market.

Tables

Table 1

Annual U.S. Unemployment Rates

Year	Bureau of Labor Statistics	Corrected BLS
1929	3.2	3.2
1930	8.7	8.7
1931	15.9	15.3
1932	23.6	22.5
1933	24.9	20.6
1934	21.7	16.0
1935	20.1	14.2
1936	16.9	9.9
1937	14.3	9.1
1938	19.0	12.5
1939	17.2	11.3
1940	14.6	9.5
1941	9.9	6.0

Darby(1976, p.8)

Table 2

Annual Panel

Dependent variable: real private earnings[10]

Variable	Lagged dependent variables & City Effects	**Lag. Dependent variables, Relief Measures & City Effects**
Lagged Dependent Variable	0.835**	**0.513****
Direct Relief spending per capita	--	**0.063****
Work Relief Jobs per capita	--	**0.046****
Work Relief Income per job	--	**0.005**
Average 6-month change in employment	--	**-0.000**
Variance of monthly employment changes	--	**-0.002**
Construction per capita	--	**0.016****
Degrees of Freedom	292	**247**
Log Likelihood	274	**286**
Autocorrelation	-.16	**-.07**

*** indicates significance at the 5 percent confidence level*

[10] The private wage index as of January of each year is used as the dependent variable in the regression. The prevailing wage rate as of January should be a function of the previous year's relief spending. The model is also estimated using the average monthly wage for a given year regressed on the previous years relief spending. Results are nearly identical in sign, magnitude, and significance.

Table 3

Monthly Panel

Dependent variable: real private earnings

Variable	Lagged dependent variables, month & City Effects	**Lag. Dependent variables, Relief Measures, month[11] & City Effects**
Lagged Dependent Variable	0.885**	**0.877****
Second Lagged Dependent Variable	0.053**	**0.063****
Per capita Direct Relief spending	--	**0.004***
Work Relief Income per job	--	**0.003****
Work Relief Jobs per capita	--	**0.001****
Average 6-month change in employment	--	**-0.001****
Variance of monthly employment changes	--	**-0.000**
Construction per capita	--	**0.001****
Degrees of Freedom	3957	**3916**
Log Likelihood	6316	**6450**
Autocorrelation	0.01	**0.00**

*** indicates significance at the 5 percent confidence level*

Table 4

Total Effect for Relief Variables

	Annual Panel			Monthly Panel		
Variable	*B*	Standard Deviation	Change in Private Wage	*B*	Standard Deviation	Change in Private Wage
Direct Relief per capita	0.12	0.36	4.3 %	0.06	0.51	3.1 %
Work Relief Jobs per capita	0.09	0.97	8.7 %	0.02	3.31	6.9 %
Work Relief Income per job	0.01	1.27	1.2 %	0.04	1.34	5.6 %
Change in Private Employment	0.00	9.07	0.6 %	-0.02	2.81	-4.4 %

[11] The month effects are together significant.

Figures

Figure 1

Real Private Average Monthly Earnings

January 1935=1

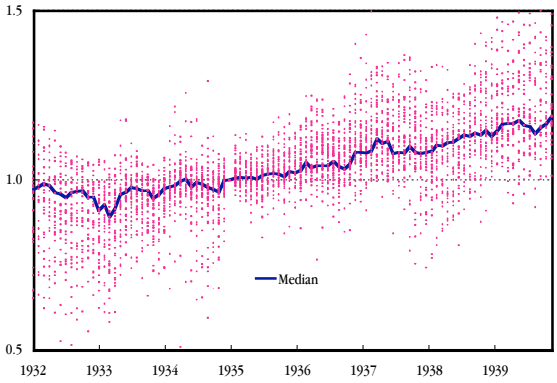


Figure 2

Employment

January 1935=1

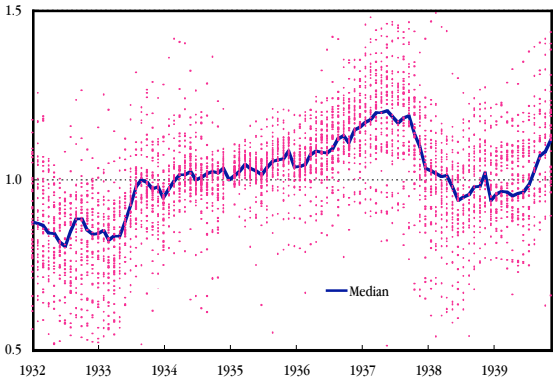


Figure 3

Direct Relief Spending per capita

January 1935=1

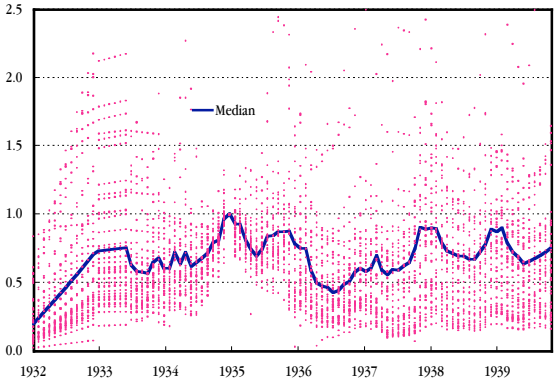


Figure 4

Work Relief Spending per capita

January 1935=1

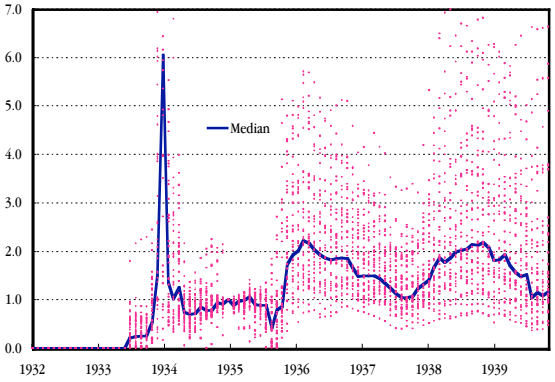


Figure 5

Springfield, MA

January 1935=1

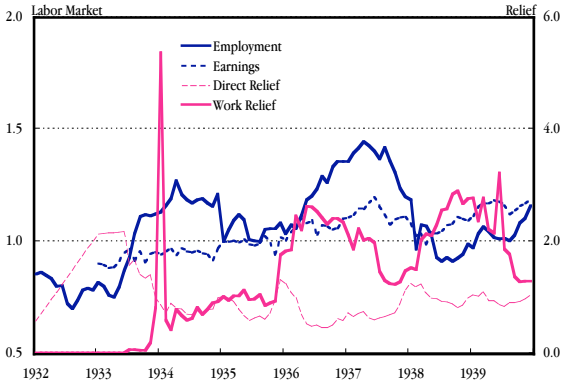


Figure 6

Baltimore, MD

January 1935=1

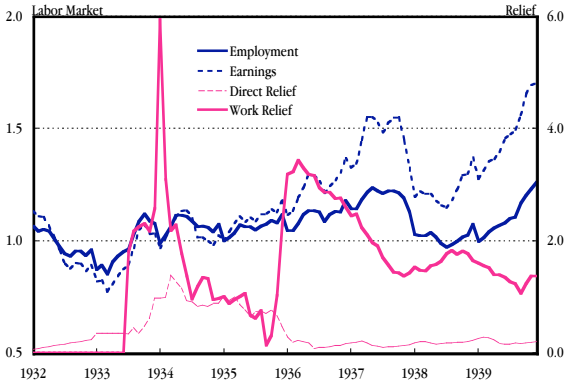


Figure 7

Private Labor Market

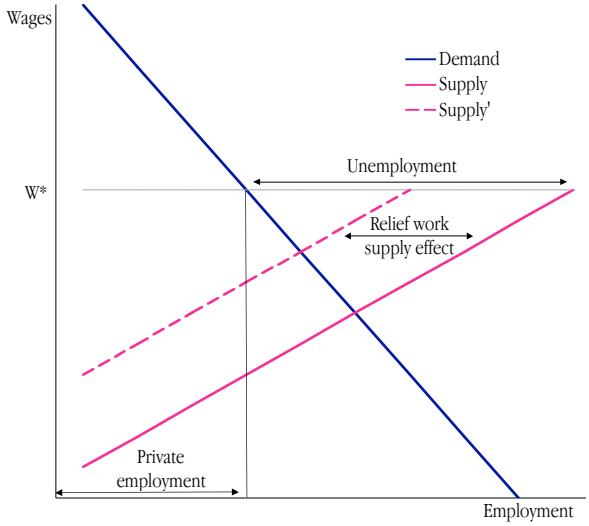
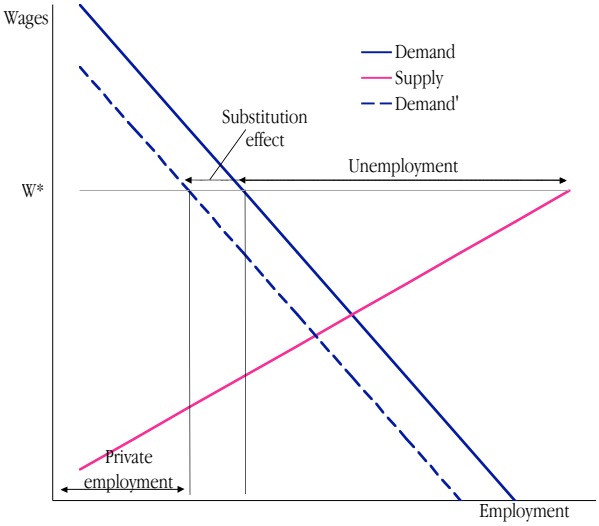


Figure 8

Private Labor Market



A. Data Appendix

Both data samples consist of observations of various lengths on 44 cities between 1932 and 1940. The sample constituted these specific 44 cities[12] 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		

[12] 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

The relief measures described in **Data & Variables** are constructed for each time periods as shown below.

I. Monthly Data[13]

a. Direct Relief per capita or $\theta^{l}D$ from the search model.

i. January 1932 – December 1933: This is computed by dividing an annual observation of direct relief in a certain city by 12 and then weighting that to achieve a linear trend up to the computed January 1933 figure. $Trend\left[\frac{\$G_{iy}}{12 \cdot pop_{im}}\right]$[14]

ii. January 1933 – May 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. $\frac{\$G_{yi} - \sum_{m=1}^{6} \$G_{im}}{6 \cdot pop_{im}}$

iii. June 1933 – June 1935: This is computed by scaling monthly observations on total direct and work relief spending in FERA, state, & local programs by the share spent in the respective state on just direct relief. $\frac{\$G_{im}(\$G_{sm} - \$GW_{sm})}{\$G_{sm} \cdot pop_{im}}$[15]

iv. July 1935 – March 1937: This is simply total general spending in a city during a month divided by the population. $\frac{\$G_{im}}{pop_{im}}$

v. April 1937 – December 1940: This is computed by weighting an annual observation on direct relief in a city by the share spent each month in that city's state. $\frac{\$G_{iy} \cdot \$G_{sm}}{\$G_{sy} \cdot pop_{im}}$

b. Work Relief Jobs/Cases per capita or θ^{w} from the search model.

i. June 1933 – May 1935: This is computed by weighting total FERA case loads by the share in the city's respective state that are devoted to work relief plus

[13] Variables are referenced by i or s, indicating whether it is data on the city or state level (no subscript indicates national data) & by *m* or *y*, indicating whether it is data at monthly or annual frequency.

[14] *$G* is general relief spending from the Final Statistical Report of the FERA (FSRFERA). At different points in time this includes, FERA direct, FERA work, state and local relief. *Pop*. represents population computed by a simple linear interpolation between 1930 & 1940 census numbers.

[15] *$GW* is FERA work relief spending from the FSRFERA.

the number of monthly CWA cases times each city's share of national CWA spending. $\frac{CG_{im} \cdot CGW_{sm}}{CG_{sm} \cdot pop_{im}} + \frac{CCWA_{m} \cdot \$CWA_{iy}}{pop_{im} \cdot \$CWA_{y}}$ [16]

ii. June 1935 – December 1935: This is computed by weighting total FERA case loads by the share in the city's respective state that are devoted to work relief plus WPA jobs per city. $\frac{CG_{im} \cdot CGW_{sm}}{CG_{sm} \cdot pop_{im}} + \frac{JWPA_{im}}{pop_{im}}$ [17]

iii. January 1936 – End[18]: This is the number of WPA jobs computed by multiplying each city's WPA hours by the number of hours per WPA job in each respective state. $\frac{JWPA_{im}}{pop_{im}} = \frac{HWPA_{im} \cdot JWPA_{sy}}{pop_{im} \cdot HWPA_{sy}}$

c. Work Relief Monthly Income per job or *W* from the search model.

i. June 1933 – June 1935: This is computed by weighting total FERA spending by the share in the city's respective state that is devoted to work relief, divided by the number of FERA work cases in a city plus annual CWA spending in a city weighted by the share of national CWA spending to occur in that month. $\frac{\$G_{im} \cdot \$GW_{sm}}{CGW_{im} \cdot \$G_{sm}} + \frac{\$CWA_{iy} \cdot \$CWA_{iy}}{CCWA_{im} \cdot \$CWA_{y}}$

ii. June 1935 – December 1935 [19]: This is computed by weighting total FERA spending by the share in the city's respective state that is devoted to work relief, divided by the number of FERA work cases in a city plus WPA spending in a city during a month divided by WPA jobs in that month. $\frac{\$G_{im} \cdot \$GW_{sm}}{CGW_{im} \cdot \$G_{sm}} + \frac{\$WPA_{im}}{JWPA_{im}}$

iii. January 1936 – End: This is WPA spending in a city during a month divided by WPA jobs in that month. $\frac{\$WPA_{im}}{JWPA_{im}}$

[16] *CG* is general relief cases from FSRFERA. At different points in time this includes, FERA direct, FERA work, state and local relief. *CGW* is FERA work relief cases from FSEFERA. *CGW* is FERA work relief cases from FSEFERA. *CCWA* is CWA cases from Security, Work, and Relief Policies. *$CWA* is CWA spending from Security, Work, and Relief Policies.

[17] Computed WPA jobs.

[18] *HWPA* is WPA hours and *JWPA* is WPA jobs from National Archives in Washington and Report on Progress of the WPA Program

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

II. Annual Data

a. Direct Relief per capita or $\theta^{j}D$ from the search model.

i. 1932: This is simply annual general relief spending. $\frac{\$G_{iy}}{pop_{iy}}$

ii. 1933 – 1935: This is computed by subtracting the total work relief spending from total relief. $\frac{\$G_{iy} - \sum_{m=1}^{12} \$GW_{im}}{pop_{iy}}$

iii. 1936 – 1940: This is simply annual general relief spending. $\frac{\$G_{iy}}{pop_{iy}}$

b. Work Relief Jobs per capita or θ^{w} from the search model.

i. 1933 – 1934: This is the average number of general cases per month in a city weighted by the city's state share of general cases that are work relief plus each city's annual CWA spending times the number of CWA cases per dollar in each city's respective state. $\frac{\sum_{m=1}^{12} CG_{im} \cdot CGW_{sy}}{12 \cdot pop_{iy} \cdot CG_{sy}} + \frac{\$CWA_{iy} \cdot CCWA_{sy}}{pop_{iy} \cdot \$CWA_{sy}}$

ii. 1935: This is the average number of general cases per month in a city weighted by each city's state share of general cases that are work relief plus average monthly WPA jobs per city in a year. $\frac{\sum_{m=1}^{12} CG_{im} \cdot CGW_{sy}}{12 \cdot pop_{iy} \cdot CG_{sy}} + \frac{\sum_{m=1}^{12} JWPA_{im}}{12 \cdot pop_{iy}}$

iii. 1936 – End: This is average monthly WPA jobs per city in a year. $\frac{\sum_{m=1}^{12} JWPA_{im}}{12 \cdot pop_{iy}}$

c. Work Relief Annual Income per job or W from the search model.

i. 1933 – 1934: This is total general relief spending in a year weighted by a city's state share of general relief spending that is work relief and divided by total cases of FERA work relief, plus annual CWA spending divided by total CWA cases. $\frac{\sum_{m=1}^{12} \$G_{im} \cdot \$GW_{sy}}{\sum_{m=1}^{12} CG_{im} \cdot \$G_{sy}} + \frac{\$CWA_{iy}}{CCWA_{iy}}$

ii. 1935: This is total general relief spending in a year weighted by a city's state share of general relief spending that is work relief and divided by total cases

of FERA work relief, plus annual WPA spending divided by total WPA

$$\text{jobs.} \frac{\sum_{m=1}^{12} \$G_{im} \cdot \$GW_{sy}}{\sum_{m=1}^{12} CG_{im} \cdot \$G_{sy}} + \frac{\sum_{m=1}^{12} \$WPA_{im}}{\sum_{m=1}^{12} JWPA_{im}}$$

iii. 1936 – End: This is annual WPA spending divided by total WPA

$$\text{jobs.} \frac{\sum_{m=1}^{12} \$WPA_{im}}{\sum_{m=1}^{12} JWPA_{im}}$$

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 is converted into chain indices with January 1935 as the base year.

Relief and employment data are next matched with population, construction, 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. Construction data come from Federal Housing Administration and the U.S. Office of Government Reports. Monthly data on the value of new and refurnished residential and non-residential construction are combined to create a total value of building construction measure. For the annual regressions this figure is summed up to an annual number.

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