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DEFENSE SPENDING AND THE ECONOMY

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ABSTRACT

While the impact of across-the-board federal defense-spending cuts on national security may be up for debate, claims of these cuts' dire impact on the economy and jobs are grossly overblown. The nation's experience with much larger defense-spending drawdowns—including those following World War II and the end of the Cold War—is that they do not result in predicted economic declines. This study surveys existing research on the "multiplier effect" of an extra dollar of federal spending on GDP to examine the economic impact of changes in federal defense spending. We find that a dollar increase in federal defense spending results in a less-than-a-dollar increase in GDP when the spending increase is deficit-financed. Combining this with a tax multiplier that is negative and greater than one, we estimate that over five years each $1 in federal defense-spending cuts will increase private spending by roughly $1.30.

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In 1943, Keynesian economist Paul Samuelson predicted the economic effects from reduced defense spending and the reintegration of 10 million servicemen into the civilian labor force after World War II. According to Samuelson (1943):

> When this war comes to an end, more than one out of every two workers will depend directly or indirectly upon military orders. We shall have some 10 million service men to throw on the labor market. We shall have to face a difficult reconversion period during which current goods cannot be produced and layoffs will be great. Nor will the technical necessity for reconversion necessarily generate much investment outlays in the critical period under discussion, whatever its later potentialities. The final conclusion to be drawn from our experience at the end of the war is inescapable—*were the war to end suddenly within the next 6 months, were we again planning to wind up our war effort in the greatest haste, to demobilize our armed forces, to liquidate price controls, to shift from astronomical deficits to even the large deficits of the thirties—then there would be ushered in the greatest period of unemployment and industrial dislocation which any economy has ever faced.* [Italics in original.]

Samuelson went on to suggest that this weak economic outcome could be avoided if the government maintained wartime price controls; if it implemented "income maintenance in the form of dismissal pay for soldiers, unemployment compensation, direct and work relief expenditure"; and if it engaged in large-scale public works.

As we know now, Samuelson's dire economic predictions never came to pass. As Henderson (2010) notes, despite plunging war production and massive discharges of soldiers, the government offered no dismissal pay for soldiers (although the G.I. bill helped many veterans go to college), it removed direct controls on the private economy fairly quickly, and it did not implement any large-scale public-works programs. Henderson (2010) writes:

> Between FY1945 and FY1947, federal government spending was cut by 61 percent. This was a 27-percentage-point drop from 41.9 percent of GDP to 14.7 percent of GDP. Yet the unemployment rate over that same time rose from 1.9 percent to only 3.6 percent. The postwar bust that so many Keynesians expected to happen never did.

Today the question of how defense cuts would impact the economy is back at the center of the political and economic debate. As required by the Budget Control Act of 2011, the federal government is scheduled to cut $1.2 trillion from its current baseline over the next nine fiscal years starting in March 2013.[1] The automatic reductions in spending, through a process called sequestration, are to be divided equally between discretionary defense and nondefense spending categories. These scheduled cuts are much smaller compared to GDP than were the vast reductions in military spending after World War II. Thus, projections of economic doom from the current cuts should be viewed skeptically from the perspective of Samuelson's failed prediction of economic disaster due to the much larger post-WWII demobilization.

The estimated economic impacts from cuts in defense or other federal spending depend on assumptions about the macroeconomic impact of government spending and taxes. A central element is the spending multiplier for defense and other government purchases. In later sections, we survey and use the main empirical evidence about the sizes of these multipliers.

The spending multiplier is defined as the effect of an extra dollar of defense or other government purchases on total economic output, gauged by real GDP. Specifically, we can think of the multiplier as it applies to deficit finance, where current taxes do not change when government spending rises. If the spending multiplier is positive and greater than one, the increase in GDP is so large that private-sector portions of GDP (notably personal consumer expenditure and private domestic investment) rise when government purchases go up. If the multiplier is positive but less than one, GDP rises, but not by enough to maintain the private-sector portions of GDP, which are crowded out when government purchases increase. Finally, if the multiplier is negative, GDP declines, and the private-sector portions of GDP must fall by more than the expansion of government purchases.

A common view is that the defense-spending multiplier is large and, hence, a reduction in defense outlays has not only a direct negative effect on military contractors but also major, harmful secondary effects on contractors' clients, on services that cater to defense-sector workers, and so on.[2] This argument can be made for any type of government spending. The missing part of these kinds of analyses is that the

1. See Rumbaugh (2013). The original deadline for the cuts was January 1, 2013, but the fiscal cliff deal enacted on January 2 postponed that deadline to March 2, 2013.
2. See, for example, Fuller (2012) and National Association of Manufacturers (2012).

resources no longer used for defense or other public purposes become available to private businesses and households throughout the economy.

Measuring the direct effects of government programs on production and employment is comparatively easy.[3] But tracing out the exact channels by which the private sector uses the resources no longer absorbed by the government to expand its private production and employment is impossible. We know, however, from the failed Samuelson prediction cited earlier that these indirect effects were dominant in the military demobilization after World War II. More generally, we can assess these effects on the rest of the economy by estimating economy-wide spending multipliers for defense and other government purchases. The key issue is not how government outlays can have beneficial direct and indirect effects, but whether these economy-wide spending multipliers are greater than one, positive but less than one, or negative.

EMPIRICAL STUDIES OF SPENDING MULTIPLIERS

Many economists have used economic data to infer the effect of an increase in some category of government spending on output, typically gauged by real GDP. In the background, taxes are sometimes held constant, as in cases of deficit finance or of funding of one state's spending by levies on residents of other states. The analysis may also consider responses of output in the short run (perhaps within a year) or the long run and may distinguish changes in government spending that are viewed as temporary versus permanent.

In all these cases, a key empirical challenge is to distinguish the effects of government spending on output from reverse causation (government spending responding to output) and from common influences of other variables (such as a natural disaster or war affecting both government spending and output). As an example, it is well known that US state and local governments tend to increase many forms of purchases when the economy is doing well and to curtail these purchases when the economy is doing badly. That is, state and local government purchases tend to be *procyclical*. Ignoring this tendency can lead to high estimated spending multipliers that are not meaningful. Similarly, governments tend to increase some transfers—such as unemployment insurance, food stamps, child welfare, and disability payments—when the economy is doing badly. That is, these types of government spending tend to be *countercyclical*. Failure to consider this tendency can lead to estimated spending multipliers that are misleadingly low and even negative.

Because of the seriousness of these estimation issues, we consider in table 1 (see page 16) only empirical studies that make a serious attempt at *identification*,

3. For example, recovery.gov, the US government's website for tracking stimulus spending, attempts to measure these kinds of direct effects of government spending on economic activity.

or distinguishing the effect of government spending on output from the reverse effect.[4] The studies considered fall into three broad areas: aggregate defense spending (primarily for the United States), the timing of loan disbursements from the World Bank, and differential effects of federal spending programs across the US states.

A familiar claim is that the US economy finally extricated itself from the Great Depression of the 1930s only because of the expansionary influence from the vast public expenditures on defense during World War II.[5] The more general notion is that expansions (or contractions) of defense purchases tend to raise (or lower) real GDP through a multiplier process. Many economists have used the US data on defense outlays to quantify this spending multiplier. The general idea is that these kinds of multipliers can be estimated more reliably than those applicable to other forms of government spending. These estimates would then apply directly to changes in defense spending but perhaps also to cases—such as changes in nondefense government purchases—that do not involve the defense budget.

For estimation purposes, a useful feature of defense outlays is that the major variations—especially those driven by war and peace—can be treated as nearly independent of economic conditions. This property means that reverse causation is unlikely to be an important problem. Other desirable properties from the standpoint of scientific inference are that the fluctuations in government purchases associated with war and peace are very large and involve increases (in the buildup of a war) as well as decreases (in subsequent demobilizations).

On the downside from a scientific perspective, wars entail major destruction of property and persons. In fact, the world history for the 20th century shows that many of the worst macroeconomic disasters relate to World Wars I and II.[6] In these cases, dramatic expansions of military outlays are associated with sharp declines in real GDP—not because of a negative spending multiplier, but because of contractions on the "supply side." This consideration means that good evidence from war and peace on spending multipliers can be garnered only from cases in which large changes in defense spending occurred but where destruction of domestic capital and labor was comparatively minor. The US experiences in World Wars I

4. In particular, we do not consider a number of studies that follow Blanchard and Perotti (2002) in assuming that contemporaneous relations between overall government spending and real GDP reflect entirely the influence of the former on the latter. Barro and Redlick (2011, table 3) show that this assumption can generate a misleadingly high multiplier for nondefense government purchases—around 2.6—in the period 1950–2006. This high estimate likely reflects the procyclical nature of this type of government spending, not the effect of government spending on output.

5. The data belie the common view that the US economy had barely recovered from the 1929–32 depression by World War II. From 1933 to 1940, real GDP grew by a remarkable 7 percent per year, despite the 1937–38 recession. This growth rate was the highest of any peacetime period of comparable length in US history at least back to 1800. This pattern illustrates the general tendency for recoveries to be stronger when the preceding contraction is larger.

6. See Barro and Ursúa (2008).

and II (and the Korean War) stand out here. Other promising case studies include Canada, Australia, and New Zealand, as well as the neutral countries Switzerland and Sweden, during the world wars.

Less serious issues related to the wartime data are the influences of the military draft, production mandates, and rationing, as well as possible shifts in the labor supply due to patriotism. The net effect of these forces on estimated defense-spending multipliers is ambiguous.

Empirical estimates of government spending multipliers based on US defense outlays are in the upper part of table 1. Barro (1984, table 13.2) finds that the increases in defense spending during World War I (peak spending in 1918), World War II (peak spending in 1944), and the Korean War (peak spending in 1952) were associated with increases in real GDP. However, the increase in real GDP was only about 60 percent of the rise in defense outlays; that is, the spending multipliers were around 0.6. Because real GDP rose by less than government purchases, other components of GDP were crowded out by 40 cents per dollar of additional defense spending. Empirically, the main reductions were in a broad measure of private domestic investment, including business nonresidential fixed investment and inventory accumulation, residential construction, and purchases of consumer durables. Hall (1986, 2009) finds similar spending multipliers using the US data on defense outlays for 1920–42 and 1947–82 and for 1930–2008, respectively.

Ramey (2011) uses a narrative approach based on articles in *Businessweek* and other publications to assess news about prospective changes in defense outlays. For example, in 1940 and 1950, information was widely available about coming expansions of defense spending during World War II and the Korean War. She assesses how these defense-news shocks showed up over time in changes in defense spending and real GDP from 1939 to 2008. By comparing the changes in GDP with the changes in defense outlays, she calculates spending multipliers of around 0.6 in the short run, cumulating to a peak of about 1.2 after two to three years. Because the analysis holds fixed the behavior of taxes, these spending multipliers apply to deficit-financed changes in defense purchases. The results also apply to spending changes that are anticipated to be long-lasting (as reflected in the defense-news variable).

Fisher and Peters (2010) use excess stock returns of defense contractors for 1947–2008 as an alternative to Ramey's defense-news shocks to get information about prospective shifts in defense outlays. However, this approach turns out to have much less explanatory power than the narrative approach for changes in defense spending, especially during the Korean War. Therefore, although the computed relation between changes in defense spending and changes in real GDP is positive—indicating a positive spending multiplier—it is not possible to use this evidence to get reliable estimates of the government-spending multiplier.

Barro and Redlick (2011) estimate spending multipliers for US macroeconomic data for 1917–2006. The key assumption is that variations in defense outlays—particularly those associated with war and peace—could be treated as exogenous. This

study uses the Ramey (2011) defense-news variable to distinguish defense spending that was viewed as temporary from defense spending that was viewed as largely permanent. (A permanent increase applies when actual and Ramey's anticipated future spending variable rise together.) Specifically, with the availability of the Ramey variable, they assess the consequences of the changes in expected future spending that occurred in 1940 and 1950, before significant increases in actual outlays occurred. (The anticipations of higher future spending tended to raise real GDP before any change occurred in actual defense spending.)

Barro and Redlick (2011, table 1) estimate spending multipliers of 0.4 to 0.5 within a year, rising to 0.6 to 0.7 over two years, and expanding further by 0.1 to 0.2 when the public viewed the changes as largely permanent. Because their analysis holds changes in taxes constant, the estimates apply to deficit-financed defense spending. The overall effect over two years from a rise in defense spending that was viewed as permanent—a multiplier of 0.7 to 0.9—is comparable to that found by Ramey (2011).

Barro and Redlick (2011) find that the estimated defense-spending multipliers applied to decreases in spending (such as in the demobilizations of 1946–47 and 1954–55) as much as to increases. Therefore, analysts can use the results to estimate the likely impact of prospective cuts in the defense budget for 2013 and later years.

Owyang, Ramey, and Zubairy (2013) extend the Ramey (2011) study to a longer time frame for the United States (1890–2010, thereby including World War I) and to Canada (1922–2011). The new research focuses on whether the size of the defense-spending multiplier depended on the amount of slack in the economy. Consistent with Barro and Redlick (2011), the US data show no significant linkage of the spending multiplier with the unemployment rate, even though the start of the defense buildup in 1941 coincided with an economy still in recession. However, Owyang, Ramey, and Zubairy (2013) find some indication from Canada that defense-spending multipliers were higher when the unemployment rate was higher. The Canadian data are particularly useful here because Canada's entry into World War II occurred in 1939, when the unemployment rate was higher than that at the time of US entry in 1941. Further analysis of the Canadian data, along with those for Australia and New Zealand and possibly Sweden and Switzerland, could help to pin down this finding.

The second part of table 1 refers to Kraay's (2012) study of the responses of real GDP in 29 developing countries to World Bank loan disbursements from 1985 to 2009. The World Bank loans are difficult to use directly to assess spending multipliers, because the amount and timing of loan approvals depend on each country's economic situation (so that reverse-causation was a serious issue). Kraay avoids this problem by considering the sizable variations in the timing of disbursements over several years following loan approvals. The basic idea is that much of this variation reflected arbitrary bureaucratic procedures and can, therefore, be regarded as essentially random. (Thus, random bureaucrats can sometimes be scientifically useful.) The analysis shows a positive response of real GDP within a year to

government expenditure driven by the timing of World Bank disbursements on loans approved in prior years. The estimated spending multiplier is between 0.5 and 0.7, within the ballpark of the estimates derived from the macroeconomic studies of defense spending.

The last part of table 1 concerns responses of real GDP and other variables in US states to government spending determined and financed by the federal government. The basic idea is that, under some circumstances, these kinds of changes in government spending at the state or local level can be viewed as independent of the state's economic conditions. Therefore, it is easier than usual to assess effects of government spending on the economy, rather than the reverse. A serious shortcoming, however, is that the computed spending multipliers cannot readily be applied to the national context. At the state level, federally financed expenditure is nearly free, not only currently (as would be sort of true for deficit-financed federal spending) but also prospectively. Therefore, the computed state spending multipliers do not include the income and substitution effects associated with the higher current and future taxes that apply at the federal level.

Nakamura and Steinsson (forthcoming) gauge the response of state real GDP and other variables to federal defense contracts applicable to each state. The key idea is that variations in aggregate defense spending occur differentially across states and in a way that is predictable from the history of defense contracts. For example, when federal defense spending rises, this spending tends to be allocated disproportionately (in relation to each state's GDP) to California and Connecticut rather than Illinois. This pattern has allowed Nakamura and Steinsson to isolate effects of federal defense spending within a state on the state's real GDP. Specifically, they filter out the possible reverse causation, whereby poor economic conditions in a state may raise that state's share of the overall defense budget.

The result for 1966–2006 is an estimated state spending multiplier around 1.4 over a two-year period.[7] This estimate is higher than that found at the national level by Barro and Redlick (2011), in which the comparable number over two years for deficit-financed spending expected to be permanent was 0.7 to 0.9. One reason that the state spending multiplier could be higher is that this spending is nearly free for the state where it occurs, coming from current or prospective taxes levied mostly on residents of other states. Another consideration is that the responses of a state's real GDP could reflect mobility of labor and capital from other states—an effect that would not operate substantially at the national level.

Cohen, Coval, and Malloy (2011) examine an array of federal spending programs (earmarks, transfers, and government contracts) in US states. They focus on the substantial response of these outlays to states' changing political power in Congress.

7. The estimated spending multiplier (Nakamura and Steinsson 2012, tables II and III) is sensitive to the statistical procedure. The simple relation between a state's added defense spending and its added GDP (without filtering for reverse causation) was close to zero. However, with a different method for ruling out reverse causation, the estimated spending multiplier was even higher, around 2.5.

They find over 1967 through 2008 that added state spending driven by a state's enhanced political power in Washington, DC, led to declines in investment and employment by corporations headquartered in the state. They also find declines in overall state real GDP and employment. Hence, their estimated state spending multipliers are negative.

Wilson (2012) and Chodorow-Reich et al. (2012) focus on effects across US states from spending under the American Recovery and Reinvestment Act (ARRA) in 2009–10. The key idea is that many forms of spending under the ARRA were driven by formulas that allocated funds to states in relative amounts that were predictable from conditions applying before the 2007–09 recession. For example, in 2009–10, states with more highways got a disproportionate share of highway spending, places with a large youth population got a disproportionate share of educational funding, and states with a history of many Medicaid recipients got a disproportionate share of Medicaid payments. Using these patterns, researchers can filter out the parts of ARRA spending that are driven by state economic conditions in 2009–10 and, hence, subject to the reverse-causation problem. Wilson (2012) covers all state spending under the ARRA except that by the Department of Labor, notably unemployment insurance. Chodorow-Reich et al. (2012) examine only outlays under the Medicaid program.

The main finding from both studies is that added ARRA spending in a state led to an increase in the state's total employment in 2009–10. Hence, the implied spending multipliers (not quantified in these studies) would be positive. However, as with Nakamura and Steinsson (forthcoming), this evidence relates to state spending that is financed by other states and is, therefore, nearly free. Also, it is unclear in Wilson (2012) and Chodorow-Reich et al. (2012) whether the patterns of state employment growth in 2009–10—essentially one year of observations across the states—depend directly on the factors used to predict ARRA spending (such as highway miles or prior Medicaid population), rather than on ARRA spending, per se. Thus, the statistical procedures are inherently less reliable than those used in Nakamura and Steinsson (forthcoming) and in Cohen, Coval, and Malloy (2011).

AGGREGATE EFFECTS OF THE 2009–10 FEDERAL STIMULUS PACKAGE

In an earlier analysis, Barro (2010) uses estimates of defense-spending multipliers of the type shown in table 1 to assess the macroeconomic consequences of the federal stimulus package of 2009–10 (the ARRA). Since that assessment looks good in retrospect, we report it here with only expositional changes.

The idea is that the US government spent roughly $300 billion (2.1 percent of GDP) extra in each of 2009 and 2010 on outlays whose macroeconomic effects could be gauged by empirical estimates of defense-spending multipliers. Since the stimulus spending was largely deficit financed, it is reasonable to use a spending multiplier of 0.4 within the current year and 0.6 over two years (as in Barro and Redlick

[2011], which is included in table 1). Therefore, the estimate was that the boost in GDP was $120 billion in 2009 and $180 billion in 2010 (0.9 percent and 1.2 percent of GDP, respectively). Since the multipliers are less than one, the heightened public outlay is estimated to reduce private-sector portions of GDP, notably private domestic investment and personal consumer expenditure. However, the short-term deal is favorable, because the added public outlays of $600 billion over two years come at a cost of $300 billion in private spending—that is, 50 cents on the dollar. Thus, we have to value each extra dollar of added public outlays at only 50 cents to make this part of the stimulus-spending program look attractive.

One assumption (unrealistic, as it turns out) in Barro (2010) is that the extra federal spending was temporary, so that spending would go back down by $300 billion as of 2011. In any event, the public debt had increased by $600 billion (neglecting some interest payments), and this debt had to be paid by raising taxes eventually (if government spending did not change after going back down by $300 billion). Barro (2010) works out an example in which taxes rose by $300 billion in 2011 and 2012 and then reverted to the initial level. While this assumption has proved unrealistic, the critical point is that taxes have to rise sometime.

Barro (2010) uses available estimates of "tax multipliers" to calculate the full effects of the ARRA spending on real GDP. Specifically, the findings of Romer and Romer (2010) and Barro and Redlick (2011) suggest tax multipliers with a one-year lag around −1.1; that is, GDP falls the next year by $1.10 for each increase in federal taxes by $1.00.[8] Adding this tax effect to those effects already described results in a full "five-year fiscal plan" for 2009–13.

The path of incremental government outlays in billions of dollars over the five years was +300, +300, 0, 0, 0, which adds to +600. The path of estimated effects on real GDP (compared to a baseline) was +120, +180, +60, −330, −330, which adds to −300. Real GDP falls overall because the "balanced-budget multiplier" is negative,[9] given that the government-spending multiplier is between 0.4 and 0.6 and the tax multiplier is −1.1. The sequence of effects on the private-sector portions of GDP was −180, −120, +60, −330, −330, which sums to −900. Thus, viewed over five years, the stimulus package of 2009 was a way to get an extra $600 billion of public spending at a cost of $900 billion in private spending—probably not an attractive deal.

We can, of course, change some of the assumptions. We could make permanent the added government spending or delay further the tax increases needed to finance the added public debt. But a similar message would emerge: more government

8. Romer and Romer (2010) used a narrative approach to isolate exogenous changes in average federal tax rates. Barro and Redlick (2011) considered an average marginal income-tax rate. The computation of a tax multiplier then relied on the historical relation between changes at the federal level in average marginal income-tax rates and average tax rates.

9. The balanced-budget multiplier is the effect on GDP when government spending and taxes rise together by $1.00.

outlay accompanied eventually by correspondingly higher taxes leads in the long run to lower real GDP. This result accords with findings on the determinants of long-term economic growth for a large panel of countries: the effect on the growth rate of real GDP from a higher ratio of government consumption to GDP tends to be negative.[10]

AGGREGATE EFFECTS FROM CUTBACKS IN DEFENSE SPENDING

The analysis from the previous section can be applied with the signs reversed to current and prospective cutbacks in federal spending, including the effects of sequestration in 2013 on the defense budget. Recall that a crucial point is that the estimated defense-spending multipliers in Barro and Redlick (2011) apply to reductions in government spending as well as to increases.

For present purposes, we treat sequestration as a cut, starting in 2013, of 5 percent in defense outlays and 5 percent in various categories of other federal spending. We consider here only the effects on real GDP from the defense-spending cuts. The other spending reductions—some in transfers to persons or state and local governments and some in federal purchases—could be added to the analysis.[11] Since real defense spending in 2012 (in 2005 dollars) was $677 billion, a 5 percent cut from that benchmark reduces real outlays by $34 billion per year, starting in 2013.

For given taxes and other federal spending, the defense spending cut lowers the federal deficit. Hence, the public debt is lower than otherwise, and this reduction means that, in the long run, taxes will decrease correspondingly when compared to a benchmark path (if other federal spending does not change). We can make various assumptions about the timing of the decreases in taxes. We assume here that federal taxes decline (relative to the benchmark path) by $34 billion per year starting in 2013, thereby paralleling the reductions in defense outlays. Similar long-run conclusions emerge if the government delays the tax decreases.[12]

As before, we use a defense-spending multiplier of 0.4 within a year and 0.6 over two years. We assume, also as before, that taxes have a multiplier effect on GDP of −1.1 with a one-year lag. These assumptions imply that real GDP falls compared to the benchmark path by $13.6 billion in 2013 (because of the spending multiplier) but rises by $17 billion in 2014 (because the effect from the tax multiplier more than offsets the spending effect). Private-sector portions of GDP rise by $20.4 billion in 2013 (60 cents on the dollar compared to the spending cut) and $51 billion in 2014 (because GDP is now above its benchmark).

10. See, for example, Barro (2013, table 1).

11. However, we lack reliable estimates of the effects on real GDP from a reduction in federal transfers to persons.

12. If the government budget constraint is expected to hold, then an increase in government spending today means an increase in taxes today or tomorrow, and a spending cut today means lower taxes today or tomorrow. This perspective relates to the notion of "Ricardian Equivalence," described in Barro (1974).

The effect of +$17 billion on real GDP continues into each future year. Therefore, if one computes changes over five years (out to 2017), the overall effect, relative to the benchmark path, is a reduction in defense spending by $170 billion, a cut in taxes also by $170 billion, an increase in real GDP by $54 billion, and a rise in private sector portions of GDP by $224 billion. In other words, over five years, we get roughly $1.30 of extra private spending for each $1.00 reduction in defense spending. Whether this exchange is a good deal depends on how much society values defense spending as a contributor to national security. It is this kind of economic and political calculation—and not the Keynesian vision described at the beginning of this paper—that dictates how large the defense budget and other parts of government spending should be.

FINAL THOUGHTS ABOUT DEFENSE-SPENDING CUTS

The question of whether defense-spending cuts will hurt the economy is at the forefront of the 2013 debate about the sequester. For plausible estimates of short-term spending multipliers—positive but less than one—the adverse effects on real GDP will be minor even in the short run. Moreover, with multipliers below one, private-sector portions of GDP—personal consumer expenditure and private domestic investment—will rise at least modestly. In the longer run, when reduced public debt and taxes (compared to a benchmark path) are factored in, real GDP should be higher than otherwise. This conclusion is consistent with findings that a smaller share of government consumption in GDP tends to enhance long-term economic growth.

Our conclusions are consistent with the historical pattern in which the US economy responded well to much larger defense cuts. Particularly compelling is the economy's strong performance after the massive post-WWII demobilization. But a similar pattern applies to more recent defense cutbacks. From 1987 to 2000, under the first Bush administration and the Clinton administration, the share of defense spending in GDP fell from 7.4 percent of GDP to 3.7 percent. The average growth rate of real GDP over this period was a respectable 3.3 percent per year, despite the 1991 recession.

There may be grounds for objecting to defense cuts based on reasoned arguments that these spending reductions would impair national security. But Keynesian arguments that a smaller defense budget will retard economic growth are not convincing.

TABLE 1. ESTIMATES OF SPENDING MULTIPLIERS

Author and Study	Multiplier estimate	Notes
Time-Series Macroeconomic Studies Based on Defense Spending		
Barro (1984)	≈ 0.6	US defense spending increases in WWI, WWII, Korean War
Hall (1986)	≈ 0.6	US defense spending, 1920–42, 1947–82
Ramey (2011)	0.6–1.2	US defense spending, 1939–2008, estimates based on defense-news variable, short-run versus long-run, deficit-financed
Fisher and Peters (2010)	> 0	US defense spending, 1948–2007, estimates based on stock returns of defense contractors, cumulative effects over five years for 1959–2007
Barro and Redlick (2011)	0.4–0.8	US defense spending, 1917–2006, short-run versus long-run, temporary versus permanent (based on defense news), deficit-financed, applies to increases or decreases
Hall (2009)	≈ 0.5	US defense spending, 1930–2008
Owyang, Ramey, and Zubairy (2013)	≈ 0.6–0.9	US (1890–2010) and Canada (1922–2011) defense spending, based on defense news, short-run versus long-run, deficit-financed, interactions with unemployment rate
A Panel Study Based on the Timing of Loan Disbursements from the World Bank		
Kraay (2012)	0.5–0.7	Uses timing of World Bank loan disbursements to 29 developing countries, 1985–2009, short-run
Panel Studies for US States		
Nakamura and Steinsson (forthcoming)	≈ 1.4	US defense spending across US states, 1966–2006, responses of state real GDP over two years
Cohen, Coval, and Malloy (2011)	< 0	Federal spending in US states, driven by states' political power in US Congress, effects on corporate investment and employment and on state GDP and total employment, 1967–2008
Wilson (2012)	> 0	ARRA cross-US state spending except for UI, 2009–10, effects on state employment
Chodorow-Reich et al. (2012)	> 0	ARRA cross-US state spending on Medicaid, 2009–10, effects on state employment

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