The Impact of Federal Government Expenditures in the 1930s*

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I. Introduction

There is no shortage of explanations for the depressed conditions of the 1930s. Most of the recent literature has focused on the causes and severity of the Great Depression of 1929–33, but the failure of production to expand much beyond its 1929 peak by 1937 and the sharp 1937–38 recession have also received attention.[1] At the heart of the discussion has been the debate over monetary versus nonmonetary forces and the role of the Federal Reserve. Yet among possible nonmonetary forces, little attention has been given to the role of Federal government expenditures. To cite only one example, in the comprehensive study of this era edited by Brunner [9], the role of government spending is ignored—or, at best, mentioned only in passing—in the volume's seventeen papers and comments.

The story of this period is a familiar one. A cyclical peak in the economy occurred in the summer of 1929, only months before the famous stock market crash of October. While the contraction did not proceed uniformly, the economy plunged into a deep depression with precipitous declines in production, prices, and the money supply, waves of bank failures and bankruptcies, and an unprecedented increase in the unemployment rate. The trough was reached in March 1933 at the time of President Roosevelt's election and the start of the New Deal. Then followed a cyclical expansion of 50 months, one of the longest in American history; yet unemployment remained exceptionally high and per capita output was actually lower at the peak in 1937 than at the preceding peak in 1929. During this expansion wages and prices rose rapidly despite substantial

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1. Many writers analyze the 1938–41 expansion as well. In this paper we define the interwar period as July 1921–June 1938 and thus omit any consideration of the 1938–41 period.

unused resources. The expansion was then interrupted by another recession of major proportions in 1937–38, before recovery began once again.[2]

Perhaps the best known explanation of the 1929–33 contraction is the monetarist interpretation of Friedman and Schwartz [18]. Their main theme is that whatever economic forces initiated the contraction, their effects were magnified by the unprecedented decline in the money supply that resulted from the banking crises [41]. The massive decline in the money supply disrupted real economic activity. The subsequent expansion in money, attributable largely to an inflow of gold, facilitated the long expansion that followed, while the Federal Reserve's decision to double reserve requirements in three stages in 1936–37 was a major factor in the timing and severity of the following recession.

Temin [48; 49] has mounted a major attack on the monetarist view, but one which focuses narrowly on the crucial two years prior to the British abandonment of the gold standard in September 1931. In this period, he argues, there is no evidence of deflationary pressures from the banking system. Rather, nonmonetary forces played the primary causal role with money being a passive, endogenous variable. The basic force was the autonomous behavior of consumption in 1929.[3] Gordon and Wilcox [22] add residential housing as another important nonmonetary force in the early years of the depression, a variable also emphasized in Bolch and Pilgrim [7] and R. A. Gordon [21].

In an important paper, Gordon and Wilcox [22, 50] conclude that ". . . both extreme monetarist and nonmonetarist interpretations of the decade of the 1930s are unsatisfactory. . . ." In particular, their empirical work suggests an important role for money in explaining the declines in output in 1931–33 and 1937–38. They do not attribute to money an important role in 1929–31, but rather emphasize the role of nonmonetary factors in this episode. Burbidge and Harrison [10] use historical decompositions in evaluating the role of money. In general, they find money to have a major, but not a solitary, role. In particular, they agree with Gordon and Wilcox for 1929–31 and 1937–38, but assign less importance to the role of money in explaining 1931–33.

Other important explanations of the causes and/or severity of the 1930's experience are found in the works of Meltzer [31]—the shock effects of the Hawley-Smoot tariffs—Bernanke [5]—the importance of nonmonetary financial variables—and Romer [38]—the effect of uncertainty due to stock market variability on consumption. Although impossible to quantify, a recurrent theme in the literature is the disincentive effects of many New Deal programs during the recovery period—e.g., the NIRA codes, minimum wage laws, a wide variety of tax increases, increased regulation of business, and government expansion into areas previously reserved for private enterprise.[4]

Certainly, this brief recapitulation does not exhaust the plausible stories of the depressed 1930s. Given the variety of explanations offered, however, it is interesting that so little has been said about the role of fiscal policy in either exacerbating or ameliorating the conditions of this period. Certainly, many contemporary writers of this era saw government expenditures and taxes as playing important roles in the events of the time.[5] The apparent current lack of interest in this topic (at least by omission) may be in no small part attributable to an acceptance of the now classic study of this period by Brown [8]. In this often-cited paper, Brown's study, using annual

2. For detailed descriptions of this era, see Friedman and Schwartz [18, 299–545] and Chandler [11].

3. For strong attacks on Temin's thesis, see Mayer [29] and Hamilton [25].

4. For example, see Roose [39, 59–69], Friedman and Schwartz [18, 493–99], and Meltzer [32]. Weinstein [52], in particular, argues strongly that the NIRA codes (in place from June 1933 to May 1935), while not independently altering government expenditures or taxes, caused a substantial loss in output and employment.

5. See, for example, the discussion of fiscal policy in Roose [39, 70–86].

data and a relatively crude measure of the full employment surplus, indicated that fiscal policy was neither a primary factor in the collapse of aggregate demand, and thus not an important cause of the Great Depression, nor a major factor in the recovery. Brown concluded [8, 863 and 866] that: "Fiscal policy, then, seems to have been an unsuccessful recovery device in the 'thirties—not because it did not work, but because it was not tried." This conclusion, however, was based on the combined impact of federal, state, and local government policies. When focusing on the federal level alone, Brown concluded that the federal government's fiscal action was at least more expansionary throughout the 1930s than it was in 1929. While 1929 was a year of "substantial net drag," this changed sharply to an expansionary effect in 1931. The expansionary effects were greatest for 1934, 1935, and 1936.

Peppers [35], focusing on the federal level only, reestimated the full employment surplus and disputed Brown's modest conclusion that federal fiscal policy was consistently more expansionary during the 1930s than it was in 1929. Like Brown, he also traced an erratic path for fiscal policy that conveyed no clear or consistent message, and judged fiscal policy to be most expansionary in 1931 and 1936, both years of large veterans' bonuses.

Stein [46, 3–130; 47, 27–63] and others have described in detail the attitudes, philosophies, and preconceptions of Hoover, Roosevelt, and other influential people of the time that stood in the way of a systematic approach to government expenditures as a stabilization device. Hoover's policy response to the depression has been described as a combination of confidence-building publicity, coordination and promotion of voluntary efforts, and modest federal spending on public works [23]. Roosevelt, like Hoover, had strong concerns for a balanced budget. Roosevelt justified his earliest budget deficits by separating the regular (balanced) budget from an "emergency" budget for relief spending (although by 1938 he was defending deliberate deficit finance as a contribution to rising income.)

In this paper we take a fresh look at an old question of the impact of federal government expenditures in the context of a model that controls for the distortionary effects of marginal tax rates. We first look at the impact of government expenditures over the entire interwar period, which we define as July 1921–June 1938.[6] Using monthly data, we evaluate the effects of government expenditures on output and prices within the context of a small macro model. In lieu of a structural model approach, we estimate a five-variable vector autoregression (VAR) model in order to avoid the use of possibly spurious restrictions. Our five-variable VAR comprises federal government expenditures, the money supply, interest rates, prices, and industrial production. We control for the distortionary effects of changes in marginal tax rates by including measures of average personal and corporate marginal tax rates as deterministic variables.

The primary motive for much of the recent research in macrotheory has been the Ricardian equivalence hypothesis. In contrast to standard neoclassical and Keynesian models in which an increase in government debt affects the economy because households view it as net wealth, the Ricardian equivalence hypothesis, as developed by Barro [1], does not view an increase in government debt as an addition to private sector wealth. Rather, an increase in government debt is seen by households as equivalent to a future increase in taxes to service and repay the debt. The present generation will altruistically save more in order to increase its bequest to the next generation so that future generations can pay the higher future taxes. Thus, private saving will

6. We define the interwar period as July 1921–June 1938 in order to eliminate the effects of war-related government expenditures and focus exclusively on a peace-time economy. This sample period corresponds to the four peace-time cycles identified by Firestone [16]. For a more detailed explanation of this choice of time period, see Beard and McMillin [4].

increase by the size of the deficit. Budget deficits are a matter of indifference in that a switch from lump sum tax to debt finance of a given level of government purchases has no effect on interest rates, output, or prices. Since previous research has indicated small and insignificant effects of deficits over the interwar period when both deficits and government expenditures were included in the same model,[7] we feel justified in omitting a deficit variable in this paper. Even in the Ricardian framework, changes in government purchases and in distortionary, or nonlump sum, taxes have real effects. For example, in Barro's [2; 3] "market clearing" approach, an increase in government purchases unambiguously raises real output, although these effects are likely small in peace-time periods, while the effects on other key macro variables are either ambiguous or depend on whether the purchases are considered permanent or temporary.

In this paper, we analyze the effects of real federal government expenditures in the interwar period through the computation of variance decompositions (VDCs). A relatively unique feature is the use of a Monte Carlo simulation technique to estimate standard errors for the VDCs. This allows a judgment as to the significance of the results. We next compute historical decompositions (HDCs) so as to focus more closely on the role of government expenditures in the 1930s. We report HDCs for a number of alternative subperiods in order to see if the role of government expenditures was different in the Hoover administration and the early New Deal.

II. Model Specification

As noted in section I, the macroeconomic effects of government expenditures are examined by estimating and analyzing a five-variable VAR model. The reduced form VAR methodology is employed for the following reasons. First, the VAR technique avoids the imposition of potentially spurious a priori constraints that are employed in the specification of structural models. Second, as pointed out by Fischer [17] and Genberg, Salemi, and Swoboda [20], since few restrictions are placed on the way in which the system's variables interact, VARs are well-suited to an examination of the channels through which a variable operates. However, since the VAR technique is a reduced form procedure, it is difficult to distinguish sharply among structural hypotheses. Other limitations have been discussed by Cooley and LeRoy [12]. Since our purpose is to understand the channels through which government spending operates, the VAR procedure is appropriate.

As noted earlier, the model employed in this study consists of five variables: output, the price level, the interest rate, the money supply, and government spending. Additionally, measures of average personal and corporate marginal tax rates are added as deterministic variables. The variables chosen were included for the following reasons. The rate of interest summarizes conditions in financial markets while output and the price level reflect the state of the goods market. Monetary policy is represented by the money supply while the stance of fiscal policy is indicated by government spending and by the inclusion of the average marginal tax rates.

The empirical counterparts to the model variables are as follows. The interest rate (*BAA*) is Moody's corporate *BAA* series and comes from *Banking and Monetary Statistics 1914–1941* (Table 128, Board of Governors of the Federal Reserve System, 1943). A long-term rate was chosen since most theoretical treatments of investment behavior focus on long-term interest rates. We felt that a risky rate like the *BAA* rate was more relevant to the investment decisions of most

7. See Raynold, Beard, and McMillin [36] and Beard and McMillin [4]. For another study that finds only modest effects of deficits, see McMillin and Beard [30].

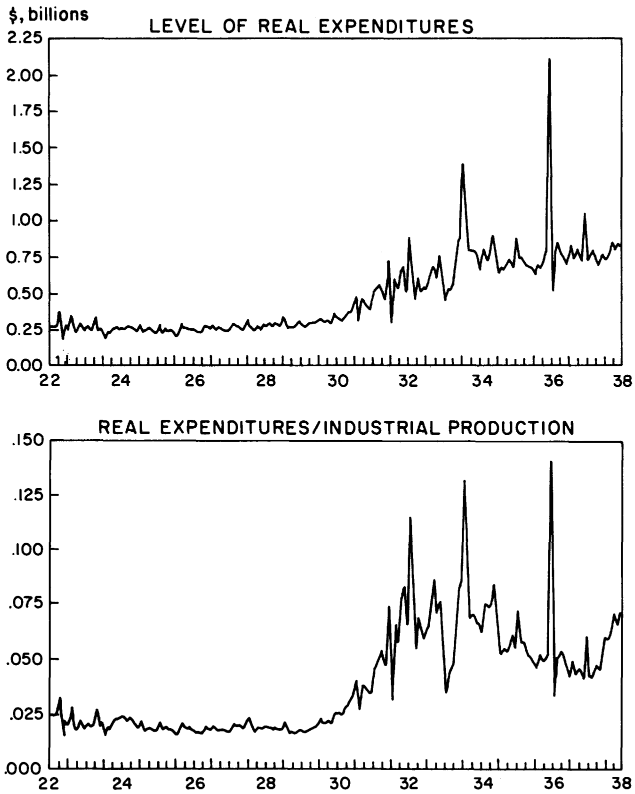

Figure 1. Real Federal Expenditures

firms than was a rate like the *AAA* bond rate. However, the sensitivity of our results to the use of the *BAA* rate was checked, as discussed later in the paper. The price level measure (*WPI*) is the wholesale price index and comes from the 1933, 1938, and 1943 editions of the *Statistical Abstract of the United States*.[8] Output is measured by the industrial production index (*IP*) with 1977 as the base year. Data for *IP* are taken from the 1985 revision of *Industrial Production* (Board of Governors of the Federal Reserve, 1985). The sensitivity of our results to Miron and Romer's [33] alternative measure of *IP* is also examined. Money (*M2*) is Friedman and Schwartz's [18] measure of *M2* from their Table A-1.[9] Government expenditures (*EXP*) include both purchases of goods and services and transfer payments. Although it would be preferable to focus on government purchases alone, separate series on purchases and transfer payments were unavailable. *EXP,* which are taken from Firestone's [16] Table A-3, are measured in billions of dollars, and are deflated by *WPI*. This series is plotted for our estimation period 1922:7–1938:6 in Figure 1.

8. As noted by the referee, a series like the consumer price index might be preferable to the *WPI*. However, apparently no reliable monthly index of consumer prices exists for our period. The *Monthly Bulletin of Statistics* published by the League of Nations contains a monthly series for an index of retail food prices and a broader *quarterly* index of the cost of living. The food price index does not seem comprehensive enough for our purposes, and the quarterly index would have to be interpolated in some fashion to generate a monthly series. We note that other studies of this period that used monthly data also employed the *WPI*.

9. *M2* is the preferred measure of Friedman and Schwartz. However, at the suggestion of the referee, who argued that high-powered money is a better measure of monetary policy than is *M2*, we substituted high-powered money for *M2*. The results were essentially unchanged; in particular, the point estimates of the VDCs for the system with high-powered money were well within one standard deviation of those in column 1, Table II.

We note that real expenditures were relatively constant from 1922—mid-1930. They rose fairly steadily during most of the Hoover administration, but were relatively constant during the portion of the Roosevelt administration included in our sample. The large spike in expenditures during 1936:6 is due to advanced payment of veterans' bonuses. Inclusion of a dummy variable that took on a value of 1 in 1936:6 and 0 in other periods in the model described later had no material effect on our results. The variability of real expenditures appears to have increased after 1930; however, Breusch-Pagan tests for heteroskedasticity in the real expenditure equation of our model yielded no evidence of heteroskedasticity in this equation.

As noted above, the levels of the average personal and corporate marginal tax rates were added to each equation as deterministic variables. The tax rate series were derived from the yearly rates calculated by Seater [43; 44]. Monthly series were constructed by setting the monthly values for a year equal to the yearly value calculated by Seater. The decision to include the tax variables as deterministic variables was largely due to the nature of tax policy over our sample. The tax rate was varied infrequently over our sample so that the rate often remained constant for several years at a time. Including the levels of the tax rates controls for the effects of tax rates within the system but does not allow us to examine how shocks to these variables affect the system.

It is necessary to render the data stationary prior to specification and estimation of the VAR. Augmented Dickey-Fuller tests for first order unit roots of the type described by Nelson and Plosser [34] were performed in order to determine the appropriate transformation of the variables. These tests, which employed 14 lags, were performed over the 1922:7–1938:6 period, and the estimated test statistics are reported in Table I,A. Following the suggestion of Schwert [42], the lag length was determined by taking the integer value of $12(T/100)^{.25}$ where T = number of observations (192, for our sample).[10] The tests indicate first order unit roots for the log levels of *IP*, *WPI*, *M2*, and *EXP* and the level of *BAA*. This suggests that first differences of the logs of *IP*, *WPI*, *M2*, and *EXP* and the first difference of the level of *BAA* should be used in specifying and estimating the models.

Engle and Granger [15] have emphasized the importance of testing for cointegration among the variables included in a VAR model. These variables are said to be cointegrated if each, taken separately, is nonstationary but some linear combination of the variables is stationary. Engle and Granger point out that a VAR estimated with only differenced data will be misspecified if the variables are cointegrated. The Dickey-Fuller tests described earlier indicated that the nondifferenced variables are nonstationary, and the procedure described in Engle and Yoo [14] is used to test for cointegration. In this procedure, a cointegrating regression is first estimated. In this regression, the contemporaneous value of one of the model variables is regressed on a constant and the contemporaneous values of the remaining variables in the model. The residuals from this equation are then subjected to a Dickey-Fuller test. Failure to reject the null hypothesis of a unit root is evidence against cointegration since a unit root indicates that the linear combination of the variables is nonstationary. There are as many cointegrating regressions as there are variables in the system.

The cointegrating regressions were estimated over 1922:7–1938:6, and the second stage Dickey-Fuller tests, which again employed 14 lags, were estimated over 1923:10–1938:6. The results of these tests are reported in Table I,B in the column labeled Engle-Yoo. Based upon a

10. Schwert [42] has emphasized the importance for all series under consideration of allowing for long lags in Dickey-Fuller unit root tests since results from tests with long lags are less sensitive to the presence of an unknown moving average component than are tests with short lags. His criterion allows for lag length to grow with sample size.

Table I. Unit Root and Cointegration Tests

A. UNIT ROOT TESTS[a]

Variable	Estimated Test Statistic
LIP	−2.32
LWPI	−2.07
LM2	−2.68
LEXP	−1.96
BAA	−2.30

B. COINTEGRATION TESTS

Variable	Estimated Test Statistic	
	Engle and Yoo[b]	Hansen[c]
LIP	−3.79	−3.12
LWPI	−2.18	−1.01
LM2	−2.15	−1.72
LEXP	−1.80	−3.42
BAA	−3.51	−2.73

a. *LIP*, *LWPI*, *LM2*, and *LEXP* are the log levels of *IP*, *WPI*, *M2*, and *EXP*, respectively. The critical value of the test statistic at the 5% level is ≈ −3.45 and is taken from Table 1 of Guilkey and Schmidt [24]. Fourteen lags are employed in the tests. The lag length was determined in the manner suggested by Schwert [42] by taking the integer value of $12(T/100)^{.25}$ where T = number of observations (192).

b. Fourteen lags are employed in the tests. The critical value at the 5% level is −4.36 and is taken from Table 3 of Engle-Yoo [14].

c. Fourteen lags are employed in the tests. The critical value at the 5% level is −3.46 and is taken from Table 1 of Guilkey and Schmidt [24]. This is the value for 175 observations; our second-stage Hansen test regressions used 177 observations.

comparison of the estimated test statistics with a critical value of −4.36 (the value from Engle and Yoo [14] for a 5 variable system with 100 observations), there appears to be no evidence of cointegration in the system.

The Engle-Yoo procedure is widely used in testing for cointegration. However, Hansen [26] has pointed out that this test, as well as the test proposed by Johansen [27], suffers from a "curse of dimensionality." That is, the power of these tests falls substantially as the size of the system increases. Hansen suggests a two-stage test similar to that of Engle-Yoo whose power is unaffected by the dimension of the system. In the first stage of this test, the cointegrating regression is estimated using an iterative Cochrane-Orcutt procedure rather than with ordinary least squares as is done by the Engle-Yoo procedure.[11] A set of residuals is then generated by subtracting the product of the parameter estimates (from Cochrane-Orcutt) times the values of the right-hand side variables in the cointegrating regression from the values of the dependent variable in the cointegrating regression. In the second stage, either a Dickey-Fuller or an augmented Dickey-Fuller test for a unit root is performed on these residuals. Critical values from Fuller [19] or Guilkey and Schmidt [24] can be used to test the hypothesis of no cointegration. If it is desired, the variables can be detrended and demeaned prior to implementation of the first stage of the procedure.[12]

11. Hansen shows that under the null hypothesis of no cointegration the Cochrane-Orcutt estimates of the parameters of the cointegrating regression converge to constants while the ordinary least squares estimates do not converge to constants but stay random. These random elements depend upon the size of the system, and this is the source of the "curse of dimensionality" in the Engle-Yoo procedure. Since the Cochrane-Orcutt estimates converge to constants, the "curse of dimensionality" is avoided in Hansen's procedure.

12. The variables in the system are regressed on a constant and trend, and the residuals from these equations are used in the Hansen procedure [26, section 7].

Implementation of Hansen's procedure, again using 14 lags in the second stage regression, for detrended and demeaned data for our system yielded no evidence of cointegration. The estimated test statistics are presented in Table I,B in the column labeled Hansen.

Since both the Hansen and Engle and Yoo procedures reveal no evidence of cointegration, the system is estimated with the differenced variables. These variables are defined as follows: *DLIP*, *DLWPI*, *DLM2*, and *DLEXP* are the first differences of the logs of *IP*, *WPI*, *M2*, and *EXP*, respectively, and *DBAA* is the first difference of *BAA*.

Following Lutkepohl [28], Akaike's AIC criterion is used to determine the lag length of the VAR model. The lag length chosen is the one that minimizes

$$AIC(k) = \ln \det \sum_{k} + (2d^2k)/T, \tag{1}$$

$k = i, \ldots, m$ where d = the number of variables in the system, m = maximum lag length considered (set to 12 months), $\det \Sigma_k$ = determinant of Σ_k, and Σ_k = estimated residual variance-covariance matrix for lag k. Use of the AIC criterion suggested a lag of 10 months for the estimation period 1922:7–1938:6. Q statistics indicated the absence of any serial correlation in the residuals of the model.

III. Empirical Results

As indicated in the introduction, the effects of government expenditures are analyzed through the computation of VDCs and HDCs. Both VDCs and HDCs are based on the moving average representation of the VAR model and capture both direct and indirect effects. VDCs show the proportion of forecast error variance for each variable that is attributable to its own innovations and to shocks to other system variables. The VDC for *DLIP*, for example, will indicate the percentage of the forecast error variance of *DLIP* accounted for by *DLEXP* and the other variables in the system. If *DLEXP* is an important determinant of movements in industrial production, one would expect this variable to explain a significant fraction of the forecast error variance of *DLIP*. In fact, Sims [45] has suggested that the strength of Granger-causal relations can be measured by VDCs. If *DLEXP* explains only a small portion of the forecast error variance of *DLIP*, this could be interpreted as a weak Granger-causal relation.

While it is customary to report VDCs without confidence intervals or standard errors, Runkle [40] has argued that this is equivalent to reporting regression coefficients without t-statistics. Therefore, in order to provide some indication of the precision with which the VDCs are estimated, we used a Monte Carlo integration procedure similar to that described in Doan and Litterman [13] to generate standard errors for the VDCs. Five hundred draws were employed in the Monte Carlo procedure. The estimates of the proportion of the forecast error variance explained by each variable are judged to be "significant" if the estimate is at least twice the estimated standard error.

HDCs are used to assess the impact of *DLEXP* on *DLIP* and *DLWPI* over several subperiods of our sample. As noted by Burbidge and Harrison [10], the HDC assigns credit for the difference between what can be called the base projection for a series and the actual series to the shocks to the system's variables. The extent to which a series that adds the shocks to a particular variable(s) to the base projection is closer to the actual series than is the base projection alone is a measure of the importance of that variable or that set of variables.

Like VDCs, HDCs are based upon the moving average representation of the VAR. The moving average representation of the VAR can be written as:

$$\mathbf{x}_t = \sum_{i=0}^{\infty} \mathbf{M}_i \boldsymbol{\mu}_{t-i} \tag{2}$$

where $\mathbf{x}_t$ = column vector of the variables in the system, $\boldsymbol{\mu}_{t-i}$ = column vector of shocks to the elements of $\mathbf{x}$ in period $t - i$, $\mathbf{M}_i$ = matrix of impulse response weights conformable to the dimensions of $\mathbf{x}$ and $\mathbf{u}$. Consider a base period which runs from observation 1 to observation T. The value of $\mathbf{x}$ in periods subsequent to T can be written as:

$$\mathbf{x}_{T+j} = \sum_{i=j}^{\infty} \mathbf{M}_i \boldsymbol{\mu}_{T+j-i} + \sum_{i=0}^{j-1} \mathbf{M}_i \boldsymbol{\mu}_{T+j-i} \tag{3}$$

where $\Sigma_{i=j}^{\infty} \mathbf{M}_i \boldsymbol{\mu}_{T+j-i}$ = base projection or forecast of $\mathbf{x}_{T+j}$ based only upon information available at time T, and $\Sigma_{i=0}^{j-1} \mathbf{M}_i \boldsymbol{\mu}_{T+j-i}$ = the part of $\mathbf{x}$ accounted for by shocks since T. The elements of the second term are used to determine the extent to which addition of the shocks to a particular variable(s) to the base projection generates a series that is closer to the actual series ($\mathbf{x}_{T+j}$) than is the base projection alone (first term).

Since no contemporaneous terms enter the equations of the VAR, any contemporaneous relations among the variables are reflected in the correlation of residuals across equations. In calculating the VDCs and HDCs, the variables are ordered in a particular fashion, and the variance-covariance matrix is orthogonalized by the Choleski decomposition. Because of the cross-equation residual correlation, when a variable higher in the order changes, variables lower in the order are assumed to change. The extent of the change depends upon the covariance of the variables higher in the order with those lower in the order.

In this paper, we focus on the following ordering: *DLEXP*, *DLM2*, *DBAA*, *DLIP*, and *DLWPI*. We thus place the policy variables prior to the interest rate, and the interest rate precedes the goods market variables *DLIP* and *DLWPI*. Since we focus on the effects of government expenditures, the critical element of this ordering for our purposes is the placement of *DLEXP* first. As long as *DLEXP* is placed first in the ordering, rearrangement of the other variables will not affect the estimates of the effects of *DLEXP* on these variables.

In placing *DLEXP* first, we assume that any contemporaneous relations between *DLEXP* and the other variables are due to the effects of *DLEXP* on these variables. The nature of the fiscal policy process suggests that real government expenditures do not respond within the same month to shocks to macrovariables. This is a common assumption in macro models, but we note that this assumption is made only for *contemporaneous* relations among the variables. In the VAR model, *DLEXP* are treated as jointly determined with the other variables; hence, feedback from the other variables to *DLEXP* is allowed with a lag. An examination of the correlation coefficients for the residuals of the *DLEXP* equation with those for the other equations indicates that, with the exception of the residual for the *DBAA* equation, the correlation coefficients are extremely low. These correlation coefficients are: .009 (*DLM2*), .217 (*DBAA*), .03 (*DLIP*), and −.022 (*DLWPI*). A simple ordinary least squares regression of the *DLEXP* equation residuals on the residuals of the other equations indicated that only the coefficient on the *DBAA* residual was significant as gauged by a standard t-test. This coefficient was positive. We interpret this positive and significant coefficient as indicating a contemporaneous effect of government expenditures on the interest rate. We regard it as implausible that this contemporaneous correlation reflects causation from the interest rate to expenditures. Even though expenditures include interest payments on government debt,

Table II. Variance Decompositions: Percentage of Forecast Error Variation in *DLIP* and *DLWPI* Explained by Shocks to Real Government Expenditures[a]

Relative Variation in	Months Later	Basic Model (1)	Model With Alternative Expenditure (2)	Model With Miron-Romer Industrial Production (3)	Model With AAA Rate (4)
DLIP	6	2.8 (3.0)	2.3	4.5	6.9
	12	8.3 (3.6)*	6.0	6.3	8.9
	24	10.1 (4.0)*	8.7	9.2	10.7
	36	10.1 (4.1)*	8.8	9.6	10.9
	48	10.1 (4.2)*	8.8	9.8	10.9
DLWPI	6	1.8 (2.3)	0.8	3.1	4.0
	12	3.6 (2.5)	1.5	4.5	4.0
	24	4.8 (2.7)	2.4	7.0	4.8
	36	4.8 (2.8)	2.4	7.1	4.8
	48	4.8 (2.9)	2.4	7.2	4.8

a. The point estimates of the VDCs are presented in columns 1–4. The numbers in parentheses in the Basic Model column are the estimated standard errors. The * indicates the point estimate is at least twice the standard error. Column 2 presents results for the model with the first difference of the ratio of government expenditures to industrial production. Column 3 substitutes the Miron-Romer industrial production series for the Federal Reserve series. Column 4 contains results for the model with the first difference of the *AAA* interest rate replacing *DBAA*. The optimal lag for the models in columns 1–4 are 10, 10, 12, and 8 months, respectively.

outstanding debt was small in our sample and any effects of interest rates on expenditures are likely to be felt only with a lag.

Nothing is gained in the analysis of the effects of *DLEXP* on the other variables by adopting the "structural" VAR approach of Bernanke [6] if the assumption that government expenditures are not contemporaneously affected by the other model variables is maintained in specifying a "structural" model of the residuals. As in the Choleski decomposition with *DLEXP* ordered first, if this assumption about government expenditures is made, any contemporaneous relations between *DLEXP* and the other variables in the "structural" approach will be due to the effects of *DLEXP* on these variables and not vice versa. The residuals of the *DLEXP* equation will thus be measures of "structural" shocks to expenditures. For this reason we do not estimate a "structural" VAR.[13]

The VDCs are presented in Table II. The estimated standard errors are shown in parentheses next to the point estimates. A * indicates the point estimate is at least twice the standard error. VDCs at horizons of 6, 12, 24, 36, and 48 months are shown in order to convey a sense of the dynamics of the system. Only the effects of government expenditures on the goods market variables *DLIP* and *DLWPI* are presented.

We note that the effects of *DLEXP* on *DLIP* are small but generally significant. Innovations in *DLEXP* explain 8–10% of the forecast error variance in *DLIP* at the 12-through 48-month horizons. The effects on *DLWPI,* however, are weaker and are not significant. It appears, then, that over our entire sample government expenditures had significant but small effects on output and no discernible effects on the price level.

The sensitivity of these results was checked in several ways. The column 1 results are for

13. Of course, the effects of the other variables in the system on each other may well differ in a Choleski decomposition from those based on a "structural" VAR decomposition.

Table III. Historical Decompositions[a]

	(1) 1929:9–1938:6		(2) 1930:6–1938:6		(3) 1929:9–1933:3		(4) 1929:9–1933:7	
Variable	*BP*	*BPEXP*	*BP*	*BPEXP*	*BP*	*BPEXP*	*BP*	*BPEXP*
DLIP	.0408	.0351 (.86)	.0429	.0372 (.87)	.0281	.0232 (.83)	.0446	.0370 (.83)
DLWPI	.0128	.0122 (.95)	.0137	.0133 (.97)	.0099	.0095 (.96)	.0145	.0137 (.94)

	(5) 1930:6–1933:3		(6) 1930:6–1933:7		(7) 1933:4–1938:6		(8) 1933:7–1938:6	
Variable	*BP*	*BPEXP*	*BP*	*BPEXP*	*BP*	*BPEXP*	*BP*	*BPEXP*
DLIP	.0317	.0269 (.85)	.0499	.0421 (.84)	.0298	.0278 (.93)	.0297	.0281 (.95)
DLWPI	.0121	.0117 (.97)	.0168	.0160 (.95)	.0100	.0099 (.99)	.0093	.0092 (.99)

a. The results in the columns labeled *BP* are the root-mean squared errors (RMSEs) for the base projection for the period. The results in the columns labeled *BPEXP* are the RMSEs for the *BP* plus the contribution of the shocks to real government expenditures. The numbers in parentheses are the ratios of the RMSEs for *BPEXP* to the RMSEs for *BP*.

the growth rate of real expenditures. An alternative measure is the share of expenditures in output. The results in column 2 are based upon a model that substitutes the first difference of the ratio of government expenditures to industrial production for *DLEXP*. The effects of a shock to this alternative expenditure measure are somewhat smaller than for *DLEXP* but are within one standard deviation of those for *DLEXP*. The sensitivity of the results to the measurement of output was also checked. Miron and Romer [33] have recently constructed an industrial production index for 1884–1940. This series is based upon 13 physical output series for which consistent data exist back to 1884. Their monthly series is more volatile than the Federal Reserve measure, which they note may be due to the fact that their measure is less inclusive than that of the Federal Reserve. The results for substituting the Miron-Romer industrial production measure for the Federal Reserve measure are presented in column 3. The effects of shocks to *DLEXP* on *DLIP* are generally somewhat weaker than those in column 1 while the effects on *DLWPI* are stronger. In both cases, the effects in column 3 are within one standard deviation of those in column 1. Finally, we checked the sensitivity of the results to the interest rate we employ. The results for the system with the first difference of the *AAA* rate are presented in column 4. With the exception of the results for the 6 month horizon, the results are nearly identical to those in column 1.

Since our results are not sensitive to the alternatives considered in Table II, we use the model that generated the column 1 results in computing HDCs.

The results of the HDCs for several subperiods of our sample are summarized in Table III. The root-mean-squared errors (RMSEs) for the base projection (*BP*) for each subperiod are presented, as are the RMSEs for the *BP* plus the contribution of the shock to real government expenditures (*BPEXP*). The ratios of the RMSE for *BPEXP* to the RMSE for *BP* are in parentheses.

The two longer subperiods—1929:9–1938:6 and 1930:6–1938:6 (columns 1–2)—provide an indication of the role of real federal government expenditures in explaining output and prices during the Great Depression, the subsequent recovery in economic activity, and the downturn in

1937–38. The first subperiod begins in the month following the cyclical peak of August 1929 and continues through the end of our sample. As an alternative, a second subperiod begins slightly later in 1930:6, when real government expenditures began a sustained increase from the relatively constant levels of 1922–1929. The results for the two longer subperiods are quite similar. We observe that the addition of shocks to real government expenditures to the *BP* reduces the RMSE for the *BP* by 13–14% for *DLIP* and by only 3–5% for *DLWPI*.

In order to see if the role of government expenditures was different in the Hoover administration and the early New Deal, we also report HDCs for six shorter subperiods. Alternatively, we date the New Deal as beginning in 1933:4, the month after President Roosevelt's inauguration and the trough of the Great Depression, and in 1933:7, the later date allowing for a brief transition before the "effective" beginning of New Deal expenditure programs. We thus represent the "Hoover era" by four alternative subperiods—1929:9–1933:3, 1929:9–1933:7, 1930:6–1933:3, and 1930:6–1933:7 (columns 3–6). In all cases, the addition of shocks to real government expenditures to the *BP* reduces the RMSEs for the *BP* for *DLIP* by 15%–17%. For the New Deal periods—1933:4–1938:6 and 1933:7–1938:6 (columns 7–8)—shocks to expenditures reduce the *BP* RMSEs for *DLIP* by only 5–7%. Perhaps surprisingly, government expenditures played a trivial role in explaining output over the New Deal periods, and the modest impact of expenditures on production over the longer subperiods reflects their impact in the "Hoover" years. Regardless of the subperiod considered, government expenditures played a trivial role in explaining prices.

IV. Summary and Conclusions

Monetary forces alone are not sufficient to explain important macro developments in the 1930s and various nonmonetary explanations have been discussed in the literature. In this paper we look at the possible role of real federal government expenditures in explaining output and prices. The effects of expenditures over the entire interwar period are analyzed through the computation of VDCs. We find small but generally significant effects on production and insignificant effects on prices. Historical decompositions are computed so as to focus more closely on the role of government expenditures in the 1930s. Again, we find only a modest impact on production and a trivial impact on prices. These results are not inconsistent with Barro's model [2; 3] where the effects of government purchases on output are likely to be small in peace-time periods. Our results are also not at odds with earlier studies by Brown [8] and Peppers [35] which found an inconsistent role for fiscal policy in the 1930s.

One of the most interesting results of this study is the finding that government expenditures played an even smaller role in explaining output in the Roosevelt administration (at least until the end of our sample period) than in the Hoover years. It has been said that before 1938 Roosevelt did not regard spending as a means to bring about recovery but rather as a way to provide relief. Reading [37], Wright [53], and Wallis [50; 51] have considered the extent to which political goals, rather than humanitarian or social goals, motivated New Deal spending. But, of course, the effects of expenditures on output and other macro variables should depend much more on the scale of spending than on how the spending was regarded or the true purpose of the spending. The behavior of real government expenditures and the ratio of real government expenditures to industrial production in the New Deal period (see Figure 1) do not suggest a serious attempt to use government spending to combat the depressed conditions of the time.

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