

**Federal Programs in Times of Crisis:
The Impact of the New Deal on Local Economies During the Great Depression**

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During times of economic crisis, how effectively do various government programs promote local economic growth? During the current economic downturn, the inevitable question has arisen: What can the federal government do? This paper revisits this classic debate by looking to a time when the U.S. economy was in severe crisis and the government's response was remarkable. The New Deal launched the most dramatic peace-time expansion in federal spending in U.S. history. Annual federal outlays outside of the traditional categories of national security and international affairs were four to six times higher in the 1930s than in 1929 (U.S. Census Bureau 1975, 1115, series Y471). In response to the Great Depression, the Roosevelt Administration introduced a myriad of different programs, funded on an unprecedented scale, in an attempt to revive economic activity. The Public Works Administration (PWA) handed out grants to build huge civil infrastructure projects, while the Federal Emergency Relief Administration (FERA), the Civil Works Administration (CWA), and the Works Progress Administration (WPA) gave state and local governments grants to provide work relief and direct relief. The New Deal launched the farm programs that provided farmers with payments to alter their production decisions. New Deal agencies lent funds to state and local governments, banks, homeowners, farmers, and to industry in order to provide needed liquidity. Through the Federal Housing Administration (FHA) the federal government sought to prop up the housing sector by insuring home improvement and mortgage loans.

While the New Deal was a federal response to the Great Depression, not all states or counties within states fared equally in terms of federal support. Economists have attempted to uncover what determined the geographic distribution of the New Deal funds, yet surprisingly very little is known about the impact of the various New Deal programs on local economies. There have been a handful of macroeconomic studies of the New Deal, but they have been too general to capture the geographic differences in the degree of the economic downturn and recovery across the country and the significant variety in the form and geographic distribution of the federal government's response.¹ Several studies of

specific programs examine their effects on employment and banking activity, but each has largely focused on one program.² Thus, to date, New Deal programs have been considered in isolation, when in fact there were many New Deal programs that were simultaneously affecting local economies, but perhaps in disparate fashion.

The New Deal offers a unique experiment to test the impact of various forms of government intervention. Nearly all of these programs were introduced for the first time between 1929 and 1939, there was substantial geographic variation in how they were distributed, and there were great differences in the recoveries of local economies during the course of the New Deal. We have developed a data set that measures the federal spending on over 30 New Deal programs in over 3,000 counties from 1933 to 1939. We use the data to examine how the various types of New Deal programs affected changes in retail sales, a key economic indicator used by New Deal administrators to assess the health of local economies. Retail sales serve as a strong proxy for personal consumption of durable and nondurable goods, which has been considered a key variable in understanding the Great Depression (see Temin 1976, Romer 1990). The impact of the New Deal was likely to show up relatively quickly in the retail sector because the funds going to relief workers and the needy were likely to be used to purchase food, clothing, some durable goods, and other merchandise.

In the process of estimating the impact of the New Deal on local economic activity, we are required to deal with two econometric issues. First, New Deal spending can not be treated as exogenous. New Deal spending was distributed in response to a combination of political and economic factors, including the performance of the local economy. If this potential endogeneity is ignored, the estimates of the impact of New Deal spending will be biased. Second, in focusing on county expenditures we need to pay attention to the potential spatial correlation across counties. In other words, the random economic shocks that affected one county may have spilled over into their neighbors. We, therefore, apply a spatial econometric approach developed by Harry Kelejian and Ingmar Prucha (1998) that combines both instrumental variables and a spatial error correction.

I. A Brief Overview of the Geographic Variation in the Great Depression

The 1930s was a decade of lost output. By 1933 both real per capita GDP and per capita retail sales had fallen to approximately two-thirds of their 1929 peaks. In per capita terms real retail sales returned to its pre-Depression level by 1939, while real GDP returned to its 1929 level by 1940.³ In this paper we use per capita retail sales as our measure of economic activity.⁴ New Deal administrators used retail sales as one of their key measures of the health of local economies, in part because many of the modern indices of economic activity, such as unemployment rates and personal income, were unavailable during the 1930s.⁵ For our purposes here retail sales has the added advantage in that it represents a major component of consumption and the data were reported for every county in the U.S. in 1929, 1933, 1935, and 1939. Retail sales is clearly an important measure of macroeconomic activity because even today the U.S. Bureau of Economic Analysis uses retail sales figures in its interpolations to create annual estimates of personal consumption expenditures of durable and nondurable goods for the National Income and Product Accounts (U.S. Bureau of Economic Analysis 1987, 11). Comparisons of annual national aggregates of retail sales and all personal consumption expenditures over the period 1929 through 1969 show correlations above .99 for the levels of the series (in both real and nominal terms), .96 for first differences of the nominal values, and .91 for real values (U.S. Bureau of the Census 1975, series T79, G416, G417, G418, and E135). Retail sales also are strongly related to personal income. Correlations of state-level per capita personal income and retail sales for the years 1929, 1933, 1935, and 1939 are .87, .89, .88, and .90, respectively.

The national aggregate data disguise quite a bit of variation in the experiences of states and counties across the United States. The ratio of 1939 to 1929 retail sales at the state level ranged from a low of 77 percent in Mississippi to a high of 124 percent in South Carolina. The New England states appear to have had the most success in recovering to their pre-Depression levels as every state had higher real retail sales in 1939 than in 1929. Within many states there was substantially more variation than we see across the states. Table 1 contains information on the distribution of the ratio of 1939 to 1929 retail sales across counties within each state. Texas experienced some of the greatest variation across its

counties as the discovery of new oil fields led to an explosion of economic activity in some counties, while the Dust Bowl and its aftermath contributed to continued Depression in some agricultural counties.

II. New Deal Expenditures, Loans, and Mortgage Insurance

The myriad of economic problems arising from the Great Depression led the Roosevelt administration to develop a variety of New Deal programs, ranging from the building of infrastructure to the regulation of employment, industry, and the financial sector. Our specific focus is on the New Deal programs that distributed federal money in the form of nonrepayable grants, repayable loans, and insurance of mortgage loans. In 1940 the U.S. Office of Government Reports (OGR) compiled a detailed statistical description of the federal government's expenditures in over 3,000 counties for the period March 3, 1933, through July 30, 1939. Most prior research based on the OGR data has made use of the state-level reports. Only recently have scholars begun to use the county-level information.⁶

The federal government distributed \$16.5 billion in nonrepayable grants over the six-year period, provided \$10.4 billion in repayable loans, and the Federal Housing Administration insured \$2.7 billion in mortgage loans. The grants and loans accounted for 61 percent of all federal expenditures during this time span and represented an unprecedented amount of spending during peacetime. For example, the New Deal increased the federal government's expenditures as a share of GDP from about 4 to 8 percent. Furthermore, the federal government began spending large amounts of money where it had spent very little before, setting the stage for a long-term structural shift in the financial responsibilities of the national, state, and local governments.⁷ As a share of government expenditures at all levels, the New Deal raised the proportion of federal spending from 30 percent in 1932 to 46 percent by 1940 (Wallis 1984, 141-2).

The bulk of New Deal grant spending was devoted to relief programs such as the Works Progress Administration (WPA), the Federal Emergency Relief Administration (FERA), the Civil Works Administration (CWA), and Social Security Administration's Aid to the Blind, Aid to Dependent Children, and Old-Age Assistance programs. The primary goal of these programs was to provide

immediate relief to the unemployed and low-income people. As a result, millions of unemployed workers were placed in jobs on projects that ranged from make-work activities to maintenance activities to the building of sidewalks, post offices, schools, local roads, and other additions to local infrastructure.

Public works spending included expenditures by the Public Works Administration (PWA), Public Buildings Administration, and the Public Roads Administration. These programs were administered differently as they focused less on providing immediate work relief in response to greater unemployment needs, but more on building long-term and large-scale projects like dams, roads, schools, sanitation facilities, and other forms of civil infrastructure (Clarke 1996, 62-68; Schlesinger 1958, 263-96). The large scale of the PWA projects and the greater allowance for expenditures on specific materials and personnel meant that the projects had the potential to influence economic activity outside the county where the money was spent.

The primary New Deal federal expenditures designed to aid the farm sector came through the Agricultural Adjustment Administration (AAA). To obtain AAA benefits farmers were required to take land out of production. The impact of the AAA spending on retail sales was likely to be much smaller than the impact of the relief grants and potentially even negative. On the one hand, farm owners might have received higher net incomes from the AAA program. Payments typically exceeded the incomes farmers would have earned on the land they took out of production because the least productive land was removed first and farmers had an incentive to raise yields on the land they kept in production. On the other hand, the AAA might well have had an adverse effect on the incomes of farm laborers, tenants, and sharecroppers. There is evidence that croppers and tenants did not get their full share of the AAA payments on the lands that they had cultivated and that some were demoted to wage laborers (Whatley 1983, Biles 1994, 39-43; Saloutos 1974). Further, by reducing acreage under cultivation and encouraging mechanization, the AAA payments might have reduced the demand for labor and, hence, the incomes of these workers (Alston 1981). Thus, the ultimate impact of the AAA on retail sales in a county depended on whether the increased spending by the actual recipients of the payments was offset the reduced spending of farm workers, tenants, and sharecroppers.

The federal government sponsored a series of loans programs during the 1930s that lent money to banks, railroads, businesses, distressed farmers, agricultural credit institutions, homeowners, and state and local governments for public works, some relief, and public housing projects.⁸ The full impact of the loans on economic activity is difficult to predict. The loans potentially had a limited effect on retail spending because the majority of loans to state and local governments and businesses were short-term and were repaid by the end of the 1930s (Olson 1988). Even in the case of longer-term loans, the borrower may have anticipated repayment and thus increased the collection of taxes or set aside resources to repay the loans. On the other hand, the loans may have had a positive impact on income and retail spending by providing immediate opportunities for the recipients to undertake projects that may have been stalled because of short-term liquidity problems. The actual financial benefit that borrowers received from the federal government is also difficult to determine. It may have come in the form of an interest rate subsidy or in some cases the loans may have been made with the implicit understanding that they would be forgiven or that repayment could be delayed. Unfortunately, the OGR data only record the aggregate amount of loans provided in each county and not the terms of the loans, which could help to predict the loans' impact on the local economies.

The final form of financial support described by the OGR was the value of mortgages insured by the Federal Housing Administration (FHA). The FHA began insuring loans for modernization and repair of housing in August 1934 (Title I) and loans for building new homes or purchasing or refinancing existing homes in 1935 (Title II). By insuring mortgages the FHA sought to give incentives to banks and other mortgage lenders to make more loans that would stimulate the recovery of the building industry (FHA Annual Report 1935, 1-2). In the process of insuring mortgages, the FHA fundamentally changed the nature of home financing by allowing lower down payments and long-term amortized loans. It is important to emphasize that the FHA sought to insure credit-worthy mortgages and rejected a significant number of applications (FHA Annual Report 1938, 13-15). FHA loans tended to help families well into the upper half of the income distribution. The largest number of loans in the late 1930s went to families with incomes in the \$2,000 to \$2,500 range, while average annual earnings for full-time employees in

nearly every sector of the economy were below \$1,500 (FHA 1938, 13; U.S. Census Bureau 1975, 166-67). The FHA allowed relatively high-income borrowers to pay a smaller down payment and to extend the length of the mortgage repayment, thus lowering the monthly payment. Also, by assuming some of the default risk of the mortgage, the FHA may have lowered the interest rates that borrowers would have otherwise paid. Thus, the combination of the institutional innovation and interest-rate subsidy associated with FHA loans may have directly freed up income for consumption. The indirect effects of the FHA-insured loans may have been large if the home construction and remodeling industries were stimulated by the increased liquidity in the mortgage industry. This effect could well have been large because a substantial fraction of the unemployed during the 1930s listed their occupations as construction workers.

As was the case with the recovery in retail sales, there was substantial variation in the extent of per capita New Deal spending across geographic regions. The patterns of New Deal spending across states differed for the various programs (see Table 2). Spending on work relief was over \$100 per person in the largest urban states in the Northeast, Midwest, and in many western states. Relief expenditures were below \$50 per person in many southern states. Per capita public works expenditures were highest in Nevada, Arizona, and Wyoming, and lowest again in the South. Meanwhile, AAA expenditures were highest in agricultural regions, particularly the West North Central region and the Mountain West. The South received substantially higher amounts per capita than did the Northeast, but much less than the amounts spent in the West and the West North Central. Loans were highest in a number of midwestern states, while the per capita value of FHA mortgages ranged from \$83 in California to a low of \$5.40 in North Dakota.

III. Modeling the Impact of New Deal Funds on Retail Sales

Given the absence of county-level data on income and state and local government spending, we have designed the empirical study to obtain reduced-form coefficients that summarize the impact of federal New Deal spending as it affected retail sales.⁹ The coefficients we estimate therefore are the combined effects of a series of structural relationships between New Deal spending and retail sales,

income, private spending on goods produced in the county, exports of goods outside the county, imports of goods from outside the county, private production, and state and local government spending. While we believe it would be interesting to identify the individual effects on these various relationships, data limitations force us to ask the much simpler question, what was the overall impact of New Deal spending on consumption?

Our analysis is based on a model of fiscal federalism suggested by Wallace Oates (1973, 22-29), which itself is based on a standard regional model in which citizens consume a mixture of goods produced inside and outside their counties and export goods to other counties. Since we estimate the New Deal's impact on retail sales using a difference equation to control for unmeasured heterogeneity across counties, we develop this discussion in terms of changes in the relevant variables.

Increases in after-tax income cause increases in the demand for goods (retail sales), which lead to increases in the total revenue of retail sales, assuming an upward sloping or flat supply curve in the retail market. Thus, the relationship between changes in retail sales and changes in after-tax income can be expressed as

$$\Delta R_i = r (1 - t_f) (\Delta Y_i - \Delta T_{i,sl}) \quad (1),$$

where i indexes the county, ΔR_i is the change in per capita retail sales, r is the share of income devoted to retail sales, ΔY_i is the change in per capita income, t_f is the federal income tax rate, and $\Delta T_{i,sl}$ is the change in state and local taxation per capita, which is assumed to be tax deductible for federal tax purposes.

We further assume the identity for the change in county i 's income as the sum of the various forms of spending on final goods and services in the county:

$$\Delta Y_i = \Delta PI_i + \Delta X_i - \Delta M_i + \Delta ND_i + \Delta SL_i \quad (2),$$

where ΔPI_i is the change in private spending on private goods and services produced within the county, ΔX_i is the change in private production exported outside the county, ΔM_i is the change in goods and services imported from outside the county, ΔND_i is the change in federal New Deal spending (or loans

and insurance), and ΔSL_i is the change in state and local government spending. All of these variables are measured in per capita terms.

Both the change in private spending on goods and services produced within county i , ΔPI_i , and the change in goods imported from outside the county, ΔM_i , are influenced by changes in disposable income and in New Deal spending.

$$\Delta PI_i = y (1 - t_f) (\Delta Y_i - \Delta T_{i\text{sl}}) \quad (3)$$

and

$$\Delta M_i = m (1 - t_f) (\Delta Y_i - \Delta T_{i\text{sl}}) + m_s \Delta SL_i + m_n \Delta ND_i \quad (4),$$

where y reflects the share of after-tax income allocated to employing unemployed resources and purchasing local goods in county i and m denotes the share of additional after-tax income that the populace used to purchase goods and services outside the county. Similarly, m_s and m_n are shares of state/local and New Deal spending, respectively, that government decision-makers devoted to purchases from outside the county.¹⁰

Private production of goods sold outside the county, ΔX_i , might also be influenced by changes in New Deal spending, such that

$$\Delta X_i = b \Delta ND_i \quad (5).$$

New Deal spending within county i might have positive effects ($b > 0$) on private production for export if it was devoted to building infrastructure that provided externalities that raised the productivity of private production.¹¹ On the other hand, the effect might have been negative ($b < 0$) if increases in New Deal spending led to reductions in private production. Such reductions, for example, were explicitly built into the AAA grants to farmers who received payment for taking land out of production. Less directly, Wallis and Benjamin (1981 and 1989) found that an extra work relief job crowded out approximately half a job in the private sector.

An influx of federal spending can lead to responses by state and local governments, so we assume the functional relationship

$$\Delta SL_i = c \Delta ND_i \quad (6).$$

The literature on flypaper effects and matching grants suggests that increases in federal spending like the New Deal can lead to increases in state and local spending, which would imply that c is positive.

Alternatively, the literature on crowding out suggests that increases in federal spending might cause state and local governments to reduce their own spending and taxation.¹² In this case, c would be negative.

An important factor that would have influenced the ultimate impact of New Deal spending on state and local spending during the Depression would have been the extent to which they were funded by state and local taxation. As a result, we specify

$$\Delta T_{i,sl} = w \Delta SL_i + a \Delta ND_i \quad (7).$$

Substituting equation (6) into (7) yields

$$\Delta T_{i,sl} = w c \Delta ND_i + a \Delta ND_i = (w c + a) \Delta ND_i \quad (8).$$

The a parameter reflects the extent to which the New Deal funds required matching or repayment. For non-matching, non-repayable grants a would equal 0. The a parameter might equal 1 if New Deal loans required full repayment at market interest rates and local officials anticipated the eventual repayment of the loans in their fiscal decisions. The parameter a would be less than one if the New Deal loans involved an interest subsidy, if there were expectations that the loans might not be fully repaid, or if the term of repayment was lengthened.

State and local governments faced significant legal restrictions in their ability to run deficits during the 1930s. Even when they did, however, the bulk of the bonds were sold in markets outside the local jurisdiction and thus repayment of the debt at market interest rates was likely to be expected. Thus, a balanced budget ($w = 1$) assumption, where increases in state and local spending are matched by increases in taxation, is reasonable. To the extent that state and local governments had more leeway to run deficits and obtain subsidized interest rates, w might be assumed less than one.¹³

After substituting the change in income equation (2) and the other relevant change equations into the retail sales equation (1), we obtain a reduced-form equation representing the effect of the introduction of New Deal spending on the change in retail sales:

$$\Delta R_i = r (1 - t_f) [1/(1 - (1 - t_f) (y - m))] * [1 - m_n + b + (1 - m_s - w) c - a] \Delta ND_i \quad (9).$$

Define Π to be the base multiplier:

$$\Pi = (1 - t_f) / [(1 - (1 - t_f) (y - m))]. \quad (10).$$

The base multiplier increases in value as the share of extra spending on purchases of unemployed local resources and local goods and services (y) produced inside the county rises and as the share of goods and services imported into the county (m) falls. The multiplier is not greatly affected by the federal tax rate on income (t_f), because less than 7 percent of households paid income taxes and federal taxes accounted for roughly 7 percent of national income at the time. Estimates of base multipliers for employment with more recent county-level data suggest values ranging from 1 to 2.5 (Vias and Mulligan 1997).¹⁴ There is the potential for higher estimates during the 1930s because the unemployment rates were at record levels.

Let β be the reduced-form relationship measuring the impact of an additional dollar of New Deal spending in county i on retail sales. That is,

$$\beta = r \Pi [1 - m_n + b + (1 - m_s - w) c - a] = r \mu. \quad (11).$$

The reduced form parameter β summarizes the various channels by which New Deal spending influenced retail sales, and μ is the income multiplier after all influences are accounted for. To get a sense of what might be reasonable values to expect for β and μ , we develop Table 3, which shows how the impact of various forms of New Deal spending on retail sales would be influenced by different assumptions. Although the reduced-form parameter is composed of several underlying parameters, we can make educated guesses about the values of some parameters, while others have very little impact on the predicted value of β . The parameters that have the largest impact on β are the extent to which added income is spent on local versus external goods and services (m and y), the productivity or crowding out effects (b) and the extent of loan repayment (a).

In general, we have a pretty good sense of the following parameter values, none of which are specific to the type of New Deal program. Estimates from national data suggest a ratio of retail sales to personal income of roughly .53, thus we can reasonably assume an r of .53. If we find that an additional dollar of New Deal spending raised retail sales by .53, it would suggest that income in the county rose by approximately one dollar, i.e. an income multiplier (μ) of 1. In Table 3 we assume a slight state and local government deficit, $w = 0.9$, and a state and local crowding out effect ($c = -0.1$). Changes in the c parameter have little effect on the reduced-form coefficient because state and local governments faced restrictions on deficit spending. If state and local governments had balanced budgets and focused their spending on goods and services produced inside the county, $w = 1$ and $m_s = 0$, the estimated coefficient will reflect no flypaper or crowding-out effects. We assume that state and local governments focused their spending on local workers and goods and services, such that only 20 percent of the spending went toward the purchase of goods and services from outside the county. Changes in this assumption also tend to have very small effects on the final reduced-form parameter.

The income multiplier (μ) is the product of the base multiplier, which is common to all programs, and the expression that summarizes the productivity, crowding out, and other effects that typically were specific to each program. The key parameters that determine the base multiplier for all programs are y and m , which measure the extent to which people used additional income to buy goods and services locally or outside the county. In the top part of the table, we show the impact of an additional dollar of disposable income when we assume a 25/75 split between purchases from outside the county and spending on local nontradeable goods and unemployed resources ($y=.75$ and $m=.25$). When combined with a federal tax rate of 7 percent, the base multiplier is roughly 1.74. When the share of income devoted to local spending falls to 50 percent in the bottom panel of Table 3, the base multiplier falls from 1.73 to 0.93.

Given the potential base multipliers, we can now see how the income multipliers change from program to program. We will focus the discussion on the top part of Table 3 where extra income is spent

75 percent locally. Consider the impact of the public works grants. Given the programs' focus on building civil infrastructure, we might expect positive externality effects for private production of as much as 30 percent ($b = 0.3$). On the other hand, roughly 20 percent of public works spending went for materials and we will assume half of the materials were purchased outside the county ($m_n = .1$). These effects would lead to a reduced-form effect in which an added dollar of public works spending raised income by the income multiplier (μ) of 2.10. The ultimate reduced form retail sales effect is obtained by multiplying an r of 0.53 by the income multiplier to obtain a coefficient of 1.11 (see row 1 of the table). If public works spending had a smaller positive productivity effect of $b = 0.1$, as in line 1a in Table 3, the retail sales effect falls to 0.93 and the income multiplier falls to 1.76.

The relief grants probably had relatively smaller productivity effects because the projects were designed to put people to work, not necessarily to build large-scale infrastructure. The relief rules required that the vast majority of funds go to the relief workers, leading us to assume $m_n = 0.05$. If we were to assume relief projects increased productivity by 10 percent ($b=0.1$), the income multiplier is 1.84 and the ultimate reduced-form effect on retail sales would be 0.98. Had the relief grants and public works spending caused crowding-out of private employment of 50 percent ($b=-0.5$ in line 2a of Table 3), similar to what Wallis and Benjamin (1989) found, the income multiplier would fall to 0.80 and the reduced-form retail sales effect would fall to 0.42

Consider the AAA program. If the AAA payments simply replaced the lost income from land taken out of production ($b=-1$), the retail sales coefficient would be 0. However, the additional reductions in the demand for labor and the decline in the economic status of tenants associated with the AAA could have caused b to be even more negative, say -1.2. As we see in Line 3a of Table 3, this would imply a retail sales coefficient of -0.18.

For New Deal loans that were fully repaid from state and local taxation ($a=1$), there would be a slightly negative β coefficient of -0.08. The impact of the loans would not be diminished as much if there were interest subsidies for the loans, local areas anticipated forgiveness on loan repayment, or if the

loans enhanced a state/local government's short-term liquidity ($a < 1$). When a falls to 0.8, the reduced-form retail sales coefficient becomes positive at 0.10.

The FHA funds operated differently from the other programs by insuring private loans and enhancing liquidity in the mortgage finance system. Our measure of FHA funds is the value of private loans insured by the FHA, which is quite different from our measure of other New Deal funds. If we treat the FHA funds as new money that would not have been in the system in the absence of the FHA, we can treat the FHA as we did New Deal loans. Incorporating the interest subsidy, the repayment parameter (a) might be 0.8. In this case, an additional dollar of FHA lending would cause retail sales to grow by .14. However, we expect much stronger positive effects from the FHA funds than from the loans because we know that the FHA loans carried subsidized interest rates and they may have added substantial liquidity to the system.

In summary, the predictions in Table 3 suggest that it is not unreasonable to expect that public works and relief spending might raise retail sales by as much as a dollar for every dollar spent if the internal/external split in spending is 75/25. After dividing this relationship by 0.53, we would obtain income multipliers approaching 2. Adjustments up and down from there would give an indication of the extent to which the programs stimulated private productivity or crowded out private production. AAA spending and New Deal loans were likely to have at best a small positive effect and potentially negative effects on retail sales and income. Finally, FHA insurance was likely to have a relatively small effect.

IV. The Estimation Equation

The Census Bureau collected retail sales information for each county in 1929, 1933, 1935, and 1939. The OGR, however, only reported aggregate New Deal information by program by county for the period March 3, 1933, to June 30, 1939 (February 2, 1932, was the starting point for the RFC Loans). Because we do not have information on New Deal spending for the specific calendar year 1933, our most reliable estimates of the impact of the New Deal will come from an equation estimating the difference in retail sales between 1929 and 1939 because we know that there was no New Deal spending in 1929.

Focusing on the difference between 1929 and 1939 allows us to look at the extent to which the New Deal promoted a return to the pre-Depression 1929 peak in retail spending. To the extent that there was “mean reversion” in retail spending, using the relatively abnormal 1933 spending as a basis for judging the impact of the New Deal would likely produce biased results (we further take up this question below).

Because there was no New Deal spending in 1929, the difference between 1939 and 1929 spending is simply the level of New Deal expenditures in 1939. Moreover, since the precise figures for 1939 expenditures are not available, we substitute the average annual New Deal spending reported by the OGR for the period March 3, 1933, through June 30, 1939. To capture the differences in effects across the different New Deal categories, we include separate variables representing five distinct categories of New Deal spending: relief grants (N_R), public works grants (N_{PW}), AAA grants (N_A), loans (N_L), and insurance of home improvement loans and mortgages (N_I).

Thus, the estimation equation is expressed as the difference in the log value of retail sales per capita between 1929 and 1939:

$$R_{i39} - R_{i29} = N_{Ri}^{avg} \beta_1 + N_{PW_i}^{avg} \beta_2 + N_{A_i}^{avg} \beta_3 + N_{L_i}^{avg} \beta_4 + N_{I_i}^{avg} \beta_5 + (Z_{i39} - Z_{i29}) \delta + D_i \theta + (\varepsilon_{i39} - \varepsilon_{i29}) \quad (12).$$

The β coefficients in the log-difference equation can be read as the percentage change in per capita retail sales over the decade that was associated with an additional dollar of per capita New Deal spending on the various programs in each year. If we multiply the coefficients by the mean value of per capita retail sales in 1929, we can derive an estimate of the dollar-for-dollar impact of New Deal spending on retail sales.¹⁵

To control for market and cultural factors that influenced changes in retail sales from 1929 to 1939 we have included a set of differenced variables – $Z_{i39} - Z_{i29}$ – that we expect would have affected changes in retail purchases. We include the percentage of the population living in urban areas, which is likely to influence income and which also controls for the impact of agglomeration economies for retail stores. The variable also captures differences in the spending patterns of agricultural and nonagricultural areas. To capture similar differences for farm and nonfarm populations in rural areas, we include changes

in the percentage of a county's landmass devoted to farms between 1929 and 1939. Changes in percent black, percent foreign-born, and percent illiterate control for racial, cultural, and educational factors that might have influenced consumption patterns.

There are a variety of unmeasured factors that may have influenced retail sales that did not change over time within each county, but varied across counties. These might include long-term differences in the cost of living, licensing laws for retail sales, the use of retail stores as opposed to home consumption, or local sales taxes. Estimating the equation in differenced form helps to control for this unmeasured heterogeneity.

In the estimation equation we have also included a vector of state dummy variables, D , to capture changes in state policies during the 1930s that were common to all counties in that state but may have varied across states. These changes might have included the introduction of state resale price maintenance laws, state-wide changes in the cost-of-living, state income or sales tax policies, or state policies for relief and public works spending. We know that there were significant changes in all of these factors which suggests that it is worthwhile to include the state dummy variables.

With some degree of measurement error, we can also examine the initial, short-run effects of the First New Deal (1933-1935) by estimating a difference equation for retail sales between 1929 and 1935. Although the OGR only reported aggregate New Deal spending from 1933 to 1939, we can separate the overall New Deal spending into the First New Deal (1933-1935) and the Second New Deal (1936-1939) using reports from the various programs on their activities as well as state-level spending data which are available annually.¹⁶ With much higher levels of measurement error, we can also examine the impact of the New Deal from 1933 and 1939 and, thus, focus more closely on the impact of the New Deal from trough to recovery. The measurement error comes primarily from our inability to precisely delineate spending in 1933 because the starting date for the aggregate county data is March 1933, an issue that we will discuss further in the results section. Therefore, in our analyses below we focus on the 1929 to 1939 and 1929 to 1935 difference-equation results, but report the results of a 1933 to 1939 analysis for completeness.

IV.1 Instruments for the New Deal Variables

Estimating the impact of New Deal spending on changes in economic activity is complicated by potential simultaneity bias. New Deal administrators, like Harry Hopkins, argued that they responded to continuing slumps in economic activity and used retail sales as one of their measures to gauge the economic climate in particular areas. Therefore, this endogeneity would imply a negative relationship between the change in retail sales and New Deal spending. On the other hand, state and local governments often proposed projects and in some cases were required to provide matching spending. Counties with greater economic activity may have had more resources at their disposal for attracting federal matching grants, thus imparting a positive relationship between economic activity and New Deal spending.¹⁷ We performed a series of Hausman tests that could not reject the hypothesis that each of the New Deal spending variables was endogenous.

To eliminate the simultaneity bias we develop instruments for the average annual amount of New Deal spending for each program. Our goal is to find variables to use as instruments that were not caused by variations in average New Deal spending over the period from 1933 to 1939 and that were not caused by changes in retail sales from 1929 to 1939. There is already an extensive literature on the factors determining the geographic distribution of New Deal funds across states. This literature, therefore, can be used as the basis for generating a set of variables that could be used as instruments for the distribution of New Deal spending across counties.¹⁸ Robert Fleck (1999b) followed a similar procedure in his study of the impact of relief programs on county unemployment statistics.

Previous research has suggested that presidential politics was an important determinant of how New Deal spending was distributed. To capture various aspects of the presidential influence, we include the following variables: mean percentage voting for the Democratic presidential candidate from 1896 to 1932 as a measure of long-term loyalty to Democrats; the standard deviation of the percent voting for the Democratic presidential candidate from 1896 through 1932 as a measure of the volatility of that support; the difference between the percentage voting for Roosevelt in 1932 and the mean Democratic vote from 1896 through 1932 as a measure of the support Roosevelt received from swing voters; the percentage of

the adult population that turned out to vote in 1932; and the state's electoral votes weighted by the county's share of the state population as a measure of the importance of the county in the electoral college.¹⁹ Anderson and Tollison (1991) suggested that congressmen in key positions could influence the distribution of New Deal funds, so we include dummy variables reflecting whether the county's representatives sat on key committees in the House of Representatives in the spring of 1933. The committees include Agriculture, Appropriations, Banking and Currency, Flood Control, Irrigation, Labor, Public Buildings, Public Lands, Rivers and Harbors, Roads, and Ways and Means. These political variables are all factors that might have directly influenced the distribution of New Deal spending, but would not have directly affected retail sales. Nor would these political factors have been directly caused by variations in New Deal spending because all are measured prior to the distribution of the New Deal funds that began after the Congressional committees were set in 1933. The state dummy variables control for the political influence of a state's senators and each state's attitude toward New Deal spending more generally.

Previous research suggests a series of structural variables that might also have influenced New Deal spending. Wallis (1998) argued that New Deal administrators tried to insure a base level of spending in every jurisdiction, so when the model is converted to per capita spending this base parameter becomes the coefficient of the inverse of population. Fleck (2001b) and Wallis (2001) have discussed the inclusion of a measure of land area per capita. Along with these two variables, we include a series of variables to control for structural differences in the distribution of New Deal funds, including the percentage of land in farms in 1929, and percent urban, percent illiterate, percent black, and the percent foreign born, all in 1930. These factors were unlikely to have been caused by changes in retail sales between 1929 and 1939 and could not have been caused by the introduction of New Deal spending because they preceded the federal program. The structural economic and political variables have been found to be important determinants of the distribution of New Deal funds and R-squareds calculated from the first-stage regressions of the two-stage-least-squares procedure are all above 0.29.²⁰ Appendix Table 1 reports the coefficients from the first-stage regressions.

IV.2 Controlling for Spatial Autocorrelation

We cannot treat each county economy in isolation from the rest of the U.S. because local economies tend to be integrated. In the context of our model, these interactions are likely to create correlations among the error terms associated with the county observations. If one interprets the usual regression error as an unmeasured economic shock to an individual county, then it would be reasonable to envision that these shocks would have been correlated across U.S. counties that were in close proximity. In the spatial economic literature these correlations manifest themselves in the covariance structure of the regression error and result in estimation inefficiency when ignored. Additionally, the usual standard errors may be unreliable. Therefore, to ensure the efficiency of our estimates and validity of our test statistics, we employ a method of moments estimator developed by Kelejian and Prucha (1999) that accounts for spatial interactions in the error structure.

For notational simplicity let the error term in equation (12) be $\varepsilon_i = \varepsilon_{i39} - \varepsilon_{i29}$. In the context of Kelejian and Prucha (1998) a spatially correlated error term is then:

$$\varepsilon_i = \rho g_i(\varepsilon_j, j \neq i) + u_i, i = 1, \dots, n \quad (13),$$

where u_i is a zero-mean disturbance with variance σ^2 . The ρ is a scalar autoregressive parameter. The function g_i is called a “spatial lag” and is typically based on some geographic proximity measure such as physical distance. For example, economic shocks in neighboring counties j and k may have had an effect on the economic shock in county i , but the effects of the shock may have been stronger for neighboring county j than for county k if county j was closer to county i than county k . Moreover, if county i were in New York and county k in California, economic circumstances in county k might not have had any effect on county i . To parameterize a spatial lag, one typically assumes:

$$g_i(\varepsilon_j, j \neq i) = \sum_j^n \alpha_{ij} \varepsilon_j, i = 1, \dots, n \text{ where } \sum_j^n \alpha_{ij} = 1 \text{ and } \alpha_{ii} = 0 \quad (14).$$

In other words, the shock experienced in county i is a weighted average of the shocks ε_j in nearby counties. The requirement that $\alpha_{ii} = 0$ ensures that shocks in the county of interest i are not spatially

correlated with itself and the requirement that the α_{ij} sum to one is a normalization so that relative (and not absolute) relationships between counties matter. We ex ante select the α parameters based on geographic contiguity, which is but one commonly accepted parameterization in the spatial analysis literature. For example, Dowd and LeSage (1997) suggest that geographic contiguity parameterizations may be useful in analyzing price level variation across U.S. states.

For a weighting scheme based on contiguity, let

$$\alpha_{ij} = \frac{1}{\sum_j \alpha_{ij}} \text{ if the } i^{\text{th}} \text{ and } j^{\text{th}} \text{ counties are contiguous, } \alpha_{ij} = 0 \text{ otherwise} \quad (15).$$

This commonly used weighting scheme implies that if county i , for example, has 8 contiguous neighbors then each of the neighboring counties would receive a weight of one-eighth in the error-correction procedure. For our county-level data the distribution of the number of neighbors under the contiguity scheme was highly symmetric. Across the 3,059 counties analyzed, the median number of contiguous neighbors was 6, while the average was 5.9. The maximum number of contiguous neighbors was 14, while the minimum was 1.²¹

Stacking observations over i , the error process can be written more compactly as:

$$\varepsilon = \rho W \varepsilon + u \quad (16),$$

where W is an $(n \times n)$ spatial weighting matrix, consisting of typical element

$$w_{ij} = \frac{\alpha_{ij}}{\sum_j \alpha_{ij}}, \quad i, j = 1, \dots, n \quad (17),$$

and ε and u are $(n \times 1)$ unobserved vectors. Finally, stacking observations in equation (12), the entire system can be written as:

$$\begin{aligned} dR &= N_R \beta_1 + N_{PW} \beta_2 + N_A \beta_3 + N_L \beta_4 + N_I \beta_5 + dZ \delta + D \theta + \varepsilon \\ \varepsilon &= \rho W \varepsilon + u, \end{aligned} \quad (18),$$

where $dR = R_{39} - R_{29}$, $N_R, N_{PW}, N_A, N_L, N_I, \varepsilon, u$ are $(n \times 1)$, $dZ = Z_{39} - Z_{29}$ is $(n \times k)$; D is $(n \times 47)$; θ is (47×1) , δ is $(k \times 1)$, and the rest of the parameters are scalar. Under suitable conditions outlined in Kelejian and

Prucha (1999) and satisfied here, a generalized two-stage least squares (G2SLS) procedure produces consistent estimates of the model's parameters (see the Estimation Appendix for details on the estimation procedure).

V. Estimation Results

Table 4 shows the estimated coefficients for the 1929 to 1939 difference equation using various estimation procedures. We show the regression results using OLS, 2SLS without a spatial error correction, and generalized 2SLS estimation with spatial corrections of the error term. The coefficients for the economic structural determinants of retail sales seem consistent with expectations. Per capita retail sales were generally higher in urban areas, which is consistent with normal patterns of income and agglomeration economies for urban areas. Greater illiteracy in a county had a strong negative influence on retail sales. A higher percentage of the population that was black or foreign-born led to lower retail sales, although the effects are not precisely estimated.²²

The comparisons of the OLS, 2SLS, and G2SLS coefficients and t-statistics in the table show clearly that using an instrumental variables approach leads to substantial changes in the interpretation of the impact of New Deal programs. The OLS estimates suggest that the public works programs and the FHA insurance programs had positive effects on the growth rate of retail sales, while relief spending, loans, and the AAA farm programs had statistically significant and negative effects. When we control for the endogeneity of the New Deal spending variables using the 2SLS and G2SLS models, however, we see a substantial increase in the impact of public works spending and a strong positive effect of relief spending. Because we believe that controlling for both endogeneity and spatial autocorrelation is important, we will focus our discussion on the G2SLS results in the remainder of the analysis.

Each of the New Deal program categories had quite different effects on depressed local economies in the 1930s. The programs with strong positive and statistically significant effects were the nonrepayable public works and relief grants. FHA insurance had a strong positive effect, but we cannot statistically reject the hypothesis that the effect was zero. AAA spending did virtually nothing to raise

retail sales and loans had a negative effect. The G2SLS coefficient for the public works grants in Table 4 suggests that an additional per capita dollar in public works grants was associated with an increase in per capita retail sales of approximately 0.23 percent. When this percentage increase is multiplied by average real retail sales in 1929 of \$542, the coefficient implies that the additional dollar of New Deal public works spending increased retail sales by \$1.25. Given that the typical ratio of retail sales to income was roughly .53, the coefficient suggests that a dollar increase in public works spending per capita was associated with a rise in per capita income of \$2.36. We report the implied income multipliers from the regression coefficients in Table 5.

Similarly, an additional dollar of relief spending raised the growth rate of retail sales by 0.169 percent, or \$0.92. This translates into an income multiplier of 1.79, as reported in Table 5. The public works and relief multipliers are both on the higher end of the range of employment multipliers (1 to 2.5) that Vias and Mulligan (1997) found in their studies using modern county-level data. We might expect the multipliers during the New Deal to have been relatively higher than modern ones because of the extraordinary unemployment rates during the Depression. The income from public works and relief projects had much greater prospects for stimulating local economies when unemployment was high and when there were disproportionately high numbers of discouraged workers.²³ The actual multipliers might have been still higher had it not been for the relief jobs' partial crowding-out of private employment (Wallis and Benjamin 1989). Our sense is that the public works spending had a stronger effect because it was more focused on building infrastructure that enhanced the productivity of the private economy than relief spending, which was more prone to employ workers in make-work activities.²⁴

The AAA program offers a sharp contrast to the public works and relief programs. The AAA coefficient in Table 4 is very small and statistically insignificant. An additional dollar of AAA spending raised retail sales growth by 0.016 percent, which implies an income multiplier of 0.16 (see Table 5). The small effect is not surprising. First, AAA funds were paid to farmers with the explicit goal of taking land out of production. Thus, a dollar of AAA benefits was offset by the value of foregone output on the land taken out of production. Although descriptions of the AAA suggest that on net the recipients benefited

from accepting the payments. Second, the AAA-required reduction in land under production led to a decline in the demand for labor that would have had an adverse effect on farm workers' incomes (Alston 1981). Finally, the negative effects of the AAA were further exacerbated to the extent that tenants did not receive their expected shares of AAA payments and were demoted to wage labor (see Saloutos 1974, Whatley 1983). In the final analysis, the impact of the AAA was determined by whether the gains in income to AAA recipients outweighed the losses to farm workers. Our results suggest that they did not.

We had anticipated that the impact of the New Deal loan programs was likely to be small because many of the loans to state and local governments were either repaid during the 1930s or required repayment at a later date. The actual repayments or anticipated repayments were likely to impose a drag on state and local spending because they would have limited the governments' ability to run deficits or issue debt in national credit markets. We were surprised to see a strong negative effect of New Deal loans on the county's retail sales. The G2SLS coefficient in Table 4 suggests that for each additional per capita loan dollar, retail sales shrank by 0.26 percent. Or, according to the income multiplier calculated in Table 5, income fell by \$2.6. The strong negative effect might have been driven in part by our use of the difference between 1939 and 1929. By 1939 many of the New Deal loans were likely to be in the repayment phase, which would have led to a reduction in 1939 income. We explore this issue further below by comparing 1935 to 1929.

The value of FHA-insured home mortgages had a strong, although statistically insignificant, effect on retail sales. On a per capita basis, an additional dollar of loans insured by the FHA was associated with a 0.35 percent rise in retail sales, which translates into an income multiplier of 3.5 (see Table 5). It would not be surprising if the FHA-insured loans stimulated the economy by jump-starting new home construction and remodeling activity because all of the Title I home repair and modernization loans were for specific projects and half of the FHA's Title II home mortgages were for new homes (FHA Annual Report 1937, 27). The impact of the new loans on the building sector was likely to have been strong because a significant proportion of the unemployed had been in the building sector.

It is important to note that FHA loans were private loans and the primary role of the federal government was to insure the lender against default risk. The question naturally arises: How much credit should the FHA program receive for the resulting expansion in economic activity? The estimates above provide an upper-bound measure of the impact of the FHA program. Given that the FHA monitored the risk of the loans that they accepted for insurance very carefully, a substantial percentage of the loans that were insured would likely have been made in the absence of the FHA program. The government's share of the credit might be as small as its cost of insuring the loans, which would have been in the range of 1 to 5 percent of the value of the loans. On the other hand, given the moribund state of the mortgage market in 1933, the FHA's reorganization of the standard mortgage terms and its insurance of billions of dollars worth of loans might have jump-started the mortgage and home modernization industry.

Because the loans and the FHA insurance were so different from the grants, we have also explored estimations where we exclude them from the analysis. The G2SLS results for public works and relief spending do not change much when the loans and FHA insured loans are eliminated. The income multipliers implied by the coefficients become 2.06 and 1.83, respectively. Meanwhile, the effect of the AAA becomes negative with an income multiplier of -0.6 .²⁵

V.1 Estimates of the Short-Run Effects: 1929 to 1935 Difference Equations

The 1929 to 1939 difference equations might miss some of the important short-run effects of the New Deal because we have used average New Deal spending over the entire period to infer the spending in 1939. The First New Deal from 1933 to 1935 was a response to the depths of the Great Depression, so the initial impact of the various programs might have been greater from the effects we measured for the change from 1929 to 1939. We have developed county-level estimates of New Deal spending through the end of 1935 using information on the timing of the various programs and using some state-level information from the OGR on federal spending on programs that extended beyond the end of 1935 (see Data Appendix). There is some measurement error in this approach because in some cases it is difficult to precisely time the beginning and end of some programs and the use of annual state-level data to impute

values for the counties is somewhat rough. We are confident about spending in the base year, however, because the New Deal values in 1929 were zero.

In Table 6 we report the results of the 1929 to 1935 log-difference equation using G2SLS, which includes the spatial error correction. We have also reported the associated multiplier estimates in Table 5. The results are similar to those reported for the 1929 to 1939 difference equation in Table 4. Public works and relief spending again have positive and statistically significant effects. The relief spending coefficient is roughly the same, with an implied income multiplier of 1.5. The public works coefficient, on the other hand, is cut in half and the income multiplier is 1.3. The smaller public works coefficient might be expected because many of the public works projects were still in the building phase by 1935 and, thus, their effects on economic productivity prior to completion were likely to be smaller than they would be in 1939. It appears that the AAA in the early period had a more negative effect than it did for the entire New Deal, as the coefficient is -0.0004 and statistically significant. The strong negative result could be related to the problems tenants faced in obtaining their share of the AAA proceeds. Warren Whatley (1983) suggests that tenants did not receive their proportionate shares of the first AAA payments, but fared better under the version of the AAA established after 1935. Thus, the problems engendered in the farm labor markets may have been more severe in the earlier period. Finally, the loans had a less negative effect in the 1929 to 1935 period than they did in the 1929 to 1939 period. Our sense is that the negative effects of lending might have been greater by 1939 because the loans were more likely to be in repayment than they were in 1935.

V.2 Estimates of Trough to Recovery

By using 1929 as the base year for the difference equations, the analysis focuses on how the New Deal influenced a county's return to its pre-depression peak in retail sales. Between 1929 and 1933 the economy experienced a stunning downturn and the extent of this downturn varied across counties. The correlation coefficient for per capita retail sales in 1929 and 1933 is .84, so we know that the relative positions of the counties differed to some degree when the comparison point is 1933 rather than 1929.

We therefore estimate another set of difference equations using 1933 as the benchmark year.

Investigating the change in retail sales from 1933 to 1939, however, introduces significant measurement error as we attempt to determine the level of New Deal spending in 1933. The measurement error comes primarily from attempts to parse the aggregated county-level New Deal information from July 1, 1933, through June 30, 1939 into 1933 and 1934-1939 spending. If one can accept that the initial effect of any New Deal spending would not be felt for approximately 6 months, problems with measurement error might not be severe. Except for RFC loans, very few of the New Deal funds were distributed prior to July 1, 1933, so that we can effectively substitute zero for all forms of New Deal spending during the fiscal year July 1932 to June 1933.²⁶ Allocating the RFC loans between 1933 and later years is much more difficult because the national totals show that 40 percent of the RFC loans awarded from February 1932 to June 1939 were handed out prior to June 1933. For that reason, and because much of the early RFC activity was associated with the Hoover administration, we have tried estimating the 1933 to 1939 model both with and without the RFC loans. Because the results are similar in either case, we report only the results with the RFC loans included. We should emphasize, however, that if New Deal spending had an immediate impact on retail sales, then our inability to precisely account for spending in the latter half of 1933 introduces significant noise in the New Deal variables.²⁷

In examining the change from 1933 to 1939, we also may need to explicitly control for the extent of the downturn from 1929 to 1933. Both Bernanke and Parkinson (1989) and DeLong and Summers (1988) suggest that mean reversion was a feature of the 1930s recovery. If there is reversion to the mean, then we might expect a stronger bounce-back effect for counties that had larger drops in retail sales during the Depression. It is useful to test this hypothesis because Romer (1992), in her study of the impact of monetary growth on national aggregate measures of real GDP, finds that mean reversion and such bounce-back effects were not significant in the 1930s.

To control for the extent of the downturn, we include the log difference in retail sales between 1929 and 1933 as a right-hand side variable in two ways. In one equation we treat the downturn as exogenous. In another version, we assume the downturn was endogenous and developed instruments for

the 1929-1933 downturn variable. In addition to the list of instruments described earlier, we include log 1929 retail sales per capita, the level of unemployment in 1930, and 1929 tax returns divided by population as instruments for the log difference in retail sales from 1929 to 1933. All of these factors are correlated with the downturn variable but would not be correlated with the error term of the 1933-1939 difference regression because these variables all precede 1933.

Table 6 shows the results of the 1933 to 1939 difference equation without the downturn variable, with it included as an exogenous variable, and then as an endogenous variable. The results suggest that a dollar increase in relief spending increased retail sales growth by 0.2 percent, which implies an income multiplier of about 2.2. The public works coefficient is smaller in all specifications than the estimates for 1929 to 1935 and 1929 to 1939 and the statistical significance of the effect is sensitive to how we treat the downturn variable. Meanwhile, the AAA coefficient is more positive than those found in the 1929 to 1935 and 1929 to 1939 estimations. The loan and FHA effects are small and statistically insignificant under all specifications. Our results are inconclusive as to the impact of the downturn on subsequent growth. When the downturn is treated as exogenous, there is a strong bounceback effect, such that a one percent decline from 1929 to 1933 is associated with an increase in the 1933 to 1939 growth of 0.416 percent. On the other hand, when we use instruments for the downturn, we get a much smaller bounceback effect that is not statistically significant.

VI. Conclusion

The New Deal represents a classic experiment in which the federal government tried a variety of different approaches to stimulate the depressed economy. The federal government handed out substantial grants to state and local governments to build large-scale public works, to provide employment on smaller-scale public works, and to provide direct relief. It distributed monies to farmers to change their production patterns. It made loans to state and local governments, to homeowners, to banks, to farmers, and to various industries. It attempted to stimulate the housing sector by insuring mortgages and home improvement loans. The federal government spent money on an unprecedented scale, which makes an

analysis of the role of the New Deal somewhat easier because we are not trying to tease out the impact of small fiscal changes on a large, robust economy.

Our results suggest that the economic effects of the various forms of New Deal spending were quite different. In fact, these contrasts help to answer questions about today's political debates about the role of fiscal policy. The finding that is most robust to alternative specifications and estimation procedures is that the public works programs that built large-scale civil infrastructure projects had strong positive effects on the economy. At the margin, an additional dollar spent on dams, roads, schools, and buildings by the PWA, PRA, and PBA had an income multiplier over two for the entire decade of the 1930s. The short-term effects of the public works projects through 1935 were somewhat smaller, which suggests that some of the most dramatic effects of these projects were not felt until completion when they were able to stimulate productivity in the private sector. The impact of the relief programs on the growth of retail sales over the 1929-1939 decade was more sensitive to the choice of specification. Our best estimate, which includes controls for a variety of factors that influenced retail sales and the potential endogeneity of New Deal spending, suggests that the relief programs also had strong multiplier effects of around 1.7. Both relief and public works spending provided employment and income for millions of workers, which in turn appears to have stimulated other economic activity. In addition, the programs to varying degrees may well have stimulated long-term productivity by building roads, dams, and other physical capital that contributed to higher productivity in the private sector. These grant programs had much stronger positive effects than the loan programs, probably because the actual and anticipated repayment of the loans from state and local governments and private borrowers to the federal government limited their impact.

The federal spending on the AAA program, the basis for our modern farm programs, had at best a small positive effect on local economies and possibly a negative effect. The AAA payments required farmers to take land out of production, so the net increases in income were much smaller than for the relief and public works spending. A substantial literature on the AAA suggests that non-landowners at the lower end of the agricultural income distribution suffered declines in income as a result of the AAA.

Our results are consistent with this view, as they suggest that the decline in retail sales from lowering the incomes of farm laborers outweighed the rise in retail sales expected of the farm owners who received most of the payments.

The Federal Housing Administration's insurance of home mortgages and home improvement loans also may have contributed to stimulating local economies. Our point estimate of the multiplier effect is relatively large, but we cannot reject the hypothesis that there was no effect. The FHA insurance program had the potential to stimulate a construction industry that had experienced a greater downturn than almost any other industry. This should be seen as an upper bound for the impact of government insurance. The loans themselves originated in the private sector and the FHA was very careful to lend to low-risk borrowers, so only a fraction of this effect can be attributed to FHA insurance. But on the margin, by insuring mortgages the FHA enabled lenders to lower the interest rates on their loans which, in turn, helped stimulate new activity in the housing construction sector. It is clear that FHA insurance led to a fundamental institutional innovation in mortgage credit by lengthening the amortization of the loans and altering the terms of the down payment. Further investigation of the specific impact of the FHA on housing values, rents, and homeownership rates is needed before we can fully establish the impact of the changes wrought by the FHA program.

The New Deal encompassed a wide range of programs, each designed to accomplish different ends. The results in this paper seem to highlight a variety of general results for different types of federal spending. The programs with the strongest impact on the economy were the large-scale public works projects. Such projects built physical capital that appears to have raised the overall productivity of the private sector and the local public sector in ways that offset any crowding out. The relief programs focused more on providing immediate relief to the unemployed and less on the end project. As a result, it seems that the economic displacement that federal spending can cause was greater for the relief programs than it was for the public works programs. The impact of the AAA program offers lessons that parallel the modern experience with foreign aid programs. The AAA funds restricted production and were distributed primarily to agricultural elites. The structure of the AAA altered landowners' incentives in

such a way that income was shifted from laborers at the lower end of the skill distribution to the owners. In this particular case, it appears that the redistribution of income engendered by the AAA payments actually served to retard the recovery of the local retail sector. Finally, the success of the FHA suggests that relatively small amounts of spending that reduce uncertainty and information costs can reap substantial rewards. Simply showering money on local economies was not enough to stimulate their recovery, the most successful programs of the New Deal helped the private sector function more smoothly or enhanced the productivity of private enterprise.

ESTIMATION APPENDIX

The generalized two-stage least squares (G2SLS) procedure to estimate equation (13) involves the following steps:

1. Simultaneity Correction: Using the linearly independent columns of $H = [Z^*, D]$ as instruments, we perform 2SLS on the retail sales equation, ignoring the spatial lag in the error term, where Z^* is an appropriately selected subset or variables from the exogenous Z (see instrument discussion in section IV.1). That is, let $Y = [N_R, N_{PW}, N_A, N_L, N_I]$ and calculate $\hat{Y} = Y P_H$, where $P_H = H(H'H)^{-1}H'$. Then, using \hat{Y} for Y on the RHS, perform least-squares on the log-difference retail sales equation.²⁸
2. Estimation of ρ : Defining the usual 2SLS residuals:

$$e = dR - N_R \hat{\beta}_1 - N_{PW} \hat{\beta}_2 - N_A \hat{\beta}_3 - N_L \hat{\beta}_4 - N_I \hat{\beta}_5 - Z \hat{\delta} - D \hat{\theta}$$

calculate:

$$\Omega = n^{-1} \begin{bmatrix} 2e'e & -\bar{e}'\bar{e} & n \\ 2e'e & -\bar{e}'\bar{e} & tr(W'W) \\ (e'\bar{e} + \bar{e}'e) & -\bar{e}'\bar{e} & 0 \end{bmatrix}$$

and

$$\omega = n^{-1} [\bar{e}'\bar{e}, \bar{e}'\bar{e}, \bar{e}'\bar{e}],$$

where $\bar{e} = We$ and $\bar{e} = W\bar{e}$. These calculations are based on three moment conditions implied by the

error structure (see Kelejian and Prucha (1999) for an explanation). Define $\theta' = [\rho, \rho^2, \sigma]$. A

consistent estimate of ρ can be calculated by solving the non-linear system:

$$[\tilde{\rho}, \tilde{\sigma}] = \arg \min_{\rho, \sigma} [\omega - \Omega\theta]'[\omega - \Omega\theta].^{29}$$

3. Generalized 2SLS: Pre-multiply the retail sales equation by $(I_n - \tilde{\rho}W)$. G2SLS proceeds by performing ordinary 2SLS on the transformed equations.

DATA APPENDIX

Retail sales information from 1933 and 1935 is from U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce, 1936 and 1939. The 1929 and 1939 retail sales information is from Historical, Demographic, Economic, and Social Data: The United States, 1790-1970, ICPSR tape number 0003, as corrected by Michael Haines. The population figures used to create our per capita estimates for 1929, 1933, 1935, and 1939 retail spending were calculated using linear interpolations of the 1930 and 1940 populations. We calculated 1929 population as 1930 minus the average change in population between 1930 and 1940; we did not use trends from 1920 to 1930 due to changes in county boundaries between 1920 and 1930. New Deal spending information is from the Office of Government Reports, 1940. The changes in percent black, percent urban, percent of land on farms, and percent illiterate for 1930 and 1940 are from the 1930 and 1940 files in ICPSR tape 0003 as corrected by Michael Haines. The census reported illiteracy for people aged 10 and above in 1930 and then reported the number of years of school completed for people aged 25 and up in 1940. For 1930 we used the illiteracy rate for people aged 10 and above. We then estimated illiteracy rates for 1940 for people aged 25 and over. We used 1947 information from the U.S. Bureau of the Census (1948, 7) to find the number of people over 24 years old with no schooling and those with 1 to 4 years of schooling. The illiteracy rate in 1947 for persons with no schooling was 78.2 percent for male and 80.72 percent for females. The illiteracy rate for 1-4 years of schooling was 22.5 percent for males and 16.68 percent for females. We assumed those with more than 5 years of schooling were all literate. U.S. Bureau of the Census, "Illiteracy in the United States, October 1947," Current Population Reports: Population Characteristics, September 22, 1948. Series P-20 no. 20.

The key instruments for the New Deal endogenous variables are the presidential voting variables: the mean Democratic share of the presidential vote from 1896 to 1932; the percent voting for Roosevelt in 1932 minus the mean Democratic share from 1896 to 1932; the standard deviation of the Democratic share of the presidential vote from 1896 to 1932; electoral votes per capita; and the percent of adults voting in 1932 were all calculated using information from ICPSR's United States Historical Election Returns, 1824-1968 (ICPSR tape number 0001). The variables measuring representation on House committees between 1933 and 1938 are from U.S. Congress, Official Congressional Directory, for the 73rd Congress 1st session through 76th Congress. We then matched the congressional information with the counties. When we estimated the equations for the 1935-1929 difference we used the committee memberships during the period 1933 and 1934.

Other instruments for the New Deal variables from the literature on the determinants of New Deal spending include the state dummies, the inverse of the population in 1930, percent of land on farms in 1929, and the percentages urban, black, foreign-born, illiterate and rural nonfarm in 1930. When we estimated the equation for the 1939-1933 difference in one of the specifications we included the percent unemployed in 1930 and per capita tax returns in 1929 and retail sales per capita in 1929 as instruments for the change in log retail sales between 1929 and 1933. All but the tax return information was found in the ICPSR tape 0003 as corrected by Michael Haines or from the Office of Government Reports (1940). The tax return information comes from U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce 1932.

All monetary variables in our analyses were translated into 1967 dollars using the Consumer Price Index (CPI) (Bureau of Census, 1975, series E-135, pp. 211-2). For the New Deal funds, we used the average annual CPI over the period 1933 to 1939 (0.412), and 1933 to 1935 (0.4). After the substantial deflation from a peak CPI of .513 in 1929 to a trough of .388 in 1933, the CPI then bounced around between .4 and .43 between 1934 and 1939 (see also Romer, 1999).

The data set consists of 3,059 counties and county/city combinations in the United States. The New Deal program information was reported for some combined counties. For example, the New Deal information was reported for all of New York City. Thus, in New York state, Bronx, King, New York, Queens, and Richmond counties were combined into New York City. Similar situations developed in other states. In Missouri St. Louis City and County were combined. In Virginia we combined the

following districts that were reported separately in the Census: Albemarle county and Charlottesville city; Allegheny county and Clifton Forge city; Augusta county and Staunton city; Campbell county and Lynchburg city; Dinwiddie county and Petersburg city; Elizabeth City county and Hampton city; Frederick county and Winchester city; Henrico county and Richmond city; Henry county and Martinsville city; James City county and Williamsburg city; Montgomery county and Radford city; Nansemond county and Suffolk city; Norfolk county with Norfolk city, South Norfolk city; and Portsmouth city; Pittsylvania county and Danville city; Prince George county and Hopewell city; Roanoke county and Roanoke city; Rockbridge county and Buena Vista city; Rockingham county and Harrisonburg city; Spotsylvania county and Fredericksburg city; Warwick county and Newport News city; Washington county and Bristol city; Arlington county and Alexandria city. A small number of counties were dropped from the sample due to missing values for the variables above. Finally, we used maps from the 1930s to determine which counties were contiguous to each other. This information was used to create the weighting matrix needed to control the spatial autocorrelation.

In estimating the model for the 1929-1935 difference we divided the information into the First and the Second New Deals. For the annual average New Deal spending up to 1935 we used the following assumptions in splitting the spending into 1935 and earlier and 1936 and later. The OGR reported county data for the AAA spending separately for the rental and benefit payments from 1933 through 1935 and for the conservation payments in 1936 and 1937, so we used only the Rental and Benefit payments for 1933 through 1935. Under relief spending, the Civil Works Administration was in place from November 1933 through March 1934 and thus can be placed in the early period. Similarly, the Federal Emergency Relief Administration ran through June 1935 with a very small amount of spending flowing over into the calendar year 1936 and 1937 to wind down some programs. Because the WPA did not start until July 1935 and less than 6 percent of the employment occurred in 1935 (see Federal Works Agency, 1940, p. 413), we did not include the WPA spending in the 1935-1929 regressions. All Social Security Administration spending for Aid to the Blind, Old-Age Assistance, and Aid to Dependent Children occurred after January of 1936, so it was not included in the earlier period's regression. The Federal Housing Administration insured loans under Title I for modernization and repair of housing in 1934 when the program started and 1935 accounted for 26.3 percent of the value for the 1934-1939 period. FHA-insured loans under Title II for building new homes or refinancing existing homes and multi-family homes in 1935 (the first year of the program) accounted for 4.6 percent of the loans from 1935 through 1939 (Federal Housing Administration 1941, p. 7). We used these percentages to determine the estimated value of loans for 1933 through 1935 in each county. For public works expenditures we used information from the OGR's Report number 9 for the states to determine the percentage of spending by the Public Works Administration, Public Roads Administration, and Public Buildings Administration that was spent prior to June 30, 1935, for each state and then applied those percentages to the counties in the state. For the loan programs, we were able to determine that the Farm Credit Administration's Drought Relief Loans program was finished by the end of 1935, and the Rural Electrification Administration, U.S. Housing Authority, and Farm Security Administration's Tenant Purchase loans were not in place until 1936 or later. For the Reconstruction Finance Corporation, the Home Owners' Loan Corporation, the PWA loans, and the remaining Farm Security Administration and Farm Credit Administration loans, we used information on the loans by state from the OGR's Report Number 9 to determine the percentage loaned prior to June 30, 1935.

FOOTNOTES

¹ Macroeconomic investigators Brown (1956) and Peppers (1973) dismiss the New Deal as a true example of Keynesian fiscal stimulus on the grounds that the federal government did not run budget deficits of the size that a Keynesian would have deemed necessary to offset the large gap in unemployment. Bernanke and Parkinson (1989) and DeLong and Summers (1988) suggest that much of the recovery in the latter half of the 1930s was the result of the natural tendency of the economy to return to its steady state. Meanwhile, Romer (1992) finds that the recovery after 1933 was primarily the result of monetary expansion.

² On employment, see Wallis and Benjamin (1981, 1989), Margo (1993), Sundstrom (1995), and Fleck (1999b). For the impact of the Reconstruction Finance Corporation (RFC) on the banking sector, see Mason (1995). Cole and Ohanian (2001) have considered the impact of the National Industrial Recovery Act.

³ See series T81 deflated by series E135 and series F4 in U.S. Bureau of the Census 1975, pp. 210-1, 224, and 843. See also Romer (1992).

⁴ Since our analysis throughout the paper focuses on per capita retail sales, we will henceforth use “retail sales” and “per capita retail sales” interchangeably.

⁵ See U.S. Senate 1936, pp. xi-xiv and Williams 1966, p. 212. In its publicity publications, the PWA (1939, 20-22) traced the paths of workers’ spending to show the impact of the PWA on retail sales and the rest of the economy.

⁶ See Fishback, Kantor, and Wallis (2002) for a table that summarizes the various studies of the distribution of New Deal funds. Couch, Atkinson, and Wells (1999) and Couch and Williams (1998) have used Alabama counties to examine the distribution of New Deal agricultural and total funds. Using another data source, Fleck (1994, 1999b, 2001a) has used county-level data to examine the distribution of relief and its impact on unemployment.

⁷ New Deal spending did not encompass all federal spending, so our analysis does not address the impact of all forms of federal expenditures. It should be noted, however, that much of the New Deal represented an entirely new role for the federal government. For example, agricultural spending, relief spending, many forms of lending to state and local governments, and insurance of mortgage loans broke new ground for the federal government. In addition, there were major increases in federal spending from the early 1930s on roads, public buildings, public works, and conservation. Federal intergovernmental and direct expenditures on education rose from 26 million in 1932 to 235 million in 1934, on highways from 217 million to 599 million, on public welfare and employment security from 2 million to 585 million, on housing and urban renewal from 0 in 1932 to 3 million in 1934 to 71 in 1936. Federal expenditures on the primary tasks of the federal government prior to the 1930s generally did not display the same marked jumps. See Wallis 1985 and U.S. Bureau of Census 1975, pp. 1124-26.

⁸ The largest loan program was the Reconstruction Finance Corporation (RFC), which was initiated under the Hoover Administration on February 2, 1932. The RFC made loans to operating banks, to closed banks to help pay depositors, to a variety of agricultural credit institutions, railroads, businesses, and rural electrification projects, and in 1932 to state and local governments to provide work relief. Approximately 40 percent of the RFC loans were made by June 1933. The Home Owners’ Loan Corporation (HOLC), which primarily operated through 1936, refinanced home loans of financially distressed homeowners. In addition to its grant programs, the PWA loaned money to state and local governments to aid in financing public works projects. In the agricultural arena the Farm Security Administration (FSA) offered a combination of grants and loans to low-income farm families who were unable to obtain credit from any other sources, while also offering loans to tenants to help them purchase farm land. The Farm Credit Administration (FCA) offered small loans in 1934 and 1935 to aid drought-stricken farm areas, made emergency crop and feed loans, and made new loans or refinanced indebtedness of farmers facing a specific set of risks (U.S. Farm Credit Administration 1935, 6, 7, 15, 16; 1936, 7). The U.S. Housing Authority (USHA) was established on November 11, 1937, under the Wagner-Steagall Act. It took over the housing projects built or started by the PWA and began making a series of loans for public housing

projects and slum clearance. The Rural Electrification Administration (REA) provided loans to finance rural electrification. The Disaster Loan Corporation (DLC), organized in February 1937, provided loans to areas hit by natural disasters (Jones 1939, 1).

⁹ Unfortunately, we only have good information at the county level on New Deal spending and retail sales. Although we have some information on manufacturing activity and agricultural output, there are significant numbers of missing observations in the sample. Further, these too would be endogenous to the system and we do not have adequate instruments that would allow us to estimate the structural model. On the other hand, we believe that we have developed a series of instrumental variables that will allow us to estimate a reduced form parameter for the impact of New Deal spending on one form of economic activity – retail sales.

¹⁰ Note that after substituting equation 4 into equation 1, the impact of an added dollar of New Deal spending on local income is $(1 - m_n)$ and that of another dollar of state and local spending is $(1 - m_s)$. We do not include government spending in the private consumption of internal county production because it would lead to double counting. The sum $m + y$ would equal one if there were no saving.

¹¹ Recent empirical work investigating the impact of public infrastructure on economic growth gives mixed support to the hypothesis that more infrastructure spending leads to substantial increases in economic growth. See Aschauer 1989; Costa, Ellson, and Martin 1987; Duffy-Deno and Eberts 1991; Hulten and Schwab 1991; Garcia-Mila and McGuire 1992; Munnell 1992; Gramlich 1994.

¹² See Hines and Thaler 1995 and Bailey and Connolly 1998 for overviews of the “flypaper” literature. See Gramlich 1977 for discussions of ways in which different types of grants influence total state and local spending. The WPA and the FERA contained matching provisions in their original legislation. The FERA matching provisions were largely ignored after November 1933. The actual share of the WPA and FERA projects financed by the state and local governments varied dramatically from project to project in ways that suggest no consistent matching formula. For information on the WPA’s matching provisions, see Howard (1943, 147) and for the FERA see Williams (1966, 217).

¹³ See Oates 1973. As of 1932, all of the states ran very large surpluses if capital outlays are excluded from their budgets. The inclusion of capital outlays led to deficits in two-thirds of the states. The states appear to have taken repayment of their debts seriously because by 1937 all but four states ran surpluses (including capital outlays as spending), some of which were very large, and three of the remaining four were very close to a balanced budget (U.S. Bureau of the Census 1935, 8-17, 28-39; 1940, 7-16).

¹⁴ For discussions of the theoretical bases for these multipliers, see McGregor, et. al. (2000), Merrifield (1987), and the sources cited therein. The multipliers based on neoclassical principles have assumed that labor markets clear at the prevailing wage. We chose a quasi-Keynesian approach in part for simplicity and in part because throughout the 1930s there were unemployment rates ranging from 10 to 25 percent.

¹⁵ The equation for the log difference in retail sales between 1929 and 1939 can actually be written as the sum of a series of difference equations for each of the intervening years. Equation (12) in the text implicitly assumes that New Deal spending in prior years did not affect retail sales in the current year. Thus, when the difference equations for all intervening years are summed, the New Deal variables for intervening years are cancelled out by subtraction. In other words, we are left with an equation that assumes the difference in New Deal spending between 1929 (which was zero) and 1939 influence the change in retail sales between the same years. We can write down a more complicated model with lagged effects of New Deal spending from prior years. Without loss of generality in the rest of the footnote, we use a simplified notation with only one form of New Deal spending. If prior year New Deal spending affected retail sales, we could write the 1939 retail sales equations as:

$$R_{39} = N_{39} \beta_{39} + N_{38} \beta_{38} + N_{37} \beta_{37} + N_{36} \beta_{36} + N_{35} \beta_{35} + N_{34} \beta_{34} + N_{33} \beta_{33} + Z_{39} + \varepsilon_{39}.$$

Following the procedure of taking first differences and summing the differences to obtain $R_{39}-R_{29}$ and noting that New Deal spending was 0 in years prior to 1933, the difference equation becomes:

$$R_{39} - R_{29} = N_{39} \beta_{39} + N_{38} \beta_{38} + N_{37} \beta_{37} + N_{36} \beta_{36} + N_{35} \beta_{35} + N_{34} \beta_{34} + N_{33} \beta_{33} + (Z_{39} - Z_{29}) + (\varepsilon_{39} - \varepsilon_{29}).$$

It is reasonable to assume that New Deal spending was distributed fairly evenly over the period. National information on the timing of New Deal AAA, relief, and public works spending, and the value of loans insured by the FHA show the figures fluctuating around either no trend or a slight positive trend, although nearly all of the series dipped in fiscal year 1938. Only loans displayed a negative trend because the RFC hit the ground running with large amounts of loans from 1932 to 1933. If we assume the impact of New Deal spending is a distributed lag such that $\beta_{38} = \delta \beta_{39}$; $\beta_{37} = \delta \beta_{38} = \delta^2 \beta_{39}$; $\beta_{36} = \delta \beta_{37} = \delta^3 \beta_{39}$; etc., then the difference equation becomes:

$$R_{39} - R_{33} = N^{\text{avg}} \beta_{39} (1 + \delta + \delta^2 + \delta^3 + \delta^4 + \delta^5 + \delta^6) + (Z_{39} - Z_{29}) + (\epsilon_{39} - \epsilon_{29}).$$

In the empirical equation we would be estimating $\hat{\beta} = \beta_{39} (1 + \delta + \delta^2 + \delta^3 + \delta^4 + \delta^5 + \delta^6)$. Thus $\hat{\beta}$ is an estimate that summarizes the impact of all New Deal spending over the period on retail sales in 1939. If $\delta = 0$, there would be no lagged effects of New Deal spending, which is the simple assumption in the text. If δ is greater than zero, $\hat{\beta}$ is the sum of all of the distributed lag effects across time. In either case, the proper interpretation of $\hat{\beta}$ is that it represents the overall percentage increase in retail sales in 1939 that can be attributed to a dollar increase in average per capita New Deal spending in the county in each year from 1933 to 1939.

¹⁶ We also face measurement error in the exogenous variables comprising the Z vector. Data are available only for the census years 1930 and 1940, so we are left to use interpolations to impute value for these variables in 1933 and 1935.

¹⁷ Wallis 1987 first pointed out the endogeneity problem in his study of the annual allocation of New Deal funds across states.

¹⁸ See Wallis 1998 and Couch and Shughart 1998 and Fishback, Kantor, and Wallis 2002 for summaries of this literature.

¹⁹ Most prior studies focus on the distribution across states, and we have developed similar measures for the county level. Using a median-voter model Wright (1974) developed a state-level “political productivity index” that took into account the long-term voting for Democratic presidential candidates between 1896 and 1932 and also swing voting. He also included measures of the electoral votes for each state and separately included various pieces of the index, particularly the standard deviation of the Democratic share of presidential votes. Anderson and Tollison (1991) and Couch and Shughart (1998) used Roosevelt’s share of the 1932 vote, which is a reasonable specification on the grounds that Roosevelt was likely to reward his supporters and that Roosevelt was seeking more than just a 51 percent victory so that he could establish a mandate. Fleck (1994, 1999c) showed the importance of adding voter turnout to the equation. Rhode, Snyder and Stumpf (2001), Fleck (2001a), and Fishback, Kantor, and Wallis (2002) suggest the importance of the swing vote in a median voter model and choose various ways to measure the swing vote. We are agnostic as to whether proper modeling should focus on a median-voter model or a mandate model. Instead, we include a series of presidential electioneering variables to allow for all of these views to be incorporated

²⁰ The R-squareds from the first-stage regressions of the 2SLS regression are .292 for public works, .414 for relief, .504 for the AAA spending, .303 for the loans, and .371 for the FHA. As is standard in a 2SLS analysis, the first-stage regressions include all of the variables that are considered exogenous in the system. Thus, the equation includes all of the variables listed in this section and the variables in the Z vector (change from 1930 to 1940 in percent urban, in percent illiterate, in percentage of land in farms, in percent black, and percent foreign born). F-tests suggest that the instruments have explanatory power for each of the New Deal categories.

²¹ We experimented with alternative weighting criteria based on the inverse distance between the county seats of each county. In other words, closer counties received greater weight in the weighting matrix than farther counties. We further imposed 100- or 200-mile cutoffs so that faraway counties were, by construction, not considered integrated. The results using these alternative weighting schemes did not change the results reported below.

²² The coefficients of the state dummies (not reported in the table) capture the effects of changes in individual states that were common to all counties within that state, but varied across states. These changes might include changes in state laws (for example, laws governing resale price maintenance), regional costs of living, state tax rates, state spending on relief and public works, changes in weather, and other factors. These state dummies capture the effects of changes in the features of the states, holding New Deal spending and the other factors in the regression constant. The coefficients suggest that unmeasured changes in the Dakotas, Kansas, and Oklahoma, where the Dust Bowl wreaked havoc, in the southern states of North Carolina and Florida, and in the mountain west states of Arizona and Nevada led to lower retail sales.

²³ Robert Fleck (1999b) has suggested that there were a large number of discouraged workers who had not been listed as unemployed by the Census who were moved onto relief jobs. This argument is a potential explanation for his finding that an additional relief job led to nearly a one-for-one increase in the measured level of unemployment, which included people on relief.

²⁴ In prior drafts of the paper we sought to assess the impact of spillover effects of New Deal spending by including measures of New Deal spending in neighboring counties. We ran into several problems in the estimation that led us to focus on the estimates reported in Table 4. By including a weighted average of New Deal spending in neighboring counties into the model, we introduce five more endogenous New Deal variables into the equation for which we have to develop instruments. The proposed solution is to take the instrument list from the 2SLS analysis without neighbors and develop additional instruments by multiplying the instrument vector by the neighbor weighting matrix to obtain a series of neighbor-weighted instruments. When we followed the procedure developed by Kelejian and Prucha for estimating the impact of the neighbors with our instrument list, we found that only one of the neighbor effects was statistically different from zero and it was strongly negative. All of the direct effects in the model were substantially reduced in size. Further investigation showed that the reduction in the coefficients for New Deal spending in the county where the money was spent was not caused by the inclusion of the five New Deal neighbor variables. Nearly all of the change in the coefficients and standard errors occurs when we move from the 2SLS estimation with our original instrument list to 2SLS estimation with the original instruments plus the neighbor-weighted instruments. Since the neighbor-weighted instruments are unnecessary if we do not include New Deal neighbor spending directly in the analysis, the change in the results is being driven by the inclusion of unneeded instruments; therefore, we did not pursue this research stream further.

²⁵ We have also explored the sensitivity of the results to alternative instrument sets. When we exclude the congressional committees or the presidential variables we see very similar results to those reported in the paper. When we exclude the economic control variables from the estimation, public works spending retains its strong positive and statistically significant effect on retail sales. The multipliers implied by the coefficients all exceed 1.6. The lack of a positive effect of the AAA is still evident. The FHA insurance effect is smaller than in the full specification. The impact of relief spending, however, is substantially diminished. We believe that the differences in the results stem from the omission of key determinants of retail sales; therefore, we place more credence on the full specification. The results of these sensitivity tests are available from the authors.

²⁶ The Public Roads Administration and the Public Buildings Administration each spent some funds prior to June 1933, but the spending accounts for at most 6 percent of the total public works spending through June 1934. See the state reports of the Office of Government Reports.

²⁷ We have considered trying to develop a panel consisting of 1929, 1933, 1935, and 1939 but the measurement error compounds the more we try to split the New Deal information into specific years. Essentially, the variation across time in the New Deal values would be determined by our assumptions and rough estimates using national and state information on the transitions between the Hoover and Roosevelt administrations in 1933 and the First and Second New Deal in 1935. To avoid these problems we followed the strategy of comparing years with New Deal spending, such as 1935 and 1939, with a year without – 1929.

²⁸ To regress Y on the linear independent columns of H we used the *OLSQR* function in GAUSS programming language, which uses the QR-decomposition to produce \hat{Y} , in light of any potential linear dependence in H . If WN_{PW} , WN_R , WN_A , WN_L or WN_I are included as endogenous regressors in equation (4), the suitable instrument matrix becomes the linearly independent columns of $H = [Z^*, D, WZ^*, WD]$. The theoretical justification is that WN_{PW} , WN_R , WN_A , WN_L and WN_I imply five additional equations in the simultaneous system, each including regressors WZ , WD . All potential equations explored are identified through exclusion restrictions.

²⁹ The optimization was performed using the *SQPSOLVE* function in the GAUSS programming language.

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Table 1

Ratios of Per Capita Retail Sales in 1933 and 1939 to 1929 and Distributional Information for Counties Within Each State

STATE	Ratio 1939 to 1929						
	Aggregate State Ratios to 1929 Value		Distributional Information for Counties in State				
	1933	1939	Mean	Std. Dev.	Min	Max.	# of Counties
NEW ENGLAND							
Connecticut	0.709	1.082	1.091	0.055	1.017	1.197	8
Maine	0.758	1.061	1.081	0.113	0.883	1.314	16
Massachusetts	0.762	1.027	1.064	0.081	0.924	1.268	14
New Hampshire	0.771	1.160	1.180	0.063	1.093	1.277	10
Rhode Island	0.677	1.028	1.120	0.144	0.909	1.300	5
Vermont	0.682	1.001	1.008	0.084	0.892	1.149	14
MID-ATLANTIC							
Delaware	0.682	1.171	1.284	0.135	1.102	1.435	3
New Jersey	0.715	1.027	1.043	0.106	0.831	1.227	21
New York	0.665	0.908	0.985	0.099	0.795	1.291	58
Pennsylvania	0.639	0.988	1.054	0.110	0.834	1.384	67
EAST NORTH CENTRAL							
Illinois	0.601	0.917	0.981	0.139	0.624	1.398	102
Indiana	0.591	1.016	1.042	0.135	0.755	1.592	92
Michigan	0.532	0.928	1.019	0.129	0.676	1.513	83
Ohio	0.648	1.011	1.043	0.103	0.743	1.386	88
Wisconsin	0.636	0.994	1.024	0.091	0.842	1.277	71
WEST NORTH CENTRAL							
Iowa	0.638	1.016	1.021	0.131	0.663	1.374	99
Kansas	0.600	0.819	0.747	0.217	0.373	1.118	105
Minnesota	0.691	1.094	1.132	0.128	0.899	1.911	87
Missouri	0.673	0.900	0.909	0.196	0.565	1.426	114
Nebraska	0.661	0.911	0.928	0.314	0.501	7.636	93
North Dakota	0.629	0.870	0.805	0.174	0.543	1.151	53
South Dakota	0.573	0.882	0.828	0.227	0.476	1.431	68
SOUTH							
Virginia	0.730	1.165	1.213	0.217	0.649	2.610	100
Alabama	0.595	0.952	0.953	0.222	0.529	1.669	67
Arkansas	0.555	0.848	0.773	0.203	0.459	1.202	75
Florida	0.608	1.154	1.131	0.249	0.547	2.525	67
Georgia	0.690	1.128	1.142	0.225	0.567	2.107	159
Louisiana	0.672	1.117	1.170	0.356	0.530	5.979	64
Mississippi	0.421	0.774	0.733	0.216	0.389	1.095	82
North Carolina	0.671	1.059	1.041	0.219	0.505	1.733	100
South Carolina	0.765	1.248	1.219	0.179	0.689	1.755	46
Texas	0.573	0.988	0.989	0.288	0.349	6.048	251
Kentucky	0.647	1.003	1.005	0.192	0.527	1.786	120
Maryland	0.761	1.103	1.214	0.165	0.767	1.537	24
Oklahoma	0.578	0.816	0.810	0.198	0.461	1.295	77
Tennessee	0.623	1.041	1.086	0.196	0.486	1.591	95

West Virginia	0.673	1.010	0.999	0.168	0.686	1.380	55
MOUNTAIN							
Arizona	0.457	0.876	0.921	0.220	0.628	1.496	14
Colorado	0.620	0.995	1.009	0.201	0.557	2.198	62
Idaho	0.602	1.085	1.078	0.154	0.692	1.434	44
Montana	0.588	1.079	1.104	0.225	0.654	3.152	56
Nevada	0.642	1.245	1.275	0.240	0.969	2.222	17
New Mexico	0.495	1.025	1.014	0.246	0.523	2.319	31
Utah	0.583	0.988	1.091	0.179	0.767	1.603	27
Wyoming	0.662	1.065	1.187	0.215	0.814	1.874	23
PACIFIC							
California	0.604	1.002	1.119	0.177	0.805	1.938	57
Oregon	0.594	1.045	1.121	0.158	0.873	1.638	36
Washington	0.593	0.974	1.007	0.157	0.663	1.349	39
Mean	0.636	1.012					
Std. Dev.	0.075	0.107					

Source: See Data Appendix.

Table 2

New Deal Funds by Purpose by State

STATE	Per Capita New Deal Spending on:				
	Public Works Grants	Relief Grants	AAA Grants	Loans	Value of Mortgages Insured
NEW ENGLAND					
Connecticut	\$31.3	\$60.3	\$2.1	\$55.0	\$20.1
Maine	50.2	52.2	1.5	109.5	7.9
Massachusetts	24.6	105.8	0.5	56.4	11.6
New Hampshire	28.5	57.4	0.8	23.5	13.7
Rhode Island	42.8	62.1	0.1	50.0	20.2
Vermont	31.6	44.6	2.4	95.9	14.1
MID-ATLANTIC					
Delaware	54.1	57.0	5.6	34.0	27.6
New Jersey	32.3	92.7	0.5	112.8	39.6
New York	31.5	119.0	0.6	96.9	24.8
Pennsylvania	23.5	111.2	1.1	51.0	15.8
EAST NORTH CENTRAL					
Illinois	30.7	102.6	12.7	99.9	20.2
Indiana	28.2	87.6	18.7	74.0	23.0
Michigan	22.5	93.7	5.0	149.9	37.1
Ohio	24.5	115.7	7.5	128.0	25.3
Wisconsin	32.7	94.1	11.5	100.1	12.9
WEST NORTH CENTRAL					
Iowa	30.6	41.7	64.7	86.1	8.9
Kansas	35.4	65.4	81.8	71.4	14.0
Minnesota	35.1	94.4	27.8	69.4	16.0
Missouri	28.1	75.6	20.8	61.1	18.6
Nebraska	38.3	64.1	74.2	114.6	9.4
North Dakota	46.7	87.8	127.7	167.4	5.4
South Dakota	46.5	112.8	100.3	143.6	7.3
SOUTH					
Virginia	52.5	28.9	6.3	43.8	19.8
Alabama	22.2	46.6	19.5	47.8	8.4
Arkansas	22.2	56.1	31.1	64.3	7.5
Florida	31.4	76.7	4.1	66.2	40.0
Georgia	21.8	43.0	18.0	52.6	13.5
Louisiana	22.2	62.6	21.9	106.4	10.1
Mississippi	22.5	39.5	28.0	50.7	8.9
North Carolina	24.0	29.8	17.5	47.5	8.3
South Carolina	38.2	52.6	21.0	68.5	8.4

Texas	33.9	44.9	37.4	72.8	18.1
Kentucky	22.3	51.8	17.6	43.6	10.4
Maryland	41.6	56.6	4.2	126.6	27.6
Oklahoma	28.5	72.8	38.5	58.5	13.3
Tennessee	24.2	38.8	14.4	77.2	15.0
West Virginia	20.4	88.3	1.6	42.5	10.3
MOUNTAIN					
Arizona	145.5	103.7	10.6	83.7	36.4
Colorado	50.8	121.9	28.7	76.2	18.1
Idaho	59.2	85.8	46.8	97.2	23.1
Montana	86.0	129.0	72.8	115.1	13.7
Nevada	483.3	104.6	5.3	162.3	55.9
New Mexico	85.8	90.8	23.9	69.5	15.5
Utah	66.7	96.6	13.6	112.7	35.4
Wyoming	105.9	108.0	31.2	132.6	38.3
PACIFIC					
California	38.1	102.7	4.8	144.1	83.0
Oregon	47.2	75.1	16.0	62.9	23.7
Washington	53.5	103.6	16.5	70.2	36.7
Mean	50.0	77.3	23.3	84.3	20.7
Std. Dev.	68.2	27.4	27.9	35.7	14.4

Notes: Per capita New Deal spending in Each State is computed as total spending in the state from 1933 to 1939 divided by the population in 1930. AAA includes payments to farmers under the Agricultural Adjustment Act, including rental and benefit payments in 1934 and 1935 and Conservation payments in 1936 and 1937. Relief includes spending under the Federal Emergency Relief Administration, the Civil Works Administration, the Works Projects Administration, and the social security programs for old-age assistance, aid to the blind, and aid to dependent children. Public works includes expenditures under the Public Works Administration, the Public Buildings Administration, and the Public Roads Administration. The loans include the Reconstruction Finance Corporation (includes loans made after February 2, 1932), Farm Security Administration, Farm Credit Administration, Home Owners' Loan Corporation, Disaster Loan Corporation, U.S. Housing Authority, the Public Works Administration, and the Rural Electrification Administration.

Sources: See Data Appendix.

Table 3

Predicted Change in Retail Sales from \$1 of New Deal Spending Based on Various Assumptions

Line	Program/Assumptions	t_f	y	m	Base mult.	c	w	b	a	m_s	m_n	r	Income mult.	Change in Retail Sales from \$1 of New Deal spending
75 Percent of Extra Income Spent Locally														
1	Public works Grant	0.07	0.75	0.25	1.74	-0.1	0.9	0.3	0	0.2	0.1	0.53	2.10	1.11
2	Relief Grant	0.07	0.75	0.25	1.74	-0.1	0.9	0.1	0	0.2	0.05	0.53	1.84	0.98
3	AAA Grant	0.07	0.75	0.25	1.74	0	0.9	-1	0	0.2	0	0.53	0.00	0.00
4	Loans Fully Repaid	0.07	0.75	0.25	1.74	-0.1	0.9	0.1	1	0.2	0.2	0.53	-0.16	-0.08
5	FHA subsidy	0.07	0.75	0.25	1.74	-0.1	0.9	0.2	0.8	0.2	0.1	0.53	0.54	0.29
1a	Public Works Grant, Small Productivity Effect	0.07	0.75	0.25	1.74	-0.1	0.9	0.1	0	0.2	0.1	0.53	1.76	0.93
2a	Relief with 50 Percent Crowding Out	0.07	0.75	0.25	1.74	-0.1	0.9	-0.5	0	0.2	0.05	0.53	0.80	0.42
3a	AAA Grant Harms Farm Workers	0.07	0.75	0.25	1.74	0	0.9	-1.2	0	0.2	0	0.53	-0.35	-0.18
4a	Loan only 80 Percent Repaid	0.07	0.75	0.25	1.74	-0.1	0.9	0.1	0.8	0.2	0.2	0.53	0.19	0.10
50 Percent of Extra Income Spent Locally														
6	Public works Grant	0.07	0.5	0.5	0.93	-0.1	0.9	0.3	0	0.2	0.1	0.53	1.13	0.60
7	Relief Grant	0.07	0.5	0.5	0.93	-0.1	0.9	0.1	0	0.2	0.05	0.53	0.99	0.52
8	AAA Grant	0.07	0.5	0.5	0.93	0	0.9	-1	0	0.2	0	0.53	0.00	0.00
9	Loans Fully Repaid	0.07	0.5	0.5	0.93	-0.1	0.9	0.1	1	0.2	0.2	0.53	-0.08	-0.04
10	FHA subsidy	0.07	0.5	0.5	0.93	-0.1	0.9	0.2	0.8	0.2	0.1	0.53	0.29	0.15

Legend:

r is the ratio of retail sales to income.

Base mult. is the base multiplier.

t_f is the federal income tax rate.

y is the share of disposable income the populace spent on goods and services from inside the county.

m is the share of disposable income the populace spent on goods and services from outside the county.

c is the marginal effect of New Deal spending on state and local spending; flypaper effects imply $c > 0$, while crowding out effects imply $c < 0$.

w is the ratio of state and local taxes to state and local spending; deficit financing means $w < 1$.

a reflects the extent to which New Deal funds were to be repaid to the federal government; for grants a is zero, full repayment of loans at market rates implies a equal to one.

b is the impact of New Deal spending on private production for export. Positive externalities from social overhead capital imply a positive b .

Replacement of production as with the AAA implies a negative b .

m_n is the share of New Deal spending spent on goods and services from outside the county.

m_s is the share of state and local spending spent on goods and services from outside the county.

μ is the income multiplier after all effects are factored in.

Table 4

Estimates of the Impact of New Deal Funding on Retail Sales, 1929 to 1939

Variables	OLS		2SLS		G2SLS		G2SLS	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
	(1)		(2)		(3)		(4)	
Intercept	0.0650	0.92	0.0172	0.22	-0.0034	-0.04	-0.0225	-0.25
Change in percentage:								
Black	-0.0063	-2.53	-0.0036	-1.21	-0.0020	-0.68	-0.0042	-1.61
Foreign-Born	-0.0092	-3.97	-0.0049	-1.52	0.0008	1.57	0.0005	1.00
Urban	0.0036	6.24	0.0033	5.04	-0.0044	-1.26	-0.0081	-2.95
Illiterate	-0.0078	-4.13	-0.0090	-4.07	0.0027	4.25	0.0030	5.19
Land on Farms	0.0006	1.28	0.0009	1.68	-0.0085	-3.74	-0.0090	-4.21
Per capita New Deal funds (1967 \$):								
Public Works	0.0003	3.24	0.0020	5.53	0.0023	6.11	0.0020	6.23
Relief	-0.0006	-2.43	0.0016	1.89	0.0017	1.79	0.0018	2.07
AAA	-0.0006	-5.10	-0.00003	-0.05	0.0002	0.31	-0.0006	-2.18
Loans	-0.0006	-3.33	-0.0022	-1.58	-0.0025	-1.70		
FHA Insurance	0.0014	3.54	0.0024	1.12	0.0035	1.53		
State Effects	Included		Included		Included		Included	
rho					0.2566		0.2771	
sigma					0.0426		0.0409	
R-square	0.3371		0.3034					
R-bar-squared	0.3245		0.2901					

Notes: There are 3,059 observations in each regression. The G2SLS refers to a generalized two-stage-least squares estimation, with corrections for spatial autocorrelation. The spatial weighting matrix gives equal value to contiguous neighbors. We have explored giving spatial weights based on the inverse distance out to 200 miles and the results are very similar to those reported here. The instruments for the 2SLS and G2SLS include the state dummies and the non-New Deal variables listed above plus the following instruments: mean percentage voting for the Democratic presidential candidate from 1896 to 1932; the standard deviation of the percent voting for the Democratic presidential candidate from 1896 through 1932; the difference between the percentage voting for Roosevelt in 1932 and the mean Democratic vote from 1896 through 1932; the percentage of the adult population voting in 1932; the state's electoral votes weighted by the county's share of the state population; a series of dummies for the county's representatives on key oversight committees in the House of Representatives in the spring of 1933 – Agriculture, Appropriations, Banking and Currency, Flood Control, Irrigation, Labor, Public Buildings, Public Lands, Rivers and Harbors, Roads, and Ways and Means – the inverse of population; land area per capita; the percent urban in 1930; the percentage of land in farms in 1929; percent illiterate in 1930; percent black; and the percent foreign born.

Sources: See Data Appendix

Table 5

Income Multipliers Implied by Coefficients

	Table 4 Specification (3)	Table 4 Specification (4)	Table 6 Specification (1)	Table 6 Specification (2)	Table 6 Specification (3)	Table 6 Specification (4)
	1929-1939	1929-1939	1929-1935	1933-1939	1933-1939	1933-1939
Form of New Deal spending:						
Public Works	2.36 *	2.06 *	1.30 *	0.13	0.73 *	0.25
Relief	1.73 *	1.83 *	1.50 *	1.10 *	1.09 *	1.02 *
AAA	0.16	-0.60 *	-0.38 *	0.42	0.30	0.50 *
Loans	-2.60 *		-0.61	-0.24	-0.86	-0.72
FHA Insurance	3.54			-0.23	0.93	0.46

Notes: The income multipliers for the percentage changes from 1929 to 1939 and 1935, respectively, were calculated by multiplying the coefficient from the G2SLS estimates in Tables 3 and 5 by the sample average retail sales per capita from 1929 of \$541.44 (1967 dollars) to obtain the dollar increase in retail sales per capita associated with a one-dollar increase in New Deal spending. That figure was then divided by 0.53, which is the typical ratio of retail sales to income. The income multipliers for the 1933-1939 were calculated using the 1933 average of \$338.49.

Table 6

Estimates of the Impact of New Deal Funding on Retail Sales, 1929 to 1935 and 1933 and 1939

	1929-1935		G2SLS		1933-1939		G2SLS, treating		G2SLS, treating	
	G2SLS		G2SLS		1929-1933 drop in		1929-1933 drop in		1929-1933 drop in	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intercept	-0.1741	-2.00	0.3234	3.82	0.1804	2.37	0.3112	3.70		
Change in percentage:										
Black	-0.0072	-2.62	0.0083	3.11	0.0041	1.77	0.0082	3.24		
Foreign-Born	0.0008	1.53	-0.0003	-0.56	0.0002	0.44	-0.0001	-0.26		
Urban	-0.0059	-2.14	-0.0014	-0.44	-0.0026	-0.92	-0.0008	-0.25		
Illiterate	0.0028	4.47	0.0019	3.41	0.0022	4.33	0.0019	3.42		
Land on Farms	-0.0079	-3.68	-0.0041	-2.02	-0.0058	-3.25	-0.0041	-2.14		
Per capita New Deal funds (1967 \$):										
Public Works	0.0013	5.39	0.0002	0.61	0.0011	3.62	0.0004	1.09		
Relief	0.0015	1.84	0.0017	2.01	0.0017	2.21	0.0016	2.17		
AAA	-0.0004	-2.08	0.0007	1.41	0.0005	1.14	0.0008	1.91		
Loans	-0.0006	-1.25	-0.0004	-0.28	-0.0013	-1.10	-0.0011	-0.87		
FHA Insurance			-0.0004	-0.18	0.0015	0.78	0.0007	0.40		
Change in log retail sales per capita, 1929-1933					-0.4231	-22.48	-0.0554	-1.01		
State Effects	Included		Included		Included		Included			
rho	0.1902		0.3392		0.3553		0.3401			
sigma	0.0444		0.0326		0.0259		0.0315			

Notes and Sources: See Data Appendix. There are 3,059 observations. We did not include FHA Insurance for the 1929-1935 analysis because most of the FHA activity took place after 1935.