In Search of the Multiplier for Federal Spending in the States

During the New Deal

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Abstract

If there was any time to expect a large peace-time multiplier effect from federal spending in the states, it would have been during the period from 1930 through 1940. Interest rates were near the zero bound, and unemployment rates never fell below 10 percent and there was ample idle capacity. We develop an annual panel data set for the 48 states from 1930 through 1940 with evidence on federal government grants, loans, and tax collections and a variety of measures of economic activity. Using panel data methods we estimate a multiplier, defined as the change in per capita economic activity in response to an additional dollar per capita of federal funds. The state per capita personal income multiplier with respect to per capita federal grants was around 1.1. Some point estimates for multipliers for nontransfer grants and nonfarm grants were higher but not statistically significantly different from one. There is some evidence that AAA farm grants had negative or no effect on personal income. Federal grants had stronger effects on consumption than on personal income, but they had no positive effect on various measures of private employment.

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The Great Recession of 2007 to 2009 has been described as the worst downturn since the Great Depression of the 1930s. In both periods the federal government sought to combat the downturns with sharp increases in federal spending. The recent federal stimulus package has led to a surge of interest in fiscal policy multipliers. The current environment for fiscal multipliers is similar to the Great Depression in two important ways. Short term interests are near the zero bound, and there is significant slack in the economy with unemployment rates over 9 percent.

The unemployment challenge was greater in the 1930s because real GDP dropped by 30 percent between 1929 and 1933 and unemployment rates exceeded 20 percent for four years and stayed above 14 percent for most of the decade. In contrast to the recent situation, in the 1930s the Hoover and Roosevelt administrations financed most of the increase in federal spending with taxes and thus ran relatively small fiscal deficits. However, as is the case today, there was substantial variation in the distribution of federal funds per capita across states that can be used to examine multipliers for federal spending in each state on state incomes.

In this paper, we construct measures of federal government spending in each state for the period 1930 through 1940 and then estimate the impact of federal government spending in the state on the per capita income, state's per capita incomes, employment, and other measures of economic activity. In estimating the multipliers, we use several different measures of federal spending: grants, grants and loans, nontransfer grants,

nonfarm grants, and Agricultural Adjustment Administration (AAA) payments to farmers to take land out of production.

The multipliers are estimated using controls for time-varying weather patterns in the states, state fixed effects, year effects, and state-specific time trends, as well as instrumental variable techniques. Multipliers from most specifications with fixed effects and no instruments were well below one. When instrumenting we use two different instrument strategies and multiple specifications and therefore report several estimates for each multiplier. After instrumenting for federal spending, the estimates of the state per capita personal income multiplier with respect to per capita federal grants was around 1.1. Some point estimates for multipliers for nontransfer grants and nonfarm grants were higher but not statistically significantly different from one. There is some evidence that AAA farm grants had negative or no effect on personal income. Federal grants had stronger effects on consumption than on personal income, but they had no positive effect on various measures of private employment.

The Recent Literature on Multipliers

Much of the focus of the recent literature on multipliers has been on macroeconomic multipliers for the national economy. The traditional Keynesian macroeconomic model predicts relatively high multipliers associated with high marginal propensities to consume, while neoclassical models predict low multipliers through crowding out of investment and consumption in part due to anticipation of future tax liabilities. Neo-Keynesian models that combine neoclassical modeling with frictions in the economy suggest multipliers somewhere in between.

Most of the empirical macroeconomic estimates for short run multipliers imply that a dollar increase in government spending is associated with an increase in income that ranges from about 50 cents to $1.30, or multipliers of 0.5 to 1.3. Some estimates range as high as 1.8. The variation comes in part from differences in strategies for resolving endogeneity problems that arise when policy makers use fiscal policy to try to counteract downturns. The methods range from use of Vector Autoregressive (VAR) models to identification of changes in military spending and tax policy that might be plausibly considered to be unrelated to the macroeconomy. Recent efforts examine the impact of unexpected changes in these plausibly exogenous factors. The results also may vary due to the period studied, as the multiplier is predicted to be larger in times of high unemployment and in periods when interest rates are fixed or near the zero bound.[1]

It is difficult to estimate a national multiplier during the 1930s because it is hard to argue that federal spending was not rising in response to the downturn, and finding an instrument for federal spending in a national regression is difficult. Any estimate is likely to be for a balanced-budget multiplier because the deficits were very small relative to the size of the problem. Scholars have repeatedly shown that the New Deal was not a

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[1] See Valerie Ramey's (2011) survey of multipliers for an upcoming issue of the *Journal of Economic Literature.* Tthe third edition of Dornbusch and Fischer's *Macroeconomics* in 1984 (p. 148) reports multiplier estimates for an increase in net government spending of 1.8 from DRI and 0.7 from the Federal Reserve Bank of St. Louis. Key papers on the macroeconomic multiplier include Aiyagari, Christiano, and Eichenbaum (1992), Barro (1981); Barro and Redlick (forthcoming 2011), Blanchard and Perotti (2002) Blinder and Zandi (2010), Cogan, Cwik, Taylor, and Wieland (2009), Devereux, Head, and Lapham (1996), Hall (1980), Pereira and Flores de Frutos (1999) Ramey (forthcoming 2011), Ramey and Shapiro (1998), Romer 1992, Zandi (2009). Average tax multipliers tend to be somewhat higher (Romer and Romer 2010). In estimates of the impact of fiscal and monetary policy for the 1920s and 1930s from a panel of countries, Almunia, Benetrix, Eichengreen, O'Rourke, and Rua.find a multiplier for military spending above 2. Ethan Ilzetski, Enrique Mendoza, and Carlos Vegh (2010) find smaller multipliers for government consumption using panel SVAR methods on a modern panel of countries, although the multipliers vary across conditions.

Keynesian attempt to deal with the Depression.2   On the other hand, there was

tremendous variation in the amount of per capita federal grants and loans distributed

across the states during the New Deal and federal funds to each state fluctuated over the

course of the decade. This variation can be used to identify the impact of distributing

additional federal funds within a typical state on the income in the state.

The state-level multiplier is not the same as the federal multiplier. Nakamura and

Steinsson (2011) suggest that state multipliers for federal spending are useful as estimates

of the multiplier in a small open economy in a currency union with free movement across

borders. However, there is likely to be cross-subsidization in the state multiplier for

federal spending that is not present at the national level. A national multiplier for federal

spending addresses a situation where all of the taxation and obligations to repay future

debt are centered within the economy where the money is spent (Barro 1982). In

contrast, a state can receive federal funds but might bear less than (or more than) its full

share of the tax and debt obligation associated with funds. In a political economy model,

the state multiplier serves another purpose by determining the benefit that the state's

residents anticipate receiving when seeking federal government funds.

Our estimates for the Depression therefore are most comparable to estimates

from a series of recent working papers on the impact of federal spending on state and

local economies between 1980 and the present.3 Juan Carlos Suarez Serrato and Phillipe

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2 Keynes chastised Roosevelt in letters to the editor for not running deficits to finance the rise in federal
spending. E. Cary Brown (1956) and Larry Peppers (1973) both performed analysis to show that the
deficits run were very small relative to the size that Keynesians would have recommended.

3Another set of studies seek to estimate the impact of state government spending
within the same state. Daniel Shoag (2010) estimates multipliers within states for state
government spending using "windfalls" in returns on state pension assets as an
instrument for state government spending. The estimates suggest an income multiplier of
$2.12 and the generation of an additional job per $35,000 spent.   Jeffrey Clemens and

Wingender develop local fiscal multiplier estimates for the modern era in the U.S. using changes in the distribution of federal spending across districts driven by updated local population estimates from the decennial Census. Using first-difference estimation on panels at the county, state, and MSA level after 1983, they report an income multiplier estimate of 1.88 and an estimated cost per job created of $30,000 per year.

Daniel Wilson (2011) estimates jobs multipliers for the federal distribution of funds under the American Recovery and Reinvestment Act (ARRA) of 2009. He relied on the exogenous formulas for allocation of spending as the source of identification and finds that the ARRA saved roughly one job per $100,000 spent and had its strongest impact on construction employment. Emi Nakamura and Jon Steinsson (2010) estimate state level multipliers based on variations in military procurement spending during periods of military buildups between 1966 and 2006. To control for endogeneity, they construct an instrument based on regressing each state's military procurement spending on total U.S. military spending; therefore, the instrument for a state in year t is the coefficient from that state's regression times U.S. military spending in year t. Their results suggest a multiplier of 1.5, which is not sensitive to how strongly monetary policy leans against the wind.

Cohen, Coval, and Malloy (2010) use changes in federal spending related to changes in key Congressional committee assignments to show that increases in federal spending are associated with reductions in private investment and employment in the states. In a study of low income countries Aart Kraay (2010) finds a multiplier of less

Stephen Miran (2011) examine the impact of state spending on state income using budget rules for state deficit finance and differences between forecasted state budgets and actual state budgets for identification. However, these sources of variation in state fiscal policy did not serve as strong instruments.

than one for World bank lending using fluctuations in approval of projects as a source of variation in later spending that is uncorrelated with current output.

Regional scientists also have developed a broad range of theoretical models that lead to multipliers for net income coming into a state.4   The models range from the early Keynesian regional models to input-output models to economic base models to neo-classical models.5   The earlier empirical work on regional multipliers led to a broad range of estimates of multipliers of between 0.5 and 2 depending on the technique used.

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4 There is a large literature on the impact of public infrastructure spending at the state level. Hulten and Schwab (1991) conclude that the link between public infrastructure and states' economic growth is weak, as the states that expanded public infrastructure the most in the 1970s were not the ones that developed faster during that period. Munnell (1992 192) finds a significant effect of public capital on state-level output, investment and employment growth, although the effects of government spending at the state level are smaller than at the national level. Garcia-Mila and McGuire (1992) constructed a panel of 48 states from 1969 until 1983 to estimate input elasticity coefficients of regional Cobb-Douglas production functions and concluded that government provided goods, such as highways and education, have a significant and positive effect on state's output. Costa, Ellison, and Martin (1987) consider a translog production function and conclude that public capital and labor are complementary inputs. The estimated elasticities of output with respect to public are around one in all states. Meanwhile, Blanchard and Katz (1992) model the effects of negative one-percent employment shocks to a wide range of variables using data from U.S. states from 1947 to 1990 and find sizeable effects on per capita income over an extended number of years.
   Duffy-Deno and Eberts (1991) study the effect of the public capital stock on the state's economic growth, first, without using capital expenditures as a proxy for capital stock, and second, considering public capital both exogenous to the firm and endogenous to the local community positing a simultaneous relationship of public capital and local economic growth. The authors find a positive and statistically significant effect of public capital on state's economic growth rate.
   Assessing a link between public capital and economic growth, Fernald (1999) studies the direction of causation between public capital and productivity and unsurprisingly concludes that road construction (which is one of the biggest components of public spending) causes a surge in productivity in industries with high motor-vehicle use. David Aschauer (1989) also finds that road construction bears the most explanatory power of the change of local productivity, while military spending has almost none.
   5 Richardson (1985) surveys all but the neoclassical models. Merrifeld (1987 and 1990) and McGregor, McVittie, Swales, and Yin (2000) for examples of neoclassical multipliers for the economic base.

Some relied on simulations that derive multipliers using input-output models and surveys that describe the degree to which different industries rely on local labor and external inputs and capital. Others rely on Ordinary Least Squares regression estimates (Mulligan (2005, 1987).

The coefficient on federal spending in a regional model will be determined by a series of factors.6   It will have positive effects to the extent that it puts to work unemployed resources; it is more productive than the private spending that is replaced by the anticipation of future obligations for taxpayers; it produces social overhead capital (like roads, sanitation, public health programs) that make the inputs in the state economy more productive; and/or it leads to multiplier effects. The Keynesian multiplier model arises as each income recipient purchases goods and services from others in the state who, in turn, spend their receipts on goods and services produced by others in the state. The regional neoclassical multiplier arises as labor demand is pushed out along an upward sloping labor supply curve.

The positive benefits of the multiplier are diminished through a variety of "leakages" when the money spent in the process is spent on goods and services outside the state economy.   Much of the federal grant spending on work relief programs, like the FERA, WPA, or CWA, had small initial leakages because over 80 percent was spent on wages for people in the state. Grants from the Public Works Administration and Public Roads Administration had larger initial leakages because more than 50 percent of the monies were spent on materials and equipment imported from other states.   More

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6The intuitive discussion of the multiplier is based on a Keynesian discussion of consumption and imports. See Cullen and Fishback (2007) and Fishback, Horrace, and Kantor (2005b) for how this works in a simple model

leakages occurred to the extent that workers on federal projects spent their wages on goods and services produced outside the state.

Federal spending will have smaller positive effects on the economy to the extent that it leads people to save in anticipation that they will have to pay future taxes. The federal spending will have an even weaker effect to the extent that it replaces local production of goods and services. An influx of federal spending might bid up local wages in ways that raised the costs of hiring labor to private producers. It may have also bid up the prices for non-labor inputs with the same effect. The most obvious crowding out came from the AAA payments to farmers to take land out of production. The stated purpose of the act was to reduce output in hopes of raising prices enough to see an increase in income. In other cases, the federal spending may have replaced state and local projects that would have been built in the absence of federal spending. The impact of the reduction in state and local spending was likely to be small because states were generally required to run balanced budgets. Even when they ran deficits in the early 1930s, the deficits were relatively small as a share of state and local spending.

The Impact of Federal Spending in the 1930s.

There have been some estimates of the impact of New Deal spending on general economic activity. At the macroeconomic level, Christina Romer (1992) calculated a fiscal multiplier of only 0.23 in a difference-in-difference estimate that examined the change in federal funds distributed after the Veterans' Bonus was passed out in 1936. In simulations from a dynamic structural general equilibrium model Gauti Eggertsson (2008) finds that the combination of increased federal spending, the move off of the gold

standard, the zero interest bound, and government efforts to raise wages and prices through the National Recovery Administration led to a strong turnaround in the economy.

In microeconomic studies using variation across time and place, Fishback, Horrace, and Kantor (2005, 2006) showed a strong positive influence of public works and relief spending on county-level retail sales and net-migration. In studies of panel data for cities between 1929 and 1940 Fishback, Haines and Kantor (2007) and Johnson, Fishback and Kantor (2010) show that relief spending contributed to reductions in mortality and crime rates and increases in birth rates. On the other hand, Agricultural Adjustment Administration (AAA) grants had a slightly negative effect on retail sales growth and net migration. Garrett and Wheelock (2006) found similar positive effects of overall New Deal spending in a cross-sectional analysis of the growth rate in state personal income per capita for the entire period 1933 to 1939 and New Deal spending during that period. However, neither paper shows the effects of a multiplier on income in the same year.

Studies of labor markets using panel data from 1930s have focused on the impact of relief spending on labor markets. Neuman, Fishback, and Kantor (2010) examine monthly data from 1933 through 1940 for over 40 cities and find that relief spending raised private employment through 1935 but reduced it afterward. Benjamin and Mathews (1992) find small crowding out effects of private employment from relief jobs through 1935 and much larger crowding out effects in the second half of the New Deal.7

Federal Spending in the 1930s

In response to the hard times between the fiscal years 1929 and 1933, the Hoover administration and Congress increased nominal government spending by 52 percent, 88

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7 We focus on the studies that use panel data here, see Neumann, Fishback, and Kantor (2010) for citations to studies relying on cross-sectional estimation.

percent after adjusting for the tremendous deflation.8   After a decade of no change in

annual real federal spending, Hoover sought to increase spending through existing

programs. As an example, the U.S. Department of Agriculture (1932, 49-50) described

the increase in highway spending in 1932 as a relief and stimulus measure: "Emergency

employment was directly provided for varying periods for nearly 200,000 men and

indirectly for a much larger number in industries that supply necessary materials and

services."9 By fiscal years 1932 and 1933 the federal government was running a deficit

of -4.7 percent of a much reduced GDP as growth in tax revenues failed to keep pace

with the rise in government spending. The sizeable tax rate increase of 1932 was

followed by a substantial drop in *income* tax revenue for fiscal 1933. The drop in income

tax collections was roughly offset by revenues from new excise taxes on cars, oil

pipelines, gasoline, fuel oil, electricity, and some other products.

   After Franklin Roosevelt and the new Democratic Congress took office in March

1933, government spending roughly doubled over the next 6 years. The rise in spending

did not lead to large budget deficits because tax revenues rose at roughly the same rate.

The recovery led to more income tax collections, but a sizeable share of the rise in

revenues came from additional collections on the new excise taxes and the renewal of

collections of taxes on alcoholic beverages after the end of Prohibition.

8See series Ea584, Ea585, and Ea586 in Wallis (2006, 5-80 and 5-81). The
federal fiscal year ran from July 1 in year t-1 to June 30 in year t. Nearly all of the
decisions made about fiscal year 1933 were made by the Hoover administration and
Congress. Roosevelt did not take office until early March 1933 and very little of the
New Deal spending occurred before July 1, 1933.
   9 In 1935 the U.S. Bureau of Agricultural Economics traced the path of $100 million of highway
spending through the economy until it was paid out as wage or salary income and calculated highway
spending multipliers of 2.7 for employment and 3 for economic activity.

E. Cary Brown (1956) and Claude Peppers (1973) have documented that the federal deficits as a share of GDP were small and fell well short of being Keynesian policies designed to stimulate the economy.10 On the other hand, the distribution of the federal spending varied enormously across states on a per capita basis. Figure 1 shows the large variation in the annual changes in per capita federal grants plotted against per capita personal income across states for the years 1930 through 1940, all measure in 1967.11   The changes over time within states after controlling for national shocks in each year is the source of variation used to identify the multipliers once instruments are incorporated.

The Composition of Federal Spending

Understanding of the impact of federal spending and taxation during the New Deal era is complicated by the great diversity of programs. The New Deal funding programs were divided into two major classifications: nonrepayable grants and repayable loans.12   The Office of Government Reports (OGR) (1940) reported the total amount spent by each program in each state in each year from July 1, 1932 through June 30, 1939.   The OGR mimeos do not document where they obtained the spending figures. To double check the OGR data and extend the series for programs back to 1930 and forward

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10 After July 1931, it appears that monetary policy was relatively uniform across the 12 regional federal reserve banks. Richardson Van Troost (2009) document differences in attitudes toward providing reserves to banks in the St. Louis and Atlanta districts in 1930 and the first half of 1931, but St. Louis began following similar policies to Atlanta's in July of that year. Meltzer (2003) notes that there were differences of opinion about how to proceed with open market operations but that nearly all open market sales and purchases were made by the New York Federal Reserve Bank.

11 The variation is even larger when Delaware is added. We left Delaware off the graph to better show the spread across states visually, as Delaware reported federal tax receipts more than $100 per capita higher than in the next highest states.

12The Office of Government Reports offered information on the value of housing loans insured by the Federal Housing Administration. Since these loans were private loans, we do not incorporate these into the analysis of net federal spending.

to 1940, we went through a large number of reports from various agencies and the Treasury department to double check the numbers. The data appendix describes the sources we used and some of the inconsistencies we found. In addition, we added information that the OGR did not report on the construction and maintenance spending on Hoover (Boulder) Dam and the Tennessee Valley Authority (TVA) and the loans and cash grants on the World War I adjusted service certificates that were associated with the Veterans' Bonus. The totals and the amounts per capita for the period 1933 through 1939 are reported in Table 1 to get a sense of the size of each program.

The main focus in the analysis is on nonrepayable grants from the federal government. About 62 percent of the grants were associated with relief programs. All of the Works Progress Administration (WPA), Civilian Conservation Corps (CCCG), and Civil Works Administration (CWA) grants and roughly half of the Federal Emergency Relief Administration (FERA) were spent on poverty relief projects with work requirements and could be considered federal expenditures because they produced a good or service. The Social Security Act Programs (SSA), and the rest of the FERA grants were New Deal programs that offered transfer payments to alleviate poverty. The Veterans' Administration (VA) and Soldiers' and Sailors' Homes (SOLD) were grant programs in place before the New Deal that provided pensions, disability payments, and living support to military veterans. Grants from the SSA programs provided matching grants to states that provided aid to dependent children (ADC), old-age assistance (OAA), and aid-to-the-blind (AB). If we performed the analysis for the U.S. as a whole, the transfer payments from the SSA, part of the FERA, the VA, and the SOLD, which account for roughly 20 percent of the grants, would not necessarily be treated as

expenditures because they are net transfers within the system. However, at the state level these transfer grants become income that influenced purchasing power within each state, and therefore the Bureau of Economic Analysis included them in their personal income estimates; therefore, we incorporate them into the analysis.

The second major grant category is public works programs, which accounted for 19.4 percent of the grants. The Public Works Administration Federal (PWAF) and Nonfederal (PWANF) programs, Public Roads Administration, Public Buildings Administration (PBA), Rivers and Harbors Grant (RH) and other smaller programs listed as public works in the table were not poverty programs. All but the PWA programs were long run federal programs established before the New Deal. Unlike the work relief poverty programs, the public works programs could hire from the labor market or the relief rolls, faced no restrictions on hours worked to limit the amount received by an individual, and paid hourly wages that were roughly double those on the work relief programs.

Approximately 12 percent of the grants were devoted to agriculture from programs run by the Agricultural Adjustment Administration (AAA), Soil and Conservation Service (SCS), Farm Security Administration (FSA), and Agriculturual Experiment Stations (AES). The AAA was the major New Deal program which was devoted to payments to farmers to take land out of production. The initial AAA program was funded with an agricultural processing tax until it was declared unconstitutional in January 1936.13 The AAA also administered the replacement program adopted under

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13AAA grants per capita were not very strongly correlated with processing tax receipts in cross-sectional correlations. The correlation for 1934 was only 0.034 and for 1935 was 0.1677.

the Soil and Domestic Conservation Act of 1936, which continued to make payments to farmers to take land out of production without the processing tax. The FSA started within the FERA relief program and was more of a poverty relief program. The SCS began before the AAA was declared unconstitutional and provided grants for training farmers about soil conservation techniques.

It is not always clear how to treat the New Deal loans in terms of developing a multiplier. They are not government spending because at the time the loans were made they all required repayment. However, loan distributions are often listed in the budget deficit figures. As one example, the OGR treated the loans for construction of irrigation projects through the Bureau of Reclamation as grants. The loans were interest-free and the repayments were often delayed for a long time period, and in a number of cases the loans were forgiven. Following the OGR practice, we treated the Bureau of Reclamation funds as grants in this analysis.

Nearly all of the rest of the loans were repaid, and thus were treated separately as a loan category.14 There was a grant feature to the loans to the extent that they provided subsidies in the form of lower interest rates and better lending terms. The Home Owners' Loan Corporation (HOLC), for example, bought over 1 million troubled mortgages and then refinanced them for the original borrower at below market interest rates charged on good loans in the housing market, even though the loans were already troubled. The

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14 There were some cases of loan forgiveness. In the case we know about, the RFC loans offered to cities for poverty relief under the Hoover administration in fiscal year 1933 were eventually forgiven by the Roosevelt administration. The HOLC likely experienced the highest loan default rate because it foreclosed on 20 percent of the mortgages that it supported. Our sense from reading the reports of the various agencies, is that they anticipated repayment and were active in seeking repayment or recovery of assets to be sold when there was a default.

HOLC also extended the standard repayment period, and allowed much smaller down payments relative to the value of the home. The Farm Credit Administration (FCA) loans provided good terms for farm mortgages and short-term loans for crops, seed, and tools.15 The subsidies in Reconstruction Finance Corporation (RFC) loans likely varied by type of loan. Given the measurement issues with loans, we add 10 percent of the value of the loans as a measure of the interest subsidy to the grants. We also run estimates where we add the full value of the loans to the grants. Given the measurement problems with loans, the loan results are treated as robustness tests of the analysis of New Deal grants.

World War I service-adjusted certificates, associated with the Veterans' Bonus of 1936, are divided into two categories, loans and death benefit grants before January 1936 and grants after that date. Since Cone and the BEA incorporated both the loans and grants in their measure of personal income, these deserve direct attention. In 1924 Congress enacted an adjusted-service certificate program for men and women who served in World War I. The program offered certificates that could be redeemed at face value twenty years after receipt. The amount to be paid was $1 for each day served in World War I inside the U.S. and $1.25 for each day overseas, and then the amount was multiplied by 1.25 to take into account the delay in payment. Certificates valued at less than $50 were paid in cash immediately and the cash value of the certificate was paid out

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15The Commodity Credit Corporation loan program provided nonrecourse loans that established a price floor for the commodities produced. The CCC loan information has been eliminated from the analysis because the loans were not reported across states in fiscal 1934 and major portions of the loans were not reported across states in other years.

to heirs at the time of the veterans' death. These payments are treated as grants throughout.

Living veterans could also borrow from the Veterans' Bureau against the certificates by accepting a lien on the value of the certificate. They could pay back the loan and receive the full certificate value in 1945 or not repay and accept the amount left after interest was deducted in 1945. After Congress lowered the maximum interest on the loans to 4.5 percent (and soon after to 3.5) and increased the amount that could be borrowed to half of the value of the adjusted service certificate on February 27, 1931, World War I veterans took out 2 million loans valued at $795 million within the next few loans followed Congressional action on February 27, 1931. In January 1936 the Veterans' Bonus Bill allowed veterans to convert the adjusted-service certificates to cash at the full face value. If veterans held them for more than one year they could receive the face value plus 3 percent interest per year until maturity on June 15, 1945. The VA received 3.3 million applications with a face value of $3.2 billion for settlement by June 30, 1936. After deducting outstanding liens from loans, the VA paid out $1.7 billion in cash. Since the payment of liens released veterans from making future payments on loans, we treat the entire $3.2 billion as grants (Administrator of Veterans' Affairs 1931, pp. 10, 42-44; 1936, pp. 1, 22-24). In the analysis that combines loans and grants, however, the $3.2 million in grants was offset by the repayment of $1.5 billion in loans, so the combined value of grants and loans for the adjusted service certificates in 1936 becomes the net value after repayment of loans of $1.7 billion.

Federal Tax Policy in the 1930s

Federal taxation in the 1930s was relatively simple in that all tax rates were the same across all states for each activity in each year. However, the tax structure during the 1930s was quite different from the post-War economy, in which the vast majority of internal tax revenues come from taxes on income in the form of corporate, personal, and employment (social security and unemployment insurance) taxes. Between 1930 and 1940 the sources of federal revenue shifted dramatically away from income taxes toward excise taxes. Less than 10 percent of households earned enough to pay personal income taxes throughout the 1930s.16 Federal revenues were small enough in 1930 that personal income taxes accounted for 38% of total internal revenue and corporate income taxes composed 42%. The Tax Revenue Act of 1932 led to several major changes. Even though income tax rates were increased, the share of revenue from personal income taxes fell markedly to 16% in 1934 and 18% in 1940, while the share from corporate income taxes fell to 15% in 1934 before rising to 21% in 1940. The big revenue sources that arose from the 1932 tax changes was an expansion in excise taxes to cover manufactures of autos, tires, gasoline, lubricants and taxes on pipelines, telephones, telegraphs, and electricity. The share of internal revenue from excise taxes rose from 19% in 1930 to 28% in 1933. In 1933 the Roosevelt administration added in processing taxes on agricultural goods, a capital stock tax and eliminated Prohibition. The excise tax share jumped to 48% in 1934. Over the rest of the decade the excise tax share fell back to 35%, although the share of revenue from alcohol taxes remained steady around 12% (Shares calculated from Wallis, 2006, p. 5-86).

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16 Households did not begin paying income taxes before income hit $2,000 for individuals and $5,000 for a family of four at a time when most workers were earning $1,000 or less.

Aside from tax rate changes, the driving force behind changes in tax revenue within a state over time were changes in economic activity. Between 1930 and 1940 the correlations across time between real per capita personal income and real per capita taxes within the same state ranged from 0.95 in Georgia to 0.3624 in South Carolina. It was above 0.8 for 22 states, between 0.7 and 0.8 in 18 states, between 0.6 and 0.7 in 5 states, 0.4913 in Nebraska, 0.4423 in Kansas, and 0.3624 in South Carolina. The taxation is so strongly tied to income levels that it is difficult to find instruments for taxation that are not also strong correlates of income.

We deal with federal taxation empirically in two ways. Since national tax rates were the same across all states, the simplest way is to incorporate year fixed effects that control for tax rate changes in a model of state per capita income as a function of national government spending per capita in the state. Essentially, the model shows the multiplier of federal spending for state personal income after controlling for the fiscal drag created by the tax rate system. We also estimate the model with a dependent variable of per capita state income as a function of real per capita national government spending minus real per capita national taxes. This is the multiplier for income from federal government spending net of taxes.

Measures of Economic Activity

The impact of federal spending is estimated for several measures of economic activity. The broadest measure is state personal income per capita, which has been estimated and reported by the Bureau of Economic Analysis since the 1930s. Frederick Cone (1940, 3, 10, 13, 39), who helped develop the early estimates, described personal

income as a measure of the "ability of consumers to purchase the new goods and services currently produced by business enterprises." With that in mind personal income was defined as national income after subtracting business savings, social security contributions from employer and employee, contributions to the Railroad Retirement Fund, and contributions to retirement systems for government employees on the grounds that these were not available for spending. To capture purchasing power, the BEA added back many transfer payments, including direct and work relief, federal pensions to veterans, adjusted service benefits (both loans in the early 1930s and the Veterans' Bonus), other government retirement allowances, unemployment compensation, railroad insurance benefits, old-age insurance benefits, and agriculture benefit payments.[17] Direct relief accounted for about 1.4 percent, retirement payments for veterans and federal workers accounted for about 1 percent, and the World War I adjusted service certificates payouts reached peaks of 1.4 percent of income (in the form of loans) in 1931 and 2.1 percent (in the form of cash) in 1936 with negligible amounts in between (Cone, 1940, 10, 24-5).[18]

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[17]Cone (1940, p. 8) made the following argument for inclusion of the World War I veterans' loans and then 1936 cash payout on adjusted service certificates as personal income: "because these disbursements of the Federal Government were in the nature of original receipts to the veterans and because, owing to the large volume of the disbursements and their wide distribution among 3,000,000 veterans, they represented an important stimulus to consumption on two distinct occasions, they have been incorporated."

[18] We are currently working on developing a measure of income that does not include transfers and thus is a more production based measure. The new measure starts with the BEA measure of personal income. We have been collecting evidence by state that will allow us to subtract transfers without a work relief component and add back payroll taxes for social insurance and contributions to government pension plans. Work relief is treated as production income because it was related to production. Since we are not privy to the exact sources used to construct personal income, there is a risk of adding measurement when we make the adjustments.

We also estimate the model with measures of wage and salary income net of payroll taxes and several other measures of economic activity. The wage and salary income, which is net of pay-roll deductions for social insurance, accounted for about 62.5 percent of personal income in the 1930s (Martin and Creamer, 1942, p. 23). Measurement error is also an issue with the wage and salary income personal income data because the information on some components had to be interpolated between census years and from scattered components. The most accurately measured information is the wage component of wage and salary income because it relies heavily on the monthly establishment surveys collected by the Bureau of Labor Statistics during this period. The BLS suggested that about 48 percent of employment was covered by the survey in 1938 (Cone, 1940, 31).

To further reduce problems with measurement error and also examine in more depth where the stimulus occurred, we examine several other series: census reports on manufacturing payroll per capita in the odd years from 1929 through 1939, and the retail census estimates of per capita retail sales for the years 1929, 1933, 1935, and 1939. These two measures are particularly valuable because they are based on nationwide censuses of all establishments. There are several additional measures of income and spending that are not available in dollar terms, including per capita measures of John Wallis's (1989) broad-based employment index built up from BLS employer surveys for 1929 through 1939,[19] newly computerized payroll indices for 1932 to 1939 for the same group of industries, and per capita auto registrations, to capture the effect of government

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19 The broad-based index includes manufacturing wages only; mining wages only; street railways; telephone and telegraph; electric light, power, and gas; insurance; brokerage; wholesale and retail trade; year-round hotels; and laundry and dry-cleaning establishments.

spending on a major consumer durable.   The employment and payroll indices rely very little on interpolation but there may be sampling error in each state because the series are based on month-to-month comparisons of the same employers from surveys that rotate employers out of the sample (Wallis, 1989; Neumann, Fishback, and Kantor 2010). Wallis (1989) used benchmarks to resolve some of these problems for the broad-based employer index, but the payroll indices have not been benchmarked. The automobile registrations also are measured well, as the states collected license revenue from the automobiles.

Estimation Procedure

Despite the variety of different models that generate income multipliers of government spending, the empirical estimations of multipliers tend to be similar and use reduced form models with a sparse set of correlates. Both Barro and Redlick (2010) and Romer and Romer (2006) express worries about omitted variable bias that might arise from the absence of exogenous factors in the model. To estimate the multiplier, we use panel data methods with a measure of real per capita state income (y_{it}) in state i and year t in 1967 dollars as the dependent variable as a function of a measure of real per capita federal spending in state i and year t (g_{it}),

$$y_{it} = \beta_0 + \beta_1\, g_{it} + \beta_2\, W + S + Y + S^*\, t + \varepsilon_{it}.$$

To control for omitted variables that might have influenced government policy and state income, we include several vectors. A vector of extreme weather (W) variables controls

for weather factors that likely influenced crop production and prices in the farm sector, construction activity, and other activities where weather was a factor. A vector of state fixed effects (S) controls for factors like geography, state constitutions, and the basic economic, cultural, and demographic structure of each state that did not change over time but varied across states. A vector of year fixed effects (Y) controls for national changes in the economy that affected all states in each year, including monetary policy changes, changes in federal tax rates, the introduction and elimination of the National Recovery Administration and other changes in national regulation. A vector of state specific time trends (S*t) control for differences in the trend paths of economic activity in each state, including the shift away from state budget deficits in the early 1930s to budget surpluses in the late 1930s. Under the complete model specification the identification of the multiplier β_1 for net New Deal spending comes from the deviation from trend across time within states after controlling for nation-wide shocks.[20]

The model can also be estimated in year-to-year first differences to control for time-invariant state effects. In the difference model state time trends are controlled with the addition of state fixed effects. Both methods lead to unbiased and consistent estimates of the multiplier in large samples, but the standard errors are more efficiently estimated by the difference estimation if there is serial correlation (Wooldridge 2006, 491-2). Since the sample is relatively small with 48 states for 11 years each, we report both sets of estimates. Following Barro and Redlick (2011), Kraay (2010) and Nakamura and Steinsson (2010), we have also estimated the model as the growth rate in income

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[20] We have also tried estimating the model while including squared terms. The estimates at the mean of the sample are very similar and there is very little gain from adding the squared terms. In addition, the instruments did not have adequate strength to separate the coefficients for the squared terms.

from the previous year as a function of the change in government spending divided by lagged income. The use of the growth rate matches more common macroeconomic specifications, while the normalization of government spending by dividing by lagged income allows the coefficient to be interpreted as a dollar-for-dollar multiplier.

There are a variety of other factors that may have changed over time in non-trend ways that may have influenced both net fiscal federal spending and per capita income in the state. There are two problems that arise in trying to control for these other factors. First, many that might be included as controls in a productivity model, such as wages, employment, and interest rates, are themselves components of personal income. By controlling for them we would be restricting the measure of the impact of net federal expenditures to the parts of state income for which we have not controlled. Second, controls for age, race, ethnicity, population, and the structure of the economy are all available typically only during the census years and thus measures would have to be interpolated between census years to provide values. Essentially, the interpolated measures of the census-year structural variables between 1930 and 1940 would be linear combinations of the state-specific time trends and/or the state effects.

Third, state and local government spending and taxation are currently controlled for with the combination of the state fixed effects and state-specific time trends. Thus deviations in trend state fiscal activity is not being controlled. The multiplier estimate therefore may incorporate influences on income that arise from correlations between deviations from trend in state and local fiscal activity and federal spending. Since federal spending drove some state decisions, this might not be a disadvantage. As it stands today, comparable annual estimates of revenues and governmental cost payments in the

states are available only up through 1931 and after 1936 for all states; therefore, any estimate incorporating controls for net state spending would miss a very large portion of the New Deal period. Information is available on cities over 100,000 people throughout the 1930s, but data for the rest of the governments is available only for 1932. We have spent the past year filling the gaps in computerized information on the large cities and the states and still have at least a year's worth of work to go.21

There still remains the possibility of biases from simultaneity and endogeneity. An ample literature on the geographic distribution of New Deal spending shows that the Roosevelt Administration tended to distribute more New Deal grants to areas where income was declining (see Wallis 1998, Fleck 2008, Fishback, Kantor, and Wallis, 2003; Neumann, Fishback, and Kantor 2010). This tendency imparts a negative bias to the multiplier coefficient. To eliminate the biases arising from the issues described above, we follow an instrumental variable strategy. To be effective, the instrument must vary annually both across time and across space and be strongly correlated with the measures of federal government funds but not with the error term in the final-stage equation.

We develop two instrument strategies each based on a similar logic about the distribution of federal spending across states. Each year the federal government decides to distribute a total number of grants while paying attention to the state of the economy

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21The federal government stopped collecting the annual information from states for the volume *Financial Statistics of the States* in 1933 after having collected information from 41 states for 1932. They restarted by collecting the data for 1937 (U.S. Bureau of the Census 1940, p. vi). John Wallis, Richard Sylla, and John Legler have posted information for 16 states for the period 1933 through 1937 with the ICPSR, but it is taking longer than we anticipated to make the data for these states comparable with the federal government's categories. Working with John Wallis we are collecting, computerizing, and categorizing the information for the remaining states for 1933 through 1937 and for the seven states in 1932 that the Census Bureau had not worked with.

but not the economy within each state. In any one year each state's share of the grants is determined by a variety of factors, including economic problems within the state but also long run differences across states unrelated to the economy. Our goal is to use these factors unrelated to state-specific economic problems as instrumental variables.

Some scholars in the modern studies have tried to use federal rules for distribution, but this will not work in the 1930s because the federal government routinely violated the original rules written into the acts setting up the programs. Nakamura and Steinsson (2010) follow a strategy of estimating a separate share coefficient for each state. This is equivalent to estimating a first stage in our panel with 47 state dummies interacted with federal spending. When we tried this, the Stock-Yogo tests for instrument strength suggested that we had weak instrument bias. We therefore, experimented with interactions of regional dummies with federal grant spending per capita based on 9 census regions and 4 census regions. The instrument tests suggested that weak instrument bias was less of a problem with the 4 census region interactions, and Hausman tests for over-identification in this case do not reject the hypothesis that the instruments do not belong in the final stage regression.

The second instrument strategy relies more heavily on the finding in the New Deal distribution literature that swing voting was the key factor influencing the cross-state distributions of funds (Wright 1974; Fishback, Kantor, and Wallis 2003; Fleck 2001, 2008, Wallis 1998, 2001). It also tries to eliminate potential correlations between the state-specific portion of the total error for total federal spending and error term for the state in the final stage regression. The instrument is the interaction of two variables: a measure of swing voting in presidential elections up to the year $t-1$ and aggregate per

capita federal grant spending in year t in an area well outside the region where the state is located. The swing voting measure is the standard deviation of the percentage voting Democrat for president in the state between 1896 and the most recent presidential election prior to year t. The variable varies across time because each state's value changes between 1932 and 1933 and again between 1936 and 1937. By using the measure calculated up through the most recent presidential election before that year, we eliminate contemporaneous correlation with factors that influenced income in the state.[22] Given the controls for state fixed effects and state time trends, it seems unlikely that swing voting in the presidential elections between 1896 and year t-1 would have been correlated with the error for income in the state in year t except through the New Deal distribution mechanism.

The second variable in the interaction term is federal grant spending in the area *outside* an geographic "moat" around the state of interest. The moat includes the state's census region (of 9) and nearby census regions to avoid spatial correlation with the error term from potential spillovers. For New England, for example, the moat includes any states in New England, the Mid-Atlantic, the East North Central, or the states of Virginia, Maryland, Kentucky, or West Virginia; therefore, the component of federal spending for the instrument for that state is based on federal spending outside that moat.[23] [24]

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22 As an example, the instrument for the year 1932 would include the standard deviation of the percent voting for the Democratic presidential candidate from 1896 through 1928.

23 For the Mid-Atlantic states the area used for the instrument does not include any states from New England, the Mid-Atlantic, the East North Central, the South Atlantic, or the states of Alabama, Georgia, Kentucky, Maryland, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia. The area used for the instrument for the East North Central states does not include any states from the Mid-Atlantic, the East North Central, the West North Central, or the states of Alabama, Arkansas, Kentucky,

Since AAA grant spending differed markedly from other grant spending in its purposes, we also estimate a model with AAA grants and nonAAA grants. In the first IV strategy the instruments using region dummies interacted with federal grant spending per capita are reasonably strong. In the second IV strategy we construct the instrument for nonAAA grant spending in a way similar to the manner described above but based on the AAA procedure for handing out grants. For each crop in each year the AAA considered past crop output and current market conditions and set a price per acre to offer farmers to take land out of production. Typically, the allotments of acreage for each state were

Louisiana, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Virginia, or West Virginia.   The area used for the instrument for the West North Central states does not include any states from the East North Central, the West North Central, the East South Central, the West South Central, and the Mountain States.   The area used for the instrument for the South Atlantic states does not include any states from the Mid-Atlantic, the South Atlantic, the East North Central, the East South Central, or the West South Central. The area used for the instrument for the East South Central does not include any states from the Mid-Atlantic, the South Atlantic, the East North Central, the East South Central, the West South Central, or the states of Iowa, Kansas, Missouri, or Nebraska.   The area used for the instrument for the West South Central states does not include any states from the East North Central, the West North Central, the East South Central, the West South Central, or Mountain regions. The area used for the instrument for the Mountain states does not include any states from the West North Central, the West South Central, the Mountain, or the Pacific regions. The area used for the instrument for the Pacific states does not include any states from the Mountain and Pacific regions or the states of Oklahoma, Texas, Kansas, North and South Dakota, and Nebraska.

The results were similar although the instrument was weaker when we created an instrument where we used three regions of the country. The first region is all states east of the states on the eastern border of the Mississippi River, and the states used for the instrument were the Mountain and Pacific states and the states of Kansas, Nebraska, North and South Dakota, and Oklahoma. The second region is all states west of the states that are on the western side of the Mississippi River; so the states used for the instrument were states east of the states on the eastern side of the Mississippi River. The third region was the states along the Mississippi River. The states used for the instrument were from the New England,,Mid-Atlantic, Pacific, and Mountain regions.

24 If the federal government had established a hard budget constraint nationwide, there might have been a negative relationship between spending in the rest of the regions and spending in the state in question. There did not appear to be a hard spending constraint at the national level because Roosevelt and the Congress often approved additional funds throughout the years and ran budget deficits in most years.

based on the average number of acres harvested and the yield per acre in the state over the previous five years. We developed an instrument that partially mimics that process while not including state-level information from the sample period on acreage, yields, or prices in the area. We set the basic structure for AAA payments in 1928 before the sample starts and then update the instrument each year based on changes in AAA spending elsewhere in the country. AAA spending well outside the region where the state was located is multiplied by an agricultural activity measure in 1928 based all crops that received AAA subsidies by 1940.[25] The 1928 activity measure is the share of U.S. crop activity in that state in 1928. The instrument is expected to have strength because it has similarity to the five-year averages that served as the basis of distribution in the actual program. By fixing the share of agricultural activity in 1928 and updating with information on AAA spending well outside the region, the instrument should not be correlated with the error in the final stage regression except through its impact on AAA spending.

Multipliers for Per Capita State Personal Income

The state personal incomes are reported on a calendar year basis, while the federal spending is reported on a fiscal year basis, covering the period from July 1 in year *t-1* to June 30 in year *t*. This automatically imparts a half-year lag into the model.[26] Table 2 shows coefficients and standard errors from a series of estimations with per capita state

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25 The AAA started providing grants for different crops in different years. By using the crops in the program as of 1940, we avoid any endogeneity related to the timing of when programs began for each crop.
26 We investigated interpolating the federal spending in each state to a calendar-year basis by using state level information on monthly employment in programs, but we could not do this for all programs. Without such state-specific information, interpolation runs the risk of incorporating information on government spending in the first half of year *t+1* into the year *t* estimate. We did not carry the process further because the interpolation might introduce biases to the extent that state income in year *t* influences the distribution of federal government spending in year *t+1*.

personal income in $1967 as a function of per capita estimates of federal government

fiscal activity in the state. The first column shows the coefficient and standard errors for

per capita federal grants in the level specification as correlates are added cumulatively

and then IV methods using the region interactions with federal spending as instruments.

The raw Ordinary Least Squares (OLS) estimate of 1.04 with no correlates implies that a

one dollar increase in per capita federal grants increased per capita state income by $1.04.

Controlling for state fixed effects raises this multiplier estimate to 1.43, and adding

weather correlates raises it further to 1.56. The addition of year effects to control for

nationwide shocks reduces the multiplier to 0.45, and the addition of state-specific time

trends lowers the estimate further to 0.16. This last estimate suggests that the federal

spending crowded out a great deal of local economic activity and/or the impact is

weakened greatly because the spending quickly leaked out of the local economy. The

results for the other regression specifications show the same pattern of changes in

coefficients as correlates are added to the analysis. This pattern typically appears when

using other measures of government funding and other measures of economic activity.

Given that the Roosevelt administration distributed more grants to areas where the

economy was in trouble, we instrument to try to eliminate negative bias in the coefficient.

The transition to instrumental variables is consistent with negative endogeneity bias, as

the instrumental variable (IV) coefficients are more positive than the non IV coefficients

from equations with the same correlates. The IV estimates that both control for a great

deal of omitted variable bias and also meet the strongest criteria for instrument strength

are the ones with controls for weather, state effects, and year effects. If we are willing to

accept up to 15 percent weak instrument bias in the size of the coefficient, the critical

value for rejecting the hypothesis of weak instrument bias at the 10 percent level developed by Stock and Yogo (2002a, 2002b) for the Kleibergan-Paap (2006) (KP) version of the Donald F-statistic when using robust standard errors is 12.83 with three instruments instrument for one right-hand side variable. The values if one is willing to accept 20 and 25 percent bias are 9.54 and 7.80. The critical values are lower if weak instrument bias is measured relative to the bias of the OLS coefficient at 9.08 for 10 percent relative bias and 6.46 for 20-percent relative bias.

The IV estimates without year fixed effects generate very high F-statistics that reject the hypothesis of weak instrument bias, but the absence of year effects means that the effects of monetary policy, federal tax rate changes, the National Recovery Administration, and other changes, which were correlated with both income and federal grant spending, are incorporated into the coefficient of federal grant spending. The addition of year fixed effects eliminates this potential problem and the KP statistics reject the hypothesis of 15 percent weak instrument bias when state and year fixed effects are included. The addition of state-specific time trends serves to eliminate potential omitted variable bias but at the cost of losing instrument strength, as the F-statistic of 5.03 is not high enough to reject 25 percent weak instrument bias. Therefore, we will focus the discussion on the IV estimates with state and year effects.

The per capita personal income multiplier estimates for per capita federal grants using the IV of region interactions with federal totals vary only slightly when using different estimating equations. The coefficient is 1.11 using level estimation, 1.10 in the first difference specification, and 1.13 in the growth rate specification. Under the moat/swing instrument strategy, the level specification leads to a multiplier estimate of

1.39. We do not report the difference specifications for the moat-swing instrument strategy because the instrument F-statistics are all less than 3, suggesting weak instrument bias. Although each of the estimates is greater than one, we cannot reject the hypothesis that the multiplier is equal to one. Thus, an additional dollar of federal grants may well have increased personal income by no more than the dollar of grant spending with no additional benefit in the private sector.

Personal Income Multipliers for Different Types of Federal Funding

The estimates of multipliers for different federal funding measures in Table 3 show variations in the point estimates for different types of spending. The multiplier for grants net of federal taxes paid from the state hovers around one. When a dollar in federal loans is treated like a dollar in grants and we estimate the effect of the combined total of grants and loans, we might expect smaller multipliers for the combination of grants and loans. There was a huge surge of loans on World War I adjusted service certificates in 1931 when the economy was falling apart, and the size of the Veterans' Bonus was cut nearly in half in 1936 when the economy was growing the cash grants of over \$3 billion was offset by the repayment of the earlier loans. The estimates under the level specifications in Table 3 are consistent with this expectation with values of .86 and .89. On the other hand, the difference and growth specifications lead to estimates of 1.54 and 1.93. When the loans are treated as providing a subsidy of 10 percent of the loan value, the multipliers for grants and 10 percent of loans are very close to the multiplier for grants alone reported in Table 2 and at the top of Table 3.

There is some sign that specific types of grants may have had stronger effects. When direct transfers to the poor are removed from the grants and the focus is on government purchases, the multiplier estimate is as high as 2.18 in the level specification using the moat-swing instrument strategy and exceeds 1.4 in two other specifications. On the other hand, the estimate is also only 0.38 in the first-difference specification.

Given that the AAA farm grants were designed to take land out of production, we expected that the nonAAA grants would have stronger effects than all grants and that the AAA potentially had negative effects on personal income. Three of the four sets of estimates are consistent with this view. The multipliers for nonAAA grants vary from 0.94 to 1.79, while the AAA grant coefficients are negative in three specifications although very large and positive in the level specification using the region interaction instruments. The findings of the positive effect of nonAAA grants and negative effects of AAA grants are similar to Fishback, Horrace, and Kantor's (2005, 2006) findings using county level data. Using a different instrument strategy, they find that public works and relief were associated with higher retail sales and net in-migration, and that AAA grants were associated with slightly lower retail sales and net out-migration. The payments would have benefitted farm owners and those tenants who received a share of the AAA payments. On the other hand, the likely reduction in the demand for farm labor would have reduced wages and employment for farm workers and share croppers (Fishback, Horrace, and Kantor (2005, 2006).27

27The policy was designed to raise prices for farmers. The negative effect on real personal income from the price rise was likely to have been felt nationwide for crops with national markets. This change would have led to a rise in the CPI and a reduction in real income nationwide that would show up in the coefficients of the year dummies. In that sense, the multiplier estimate will be overstated. There may have been differential

Even though the results in Table 3 show variations in the multiplier for different forms of spending, it is important to note that the standard errors of the estimates are large enough that we cannot reject a multiplier of one for nearly every estimate. Further, the comparisons of multipliers for different types of government funding do not match for each estimation procedure. For example, in looking down the columns for the growth rate specification, the largest multiplier of 1.93 is the one for grants and the full value of loans. In contrast, the multiplier for grants and the full value of loans is lowest in the two level specifications.

The Impact of Federal Grants on Various Measures of Economic Activity

Federal grants had differential effects on various economic measures that are aggregated into per capita personal income. Table 4 shows dollar-for-dollar IV estimates for retail sales per capita, wage and salary income per capita and manufacturing payrolls per capita. Data for retail sales per capita and manufacturing payrolls per capita e data were reported for only some years; therefore, the table includes estimates for per capita personal income for the same years to make comparisons on the same set of observations.

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effects within states due to differences in the consumer prices based on distance between farmgate and final market. Much of this effect would be control for by the state fixed effects. For some crops that sold in local markets, like corn and hay, the local prices might have risen more than at the national level with the reduction in output as long as they stayed within a price range set by transportation costs to other markets (Fox, Fishback, Rhode, 2011). This localized rise would show up in the measure of state personal income but the effect of the rise in limiting purchasing power would not because we have not adjusted for localized differences in the cost of living. Here again the multiplier effect will be slightly overstated.

Dollar-for-dollar estimates cannot be developed when the dependent variables are employment and durable good consumption in the form of automobile registrations, so we report the effects as elasticities evaluated at the mean of the sample along with elasticities for per capita state income.

The estimates suggest that the distribution of government funds had strong effects on consumption but virtually no effect on employment. The dollar-for-dollar coefficient in the retail sales per capita analysis for the four years in which it was available is in the 0.8 to 0.9 range. The coefficient is similar to the coefficient in the per capita personal income estimation for the same four years. Given that the ratio of retail sales to income during this period was roughly 0.5, it appears that the grants had a relatively much stronger effect on retail consumption than it did on income per capita.

The grants also contributed to increases in automobile purchases. The estimates of the elasticity of automobile registrations with respect to grants range from 0.047 to 0.081 while the elasticity estimates for personal income with respect to grants range from 0.086 to 0.108. To put this in dollar terms, value each car at the price of a new Ford in 1934, which was about $500 then and $1,247 when adjusted to the 1967 Consumer Price Index values used to control for the price level. An additional dollar of federal spending raised the value of car registrations by between 10 and 22 cents.

The effect of grants on wage and salary income was small relative to the effect on all personal income. The dollar-for-dollar coefficients for wage and salary income are relatively low, ranging from 0.09 to 0.66. These are much smaller than the coefficients above 1 for personal income. Wage and salary income accounts for roughly 60 percent

of all income during this period, and the wage and salary grant coefficients are less than 60 percent of the coefficients for personal income.

In contrast to their generally strong positive effects on personal income and consumption, the effects of grants on per capita employment and manufacturing payrolls are mostly negative and relatively small in size.28 29 These findings for *private* employment and payrolls are in the same range as the results for the impact of New Deal relief spending found by Neumann, Fishback, and Kantor (2010) for a city-level monthly panel from 1932 through 1939 and Benjamin and Mathews' (1992) findings for a state panel from 1932-1939. Both find that relief spending was associated with crowding out of private employment after 1935. Neumann, Fishback, and Kantor found some positive effects of relief spending on employment from 1932 to 1935, while Benjamin and Mathews find crowding out of about 20 percent. Wallis and Benjamin (1982) also provide cross-sectional estimates that cast doubt on any positive effects of New Deal spending on private employment.

28We put all measures on a per capita basis for consistency. We have also estimated the models for the payrolls and employment without putting them on a per capita basis and the magnitudes of the results are only slightly larger.

29It is possible that the statistically insignificant effect of government spending on manufacturing payrolls might reflect the fact that most manufacturing was selling to national and international markets. If so, a rise in federal spending within a state would only stimulate the demand in that state, which might be a small share of the demand for the product. We checked this hypothesis by estimating the impact of spending on manufacturing payrolls in the bread industry for the years 1929, 1931, 1933, 1935, and 1937. The bread industry was found in every state and tended to sell locally. The bread industry results also show small and statistically insignificant effects of federal spending. The dollar-for-dollar effects 0.2 cents per dollar spent, while the elasticities are smaller than the elasticities reported for manufacturing payrolls per capita in Table 4. The findings for the bread industry are therefore inconsistent with the idea that the small effects on manufacturing are being driven by the dispersed nature of manufacturing consumption.

State by State Estimates

The identification in the state and year fixed effects IV model comes from changes over time within the same state in the part of federal spending correlated with the instrument after controlling for nation-wide shocks in each year. The coefficient is therefore an average across the states. We also estimate the relationship for each individual state over the period using a difference model to reduce problems with non-stationarity. The major issue with the state-by-state estimation is how well the model can control for national shocks common to all states. Our solution is to include the difference in the real per capita national money supply (M2) and a dummy variable for the National Recovery Administration (NRA) period from 1933 through 1935. Cole and Ohanian (2004) argue that the NRA's codes of competition held wages and prices high and contributed greatly to the underemployment of resources. Changes in tax rates were important but there were multiple changes over the period and there are few degrees of freedom. To capture the impact of taxes we estimate the model with grants net of federal taxes in each state. Results are reported for estimates without the taxes for comparison purposes. In this analysis we use the moat/swing instrument, which has fewer problems with weak instrument bias than when total grants spending is used.

Table 5 shows the dollar-for-dollar effects on per capita state personal income of a one dollar increase in per capita grants net of federal taxes in each state. For comparison, when the model specification is estimated for the panel, the multiplier coefficient is 0.605 with a t-statistic of 3.19 and the instrument is strong. The instrument strength varies for each state. There were 33 states where the hypothesis of weak instrument bias of 10 percent was rejected, 7 more estimates that met the 15 percent or

less criteria, and 8 with very weak instruments. The estimates were generally consistent with a view of negative endogeneity, as the IV multiplier estimates were more positive, by an average of about .25, than the OLS coefficients in 35 of the 40 states where weak instrument bias is not a problem.   The average dollar-for-dollar coefficient for those states was 0.80.   For the 40 states with no more than 15 percent weak instrument bias, the average coefficient was .79 and the values ranged from -1.27 in Kansas to 3.4 in Idaho. The multiplier estimates for states that where weak instrument bias could not be rejected included 5 of the 7 largest values in absolute value.

There was not much of a discernable pattern in the estimates across states. Among the states with less than 15 percent weak instrument bias, the extreme values over 2 were found in Pennsylvania, New Jersey, and Idaho. Pennsylvania was probably the most self-sufficient state but Idaho was one of the smallest states. States with values between one and two included states of all sizes from several regions. Some had large populations, like New York, Massachusetts, and California, but most were in the bottom half of the population distribution, including West Virginia, Oregon, Utah, Mississippi, Washington, Maine, and Florida. Negative multipliers were found in Kentucky, Oklahoma, Minnesota, and Kansas.

Conclusions

If there was any time to expect a large peace-time multiplier from federal spending in the states, it would have been during the period from 1930 through 1940. Interest rates were at or near the zero bound for nearly the entire decade, a situation in which macroeconomic models predict stronger fiscal multiplier effects. Unemployment

rates with work relief workers treated as unemployed were never below 14 percent during the decade. Even if people on work relief were treated as employed, the unemployment rate never fell below 9 percent (Darby 1976). There was idle capital in nearly every industry. As a result, there were clearly a large number of underemployed resources that could have been soaked up by federal spending without crowding out private activity.

Estimates controlling for weather shocks, state and year fixed effects, and in some cases state-specific time trend but without using instruments imply that state fiscal multipliers were in the 0.1 to 0.5 range. Given that New Deal funds were distributed in part in response to drops in economic activity, there is ample reason to believe that the non-IV multiplier estimates are biased downward even in models with state and year fixed effects and time trends. The estimates for the state multipliers that we think are most accurate are the IV estimates controlling for state and year fixed effects. The IV estimates without controls for state and year effects are likely too high because they do not control for nationwide monetary and tax rate shocks, nor do they control for fundamental time-invariant features of the states. The addition of state-specific time trends leads to weak instrument bias.

The dollar-for-dollar multiplier estimates show the dollar change in the measure of economic activity in response to a one dollar increase in the measure of government spending. Nearly all of the state per capita personal income multiplier estimates are not statistically significantly different from one. In some estimating strategies, the point estimates of the multipliers for nonfarm grant spending and nontransfer grants are as high as 1.79 and 2.18, but estimates are below one for the same measure, and the standard errors of the estimates in all of those cases are high enough that a multiplier of one cannot

be rejected. Three of four point estimates for coefficients on AAA grants to farmers to take land out of production were negative but not statistically significant. As a general statement, the distribution of a dollar of federal funds to a state led to about a dollar increase in personal income in the state.

Federal grants had a stronger impact on consumption than on personal income. The per capita retail sales multiplier was roughly the same size as the personal income multiplier in the same sample even though retail sales are typically only about 50 percent of income. An additional dollar in federal grants contributed to an increase of about 10 to 20 cents in the value of automobiles registered in the state.

On the other hand, there were no signs that increased government grants raised nonfarm private employment or manufacturing employment. Most of the coefficient estimates were negative with small elasticities. The results are consistent with the findings by Neumann, Fishback, and Kantor (2010) and Benjamin and Mathews (1992) using alternative data sets.

If we apply the lessons of the New Deal to the federal fiscal stimulus today, it is important to realize that the estimates for the states are not for a national multiplier. Instead, they describe the impact within the state of additional federal funding in the state after all leakages are considered.30 In both periods interest rates are near the zero bound

30 Given that the states are part of a large open economy, the impact of federal spending can spill over into other states. The direction of the bias could plausibly go in either direction. The multiplier estimate might be overstated to the extent that a rise in government spending in a neighboring state leads to more spillover spending in the state of interest and federal spending in the neighboring state is positively correlated to government spending in the state of interest. The multiplier would be understated with a positive spillover from government spending in the neighboring state but a negative correlation between federal spending in the neighboring state and the state of interest. As a first cut, we estimated a model with a spatial lag for income in neighboring states. The

and unemployment rates are well above long run averages, although the problems of the

Depression were far worse. The New Deal results suggest that federal fiscal stimulus

during a modern recession would stimulate income in the states roughly dollar-for-dollar

but have little impact on private nonfarm employment in the state.

nonIV and IV results lead to substantially smaller multipliers, but we have not yet found
a strong instrument for the income in the neighboring states.

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Figure 1
Annual Changes in Per Capita New Deal Grants and Changes in Per Capita Personal
Income for the years 1930 through 1940 by State in 1967 Dollars

Table 1
Total and Per Capita Federal Spending by Program in Millions of Contemporay
Dollars for the period July 1, 1932 through June 30, 1939

	Acronym	Amounts From July 1, 1932 to June 30, 1939 (Millions $)	Per Capita	Category	First Fiscal Year with Significant Spending	Ended Before 1939
TOTAL TAXES COLLECTED FROM STATES		26,061	213.11			
NONREPAYABLE GRANTS						
Works Progress Administration	WPA	6,844	55.97	Work Relief	1936	
Veterans' Administration	VA	3,955	32.34	Relief	Pre 1933	
Federal Emergency Relief Administration	FERA	3,059	25.02	Relief and Work Relief	1934	Mar-37
Agricultural Adjustment Administration1	AAA	2,863	23.41	Agriculture	1934	
Civilian Conservation Corps	CCCG	2,130	17.42	Work Relief	1934	
Public Roads Administration	PRA	1,613	13.19	Public Works	Pre 1933	
Rivers and Harbors and Flood Control	RHFC	1,316	10.76	Public Works	Pre 1933	
Public Works Administration--Nonfederal Projects	PWANF	1,032	8.44	Public Works	1934	
Civil Works Administration	CWA	807	6.60	Relief/Public Works	1934	Mar-34
Social Security Act	SSA	759	6.21	Relief	1936	
Public Works Administration--Federal Projects	PWAF	632	5.16	Public Works	1934	
Balance from Relief Acts	BRA	376	3.08	Relief	1936	
Public Buildings Administration	PBA	324	2.65	Public Works	Pre 1933	
Bureau of Reclamation	BR	290	2.37	Public Works	1934	
Farm Security Administration	FSA	273	2.24	Agriculture	1936	
National Guard	NG	219	1.79	Military	Pre 1933	
Public Works Administration--Housing Projects	PWAH	129	1.05	Public Works	1935	
Soil Conservation Service	SCS	100	0.82	Agriculture	1934	
Agricultural Extension Work	AE	94	0.77	Agriculture	Pre 1933	
Vocational Education	VE	90	0.74	Education	Pre 1933	
U.S. Employment Service	USES	80	0.65	Relief	1934	
Indian Service - Civilian Conservation Corps	CCCIS	51	0.42	Relief	1934	

Agricultural Experiment Stations	AEX		36	0.29	Agriculture	Pre 1933
Forest Service (Roads)	FSR		34	0.28	Public Works	1937
Colleges of Agriculture and Mechanical Arts	CAM		24	0.19	Education	Pre 1933
Forest Funds	FF		17	0.14	Public Works	Pre 1933
Mineral Lease Act Payments	ML		11	0.09	Public Works	Pre 1933
Land Utilization Program	LUP		11	0.09	Public Works	1939
State Soldiers' and Sailors' Homes	SSS		4	0.03	Relief	Pre 1933
Special Funds	SF		2	0.02	Miscellaneous	Pre 1933
Office of Education--Emergency Relief Act Funds	OE		2	0.02	Education	1936
State Marine Schools	SMS		1	0.01	Education	Pre 1933
Books for the Blind	BFB	a)		0.00	Education	Pre 1933
Federal Water Project Payments	FWP	a)		0.00	Public Works	Pre 1933
Nonrepayable Grants Total			27,180	222.26		
REPAYABLE LOANS CLOSED						
Reconstruction Finance Corporation	RFC		4,782	39.11	All	1932
Farm Credit Administration	FCA		3,957	32.35	Agriculture	Pre 1933
Home Owners' Loan Corporation	HOLC		3,158	25.83	Home Finance	1934	1936
Commodity Credit Corporation	CCCL		1,186	9.70	Agriculture	1934
Public Works Administration	PWAL		508	4.15	Public Works	1934
Farm Security Administration	FSAL		337	2.76	Agriculture	1934
Home Owners' Loan Corporation and Treasury Investments in Bldg. and Savings and Loans Associations	HOLCT		266	2.17	Home Finance	1934
Federal Reserve Banks.	FRB		125	1.02	Finance	1935
Rural Electrification Administration	REA		123	1.01	Agriculture	1936
U.S. Housing Authority	USHA		56	0.45	Public Works	1939
Farm Tenant Purchases	FTP		33	0.27	Agriculture	1938
Disaster Loan Corporation	DLC		17	0.14	Relief	1937
Total Repayable			14,549	118.97		
Value of Loans Insured by Federal Housing Administration			0	0.00		
Title I --Refurbishing and Maintanence Loans			834	6.82	Home Finance	1936
Title II--Home Mortgages.			1,855	15.17	Home Finance	1936
Total Housing Loans Insured			2,689	21.99		

[a]Under 500,000 dollars.

Table 2
Estimates of Dollar-for-Dollar Effect of Per Capita Grants on State Per Capita Personal Income, 1930-1940
t-statistics Listed Below Coefficients

		Estimation Specification			
Level/Difference		Level	Difference	Growth Rate	Level
LEAST SQUARES					
No controls	Coeff.	**1.04**			**1.04**
	t-stat.	*2.70*			*2.70*
Controls state effects	Coeff.	**1.43**	**1.00**	**1.21**	**1.43**
	t-stat.	*7.24*	*7.22*	*9.17*	*7.24*
Controls state effects and weather	Coeff.	**1.56**	**1.09**	**1.33**	**1.56**
	t-stat.	*7.10*	*6.95*	*9.56*	*7.10*
Controls year effects, state effects, and weather	Coeff.	**0.45**	**0.73**	**1.18**	**0.45**
	t-stat.	*2.29*	*3.40*	*5.61*	*2.29*
Controls state time trends, year effects, state effects, and weather	Coeff.	**0.16**	**0.75**	**1.23**	**0.16**
	t-stat.	*0.69*	*3.07*	*5.18*	*0.69*

TWO-STAGE LEAST SQUARES

		Instrument Choice			
Instruments		Interaction of National with 3 Region Dummies			Moat times Swing
Controls Weather and state Effects	Coeff.	**1.94**	**1.28**	**1.29**	**2.16**
	t-stat.	*15.28*	*7.23*	*11.10*	*15.41*
	Instrument F-stat.	198.67	189.66	71.86	192.72
Controls year effects, state effects and weather	Coeff.	**1.11**	**1.10**	**1.13**	**1.39**
	t-stat.	*6.14*	*2.30*	*3.16*	*3.44*
	Instrument F-stat.	13.07	8.15	8.59	7.09
Controls state time trends, year effects, state effects, and weather	Coeff.	**0.50**	**1.12**	**1.14**	**0.97**
	t-stat.	*2.41*	*2.33*	*3.38*	*1.80*
	Instrument F-stat.	5.03	6.94	7.49	4.14

Sources: See Data Appendix.
Notes: This is a balanced panel with information for 48 states for each year from 1930 through 1940. Estimation used the STATA 10 reg and ivreg2 programs. For the calculations of t-statistics, standard errors are based on White corrections using the robust command with standard errors clustered at the state level. The instrument F-

statistic is the Kleibergen-Paap rank Wald (KP) F statistic. When compared with the Stock-Yogo critical values, the Kleibergen-Paap rank Wald (KP) F statistic can be used to test for weak-instrument bias based on the maximum weak-instrument bias that one is willing to accept. In the analyses with one instrument for one government activity measure, the critical value is 16.38 at the 10 percent confidence level if someone is unwilling to accept more than 10-percent weak instrument bias, 8.96 for unwillingness to accept more than 15 percent bias and 6.66 for 20 percent bias. When using three instruments the critical values for maximal size of weak instrument bias are 22.30 at the 10 percent level if someone is unwilling to accept more than 10-percent weak instrument bias, 12.83 for unwillingness to accept more than 15 percent bias, 9.54 for 20 percent bias, and 7.80 for 25-percent bias. The critical values for willingness to accept 10-percent weak instrument bias relative to the bias from OLS estimation is 9.08 and for willingness to accept 20% bias is 6.46.

Table 3
Estimates of Dollar-for-Dollar Effect of Per Capita Measures of Government
Funding on State Per Capita Personal Income, 1930-1940
t-statistics Listed Below Coefficients

		Level	First Difference	Growth Rate	Level
		Interaction of National with 3 Region Dummies			Moat times Swing
		Controls Weather, State and Year Effects			
Grants only	Coeff.	**1.11**	**1.10**	**1.13**	**1.39**
	t-stat.	*6.14*	*2.30*	*3.16*	*3.44*
	Instrument F-stat.	13.07	8.15	8.59	7.09
Grants plus all loans	Coeff.	**0.89**	**1.54**	**1.93**	**0.86**
	t-stat.	*5.54*	*2.20*	*2.78*	*1.98*
	Instrument F-stat.	16.38	6.46	4.48	9.41
Grants Plus 10 % of Loans	Coeff.	**1.05**	**1.14**	**1.20**	**1.08**
	t-stat.	*5.19*	*1.91*	*3.13*	*1.94*
	Instrument F-stat.	12.74	7.97	8.58	7.85
Grants Minus Taxes	Coeff.	**0.93**	**1.06**	**1.14**	**1.08**
	t-stat.	*3.59*	*1.99*	*3.04*	*1.49*
	Instrument F-stat.	12.01	7.52	8.46	5.55
Grants Minus Transfers	Coeff.	**1.67**	**0.38**	**1.46**	**2.18**
	t-stat.	*4.14*	*0.52*	*3.64*	*1.97*
	Instrument F-stat.	12.45	9.86	12.56	6.86
Grants minus AAA	Coeff.	**0.94**	**1.26**	**1.42**	**1.79**
	t-stat.	*3.00*	*2.13*	*2.08*	*2.70*
	Instrument F-stat.	11.88	7.36	7.42	4.44
AAA	Coeff.	**3.68**	**-2.17**	**-0.84**	**-0.41**
	t-stat.	*2.19*	*-0.74*	*-0.32*	*-0.62*
	Instrument F-stat.	17.58	12.07	11.91	10.28
	combined F-stat.	15.68	7.34	3.06	4.27

		Controls Weather, State and Year Fixed Effects, and State-Specific Time Trends			
Grants only	Coeff.	**0.50**	**1.12**	**1.14**	**0.97**
	t-stat.	*2.41*	*2.33*	*3.38*	*1.80*
	Instrument F-stat.	5.03	6.94	7.49	4.14
Grants plus all loans	Coeff.	**0.77**	**1.72**	**2.20**	**0.86**
	t-stat.	*2.94*	*2.16*	*2.72*	*1.26*
	Instrument F-stat.	11.72	4.04	3.44	7.73
Grants Plus 10 % of Loans	Coeff.	**0.65**	**1.19**	**1.22**	**1.08**
	t-stat.	*2.30*	*1.83*	*2.86*	*1.23*
	Instrument F-stat.	6.28	6.40	7.38	4.04
grants minus taxes	Coeff.	**0.47**	**1.08**	**1.13**	**1.21**
	t-stat.	*2.17*	*1.95*	*2.99*	*1.37*
	Instrument F-stat.	4.76	6.42	7.98	2.06
Grants minus AAA	Coeff.	**0.51**	**1.30**	**1.54**	**1.73**
	t-stat.	*2.32*	*2.01*	*1.73*	*1.98*
	Instrument F-stat.	4.49	6.11	6.60	1.87
AAA	Coeff.	**2.95**	**-2.08**	**-1.47**	**-3.52**
	t-stat.	*1.44*	*-0.63*	*-0.39*	*-1.39*
	Instrument F-stat.	10.39	9.17	8.86	4.29
	combined F-stat.	7.71	5.24	1.85	2.06
Grants minus AAA	Coeff.	**0.52**	**1.35**	**1.34**	**1.73**
	t-stat.	*2.20*	*2.21*	*2.42*	*1.98*
	Instrument F-stat.				
AAA	Coeff.	**-0.73**	**-3.89**	**-2.93**	**-3.52**
	t-stat.	*-0.75*	*-2.75*	*-1.76*	*-1.39*
	Instrument F-stat.				
	combined F-stat.	6.62	20.45	8.45	2.06

All analyses are from individual regressions except for the nonAAA and AAA results, which are estimated in the same regression. The instrument F-statistic is the Kleibergen-Paap rank Wald (KP) F statistic. See notes to Table 2 for instrument strength critical values with on variable that requires instruments. In the analysis with nonAAA and AAA grants the critical values with 10 percent confidence are 7.03 for 10 percent bias, 4.58 for 15 percent bias, and 3.95 for 20 percent bias. When loans are included the panel drops 1940 due to problems in obtaining data for loans in 1930. The measures of economic activity are on a calendar year basis

while the measures of government activity are on the July t-1 to June t fiscal year basis. Commodity Credit Corporation loans were dropped from the loan figures due to inaccurate measurement of the distribution of loans across states.

Table 4
Estimates of Impact of Government Grants on Measures of Economic Activity
t-statistics Listed Below Coefficients

		Level	First Difference	Level
		Interaction of National with 3 Region Dummies		Moat times Swing
Per Capita State Income	Coeff.	**1.11**	**1.10**	**1.39**
	t-stat.	*6.14*	*2.30*	*3.44*
	Instrument F-stat.	13.07	8.15	7.09
Per Capita Wage and Salary Income	Coeff.	**0.27**	**0.09**	**0.66**
	t-stat.	*2.01*	*0.56*	*1.59*
	Instrument F-stat.	13.07	8.15	7.09
retail sales per capita, 1929, 1933, 1935, 1939	Coeff.	**0.822**		**0.884**
	t-stat.	*1.62*		*2.05*
	Instrument F-stat.	6.13		4.36
Income per capita, 1929, 1933, 1935, 1939	Coeff.	**0.853**		**0.941**
	t-stat.	*3.73*		*1.83*
	Instrument F-stat.	6.13		4.36
Manufacturing payroll per capita 29,31,33,35,37,39	Coeff.	**-0.21**		**-0.01**
	t-stat.	*-3.09*		*-0.03*
	Instrument F-stat.	9.91		6.84
Income per capita 29,31,33,35,37,39	Coeff.	**0.89**		**1.05**
	t-stat.	*4.39*		*2.37*
	Instrument F-stat.	9.91		6.84
ELASTICITIES				
Per Capita State Income	Coeff.	**0.086**	**0.086**	**0.108**
	t-stat.	*6.14*	*2.3*	*3.44*
	Instrument F-stat.	13.07	8.15	7.09
Nonfarm Employment per Capita	Coeff.	**-0.019**	**0.001**	**-0.013**
	t-stat.	*-1.41*	*0.06*	*-0.47*
	Instrument F-stat.	13.07	8.15	7.09
Manufacturing employment per	Coeff.	**-0.082**	**-0.003**	**-0.066**
	t-stat.	*-3.51*	*-0.03*	*-2.38*

capita 29,31,33,35,37,39	Instrument F-stat.	13.07	7.09	7.09
Auto Registrations Per Capita	Coeff.	**0.075**	**0.047**	**0.081**
	t-stat.	*3.81*	*2.08*	*2.38*
	Instrument F-stat.	13.07	8.15	7.09

Table 5
Dollar-for-Dollar Estimates by State from Differenced Regressions
Using OLS and IV

State	Dollar-for-Dollar Coefficient	t-statistic	Dollar-for-Dollar Coefficient	t-statistic	KPF
Rejects 10-percent Weak Instrument Bias at the 10-percent Level					
New Jersey	0.95	0.83	2.21	2.85	19.32
Pennsylvania	0.78	0.62	2.06	2.62	41.24
West Virginia	1.26	1.66	1.80	6.13	28.82
Oregon	1.40	2.48	1.59	4.58	26.02
Utah	0.96	1.10	1.26	2.20	79.67
Massachusetts	0.54	1.48	1.22	1.88	30.50
Mississippi	0.97	2.11	1.07	2.98	37.68
Washington	0.84	1.15	1.06	2.28	68.63
California	0.85	1.04	1.03	1.76	21.15
North Carolina	0.62	0.85	0.86	1.93	22.05
Vermont	0.66	1.62	0.78	2.91	83.26
Colorado	0.79	1.61	0.76	1.98	81.61
Tennessee	0.47	0.57	0.73	1.69	41.54
New Hampshire	0.63	1	0.70	2.1	61.47
Arkansas	0.59	1.39	0.70	2.2	25.75
Ohio	0.47	0.45	0.66	0.77	25.94
New Mexico	0.51	1.10	0.64	2.27	44.83
Rhode Island	1.29	1.94	0.61	1.18	259.36
Alabama	0.56	1	0.61	1.55	44.83
Connecticut	0.57	0.5	0.61	3.19	144.60
Louisiana	0.47	0.97	0.58	1.36	31.52
Indiana	0.33	0.24	0.57	0.61	68.43
Wyoming	0.23	0.28	0.52	0.75	19.37
Georgia	0.48	1.33	0.51	1.98	67.28
Illinois	0.16	0.12	0.49	0.44	23.97
Wisconsin	0.26	0.27	0.37	0.5	118.42
South Carolina	0.55	1.84	0.37	1.67	70.08
Missouri	-0.06	-0.07	0.32	0.52	36.11
Virginia	0.00	0	0.12	0.26	62.49
Kentucky	-0.82	-0.12	-0.03	-0.06	35.22
Oklahoma	-0.14	-0.19	-0.09	-0.15	58.38
Minnesota	-0.82	-0.81	-0.50	-0.65	28.48
Kansas	-0.92	-1.03	-1.27	-1.32	25.50
Rejects 20-percent Weak-Instrument Bias at 10 percent Level					
Idaho	4.02	4.26	3.46	4.26	10.20
New York	0.34	0.47	1.50	1.51	9.33
Maine	0.84	1.08	1.29	1.69	15.30

Florida	0.83	0.92	1.19	1.24	11.06
Texas	0.10	0.13	0.67	0.79	10.49
Maryland	0.08	0.14	0.50	1.20	10.53
Arizona	0.01	0.00	0.28	1.07	14.81
Cannot Reject 20-percent Weak-Instrument Bias at 10 percent level.					
South Dakota	3.42	3.24	17.54	0.82	0.26
Nevada	0.54	1.35	3.51	1.15	0.73
Delaware	0.63	1.05	1.29	1.18	1.58
Michigan	-1.11	-1.68	1.09	0.62	3.30
Montana	1.14	1.65	-0.52	-0.68	5.97
Nebraska	0.40	0.26	-6.32	-1.53	2.94
Iowa	-3.76	-1.15	-8.30	-2.85	3.94
North Dakota	2.81	2.08	-24.79	-0.34	0.08

Notes. The estimates come from difference regressions of the change in real per capita personal income on the change in real per capita federal grants minus taxes, the change in the real money supply, a dummy for the NRA period, and a year trend. The estimates over the period 1930 through 1940 for each state.