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MACROECONOMIC EFFECTS FROM GOVERNMENT PURCHASES AND TAXES

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ABSTRACT

For U.S. annual data that include WWII, the estimated multiplier for defense spending is 0.6-0.7 at the median unemployment rate. There is some evidence that this multiplier rises with the extent of economic slack and reaches 1.0 when the unemployment rate is around 12%. Multipliers for non-defense purchases cannot be reliably estimated because of the lack of good instruments. For samples that begin in 1950, increases in average marginal income-tax rates (measured by a newly constructed time series) have a significantly negative effect on real GDP. Increases in taxes seem to reduce real GDP with mainly a one-year lag due to income effects and mostly a two-year lag due to substitution (tax-rate) effects. Since the defense-spending multiplier is typically less than one, greater spending tends to crowd out other components of GDP. The largest effects are on private investment, but non-defense purchases and net exports tend also to fall. The response of private consumer expenditure differs insignificantly from zero.

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The global recession and financial crisis of 2008-09 have focused attention on fiscal-stimulus packages. These packages often emphasize heightened government purchases, predicated on the view (or hope) that expenditure multipliers are greater than one. The packages typically also include tax reductions, designed partly to boost disposable income and consumption (through income effects) and partly to stimulate work effort, production, and investment by lowering marginal income-tax rates (through substitution effects).

The empirical evidence on the response of real GDP and other economic aggregates to added government purchases and tax changes is thin. Particularly troubling in the existing literature is the basis for identification in isolating effects of changes in government purchases or tax revenues on real economic activity.

The present study uses long-term U.S. macroeconomic data to contribute to existing evidence along several dimensions. Spending multipliers are identified primarily from variations in defense spending, especially the changes associated with the buildups and aftermaths of wars. Tax effects are estimated mainly from changes in a newly measured time series on average marginal income-tax rates from federal and state income taxes and the social-security payroll tax. Some of the results differentiate substitution effects due to tax-rate changes from income effects due to changes in tax revenues.

Section I discusses the U.S. data on government purchases since 1914, with stress on the differing behavior of defense spending and non-defense purchases (by all levels of government). The variations up and down in defense outlays are particularly dramatic for World War II, World War I, and the Korean War. Section II describes a newly updated time series from 1916 to 2006 on average marginal income-tax rates from federal and state individual income taxes and the social-security payroll tax. Section III discusses the Romer and Romer (2008) data on changes in federal tax revenues since 1945. Section IV describes our empirical framework for assessing effects on real GDP from changes in government purchases, taxes, and other variables. Section V presents our empirical findings. The analysis deals with annual data ending in 2006 and starting in 1950, 1939, 1930, or 1917. Section VI summarizes the main findings and provides suggestions for additional research.

I. The US. History of Government Purchases: Defense and Non-defense

The graphs in Figure 1 show annual changes in per capita real defense or non-defense purchases (nominal outlays divided by the GDP deflator), expressed as ratios to the previous year's per capita real GDP.[1] The underlying data on government purchases are from the Bureau of Economic Analysis (BEA) since 1929 and, before that, from Kendrick (1961).[2] The data on defense spending apply to the federal government, whereas those for non-defense purchases pertain to all levels of government. Note that we include only government spending on goods and services, not transfers or interest payments. To get a long time series, we are forced to use annual data, because quarterly values are available only since 1947. The restriction to annual data has the virtue of avoiding problems related to seasonal adjustment.

The blue graph in Figure 1 makes clear the dominance of war-related variations in the defense-spending variable. For World War II, the value is 10.6% of GDP in 1941, 25.8% in

[1]Standard numbers for real government purchases use a government-purchases deflator that assumes zero productivity change for goods and services bought by the government. We proceed instead by dividing nominal government purchases by the GDP deflator, effectively assuming that productivity advance is the same for publicly purchased goods and services as it is in the overall economy.
[2]The data since 1929 are the BEA's "government consumption and gross investment." This series includes an estimate of depreciation of public capital stocks (a measure of the rental income on publicly owned capital, assuming a real rate of return of zero on this capital).

1942, 17.2% in 1943, and 3.6% in 1944, followed by two negative values of large magnitude, -7.1% in 1945 and -25.8% in 1946. Thus, World War II provides an excellent opportunity to gauge the size of the government-purchases multiplier; that is, the effect of a change in government purchases on real GDP. The favorable factors are:

- The bulk of the changes in defense spending associated with World War II are plausibly exogenous with respect to the determination of real GDP.
- These changes in defense spending are very large and include sharply positive and sharply negative values.
- Unlike the many countries that experienced sharp decreases in real GDP during World War II (see Barro and Ursua [2008, Table 7]), the United States did not have massive destruction of physical capital and suffered from only moderate loss of life. Hence, demand effects from defense spending should be dominant in the U.S. data.
- Because the unemployment rate in 1940 was still high, 9.4%, but then fell to a low of 1.0% in 1944, it is possible to get some information on how the size of the government-purchases multiplier depends on the amount of slack in the economy.

The U.S. time series contains two other war-related cases of major, short-term changes in defense spending. In World War I, the defense-spending variable (red graph in Figure 1) equaled 3.5% in 1917 and 14.9% in 1918, followed by -7.9% in 1919 and -8.2% in 1920. In the Korean War, the values were 5.6% in 1951, 3.3% in 1952, and 0.5% in 1953, followed by -2.1% in 1954. As in World War II, the United States did not experience much destruction of physical capital and incurred only moderate loss of life during these wars. Moreover, the changes in defense outlays should again be mainly exogenous with respect to real GDP.

In comparison to these three large wars, the post-1954 period features much more modest variations in defense spending. The largest values—1.2% in 1966 and 1.1% in 1967—occur during the early part of the Vietnam War. These values are much smaller than those for the Korean War; moreover, after 1967, the values during the Vietnam War become negligible (0.2% in 1968 and negative for 1969-71). After the end of the Vietnam conflict, the largest values of the defense-spending variable are 0.4-0.5% from 1982 to 1985 during the "Reagan defense buildup" and 0.3-0.4% in 2002-2004 during the post-2001 conflicts under George W. Bush.

The times of increased defense spending since 1950 correspond well to the military dates isolated by Ramey (2008), who extended the analysis of Ramey and Shapiro (1998). (See Rotemberg and Woodford [1992, section V] for a related discussion.) The Ramey analysis uses a narrative approach, based on articles in *Business Week*, to isolate major political events since the end of World War II that forecasted substantial increases in defense spending. The dates selected were June 1950 (start of the Korean War), February 1965 (escalation of the conflict in Vietnam in response to an attack on the U.S. Army barracks), December 1979 (Soviet invasion of Afghanistan), and September 2001 (terrorist attacks on New York City and Washington DC).[3] In terms of quantitative response of defense spending, however, the dominant event is the Korean War. It seems unlikely that there is enough information in the variations in defense outlays after 1954 to get an accurate reading on the defense-spending multiplier.

The red graph in Figure 1 shows the movements in non-defense government purchases. Note particularly the values of 2.4% in 1934 and 2.5% in 1936, associated with the New Deal. Otherwise, the only clear pattern is a tendency for non-defense purchases to decline during major wars and rise in the aftermaths of these wars. For example, the non-defense purchases variable

[3]In ongoing research, Ramey (2008, Table 4) provides a longer list of such events between 1947 and 2006 and attempts to quantify the implications of each event for the present value of defense spending.

takes on values between -1.0% and -1.2% between 1940 and 1943 and then between 0.8% and 1.6% from 1946 to 1949. This broad pattern applies also to World War I, but the precise timing is puzzling. The most negative value for the non-defense purchases variable is -3.5% in 1919 (after the Armistice of 1918 but in the same year as the Versailles Treaty) and then becomes 2.5% in 1920 and 2.0% in 1921.

It is hard to be optimistic about using the macroeconomic time series to isolate multipliers for non-defense government purchases. The first problem is that the variations in the non-defense variable are small compared to those in defense outlays. More importantly, many of the changes in non-defense purchases are likely to be endogenous with respect to the determination of real GDP. That is, as with private consumption and investment, expansions of the overall economy likely induce governments, especially at the state and local level, to spend more on goods and services. As Ramey (2008, p. 4) observes, the outlays by state & local governments became the dominant part of non-defense government purchases since the 1960s. These expenditures (which relate particularly to education, public order, and transportation) are likely to respond to fluctuations in state and local revenues caused by changes in aggregate economic conditions. Whereas war and peace is a plausible exogenous driver of defense spending, we lack similarly convincing instruments for isolating exogenous changes in non-defense purchases.

A common approach in the existing empirical literature, exemplified by Blanchard and Perotti (2002), is to include government purchases (typically, defense and non-defense combined) in a vector-auto-regression (VAR) system and then make identifying assumptions concerning exogeneity and timing. Typically, the real-government-purchases variable is allowed to move first, so that the contemporaneous associations with real GDP and other macroeconomic aggregates, including real personal consumer expenditure, are assumed to reflect causal influences from government purchases to the macro variables. This approach may be satisfactory for war-driven defense spending, but it seems problematic for other forms of government purchases. More generally, it is perilous to treat an arbitrarily selected component of GDP as exogenous and then demonstrate a positive, "causal" effect of this component on overall GDP.

II. Average Marginal Income-Tax Rates

Marginal income tax rates have substitution effects that influence decisions on work effort, investment, the timing of consumption, and so on. Therefore, we would expect changes in these marginal tax rates to influence the growth rate of real GDP and other macroeconomic aggregates. To gauge these effects at the macroeconomic level, we need measures of average marginal income-tax rates—or, possibly, other gauges of the distribution of marginal tax rates across economic agents.

Barro and Sahasakul (1983, 1986) used the Internal Revenue Service (IRS) publication *Statistics of Income, Individual Income Taxes* from various years to construct average marginal income-tax rates from the U.S. federal individual income tax from 1916 to 1983.[4] The Barro-Sahasakul series that we use in the present study weights each individual marginal income-tax rate by adjusted gross income (AGI). The series takes account of non-filers, which were particularly important before World War II. The 1986 study added the marginal income-tax rate from the social-security (FICA) tax on wages and self-employment income (starting in 1937 for

[4]The current federal individual income-tax system was enacted in 1913, following the ratification of the 16th Amendment, but the available data start in 1916.

the main social-security program and 1966 for Medicare). The analysis considered payments by employers, employees, and the self-employed and took account of the zero marginal tax rate for social security, but not Medicare, above each year's income ceiling. However, the earlier analysis and our present study do not allow for any offsetting individual benefits at the margin from making social-security "contributions."

We use the National Bureau of Economic Research (NBER) TAXSIM program, administered by Dan Feenberg, to update the Barro-Sahasakul data. The TAXSIM program allows for the increasing complexity of the federal individual income tax due to features such as the Alternative Minimum Tax, the earned-income tax credit, phase-outs of exemptions and deductions, and so on. TAXSIM allows for the calculation of average marginal income-tax rates weighted in various ways—we focus on the average weighted by a concept of income that encompasses wages, self-employment income, partnership income, and S-corporation income. Although this income concept differs from the adjusted-gross-income measure used before (particularly by excluding capital income),[5] we find in the period of overlap from 1966 to 1983 that the Barro-Sahasakul and NBER TAXSIM series are highly correlated in terms of levels and changes. For the average marginal tax rates from the federal individual income tax, the correlations from 1966 to 1983 are 0.99 in levels and 0.87 in first differences. For the social-security tax, the correlations are 0.98 in levels and 0.77 in first differences. In addition, at the start of the overlap period in 1966, the levels of Barro-Sahasakul—0.217 for the federal income tax and 0.028 for social security—are not too different from those for TAXSIM—0.212 for the

[5]The Barro-Sahasakul federal marginal tax rate also does not consider the deductibility of part of state income taxes. However, since the average marginal tax rate from state income taxes up to 1965 does not exceed 0.016, this effect would be minor. In addition, the Barro-Sahasakul series treats the exclusion of employer social-security payments from taxable income as a subtraction from the social-security rate, rather than from the marginal rate on the federal income tax. However, this difference would not affect the sum of the marginal tax rates from the federal income tax and social security.

federal income tax and 0.022 for social security. Therefore, we are comfortable in using a merged series to cover the period from 1916 to 2006. The merged data use the Barro-Sahasakul numbers up to 1965 and the new values from 1966 on.

The new construct adds marginal income-tax rates from state income taxes.[6] From 1979 to 2006, the samples of income-tax returns provided by the IRS to the NBER include state identifiers for returns with AGI under $200,000. Therefore, with some approximations for allocating high-income tax returns by state, we were able to use TAXSIM to compute average marginal tax rates from state income taxes since 1979. From 1929 to 1978, we used IncTaxCalc, created by Jon Bakija, to estimate marginal tax rates from state income taxes. To make these calculations, we combined the information on each state's tax code (incorporated into IncTaxCalc) with estimated numbers on the distribution of income levels by state for each year. The latter estimates used data on per capita state personal income from Bureau of Economic Analysis.[7] The calculations from TAXSIM and IncTaxCalc take into account that, for people who itemize deductions, an increase in state income taxes reduces federal income-tax liabilities.

Table 1 and Figure 2 show our time series from 1912 to 2006 for the overall average marginal-income tax rate and its three components: the federal individual income tax, social-security payroll tax (FICA), and state income taxes.[8] In 2006, the overall average marginal rate was 35.3%, breaking down into 21.7% for the federal individual income tax, 9.3% for the social-

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[6]The first state income tax was implemented by Wisconsin in 1911, followed by Mississippi in 1912. A number of other states (Oklahoma, Massachusetts, Delaware, Missouri, New York, and North Dakota) implemented an income tax soon after the federal individual income tax became effective in 1913.
[7]Before 1929, we do not have the BEA data on income by state. For this period, we estimated the average marginal tax rate from state income taxes by a linear interpolation from 0 in 1910 (prior to the implementation of an income tax by Wisconsin in 1911) to 0.0009 in 1929. Since the average marginal tax rates from state income taxes are extremely low before 1929, this approximation would not have much effect on our results.
[8]We went back to 1912 by interpolating the federal part of the average marginal income-tax rate from 0 in 1912 to 1.2% in 1916. Since our analysis starts in 1917, this interpolation has at most a minor effect on the regression results.

security levy (inclusive of employee and employer parts), and 4.3% for state income taxes.[9] For year-to-year changes, the movements in the federal individual income tax usually dominate the variations in the overall marginal rate. However, rising social-security tax rates were important from 1971 to 1991. Note that, unlike for government purchases, the marginal income-tax rate for each household really is an annual variable; that is, the same rate applies at the margin to income accruing at any point within a calendar year. Thus, for marginal tax-rate variables, it would not be meaningful to include variations at a quarterly frequency.[10]

Many increases in the federal average marginal tax rate on individual income involve wartime, including World War II (a rise in the rate from 3.8% in 1939 to 25.7% in 1945, reflecting particularly the extension of the federal income tax to most households), World War I (an increase from 0.6% in 1914 to 5.4% in 1918), the Korean War (going from 17.5% in 1949 to 25.1% in 1952), and the Vietnam War (where surcharges contributed to the rise in the rate from 21.5% in 1967 to 25.0% in 1969). The federal average marginal rate tended to fall during war aftermaths, including the declines from 25.7% in 1945 to 17.5% in 1949, 5.4% in 1918 to 2.8% in 1926, and 25.1% in 1952 to 22.2% in 1954. No such reductions were apparent after the Vietnam War.

An extended period of rising federal tax rates prevailed from 1971 to 1978, with the average marginal rate from the individual income tax increasing from 22.7% to 28.4%. This increase reflected the shifting of households into higher rate brackets due to high inflation in the

[9]Conceptually, our "marginal rates" correspond to the effect of an additional dollar of income on the amounts paid of the three types of taxes—federal individual income tax, social-security payroll tax, and state income taxes. The calculations consider interactions across the levies; for example, part of state income taxes is deductible on federal tax returns, and the employer part of social-security payments does not appear in the taxable income of employees.

[10]However, even if the tax-rate structure is set by the beginning of year t, information about a household's marginal income-tax rate for year t arrives gradually during the year—as the household learns about its income, deductions, etc.. This type of variation in perceived marginal income-tax rates within a year is clearly endogenous with respect to the determination of income (and real GDP) during the year.

context of an un-indexed tax system. Comparatively small tax-rate hikes include the Clinton increase from 21.7% in 1992 to 23.0% in 1994 (and 24.7% in 2000) and the rise under George H.W. Bush from 21.7% in 1990 to 21.9% in 1991. Given all the hype about Bush's "read my lips, no new taxes," it is surprising that the average marginal income-tax rate rose by only two-tenths of a percentage point in 1991.

Major cuts in the average marginal income-tax rate occurred under Reagan (25.9% in 1986 to 21.8% in 1988 and 29.4% in 1981 to 25.6% in 1983), George W. Bush (24.7% in 2000 to 21.1% in 2003), Kennedy-Johnson (24.7% in 1963 to 21.2% in 1965), and Nixon (25.0% in 1969 to 22.7% in 1971, reflecting the introduction of the maximum marginal rate of 50% on earned income).

Also noteworthy is the odd behavior of the average marginal federal income-tax rate during the Great Depression. The rate fell from 4.1% in 1928 to 1.7% in 1931, mainly because falling incomes within a given tax structure pushed people into lower rate brackets. Then, particularly because of misguided attempts to balance the federal budget by raising taxes under Hoover and Roosevelt, the rate rose to 5.2% in 1936.

Although social-security tax rates have less high-frequency variation, they increase sharply in some periods. The average marginal rate from social security did not change greatly from its original value of 0.9% in 1937 until the mid 1950s but then rose to 2.2% in 1966. The most noteworthy period of rising average marginal rates is from 1971—when it was still 2.2%—until 1991, when it reached 10.8%. Subsequently, the average marginal rate has remained reasonably stable, though it fell from 10.2% in 2004 to 9.3% in 2006 (due to expansion of incomes above the social-security ceiling).

The marginal rate from state income taxes rose from less than 1% up to 1956 to 4.1% in 1977 and has since been reasonably stable. We have concerns about the accuracy of this series before 1979 because of missing information about the distribution of incomes by state. However, the small contribution of state income taxes to the overall average marginal income-tax rate suggests that this measurement error may not matter a lot for our regression results.

Our key identifying assumption is that changes in average marginal income-tax rates lagged one or more years can be satisfactorily treated as pre-determined with respect to per capita real GDP. Note that the regressions used to determine per capita real GDP in year t include a measure of aggregate economic conditions in year t-1 (the lagged unemployment rate, though we also considered the lagged growth rate of per capita real GDP). Thus, a contemporaneous association between economic conditions and changes in marginal income-tax rates need not cause problems in isolating effects from lagged tax-rate changes on real GDP growth.

One way to evaluate our identifying assumption is from the perspective of the tax-smoothing approach to fiscal deficits (Barro [1979, 1990]; Aiyagari, Marcet, Sargent, and Seppala [2002]). A key implication of this approach is the Martingale property for tax rates: future changes in tax rates should not be predictable at date t. Redlick (2009) tests this hypothesis for the data on the overall average marginal income-tax rate shown in Table 1. He finds that the Martingale property is a good first-order approximation but that some economic variables have small, but statistically significant, predictive content for future changes in tax rates.

If tax smoothing holds as an approximation, then the change in the tax rate for year t, τ_t-τ_{t-1}, would reflect mainly new information about the future path of the ratio of real government expenditure, G_{t+T} (inclusive here of transfer payments), to real GDP, Y_{t+T}. The arrival of new information that future government outlays would be higher in relation to GDP would cause an increase in the current tax rate. For our purposes, the key issue concerns the effects of changes in expectations about future growth rates of real GDP. Under tax-smoothing, these changes would not impact the current tax rate if the shifts in expected growth rates of real GDP go along with corresponding changes in expected growth rates of real government spending. Thus, our identifying assumption is that any time-varying expectations about future real GDP growth rates do not translate substantially into changes in the anticipated future path of G/Y, and, therefore, do not enter significantly into the determination of tax rates.

III. The Romer-Romer Tax-Change Series

Romer and Romer (2008) use a narrative approach based on Congressional reports and other sources to assess all significant federal tax legislation from 1945 to 2007. They gauge each tax change by the size and timing of the intended effects on federal tax revenues. Thus, in contrast to the marginal income-tax rates discussed before, the Romer-Romer focus is on income effects related to the federal government's tax collections. In practice, however, their tax-change variable has a high positive correlation with shifts in marginal income-tax rates; that is, a rise in their measure of intended federal receipts (expressed as a ratio to past GDP) usually goes along with an increase in marginal income-tax rates, and vice versa.[11] For example, an annual version

[11]A major counter-example is the Reagan tax cut of 1986, which reduced the average marginal tax rate from the federal individual income tax by 4.2 percentage points up to 1988. Because this program was designed to be

of their tax-change variable (computed from Romer and Romer [2008, Table 1, columns 1-4]), when expressed as a ratio to the prior year's nominal GDP, has a correlation of 0.78 from 1946 to 2006 with the change in our average marginal income-tax rate. Consequently, the Romer-Romer variable or our marginal tax-rate variable used alone would pick up a combination of income and substitution effects. However, when we include the two tax measures together, we can reasonably view the Romer-Romer variable as isolating income effects,[12] with the marginal tax-rate variable isolating mainly substitution effects.[13]

Because the Romer-Romer variable is based on planned changes in federal tax revenues, assessed during the prior legislative process, the variable avoids the obvious endogeneity of realized tax revenues with respect to realized real GDP. Thus, the major concern about endogeneity of the tax changes involves politics; that is, Congressional legislation on taxes may involve feedback from past or prospective economic developments. To deal with this concern, Romer and Romer divide each tax bill (or sometimes parts of bills) into four bins, depending on what the narrative evidence reveals about the underlying motivation for the tax change. The four categories are (Romer and Romer [2008, abstract]): "… responding to a current or planned change in government spending, offsetting other influences on economic activity, reducing an inherited budget deficit, and attempting to increase long-run growth." They classify the first two bins as endogenous and the second two as exogenous. Frankly, we view these classifications as

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revenue neutral (by closing "loopholes" along with lowering rates), the Romer-Romer variable shows only minor tax changes in 1987 and 1988.

[12]Ricardian equivalence does not necessarily imply than these income effects would be nil. For example, a high value of the Romer-Romer tax variable might signal an increase in the ratio of expected future government spending to GDP, thereby likely implying a negative income effect.

[13]However, for a given ratio of federal revenue to GDP, an increase in the average marginal tax-rate variable might indicate that the government had shifted toward a less efficient tax-collection system, thereby implying a negative income effect.

unpersuasive as a way to deal with endogeneity.[14] Fortunately, however, we find that results for the Romer-Romer tax variable are similar whether we include the tax changes from all four of their bins or only those changes that belong to the two bins labeled as exogenous. Thus, the main findings do not depend on their choices of which tax changes to assign to which bins or on which bins to regard as endogenous.

Romer and Romer (2008, Table 1) provide data at a quarterly frequency, but our analysis uses these data only at an annual frequency, thus conforming to our treatment for government purchases and average marginal income-tax rates. Our key identifying assumption is that changes in the Romer-Romer tax series lagged one or more years can be satisfactorily treated as pre-determined with respect to per capita real GDP. This assumption parallels the one made for average marginal income-tax rates (and, again, our analysis includes separate lagged measures of the state of aggregate economic conditions). We also consider using the Romer-Romer variables as instruments for actual changes in federal revenue.

IV. Framework for the Empirical Analysis

We estimate equations for the per capita real GDP growth rate of the form:

(1) $(y_t - y_{t-1})/y_{t-1} = \beta_0 + \beta_1 \cdot (g_t - g_{t-1})/y_{t-1} + \beta_2 \cdot (\tau_{t-1} - \tau_{t-2})$ + other variables,

[14]The first bin does not actually involve endogeneity of tax changes with respect to real GDP but instead reflects concern about a correlated, omitted variable—government spending—that may affect real GDP. Empirically, the main cases of this type in the Romer-Romer sample associate with variations in defense purchases during and after wars, particularly the Korean War. The straightforward remedy for this omitted-variable problem is to include a measure of defense spending when attempting to explain variations in real GDP. Romer and Romer (2009) include broad measures of government spending in parts of their analysis.

where y_t is per capita real GDP for year t, g_t is per capita real government purchases for year t, and τ_t is a tax rate for year t. The form of equation (1) implies that the coefficient β_1 is the government-spending multiplier.[15] We are particularly interested in whether β_1 is greater than zero, greater than one, and larger when the economy has more slack. We gauge the last effect by adding to equation (1) an interaction between the variable $(g_t - g_{t-1})/y_{t-1}$ and the first annual lag of the unemployment rate, U_{t-1}, which is an indicator of the amount of slack in the economy.

When g_t corresponds to defense spending, we instrument with the same variable interacted with a dummy for war years (1914-20, 1939-46, 1950-54, 1966-71; see the notes to Table 2). Since the main movements in defense spending are war related (Figure 1), we end up with similar results—especially in samples that cover WWII—if we include the defense-spending variable itself in the instrument list. We also consider representing g_t by non-defense purchases, but this setting leads to problems because of the lack of convincing instruments.

If τ_t is an average marginal income-tax rate, as with our data in Table 1, we expect $\beta_2 < 0$ because of adverse effects on the incentive to work and produce. As noted before, we assume that the first annual lag of the change in the tax rate, τ_{t-1}-τ_{t-2}, can be treated as pre-determined in the estimation of equation (1). Thus, this effect picks up the one-year-lagged effect of changes in marginal tax rates on overall economic activity. We also assess whether additional lags of tax-rate changes matter for GDP growth. If τ_t represents projected tax revenues collected as a share of GDP, determined from the Romer and Romer (2008, Table 1) data, we can get $\beta_2<0$ because of aggregate-demand effects. Again, we treat the first annual lag and longer lags as pre-determined.

[15]Note that the variable y_t is the per capita value of nominal GDP divided by the implicit GDP deflator, P_t (determined by the BEA from chain-weighting for 1929-2006). The variable g_t is calculated analogously as the per capita value of nominal defense spending divided by the same P_t. Therefore, the units of y and g are comparable, and β_1 reveals the effect of an extra unit of defense spending on GDP.

As mentioned before, the other variables included in equation (1) include indicators of the lagged state of the business cycle. This inclusion is important because, otherwise, the fiscal variables might just be picking up influences related to the dynamics of the business cycle. In the main analysis, we include the first annual lag of the unemployment rate, U_{t-1}. Given the tendency for the U.S. economy to recover from recessions, we expect that the coefficient on U_{t-1} would be positive. We also consider the inclusion of the first annual lag of the dependent variable (the previous year's growth rate of per capita real GDP); however, this variable turns out to be statistically insignificant in equation (1) once U_{t-1} is included.

Not surprisingly, additional variables would likely have systematic influences on GDP growth in equation (1). However, as Romer and Romer (2009) have argued, omitted variables that are orthogonal to the fiscal variables (when lagged business-cycle indicators are included) would not bias the estimated effects of the fiscal variables. The main effect that seemed important to add—particularly for samples that include the Great Depression of 1929-33—is an indicator of monetary/credit conditions. A variable that works well is the quality spread in interest rates; specifically, the gap between the interest rate (yield to maturity) on long-term Baa-rated corporate bonds and the interest rate on long-term U.S. government bonds. We think of this yield spread as representing distortions in the workings of the credit markets. The square of the spread turns out to work in a reasonably stable way over the long term in contributing to the explanation of GDP growth in equation (1).[16] Since the spread is clearly endogenous with respect to GDP growth, we instrument the current value with the first annual lag of the spread variable. That is, given the lagged business-cycle indicators already included, we treat the lagged yield spread as pre-determined with respect to real GDP. Although the inclusion of this

[16] If we think of the yield spread as analogous to a distorting tax rate, then the square of the spread would approximate the deadweight loss from the distortion.

credit variable likely improves the precision of our estimates of fiscal effects, we get broadly similar results if the credit variable is omitted.

V. Empirical Results

Table 2 shows two-stage least-squares regressions with annual data of the form of equation (1). The samples all end in 2006. (Data on marginal income-tax rates for 2007 will soon be available from TAXSIM.) The starting year is 1950 (including the Korean War), 1939 (including WWII), 1930 (including the Great Depression), or 1917 (including WWI and the 1921 contraction). The instrument lists exclude the current value of the square of the interest-rate spread and the variables that involve Δg-defense but include the lagged square of the interest-rate spread and the interaction between Δg-defense and the dummy variable for years associated with major wars (see the notes to Table 2).

A. Defense-Spending Multipliers

For all samples, the estimated defense-spending multiplier in Table 2 is significantly greater than zero, with a p-value less than 0.01.[17] For the 1950 sample, the estimated coefficient, 0.77 (s.e.=0.28), is insignificantly different from one (p-value=0.41). For any of the samples that start in 1939 or earlier, and thereby include WWII, the estimated multiplier is much more precisely determined. These estimates are all significantly greater than zero and significantly

[17]See Barro (1984, pp. 312-315) for an earlier analysis of the effects of wartime spending on output.

less than one. In the last three columns of the table, the estimated coefficient is between 0.64 and 0.67, with standard errors of 0.10 or less.

As discussed before, each regression includes the lagged unemployment rate, U(-1), to pick up business-cycle dynamics in a simple way. The estimated coefficients on U(-1) in Table 2 are significantly positive for each sample, indicating a tendency for the economy to recover by growing faster when the lagged unemployment rate is higher. If the first lag of the dependent variable, per capita GDP growth, is added to the equations, the estimated coefficient on this new variable is never statistically significantly different from zero, whereas the estimated coefficient of U(-1) remains significantly positive.

The interaction term, (Δg-defense)*U(-1) in Table 2, indicates how the defense-spending multiplier depends on the amount of slack in the economy, gauged by the lagged unemployment rate. Note that U(-1) is measured in this interaction term as a deviation from the median unemployment rate of 0.0557 (where the median is calculated from 1914 to 2006). Therefore, the coefficient on the variable Δg-defense reveals the multiplier when the lagged unemployment rate is at the sample median, and the interaction term indicates how the defense-spending multiplier varies as U(-1) deviates from its median. However, for the 1950 sample, there is insufficient variation in Δg-defense to estimate the interaction term with any precision.[18] For samples that start in 1939 or earlier, and thereby include WWII, it is possible to get meaningful estimates of the coefficient on the interaction term. For example, for the 1939 sample in column 3 of the table, the estimated coefficient of the interaction variable is 5.1 (s.e.=2.2), which is significantly different from zero with a p-value less than 0.05. Since the estimated coefficient

[18]If the interaction term is added, along with the variable Δ[τ*(g-def/y)](-1), to the 1950 equation in Table 2, the estimated coefficient on the interaction term is -23.6 (s.e.=25.3), and the estimated coefficient on Δg-defense is 0.58 (0.37).

on the Δg-defense variable in this sample is 0.67 (0.07), the point estimates imply a defense-spending multiplier of 0.67 at the median unemployment rate of 5.6%. But the estimated multiplier rises by around 0.10 for each 2 percentage points by which the unemployment rate exceeds 5.6%. Hence, the estimated multiplier reaches 1.0 when the unemployment rate gets to about 12%. Conversely, the estimated multiplier is less than 0.67 when the unemployment rate is below its median of 5.6%.

The two-stage least-squares estimates of the defense-spending multipliers in Table 2 rely only on the variations in the variable Δg-defense associated with major wars (including a year of war aftermath in each case[19]). Hence, the estimation excludes more moderate—possibly also exogenous—variations in defense spending, such as those associated with the defense buildups under Reagan and George H.W. Bush. If we can think of all of the variations in defense spending as exogenous, we can improve on the estimates by allowing for all of the sample variations in the variable Δg-defense. However, because the wartime observations already capture the principal fluctuations in defense spending, the results on defense-spending multipliers turn out to change little if we modify the instrument lists to include the variable Δg-defense. For the 1950 sample, the estimated coefficient on Δg-defense becomes 0.65 (s.e.=0.26), somewhat below the value shown in Table 2 and closer to the estimates for the longer samples. For the samples that start in 1939 or earlier, the change in the instrument list has a negligible impact. For example, for the 1939 sample in column 3, the estimated coefficient on Δg-defense becomes 0.65 (s.e.=0.07) and that on the interaction with U(-1) becomes 4.8 (2.2).

[19]For this purpose, we treated WWI as ending in 1919 and thereby included 1920 as the year of war aftermath. However, the results change little if we instead treat the war as ending in 1918, so that 1919 is the year of war aftermath.

B. Effects of Marginal Income-Tax Rates

The equations in Table 2 include the lagged change in the overall marginal income-tax rate, $\Delta\tau(-1)$. For the sample that starts in 1950, the estimated coefficient is -0.58 (s.e.=0.21), which is significantly negative with a p-value less than 0.01. Thus, the estimate is that a cut in the marginal tax rate by 1 percentage point raises the next year's per capita GDP growth rate by around 0.6% per year. We consider later additional lags of the tax-rate variable.

Samples that start earlier than 1950 show much less of an impact from $\Delta\tau(-1)$ on GDP growth. For example, for the sample that starts in 1939, in Table 2, column 2, the estimated coefficient is 0.10 (s.e.=0.16). One problem is that the dramatic rise in the average marginal tax rate from 0.069 in 1940 to 0.263 in 1944 does not match up well with the strong GDP growth during WWII (even when one factors in the positive effect from the rise in defense purchases from 1941 to 1944).[20]

Aside from the dramatic rise in defense spending and tax rates, World War II featured an expansion of direct governmental control over the allocation of private resources, including the large-scale military draft and mandates on production, such as the shift from cars to tanks and other military equipment (mostly outside of usual price mechanisms). Any positive effect of patriotism on labor supply would reinforce the command-and-control aspects of the wartime economy. Similar forces arose during World War I.

For our purposes, the key point is that the sensitivity of production, GDP, to the marginal income-tax rate is likely to be weaker the larger the fraction of GDP that is allocated by governmental directive. To gauge this effect empirically, we use the ratio of defense purchases

[20]Another problem is that the fall in the average marginal tax rate from 0.238 in 1947 to 0.193 in 1948 does not accord with the 1949 recession.

to GDP as a proxy for the extent of command and control. An interaction term between the defense-purchases ratio and the marginal tax rate, τ_t, then picks up the attenuation of the impact of tax rates on GDP. The regressions shown in columns 3-5 of Table 2 include the lagged change in this interaction term, along with the lagged change in the tax rate, $\Delta\tau(-1)$. For example, for the sample that starts in 1939 in column 3, the estimated coefficient of the interaction term is significantly positive, whereas that for $\Delta\tau(-1)$ is negative (though not statistically significant on its own). The two tax-rate variables are jointly significant with a p-value of 0.03. The point estimates imply that the net effect of a lagged change in τ on GDP growth becomes nil when the defense purchases ratio rises to 0.36 (compared to the peak of 0.44 reached in 1944).

C. The Yield Spread

Table 2 shows that the estimated coefficient on the yield-spread variable is significantly negative for each sample. (Recall that the variable is the squared difference between the Baa bond rate and the U.S. Treasury bond rate, and the lagged value of this variable appears on the instrument list.) That is, as expected, a larger gap between the Baa bond rate and the U.S. Treasury rate predicts lower GDP growth. The magnitude of the coefficient is similar across samples, except when the sample goes back far enough to include the GDP growth rates from the Great Depression, 1930-33. The inclusion of these depression years raises the magnitude of the estimated coefficient (to fit the extremely low growth rates of 1930-33). For example, for the 1930 sample, if one allows for two separate coefficients on the yield-spread variable, the estimated coefficients are -111.3 (s.e.=14.9) for 1930-38 and -48.0 (28.8) for 1939-2006. (This regression allows also for separate intercepts up to 1938 and after 1938.) The two estimated coefficients on the yield-spread variable differ significantly with a p-value of 0.051. Similar results apply if the sample starts in 1917.

For our purposes, an important result is that the estimated coefficients on the defense-spending and tax-rate variables do not change a lot if the equation excludes the yield-spread variable. For example, for the 1939 sample (corresponding to column 3 of Table 2), the estimated coefficients become 0.71 (s.e.=0.08) on Δg-defense, 0.47 (0.14) on U(-1), 6.1 (2.4) on (Δg-defense)*U(-1), -0.31 (0.21) on Δτ(-1), and 0.72 (0.31) on Δ[τ*(g-def/y)](-1). Thus, the main change is for the variable U(-1), which picks up business-cycle dynamics. For the 1930 and 1917 samples, the main changes are again in the estimated coefficients on U(-1). For the 1950 sample (corresponding to column 2 of Table 2), the deletion of the yield-spread variable raises the magnitudes of the estimated fiscal effects. The estimated coefficients become 1.01 (s.e.=0.30) on Δg-defense, 0.40 (0.19) on U(-1), and -0.69 (0.23) on Δτ(-1).

Since we think that holding fixed a measure of credit conditions sharpens the estimates of the effects of the fiscal variables, we focus on the results presented in Table 2. However, the robustness of the main results to deletion of the interest-rate spread variable heightens our confidence in the findings with regard to fiscal effects.

D. More Results on Taxes

Table 3 presents further results on tax variables for the 1950-2006 sample. Column 1 is essentially the same as column 1 of Table 2, with a minor difference in the instrument list.

Column 2 of Table 3 adds the second annual lag of the $\Delta\tau$ variable. The estimated coefficient is negative but not statistically significant, and the first lag remains significantly negative.[21]

The problem with adding the contemporaneous value of $\Delta\tau$ as a regressor is endogeneity. For a given tax structure, a rise in real GDP tends to raise τ because households are pushed into higher marginal-rate brackets. In addition, the government might respond within the year to higher real GDP by changing the tax law applicable to income for the current calendar year. To take account of the first channel, we calculated what each household's marginal income-tax rate would have been based on the prior year's (nominal) income when applied to the current year's tax law. We then averaged the marginal rates based on the previous year's incomes. We were able to construct this variable, using the NBER's TAXSIM program, for the federal individual income tax and social security from 1967 to 2006. We then formed an instrument for the current value of $\Delta\tau$ by taking the value of τ_t constructed as just described for the federal income tax and social security and subtracting the actual values of average marginal rates from the federal income tax and social security for the previous year. (This instrument assumes a value of zero for the change in the average marginal tax rate from state income taxes.) For the period 1950-66, the instrument takes on the constant value -0.0005, which is the median change from 1950 to 2006 (or, it turns out, also from 1917 to 2006).

If we add the contemporaneous change in τ to column 2 of Table 3, while adding the variable just described to the instrument list, we get that the estimated coefficients on the $\Delta\tau$

[21]Our focus is on the overall marginal income-tax rate; that is, we implicitly have the same coefficients for changes in federal and state income-tax rates as for changes in social-security tax rates. If we separate the two income-tax rates from the social-security rate, we surprisingly get larger size coefficients for social security. The hypothesis of equal magnitude coefficients for each of the two lags is rejected with a p-value of 0.02. We have no good economic explanation for this result. However, in terms of the data patterns, the strong results for the social-security tax rate seem to arise because the increases in average marginal tax rates starting in the early 1970s fit well with the recessions of the mid 1970s and the early 1980s.

variables are 0.26 (s.e.=0.30) for the current year, -0.56 (0.25) for the first lag, and -0.22 (0.19) for the second lag.[22] Thus, the contemporaneous effect differs insignificantly from zero. Moreover, the positive point estimate may still reflect endogeneity, in the sense of the government tending to raise tax rates when the economy is doing well, and vice versa.

Columns 3 and 4 of Table 3 show results for the Romer-Romer tax-change variable, while excluding the marginal tax-rate variable. The main result is a significantly negative effect for the first lag, -1.21 (s.e.=0.39), in column 3. The second-lag coefficient is negative but not significantly different from zero (column 4). This time pattern is broadly consistent with the findings of Romer and Romer (2009, Figure 9, panel c), based on a quarterly VAR system with one of their four categories of tax changes. That is, their main negative response of real GDP to a tax hike occurs with a lag of 3 to 10 quarters. They get a larger effect (say for the average response for quarterly lags between 4 and 7), but for only the one type of tax change and without holding constant the variables used in our Table 3.

The results change little if we instrument the Romer-Romer tax-change variable with the changes designated by them as exogenous. In this case, the estimated coefficient on the first lag (corresponding to Table 3, column 3) becomes -1.26 (s.e.=0.48). With the second lag included (corresponding to column 4), the estimated coefficients become -1.18 (0.50) on the first lag and -0.68 (0.37) on the second.

Columns 5 and 6 of Table 3 include the marginal tax-rate variables and the Romer-Romer variables at the same time. As noted before, we can then reasonably interpret the marginal tax-rate variable as reflecting substitution effects, with the Romer-Romer variable picking up income

[22]If we include the actual value of the current change in τ on the instrument list, the estimated coefficient on the current value of $\Delta\tau$ becomes 0.35 (s.e.=0.23), larger than before, as expected, but still insignificantly different from zero.

effects. With this perspective, the results that include two lags of each variable in column 6 are intriguing. The suggestion is that the effects from changes in marginal income-tax rates (substitution effects) show up mostly with a two-year lag (coefficient of -0.61, s.e.=0.28), whereas those from changing tax revenues (income effects) appear mainly with a one-year lag (coefficient of -0.96, s.e.=0.49). A possible explanation, worth further investigation, is that economic agents respond less rapidly to changed incentives (conveyed by shifts in marginal income-tax rates) than to changed incomes.

We observed before, in Table 2, that the effects from changes in marginal income-tax rates look weaker in samples that start before 1950. The Romer-Romer data begin in 1945, so the earliest starting date that we can consider while allowing for two lags of this variable is 1947. In practice, beginning even one year earlier than 1950 eliminates the joint significance of the Romer-Romer tax variables shown in Table 3, column 4 (where the p-value for joint significance of the two lags was 0.007). For example, for a sample that starts in 1948, the estimated coefficients on the Romer-Romer variables are -0.60 (s.e.=0.39) on the first lag and -0.27 (0.31) on the second, with a p-value for joint significance of 0.20. The key problem is that the tax cut in 1948 does not match up well with the 1949 recession. Note that Romer and Romer (2009) use a sample that starts in 1950.

The marginal tax-rate variable holds up somewhat better with an earlier starting date. For example, for a sample that begins in 1948, the estimated coefficients (corresponding to Table 3, column 2) are -0.12 (s.e.=0.20) on the first lag and -0.40 (0.19) on the second, with a p-value for joint significance of 0.052.[23] These results highlight the previous finding that the impact of a change in marginal tax rates shows up mostly with a two-year lag.

Instead of following Romer and Romer (2009) by entering their variable directly into regressions for GDP growth, it might be better to use their measures as instruments for variables involving actual changes in federal revenues. The change in total federal revenue presumably enters into the determination of the growth rate of per capita real GDP in equation (1) but has the shortcoming of being strongly endogenous. Specifically, we use the change in per capita real federal tax revenue, expressed as a ratio to the prior year's per capita real GDP (a form that parallels the one used for the defense-purchases variable in Tables 2 and 3). The results analogous to Table 3, column 4 (with the federal-revenue variables used for the right-hand-side variables but the corresponding Romer-Romer variables used as instruments) are an estimated coefficient of -0.65 (s.e.=0.31) on the first lag and -0.56 (0.34) on the second. The p-value for joint significance is 0.025. If we use instead as instruments only the "exogenous" parts of the Romer-Romer tax changes, the two coefficients become -0.65 (0.32) on the first lag and -0.81 (0.40) on the second, with a p-value for joint significance of 0.018. Note that the coefficients in these specifications can be viewed as tax-revenue multipliers. Therefore, the cumulated multipliers over two years are estimated to exceed one in magnitude. However, in all cases, the joint significance of the tax-change variables still vanishes if the sample starts before 1950.

E. More Results on Government Purchases

[23]If the marginal tax-rate and Romer-Romer variables are entered together for a sample starting in 1948, the estimated coefficients for the tax-rate variable are 0.21 (s.e.=0.29) for the first lag and -0.78 (0.30) for the second, and those for the Romer-Romer variable are -0.74 (0.54) for the first lag and 0.72 (0.49) for the second. P-values for joint significance are 0.038 for the marginal tax-rate variables, 0.139 for the Romer-Romer variables, and 0.040 for all tax variables.

The results in Table 2 seem to provide reliable estimates of multipliers for defense spending, particularly for long samples that include WWII. These multipliers are estimated to be around 0.6-0.7 at the median unemployment rate. However, to evaluate typical fiscal-stimulus packages, we are more interested in multipliers associated with non-defense purchases. The problem, already mentioned, is that these multipliers are hard to estimate because the observed movements in non-defense purchases are likely to be strongly endogenous with respect to real GDP. Given these problems, it is important to know whether the defense-spending multiplier provides an upper or lower bound for the non-defense multiplier.

Consider, from a theoretical standpoint, how the multiplier for non-defense purchases relates to that for defense spending. One point is that the movements in defense spending, driven to a considerable extent by war and peace, are likely to be more temporary than those in non-defense purchases, a property stressed by Barro (1981). However, in a baseline model worked out by Barro and King (1984), the extent to which a change in government purchases is temporary has no impact on the size of the multiplier. The key features of this model are the existence of a representative agent with time-separable preferences over consumption and leisure, the absence of durable goods, lump-sum taxation, and "market clearing."

Consider now some realistic deviations from the Barro-King setting. With distorting taxes, the government's incentive to tax-smooth implies that tax rates tend to rise less when a given size change in government purchases is more temporary.[24] Since higher tax rates discourage economic activity, we conclude that a temporary change in purchases, such as in wartime, tends to have a larger effect on real GDP than an equal-size permanent change.

[24]In World War II, defense spending was temporarily high, implying, on tax-smoothing grounds, that much of the added spending was deficit financed. However, the war likely also substantially raised the anticipated long-run ratio of government spending to GDP. This change motivated a rise in tax rates—the average marginal tax rate from the federal individual income tax increased from 3.8% in 1939 to 25.7% in 1945 (see Table 1).

However, in the empirical analysis, we already held constant measures of marginal income-tax rates. Within a Barro-King setting, but with distorting taxes, the spending multiplier estimated for given tax rates should not depend on whether the change in spending is temporary or permanent.

If we allow for durable goods and, hence, investment, the crowding out of investment tends to be larger when the increase in government purchases is more temporary. The decline in investment means that consumption and leisure fall by less than otherwise and, hence, that work effort rises less by less than otherwise. If production depends in the short run only on labor input, we conclude that the multiplier is smaller when the increase in government purchases is more temporary (as in wartime). However, this last conclusion need not hold if we allow for variable capital utilization, which tends to expand more when the increase in purchases is more temporary.

Some other considerations suggest that the defense-spending multiplier, dominated by behavior during wartime, would exceed the non-defense multiplier. During wars, governments tend to use command-and-control techniques, such as drafting people to work in the military and forcing companies to produce tanks rather than cars (all without reliance on explicit prices). Such actions tend to raise the responsiveness of real GDP to changes in government purchases. In addition, patriotism likely shifts labor supply outward in wartime (at least in an important and popular war, such as WWII). Again, this factor tends to make the wartime multiplier comparatively large.

Our conjecture is that, because of command-and-control considerations, the defense-spending multiplier associated with war and peace would exceed the non-defense multiplier. In this case, the defense-spending multiplier—for which we have good estimates—would provide an upper bound for the non-defense multiplier. However, since the comparison between the two multipliers is generally ambiguous on theoretical grounds, it would obviously be desirable to have direct, reliable estimates of the non-defense multiplier.

Table 4 shows regression results when a non-defense purchases variable—constructed analogously to the defense-spending variable—is added to the regressions shown in Table 2. Crucially, the instrument list includes the contemporaneous value of the non-defense purchases variable. The estimated multiplier is large and significantly different from zero for the 1950 sample—the estimated coefficient is 1.93 (s.e.=0.90). However, the estimated coefficients are insignificantly different from zero for the other samples, starting in 1939, 1930, or 1917.

A reasonable interpretation is that the estimated coefficients on the non-defense purchases variable pick up primarily reverse causation from real GDP to government spending but that the nature of this reverse linkage varies over time. In particular, in the 1950 sample, non-defense purchases are dominated by state & local outlays, which are particularly likely to reflect positive feedback from real GDP to government purchases.

To illustrate the potential for spuriously high estimated multipliers due to endogeneity, in Table 4, columns 5 and 6, we replaced the non-defense purchases variable by analogously defined variables based on sales of two large U.S. corporations with long histories—General Motors (which used to be important) and General Electric. In column 5, the estimated "multiplier" based on GM sales for a 1950 sample is 3.44 (s.e.=0.82). That is, this "multiplier" exceeds three. For GE sales—which are less volatile than GM's but also more correlated with real GDP—the result is even crazier, 17.9 (4.6). Moreover, unlike for non-defense purchases, the estimated GM and GE coefficients turn out to be similar when estimated over the longer samples. Clearly, the GM and GE estimates reflect mainly reverse causation from real GDP to sales of individual companies. We think that a similar perspective applies to the apparent multiplier above one estimated for non-defense purchases for the 1950 sample in column 1.

The problem is that, lacking good instruments, we cannot estimate multipliers satisfactorily for non-defense purchases.[25] The VAR literature makes identifying assumptions based typically on changes in government purchases being pre-determined within a quarter (see, for example, Blanchard and Perotti [2002]). This procedure, which has been criticized by Ramey (2008), corresponds in annual data to the kinds of estimates for non-defense purchases shown in columns 1-4 of Table 4. We think these results are not meaningful. Probably a more productive avenue is a search for satisfactory instruments for changes in non-defense government purchases for the United States or perhaps for other countries.

F. Effects on Components of GDP

Table 5 returns to the setting of Table 2, but with the dependent variables corresponding to changes in components of GDP, rather than overall GDP. For example, for personal consumer expenditure, the dependent variable is the difference between this year's per capita real expenditure (nominal spending divided by the GDP deflator) and the previous year's per capita real expenditure, all divided by the prior year's per capita real GDP. The same approach applies in Table 5 to gross private domestic investment, I, non-defense government purchases, and net

[25]Lagged changes in the non-defense purchases variable provide only weak instruments and tend to produce estimated coefficients with very high standard errors. With the first lag used as an instrument for the 1950 sample, the estimated coefficient of the non-defense-purchases variable is 5.6 (s.e.=3.1). With the second lag used as an instrument, the result is 1.9 (3.1).

exports. Note that this method relates spending on the various parts of GDP to defense spending and the other right-hand-side variables considered in Table 2 but does not allow for effects from changing relative prices, for example, for C versus I.[26] This spending approach is valid even though the GDP deflator comes from chain-weighting—since all nominal magnitudes are divided by the same price index (the GDP deflator) in each year. Moreover, in this approach, the effects found for overall GDP in Table 2 correspond to the sum of the effects for the components of GDP considered in Table 5. For example, the defense-spending multiplier estimated in Table 2 equals one plus the sum of the estimated effects on the four components of GDP in Table 5. For the other right-hand-side variables, the estimated effect in Table 2 equals the sum of the estimated effects in Table 5.

The data for the components of GDP in Table 5 come from the BEA information available annually since 1929. Therefore, the samples considered in Table 5—1950-2006, 1939-2006, and 1930-2006—do not go back before 1930. (For the 1917-2006 sample in Table 2, we used non-BEA data before 1929 for GDP and government purchases.)

Recall that the defense-spending multipliers estimated for overall GDP in Table 2 were 0.6-0.7. Correspondingly, the sum of the magnitudes of the crowding-out coefficients for the four parts of GDP considered in Table 5 is 0.3-0.4. For example, for the 1930-2006 sample, the estimated coefficient for C is -0.02 (s.e.=0.05)—not statistically significantly different from zero—that for I is -0.20 (0.06), that for non-defense government purchases is -0.06 (0.02), and that for net exports is -0.06 (0.02). Thus, the clearest crowding out from defense spending

[26]Exogenous changes in relative prices likely affect the shares of the various expenditure components in overall nominal GDP. We have not yet investigated this possibility.

applies to private investment. However, crowding out is also significant for non-defense government purchases and net exports but not for private consumer expenditure.

The negative effect from the average marginal income-tax rate on overall GDP shows up most clearly in Table 2 for the 1950-2006 sample, with an estimated coefficient of -0.58 (s.e.=0.21). Table 5 shows that this response divides up roughly equally between C (-0.29, s.e.=0.11) and I (-0.35, s.e.=0.15).

For the interest-rate spread variable, Table 2 shows negative effects on overall GDP for all samples but a larger magnitude of response in the 1930 sample, which includes the Great Depression. In Table 5, the main negative effect in the 1950 and 1939 samples is on I. The change in the 1930 sample is a sharply negative effect on C—which did not appear in the samples that exclude the Great Depression.

VI. Concluding Observations

For samples that include World War II, the estimated multiplier connecting changes in defense spending to responses of real GDP is reliably determined to be in the range of 0.6-0.7 when evaluated at the median unemployment rate. There is some evidence that this multiplier rises with the amount of economic slack, with the estimated multiplier reaching one when the unemployment rate is around 12%. In contrast, we lack reliable estimates of the multiplier for non-defense purchases, mostly because the lack of good instruments makes it infeasible to isolate the direction of causation between these purchases and real GDP.

Samples since 1950 indicate substantial and significantly negative effects from changes in average marginal income-tax rates on real GDP growth. There is some evidence that the tax effects involve a combination of income effects (mainly with a one-year lag) and substitution effects (mostly with a two-year lag). Unfortunately, these tax effects are less reliably estimated in long-term samples, for example, those that include World War II, the Great Depression, and World War I.

Since the defense-spending multiplier is significantly less than one in long samples, a rise in defense spending is estimated to crowd out significantly other components of GDP. The main crowding out shows up for gross private domestic investment but significant negative effects show up also for non-defense government purchases and net exports. The effect on private consumer expenditure differs insignificantly from zero.

An important extension would be to apply the U.S. results to long-term macroeconomic data for other countries. However, the U.S. evidence from variations in defense spending works well for isolating multipliers because the main wars—the two world wars and Korea—involved little destruction of U.S. capital stock and only moderate loss of life. The massive destruction during the disasters for WWI and WWII for most other OECD countries would preclude a similar analysis. However, some other cases—such as Canada and Latin American countries during the world wars—may work. Another challenge for these other countries is the lack of information on average marginal income-tax rates—the variable constructed in the present study for the United States. It is unclear whether the necessary underlying information exists to assemble analogous long-term time series for other countries.

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Table 1 Data on Average Marginal Income-Tax Rates				
Year	**Overall Marginal Tax Rate**	**Federal Individual Income Tax**	**Social-Security Payroll Tax**	**State Income Taxes**
1912	0.000	0.000	0.000	[0.0001]
1913	0.003	[0.003]	0.000	[0.0001]
1914	0.006	[0.006]	0.000	[0.0002]
1915	0.009	[0.009]	0.000	[0.0002]
1916	0.012	0.012	0.000	[0.0003]
1917	0.037	0.037	0.000	[0.0003]
1918	0.054	0.054	0.000	[0.0004]
1919	0.052	0.052	0.000	[0.0004]
1920	0.046	0.046	0.000	[0.0005]
1921	0.043	0.042	0.000	[0.0005]
1922	0.047	0.046	0.000	[0.0006]
1923	0.034	0.033	0.000	[0.0006]
1924	0.036	0.035	0.000	[0.0007]
1925	0.031	0.030	0.000	[0.0007]
1926	0.029	0.028	0.000	[0.0007]
1927	0.033	0.032	0.000	[0.0008]
1928	0.042	0.041	0.000	[0.0008]
1929	0.036	0.035	0.000	0.0009
1930	0.024	0.023	0.000	0.0007
1931	0.018	0.017	0.000	0.0006
1932	0.030	0.029	0.000	0.0006
1933	0.032	0.031	0.000	0.0015
1934	0.036	0.034	0.000	0.0018
1935	0.041	0.038	0.000	0.0028
1936	0.055	0.052	0.000	0.0032
1937	0.058	0.046	0.009	0.0035
1938	0.046	0.034	0.009	0.0032
1939	0.051	0.038	0.009	0.0036
1940	0.069	0.056	0.009	0.0038
1941	0.126	0.113	0.009	0.0038
1942	0.205	0.192	0.008	0.0047
1943	0.221	0.209	0.007	0.0048
1944	0.263	0.252	0.006	0.0052
1945	0.268	0.257	0.006	0.0047
1946	0.238	0.226	0.007	0.0052
1947	0.238	0.226	0.006	0.0056
1948	0.193	0.180	0.006	0.0072
1949	0.187	0.175	0.005	0.0072
1950	0.211	0.196	0.007	0.0079

Table 1, continued				
Year	**Overall Marginal Tax Rate**	**Federal Individual Income Tax**	**Social-Security Payroll Tax**	**State Income Taxes**
1951	0.248	0.231	0.009	0.0085
1952	0.268	0.251	0.008	0.0086
1953	0.266	0.249	0.008	0.0086
1954	0.241	0.222	0.010	0.0087
1955	0.250	0.228	0.012	0.0098
1956	0.254	0.232	0.012	0.0101
1957	0.255	0.232	0.013	0.0104
1958	0.253	0.229	0.013	0.0114
1959	0.265	0.236	0.016	0.0130
1960	0.265	0.234	0.018	0.0129
1961	0.270	0.240	0.017	0.0132
1962	0.275	0.244	0.017	0.0142
1963	0.280	0.247	0.018	0.0146
1964	0.253	0.221	0.017	0.0155
1965	0.244	0.212	0.016	0.0164
1966	0.251	0.212	0.022	0.0173
1967	0.256	0.215	0.021	0.0202
1968	0.286	0.238	0.026	0.0229
1969	0.298	0.250	0.024	0.0245
1970	0.286	0.237	0.022	0.0270
1971	0.278	0.227	0.022	0.0291
1972	0.289	0.231	0.025	0.0332
1973	0.305	0.239	0.034	0.0327
1974	0.325	0.247	0.042	0.0354
1975	0.333	0.254	0.043	0.0370
1976	0.340	0.257	0.043	0.0391
1977	0.361	0.277	0.043	0.0410
1978	0.369	0.284	0.043	0.0421
1979	0.384	0.273	0.068	0.0420
1980	0.400	0.286	0.072	0.0412
1981	0.418	0.294	0.084	0.0403
1982	0.404	0.275	0.087	0.0414
1983	0.391	0.256	0.091	0.0450
1984	0.393	0.254	0.095	0.0446
1985	0.399	0.260	0.095	0.0442

Table 1, continued				
Year	**Overall Marginal Tax Rate**	**Federal Individual Income Tax**	**Social-Security Payroll Tax**	**State Income Taxes**
1986	0.401	0.259	0.097	0.0447
1987	0.375	0.237	0.096	0.0422
1988	0.356	0.218	0.097	0.0418
1989	0.360	0.218	0.100	0.0421
1990	0.362	0.217	0.102	0.0421
1991	0.371	0.219	0.108	0.0438
1992	0.369	0.217	0.108	0.0448
1993	0.379	0.224	0.110	0.0446
1994	0.385	0.230	0.111	0.0446
1995	0.386	0.232	0.109	0.0445
1996	0.385	0.235	0.107	0.0441
1997	0.386	0.237	0.105	0.0440
1998	0.387	0.239	0.104	0.0440
1999	0.390	0.243	0.103	0.0442
2000	0.392	0.247	0.101	0.0442
2001	0.385	0.238	0.103	0.0440
2002	0.380	0.231	0.105	0.0436
2003	0.359	0.211	0.104	0.0441
2004	0.358	0.213	0.102	0.0433
2005	0.351	0.216	0.092	0.0433
2006	0.353	0.217	0.093	0.0432

Note: See the text on the construction of average (income-weighted) marginal tax rates for the federal individual income tax, social-security payroll tax, and state income taxes. Values shown in brackets for the federal individual income tax for 1913-15 and for state income taxes for 1912-28 are interpolations. The total is the sum of the three pieces. The construction of these data will be detailed in an appendix to be posted at http://www.economics.harvard.edu/faculty/barro/data_sets_barro.

Table 2 Equations for GDP Growth, Various Samples					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Starting date	**1950**	**1939**	**1939**	**1930**	**1917**
Δg-defense	0.77** (0.28)	0.59** (0.06)	0.67** (0.07)	0.66** (0.09)	0.64** (0.10)
U(-1)	0.52** (0.17)	0.67** (0.15)	0.62** (0.15)	0.62** (0.10)	0.48** (0.11)
(Δg-defense)*U(-1)	--	--	5.1* (2.2)	4.6 (2.9)	4.6 (3.3)
Δτ(-1)	-0.58** (0.21)	0.10 (0.16)	-0.26 (0.19)	-0.39 (0.25)	-0.32 (0.28)
Δ[τ*(g-def/y)](-1)	--	--	0.73** (0.28)	0.84* (0.38)	0.79 (0.42)
Interest-rate spread, square	-48.3* (20.5)	-56.4* (23.5)	-48.1* (22.9)	-104.1** (13.2)	-76.1** (12.4)
p-value for τ variables	0.007	0.53	0.030	0.083	0.17
R^2	0.46	0.77	0.80	0.73	0.65
σ	0.018	0.021	0.020	0.027	0.031

*Significant at 0.05 level. **Significant at 0.01 level.

Notes to Table 2

Data are annual from the starting year shown through 2006. The dependent variable is the change from the previous year in per capita real GDP divided by the previous year's per capita real GDP. Δg-defense is the change from the previous year in per capita real defense spending (nominal spending divided by the GDP deflator) divided by the previous year's per capita real GDP. U(-1) is the lagged unemployment rate. (Δg-defense)*U(-1) is an interaction between the first two independent variables, each expressed as a deviation from its respective median over 1914-2006: 0.0002 for Δg-defense and 0.0557 for U(-1). Δτ is the change from the previous year in the average marginal income-tax rate from federal and state income taxes and social security, as shown in Table 1. The lagged value of this variable appears in the equations. Δ[τ*(g-def/y)](-1) is the lagged value of the change in the interaction between the average marginal income-tax rate and the ratio of defense spending to GDP. The interest-rate spread is the difference between the yield on long-term Baa corporate bonds and that on long-term U.S. Treasury bonds. Before 1919, the spread is estimated from data on long-term Aaa corporate bonds. The square of the spread appears in the equations. Data on yields are from Moody's, as reported on the website of the Federal Reserve Bank of St. Louis.

Estimation is by two-stage least-squares, using as instruments all of the independent variables in this table, except for the square of the interest-rate spread, which is replaced by its lagged value, and the variables involving the changes in defense spending. These variables are replaced by interactions with a dummy variable for war years (1914-20, 1939-46, 1950-54, 1966-71). For WWI, the period starts with the initiation of war in Europe, 1914, goes to the signing of the Versailles Treaty in 1919, then includes one year of war aftermath. Similarly, WWII covers 1939-46. The other years designated as "war" are those with a casualty rate of at least 0.01 per thousand population or for the year following a casualty rate at least this high. The instrument list also contains the first lag of the dependent variable. The p-value is for a test that the coefficients on (one or two) variables involving the tax rate are all equal to zero. σ is the standard-error-of-estimate.

Table 3 Equations for GDP Growth, 1950-2006, More Results on Tax Variables						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Δg-defense	0.75** (0.28)	0.70* (0.28)	0.95** (0.31)	0.97** (0.31)	0.97** (0.31)	0.84** (0.30)
U(-1)	0.53** (0.17)	0.51** (0.17)	0.41* (0.18)	0.39* (0.19)	0.42* (0.18)	0.46** (0.18)
Δτ(-1)	-0.57** (0.21)	-0.45* (0.22)	--	--	-0.28 (0.26)	-0.07 (0.27)
Δτ(-2)	--	-0.27 (0.19)	--	--	--	-0.61* (0.28)
Romer: [Δ(federal tax revenue)/Y(-1)](-1)	--	--	-1.21** (0.39)	-1.20** (0.39)	-0.88 (0.50)	-0.96 (0.49)
Romer: [Δ(federal tax revenue)/Y(-1)](-2)	--		--	-0.17 (0.31)	--	0.67 (0.47)
Interest-rate spread, square	-53.6** (20.5)	-45.6* (21.0)	-55.5** (20.0)	-53.0** (20.7)	-52.1** (20.3)	-42.7* (20.5)
p-value for τ variables	0.008	0.007	--	--	0.29	0.058
p-value for Romer variables	--	--	0.003	0.007	0.086	0.055
p-value for all tax variables	0.008	0.007	0.003	0.007	0.004	0.003
R^2	0.46	0.48	0.47	0.48	0.48	0.53
σ	0.018	0.017	0.017	0.017	0.017	0.017

*Significant at 0.05 level. **Significant at 0.01 level.

Note: See the notes to Table 2. Data are annual from 1950 through 2006. Δτ(-2) is the second annual lag of the change in the average marginal income-tax rate. The Romer variable is based on the construction from Romer and Romer (2008, Table 1, columns 1-4) of legislated, projected incremental nominal federal tax revenues for year t. We use their series that neglects retroactive changes. Their data indicate the quarter in which the incremental revenue flow (expressed at an annual rate) is supposed to start. We aggregated their quarterly figures for each year to get annual numbers and then expressed these values as ratios to the previous year's nominal GDP. One or two annual lags of this variable appear in the equations. Estimation is by two-stage least-squares. See the notes to Table 2 for a discussion of the instruments

Table 4 Equations for GDP Growth, More Results on Government Purchases						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Starting date	**1950**	**1939**	**1930**	**1917**	**1950**	**1950**
Δg-defense	0.89** (0.28)	0.65** (0.08)	0.65** (0.10)	0.64** (0.10)	0.75** (0.24)	0.59* (0.26)
U(-1)	0.64** (0.18)	0.61** (0.15)	0.62** (0.11)	0.48** (0.11)	0.31* (0.16)	0.56** (0.15)
(Δg-defense)*U(-1)	--	4.7* (2.4)	4.5 (3.1)	4.6 (3.3)	--	--
Δτ(-1)	-0.54** (0.20)	-0.29 (0.20)	-0.40 (0.26)	-0.32 (0.28)	-0.36* (0.18)	-0.43* (0.19)
Δ[τ*(g-def/y)](-1)	--	0.72** (0.28)	0.84* (0.38)	0.79 (0.42)	--	--
Interest-rate spread, square	-41.4* (20.2)	-49.8* (23.0)	-104.7** (13.6)	-75.7** (12.4)	-40.5* (18.4)	-24.0 (20.1)
Δg-non-defense	1.93* (0.90)	-0.34 (0.73)	-0.13 (0.63)	0.05 (0.48)	--	--
Δ(GM sales)	--	--	--	--	3.44** (0.82)	--
Δ(GE sales)	--	--	--	--	--	17.86** (4.59)
R^2	0.49	0.80	0.73	0.65	0.51	0.56
σ	0.017	0.020	0.028	0.031	0.017	0.016

*Significant at 0.05 level. **Significant at 0.01 level.

Note: See the notes to Table 2. The first four columns include the variable Δg-non-defense, which is the change from the previous year in per capita real non-defense government purchases (nominal purchases divided by the GDP deflator) for all levels of government, divided by the previous year's per capita real GDP. The variable Δg-non-defense is included in the instrument lists for the first four columns. Δ(GM sales) is the change from the previous year in per capita real net sales of General Motors Corporation, expressed as a ratio to the previous year's per capita real GDP. Real net sales are nominal sales divided by the GDP deflator. This variable is included in the instrument list for column 5. Δ(GE sales), in column 6, is treated analogously but using net sales of General Electric Corporation. The GM and GE data come from annual reports of the two companies.

Table 5 Effects on Components of GDP				
Sample: 1950-2006				
Dependent variable	**ΔC**	**ΔI**	**Δ(G-non def)**	**Δ(X-M)**
Δg-defense	0.00	-0.13	-0.06	-0.04
	(0.15)	(0.21)	(0.04)	(0.09)
U(-1)	0.22*	0.43**	-0.06*	-0.08
	(0.09)	(0.12)	(0.03)	(0.05)
Δτ(-1)	-0.29**	-0.35*	-0.02	0.07
	(0.11)	(0.15)	(0.03)	(0.07)
Interest-rate spread, square	-10.1	-31.6*	-3.6	-2.1
	(11.2)	(14.9)	(3.2)	(6.5)
R^2	0.29	0.44	0.15	0.08
σ	0.010	0.013	0.003	0.006
Sample: 1939-2006				
Δg-defense	-0.01	-0.22**	-0.05**	-0.05*
	(0.04)	(0.06)	(0.01)	(0.02)
U(-1)	0.20**	0.48**	-0.04	-0.02
	(0.07)	(0.12)	(0.03)	(0.05)
(Δg-defense)*U(-1)	2.7*	2.4	-1.2**	1.2
	(1.1)	(1.7)	(0.4)	(0.7)
Δτ(-1)	-0.19*	-0.08	-0.09**	0.09
	(0.10)	(0.15)	(0.03)	(0.06)
Δ[τ*(g-def/y)](-1)	0.58**	0.29	-0.03	-0.11
	(0.14)	(0.22)	(0.05)	(0.09)
Interest-rate spread, square	-6.3	-30.4	-5.0	-5.5
	(11.5)	(18.0)	(4.2)	(7.3)
R^2	0.35	0.54	0.51	0.35
σ	0.010	0.016	0.004	0.006
Sample: 1930-2006				
Δg-defense	-0.02	-0.20**	-0.06**	-0.06**
	(0.05)	(0.06)	(0.02)	(0.02)
U(-1)	0.25**	0.32**	0.03	0.00
	(0.06)	(0.07)	(0.02)	(0.02)
(Δg-defense)*U(-1)	2.1	2.7	-1.3*	1.2
	(1.7)	(1.9)	(0.6)	(0.7)
Δτ(-1)	-0.25	-0.15	-0.09	0.10
	(0.14)	(0.16)	(0.05)	(0.06)
Δ[τ*(g-def/y)](-1)	0.66**	0.29	0.00	-0.11
	(0.22)	(0.25)	(0.07)	(0.09)
Interest-rate spread, square	-56.1**	-42.9**	-4.2	-0.6
	(7.6)	(8.6)	(2.6)	(3.0)
R^2	0.46	0.54	0.30	0.34
σ	0.016	0.018	0.005	0.006

*Significant at 0.05 level. **Significant at 0.01 level.

Notes to Table 5

These results correspond to Table 2 except for the specifications of the dependent variables, which are now based on components of GDP. For personal consumer expenditure, C, the dependent variable equals the change in per capita real expenditure (nominal expenditure divided by the GDP deflator), expressed as a ratio to the previous year's per capita real GDP. The same approach applies to gross private domestic investment, I, non-defense government purchases (by all levels of government), and net exports. Data since 1929 are from the Bureau of Economic Analysis.

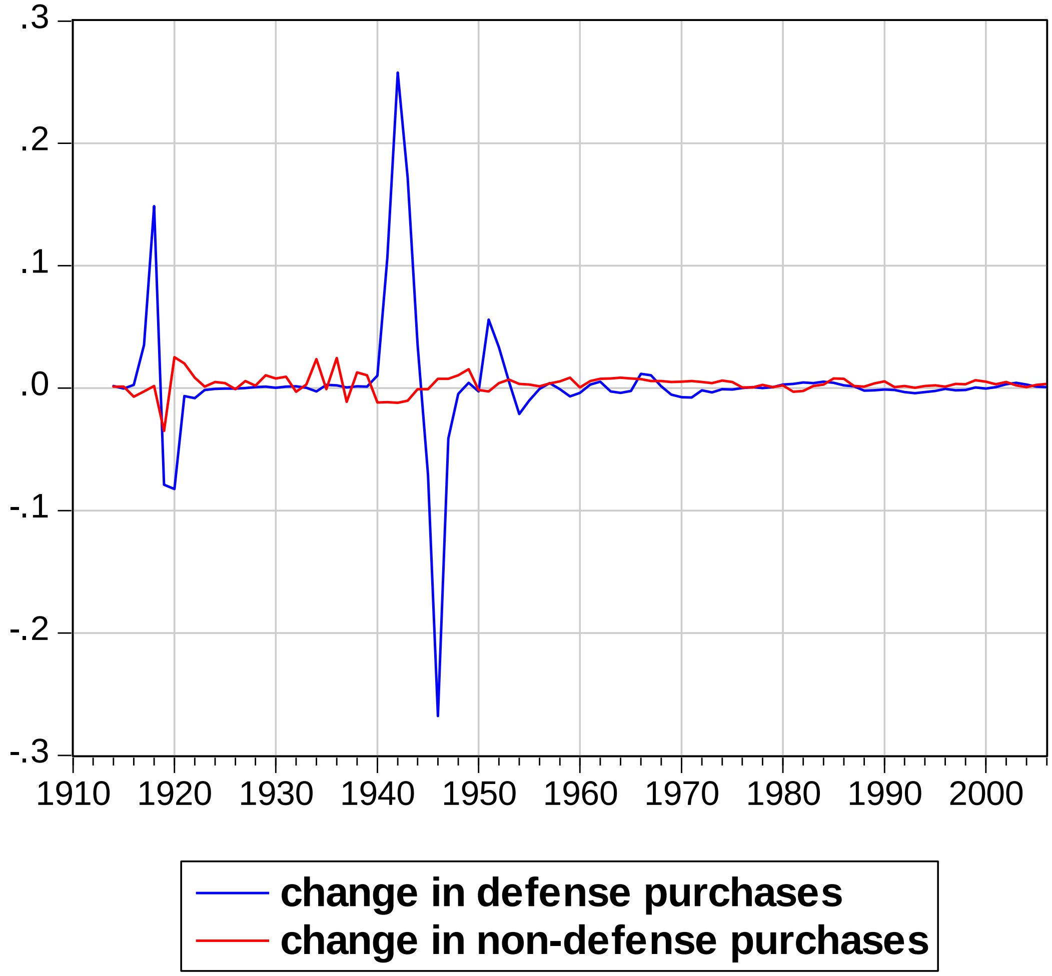

Figure 1

Changes in Defense and Non-Defense Government Purchases, 1914-2006

(expressed as ratios to the previous year's GDP)

Note: The figure shows the change in per capita real government purchases (nominal purchases divided by the GDP deflator), expressed as a ratio to the prior year's per capita real GDP. The blue graph is for defense purchases, and the red graph is for non-defense purchases by all levels of government. The government purchases data are from Bureau of Economic Analysis since 1929 and, before that, from Kendrick (1961). The GDP data are described at http://www.economics.harvard.edu/faculty/barro/data_sets_barro

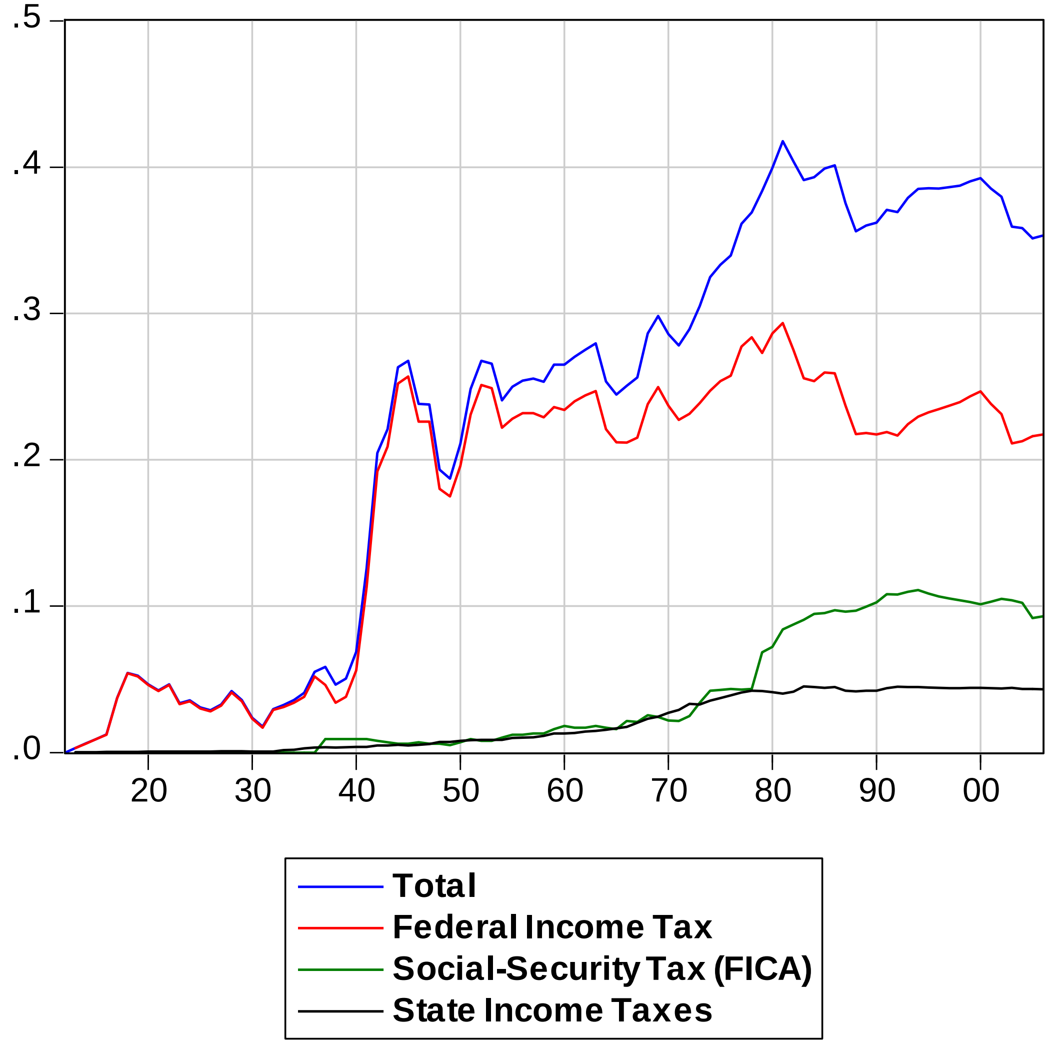

Figure 2

Average Marginal Income-Tax Rates, 1912-2006

Note: The red graph is for the federal individual income tax, the green graph is for the social-security payroll tax (FICA), and the black graph is for state income taxes. The blue graph is the total average marginal income-tax rate. The data are from Table 1.