

The Fall and Rise of Keynesian Fiscal Policy

Alan J. Auerbach[†]
University of California, Berkeley

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[†] Correspondence: Alan J. Auerbach, Department of Economics, University of California, Berkeley, CA 94720-3880, USA. Email: auerbach@econ.berkeley.edu. Phone: (510) 643-0711. Fax: (510) 643-0413.

Abstract

This paper reviews the recent evolution of thinking and evidence regarding the effectiveness of activist fiscal policy, including how policy multipliers might vary with respect to economic conditions. Like many other countries that were hit by the “Great Recession,” the United States responded initially with active fiscal policy measures. But a more positive view of fiscal intervention appears to have developed earlier in the decade, and estimated decision rules confirm that there was an increase in policy activism. While this positive view has been tested by the unclear effects of fiscal policy during the Great Recession, recent evidence does suggest that fiscal policy may be especially effective in recession. Fiscal policy activism has also been tempered by recent concerns about growing government debt, a development which potentially might also undercut the effectiveness of expansionary fiscal policy.

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I. Introduction

Like many other countries that were hit by the recent “Great Recession,” the United States responded initially with active fiscal policy measures. U.S. actions actually began early in the recession (which was dated by the NBER to have begun in December, 2007) with temporary tax cuts enacted in the last year of the Bush Administration, in February 2008, followed by a first-time homebuyers’ tax credit enacted in July 2008. The largest single action in the United States, adopted in February 2009 shortly after the accession of President Obama, was the American Recovery and Reinvestment Tax Act (ARRA). The ARRA was a combination of tax cuts, transfers to individuals and states, and government purchases estimated to have increased budget deficits by a cumulative amount equal to 5.5 percent of one year’s GDP over a period of a few years.

Smaller attempts at fiscal stimulus continued thereafter with more targeted measures, notably the temporary “cash for clunkers” program in summer 2009 aimed at stimulating the replacement of old cars with new ones, and an extension and expansion of the homebuyers tax credit in November 2009 and July 2010. Accompanying these fiscal efforts were the Troubled Asset Relief Program (TARP), enacted in fall 2008 to address the financial crisis, and a continuing array of interventions by the Federal Reserve Board that aimed to stabilize credit markets and stimulate the economy. Finally, at the end of 2010, a deal between the President and Congress led to a one-year reduction of 2 percentage points in the social security payroll tax, with this tax cut being extended further in late 2011 and again early 2012.

Many other countries also pursued active fiscal measures during this period. The prevalence of fiscal policy interventions reflects the severity of the recession and also perhaps at least a temporary optimism with regard to the potential effectiveness of activist fiscal policy.

Yet the variety of approaches adopted suggests uncertainty about which approaches might have been most effective, and the overall perspective on the effectiveness of fiscal policy as a tool for stabilization is still debated, even as the focus in many countries, including the United States, has begun to shift away from fiscal stimulus to fiscal balance, with another controversial view, that fiscal *contractions* may in certain circumstances stimulate economic activity, coming in conflict with the traditional Keynesian perspective.

This paper reviews the recent evolution of thinking and evidence regarding the effectiveness of activist fiscal policy, including how policy multipliers might vary with respect to economic conditions, such as the very low interest rates that were central features of the Great Recession.

II. The Evolution of Theory and Policy

From its heyday several decades ago, discretionary fiscal policy had until relatively recently come to be viewed with considerable skepticism. Those studying economics would start with the classical argument against short-term policy interventions -- the lags in the making of economic policy and further lags in the implementation and effects after the policy is enacted, which make it difficult for policymakers to time fiscal policy actions to stabilize the economy. Added to the problem of policy lags are the difficulties posed by policy uncertainty (Brainard, 1967) and the devastating Lucas (1976) critique, which implies that a policy's stabilizing effects can be undercut by the expectations and actions of rational agents who observe the government's policy process. For example, investment might actually drop more during a recession in anticipation of a countercyclical investment incentive to be enacted in the near future; consumption might not respond much to a countercyclical reduction in income taxes, as the wealth effects of such tax reductions are small when the reductions are seen as temporary.

Indeed, even if such tax reductions are of longer duration, concerns about future generations could still neutralize wealth effects on consumption, as explicated by the notion of Ricardian equivalence (Barro, 1974). Finally, with the automatic stabilizers already built into the government's tax and transfer systems, fiscal policy could be used even without active intervention. Moreover, the growing independence of central banks and advances in monetary theory and practice had strengthened confidence in the use of interest-rate interventions as tool not only for controlling inflation but also for the stabilization of economic fluctuations.

These arguments against activist fiscal policy clearly saw their limits as the Great Recession hit, with output dropping sharply even with the large increases in deficits associated with automatic stabilizers, and with monetary policy options curtailed as short-term interest rates approached zero, a problem that had recently been confronted in Japan but elsewhere had not been seen in several decades. But, at least in the United States, a more positive view of fiscal intervention appears to have developed earlier, even before the Great Recession. A brief review of actions taken during the past four recessions serves to illustrate the evolution of U.S. countercyclical policy.

In August 1982, after a year of deep recession that still had several months left to run, Congress passed the Tax Equity and Fiscal Responsibility Act (TEFRA), scaling back the large Reagan tax cuts that had been enacted just over one year earlier. Legislation over the same period cut near-term federal spending. During the next U.S. recession, in October 1990, a budget summit meeting of the first President Bush and Congressional leaders produced legislation aimed at reducing the deficit. Thus, in 1982 and 1990, policymakers chose to impose fiscal discipline during a recession.

This pattern changed in the 2000s, but well before the Great Recession. In 2001, as concerns about a recession developed, Congress added a set of cash rebates to the original set of tax cuts proposed by President Bush, in order to help stimulate the economy in the short run. In early 2002, in response to the 2001 recession that was not then known to have ended, Congress introduced “bonus depreciation,” the first use of countercyclical investment incentives since the 1970s. In 2003, further individual tax rebates were enacted, as part of a package that focused mainly on other changes. Finally, the first round of fiscal stimulus during the Great Recession, as already mentioned above, was adopted early in 2008, just two months after the turning point and at a time when few economic forecasts predicted a deep recession.

Simple statistical analysis confirms the evolution of policy responses that the preceding narrative provides. As in several earlier papers, most recently Auerbach and Gale (2009a), we measure discretionary changes in fiscal policy using actual changes in legislation, as reported by the Congressional Budget Office (CBO), rather than relying, for example, on the more popularly used measure based on changes in the full-employment surplus. While the latter does control for automatic stabilizers by focusing on the surplus at full employment, it fails to control for other factors that might cause the budget surplus to change, such as a change in the income distribution or in the composition of income, both factors that in the past have caused important fluctuations in the U.S. budget surplus.¹

The data on policy changes for revenues, outlays (excluding interest) and their difference, the primary surplus, come from the updates that CBO provides for its baseline revenue and expenditure forecasts for the federal budget, covering the current fiscal year and several future fiscal years. With each update, CBO estimates changes in projected revenues and expenditures due to policy actions. Using these updates, we derive a roughly semiannual series of projected

changes in revenues and expenditures. Continuous data from CBO forecast revisions are available since summer, 1984, with the last complete observation being in the winter of 2012.

For each observation, as before, we measure the policy change in question (revenue, outlays net of interest, or the primary surplus) as the discounted sum of annual policy changes adopted during the interval for the current and subsequent four fiscal years (relative to each year's corresponding measure of potential GDP, as estimated by CBO), with the five weights normalized to sum to 1.² The discount factor is 0.5, meaning that each succeeding fiscal year's policy change is accorded half the weight of the previous one.³

Figure 1 displays the resulting series for changes in revenues and outlays. From the figure, it appears that discretionary policy was very active during the 1980s, very quiet during the Clinton administration, especially during the mid-1990s, and extremely active since then. But this figure does not account for changes in the factors driving policy, that is, whether the changes in activity are attributable to changes in the willingness to use discretionary policy or simply to changes in the perceived need to use policy interventions. To distinguish between these two possible explanations, we estimate simple policy reaction functions. The main explanatory variables are the projected annual budget surpluses over the same five-year budget period, weighted using the same discounting process as that used to construct the dependent variables, and the lagged value of the full-employment GDP gap from the prior quarter, as calculated by CBO, with both variables also scaled by potential GDP. The former variable is included to account for the likely response to budgetary conditions; the latter variable is included to determine the countercyclical policy response. The predicted coefficients of both variables are negative in explaining revenues and the primary surplus, and positive in explaining outlays. The coefficients' absolute values measure the strength of the associated responses.

Table 1 reports the results of this analysis. The first three columns of Table 1 show the estimates for the full sample period, with revenues, non-interest outlays, and the primary surplus, respectively, as the dependent variables. Over the full sample period, both the GDP gap and the budget surplus exert a significant, negative impact on surplus-enhancing policy actions, indicating that policy has been both countercyclical in timing and responsive to budgetary conditions. Both revenues and outlays respond in a consistent manner. Outlays account for a greater share of the overall response, although the differences in coefficients between outlays and revenues are small and not significant. The results indicate that policy changes adopted in a representative six-month period counter slightly over one sixth of the GDP gap and a similar fraction of the projected budget surplus. The fourth column of Table 1 shows the effect of including the lagged ratio of outstanding national debt to potential GDP in the regression. The debt has the correct sign (higher levels of debt leading to policies that increase the budget surplus). However, this variable is not significant for the subsample periods yet to be considered, and on its own performs worse in the regressions than the projected surplus variable, so we exclude it from further analysis.⁴

Figure 2 repeats the series of policy changes shown in Figure 1, but now accompanied by the predicted policy changes based on the full-sample estimates for revenues and outlays in the first two columns of Table 1. As the figure shows, some of the changes in the level of activity, from active, to quiet, to very active, are attributable to changes in the underlying forces driving policy. For example, the renewed strength of policy responses after 2000 is consistent with the strength of underlying forces, in particular a weakened economy and a stronger budget position, both of which led to expansionary fiscal measures. That is, even assuming no change in policy reaction functions, we should have expected increased activism in the early 2000s.

But there is more to the recent resurgence of activity, as one sees by estimating the reaction functions separately for different time periods. The fourth, fifth and sixth columns of Table 1 present evidence for the primary surplus for three sample periods of roughly equal length. The first includes the presidencies of Ronald Reagan and George H. W. Bush, the second that of Bill Clinton, and the third that of George W. Bush plus the first months of the Obama Administration, thereby including the passage of the large stimulus measures during the Great Recession.⁵

There is no evidence from a comparison of results for the first two sub-periods for an increasing reliance on discretionary fiscal policy, as measured by the coefficient on the output gap. Indeed, the coefficient declines in absolute value between the first and second sub-period, as does the coefficient on the budget surplus, which indicates a stronger response to budget conditions in the earlier period. In the third sub-period, however, the strength of the countercyclical response grows sharply, indicating that the renewed activism of the period is due not only to stronger underlying forces, but also greater responsiveness. Interestingly, although this period was viewed as one of weak budget control, the impact of primary surpluses on policy decisions is slightly stronger than in the 1990s.

The final column of Table 1 extends the final sub-period to include the five observations since mid-2009. The impact of the budget surplus is little changed, but that of the output gap is reduced. The large standard errors associated with these coefficients leave us unable to come to any strong conclusions, but these last estimates do call into question whether the cycle of activist fiscal policy has already peaked, an issue to which we will return. While the very recent period included a large tax cut (when President Obama and Congress agreed to a temporary reduction in the payroll tax), it also included the August, 2011 budget agreement (arrived at in conjunction

with Congressional approval of an increase in the debt ceiling), which cut spending even as the model predicted an increase. Thus, although policy activism has continued to the present, the extent to which this activism is of a countercyclical nature is less certain. Indeed, there has been a tension throughout the recession between concerns about promoting economic activity and about taking measures to ensure fiscal balance, and the large deficits experienced during the recession have no doubt contributed to an increased focus on the latter concern. Contributing to this shift in emphasis have been two arguments; first, that the stimulus measures undertaken in the United States were ineffective, or at least less effective than was hoped or expected; second, that large budget deficits weaken the output effects of fiscal expansion, and indeed that fiscal consolidation may actually promote increased economic activity. While US fiscal policy has not experienced a sustained shift from expansionary to contractionary, other countries, notably the United Kingdom, have eschewed further fiscal stimulus and adopted policies aimed at significant cuts in spending and the budget deficit.

What role has economic theory and evidence played in promoting this evolution in fiscal policy, toward countercyclical activism and perhaps then more in the direction of fiscal consolidation? I review three segments of the literature to address this question, looking at general theory and evidence regarding the strength of fiscal policy multipliers, specific evidence on the impact of stimulus measures during the Great Recession, and contributions on the effectiveness of fiscal consolidation, in particular the conditions under which fiscal consolidation might be expansionary.

III. Fiscal Policy Multipliers

There is a considerable literature examining the effects of fiscal policy on different components of economic activity, in particular household consumption and business fixed

investment.⁶ But this literature does not directly inform us about the overall effects of fiscal policy, because it does not consider the indirect effects on other sectors of the economy. For the purpose of assessing the overall effectiveness of countercyclical policy, there have generally been three types of models used, with differing strengths and weaknesses: large-scale macroeconomic models, structural vector autoregressions (SVARs) and other simple time series approaches, and dynamic stochastic general equilibrium (DSGE) models.

Large-scale macroeconomic models account for relevant prices and quantities in different sectors of the economy, and relate these prices and quantities to each other and to government policy variables. While large-scale models provide considerable detail regarding the channels through which policy can operate, and are commonly used by government forecasters, their theoretical grounding has been challenged based on the argument that the structural equations describing the behavior of households and firms lack adequate micro-foundations. Of the three types of models, large-scale macro models often produce the largest multipliers, as will be discussed further when we address the literature on the effects of ARRA.

SVAR and DSGE approaches, on the other hand, have been the principal ones found in the recent academic literature. They represent alternative responses to the criticisms of large scale models, with DSGE models hewing more closely to micro-foundations, while SVARs moving away from attempts to establish strong structural restrictions.

In a standard vector autoregression, a vector of variables – say, output, taxes, and government purchases – is regressed on lagged values of the same variables. Because there is no specification of the channels through which policies affect output, it is not possible to separate the response of output to policy from the response of policy to output. In a structural vector autoregression, a limited structure is provided in the form of assumptions about the recursive

structure of the error matrix – that is, about the order in which shocks to policies and output occur. This makes it possible to identify the changes in current policy variables that are attributable to actual changes in policy rather than to endogenous responses to economic conditions. The key issue in this literature is the method used to identify “true” policy changes in attempting to obtain persuasive multiplier estimates.

An important early contribution in the SVAR literature, by Blanchard and Perotti (2002), provides estimates of multipliers for both government purchases and taxes, using the identifying assumption that these variables could respond to output within a quarter (the period of observation) only through automatic provisions, not discretionary policy. Thus, controlling for such automatic response, which could be estimated directly, the fiscal shocks within a period could be treated as exogenous. Based on such a methodology, Blanchard and Perotti estimate a GDP multiplier for government purchases of about 0.5 after one year, with longer-term multipliers depending on model specification due to differences in the estimated permanence of policies. That is, the short-term multipliers imply a net crowding-out of components of GDP other than the government purchases themselves. Estimates of tax cut multipliers are slightly larger, closer to 1.0 after one year.

As noted, a central concern with the SVAR approach is the identification of policy shocks. A change in taxes or spending identified by the Blanchard and Perotti (2002) methodology as a policy shock might have been anticipated by individuals (even if not by the econometric model), or it might not have been a policy change at all (for example, it might be due to other factors such as a change in the income distribution). Thus, one line of research extending the basic SVAR approach has been to identify policy changes through a narrative approach, applying additional information on policy decisions to help identify exogenous policy

changes, rather than treating as exogenous surprises those changes not predicted by the SVAR itself.

Using military spending build-ups as an important source of variation in government purchases that is exogenous with respect to economic activity, Ramey and Shapiro (1997) estimate the effect of these build-ups on GDP and its other components. More recently, Ramey (2011a) provides a more complete set of data on such shocks and emphasizes the importance of distinguishing the announcement dates of policy changes from their dates of implementation. Using such a series based on actual policy announcements, she estimates an output multiplier after four quarters of about 0.7. As noted above, one implication of a multiplier below 1.0 for government purchases is that other components of GDP fall in response to the increase in government purchases. Surveying the available evidence on the effects of government purchases, including the studies mentioned above as well as several others, Ramey (2011b) concludes the multiplier for a temporary, deficit-financed increase in government purchases is probably with the range (0.8, 1.5), although values below and above this range cannot be ruled out.⁷

On the tax side, the narrative approach to identifying policy shocks has been introduced by Romer and Romer (2010), who used the same method in earlier analysis identifying monetary policy shocks. They argue that the multipliers of tax changes estimated using other approaches are likely to underestimate tax policy multipliers by treating as exogenous many policy changes that were actually responding to economic conditions or government purchases. Using their narrative approach to identify policy changes that were arguably independent of such other factors, they find a GDP tax-cut multiplier of about 1.0 after four quarters rising to 3.0 after 10

quarters. This very large multiplier is associated with an enormous impact on investment. The result is striking, and clearly merits further investigation.

Although the narrative approach may yield better estimates of true policy surprises than the standard SVAR approach, both approaches are limited in certain critical respects stemming from the reduced-form nature of these models. First, the models cannot be used to examine the economy's responses to automatic stabilizers or to any already-operating rules that relate activist fiscal policy to economic conditions, since effects of both types are already incorporated in the model's estimated impulse responses. Second, these models can measure only the multipliers of policies that deviated from standard policy responses to economic conditions within the sample period and can only estimate the effects of those policies as they were actually adopted. For example, if shocks to government purchases or taxes tended to be short-lived, then we cannot draw direct inferences about the effects of more permanent shocks. New tax changes differing in composition from those examined in-sample could well have different multipliers than those estimated. This concern is especially important under the narrative approach, in light of the fact that most of the estimates of the effects of government purchases actually relate to defense spending and are based heavily – almost exclusively – on the experience during World War II or the Korean War (Hall, 2009). Third, these models can only estimate the effects of policy interventions under the economic conditions prevailing within the sample, and the multiplier effects of different policies could vary substantially with economic conditions. Investment incentives that might be strong in a boom might be ineffectual in a period of tight credit and net operating losses. Tax cuts for households might have a larger effect during periods in which liquidity constraints bind more tightly. Government spending might have larger multipliers during periods, like recent times, when the zero-interest rate bound is binding. Finally, in the

standard SVAR approach, the crucial identifying assumption that unexplained shocks to policy are actually policy innovations is typically left untested.

One recent attempt to confront some of these limitations is Auerbach and Gorodnichenko (2012a) (hereafter AG), who extend the standard Blanchard-Perotti analysis in two key ways. First, using real-time forecasts of taxes, government purchases and GDP, AG purge disturbances of predictable components in order to make the resulting, purified shocks correspond more closely to true stochastic innovations. Second, AG allow multipliers to vary by the state of the economy by using a function of a measure of economic activity (a seven-quarter moving average of lagged real GDP growth) to determine the relative weights on “expansion” and “recession” SVAR parameters, which are estimated simultaneously. Applying this approach to quarterly postwar US data, AG show that the two modifications lead to much larger multiplier estimates in recession than expansion, with values at or above the upper range of Ramey’s range in recession and smaller values in expansion. These estimates suggest that fiscal policies adopted during the Great Recession might have had stronger effects than those adopted in milder recessions or expansions.

However, the Great Recession differed from previous recessions not just in terms of severity (which the AG analysis picks up through its measure of economic activity), but also in the conditions in financial markets, including the binding zero lower bound (ZLB) on short term interest rates. Indeed, several previous recessions were associated with the relatively high interest rates associated with monetary contractions. Unfortunately, with essentially one episode of observations, one cannot use aggregate US time series data to evaluate the impact of the ZLB.

As a consequence of this limitation, much of the recent discussion and debate surrounding the potential effects of policy intervention have been based on analysis using DSGE

models. DSGE models typically feature a relatively small number of equations based tightly on microeconomic theory, with some parameters derived from empirical estimates and others calibrated to make the model consistent with observed macroeconomic relationships. Because these models specify a full economic structure, they can be used to analyze policies and policy environments in a way that is not limited by historical experience.

But to do these things, the DSGE approach leans heavily on modeling assumptions that may or may not be valid, like assumptions regarding the stickiness of wages and prices, the prevalence of liquidity constraints, the rationality of agents, the structure of markets, and so forth. Indeed, as we shall discuss, some of the recent disputes regarding the potential effects of fiscal policies can be traced to differences in the assumptions in DSGE models as well as to assumptions about the nature and timing of the policies themselves.

In a recent review of the DSGE literature and using his own model of this type, Hall (2009) concludes that plausible dynamic stochastic general equilibrium models of the “new Keynesian” variety (that is, incorporating certain nominal rigidities in wages and prices) generate government spending multipliers that are consistent with those found using time series methods—well above zero, but below 1.0. However, as Hall notes, it appears that in the DSGE approach, relatively small changes in parameter specification – within empirically plausible ranges – are capable of producing substantial shifts in estimated multipliers. For example, several recent analyses using dynamic stochastic general equilibrium models, notably papers by Eggertsson (2010) and Christiano, Eichenbaum, and Rebelo (2011), have argued that when nominal interest rates are close to zero, the government spending multiplier can be substantially larger, with estimates in the range of 3 to 4.

One apparent explanation for the larger multiplier under the zero bound is that monetary policy responses are no longer active. The typical dynamic stochastic general equilibrium model includes a Taylor rule for monetary policy: that is, a rule in which interest rates respond to the output gap and the inflation rate. In normal circumstances, a government spending increase would stimulate output and inflation, which in turn would lead to an increase in interest rates, which would reduce current consumption and investment demand. However, when nominal interest rates fall to the zero bound, this response would be absent, and the output response therefore would be larger, because the monetary authority would still wish for the nominal interest rate to be even lower.

This intuition is apparently too simple, though, because some other DSGE analyses assuming constant interest rates deliver much smaller government spending multipliers. In particular, Cogan, Cwik, Taylor, and Wieland (2010) estimate the response to a permanent increase in government spending, assuming that interest rates stay equal to zero for the first two years of the experiment and follow a Taylor rule thereafter. They find an original multiplier around 1, but that by the end of the two-year period the effect on output is only 0.4. They attribute this difference from papers finding larger multipliers to a shorter zero-bound period. This finding is consistent with the analysis presented by Woodford (2011) that multipliers are reduced to the extent that the increase in government spending extends beyond the end of the zero-bound period. Thus, the multiplier for government purchases would be largest for a temporary spending increase that extended only for the period in which interest rate policy was near the zero bound and thus not active.

In summary, the evidence from the recent academic literature does not pin down the size of the effects of countercyclical fiscal policy. However, it does support two general conclusions.

First, under normal conditions the multiplier for government purchases is positive, although it may well be below 1.0. Second, in unusual conditions, such as a deep recession and/or a binding ZLB, fiscal policy's expansionary impact may be strengthened.

IV. The Role of Fiscal Consolidation during Recessions

As mentioned above, one concern accompanying evaluations of countercyclical fiscal interventions during the Great Recession has been the U.S. fiscal balance. Seeing budget deficits around 9 percent for several fiscal years and publicly held national debt rapidly increasing as a share of GDP, many have expressed concerns about the sustainability of US fiscal policy. If one looks at the longer run, the debt accumulation during the Great Recession is small relative to the impending flow of annual imbalances arising from the growth of old-age entitlement spending (Auerbach and Gale 2009b), and low current interest rates lessen the additional cost of recent debt accumulation. However, some still express concern that the chances of a fiscal crisis increase substantially as the debt-GDP ratio continues to grow, particularly as much of the additional debt is being held abroad (unlike in Japan). Moreover, those with this concern cite results in the literature suggesting that fiscal contractions may even be expansionary, if they are adopted at high levels of public debt and focus primarily on spending cuts, rather than tax increases.

An extensive theoretical literature argues that contractionary fiscal policy adopted during periods of budget stress can even have an expansionary effect on output, essentially by shifting the economy's trajectory away from one that could be very constraining for productive activity because of high marginal tax rates or economic disruptions. Some empirical evidence, based on panel data for OECD countries, does suggest that fiscal consolidation has a less contractionary effect when adopted under fiscal stress, as measured by high debt and projected government

spending relative to GDP (Perotti, 1999). Analysis based on OECD data also indicate that fiscal contractions are more expansionary when implemented through cuts in government spending, as one might expect given the potential damage from reliance on higher marginal tax rates (Ardagna, 2004, Alesina and Ardagna, 2010).

However, at least some more recent evidence has called these findings on expansionary fiscal consolidations into question. Based on a new data set for several countries and using the Romer-Romer narrative approach to identify fiscal consolidations, rather than more standard measures based on changes in the full-employment primary surplus, IMF (2011) concludes, “Fiscal consolidation typically lowers growth in the short term...we find that after two years, a budget deficit cut of 1 percent of GDP tends to lower output by about ½ percent and raise the unemployment rate by ⅓ percentage point.” Like earlier studies, though, the IMF study does conclude that fiscal consolidations relying on spending reductions have less contractionary effects than those relying on tax increases.

Also, other recent evidence, for a panel of OECD countries (Auerbach and Gorodnichenko 2012b), finds that government purchase multipliers are higher in recession than in expansion, as Auerbach and Gorodnichenko (2012a) found for the United States, but not for countries with high debt-GDP ratios. Table 2 presents results from this paper, showing the change in real GDP in response to a one-percent government spending shock, how this varies between recession and expansion, and how the effects depend on the country’s debt-GDP ratio. The first row of the table shows a much larger response in recession than in expansion, with both mean and maximum responses (over three years) bracketing those for the linear model, which constrains multipliers to be equal across states.⁸ The second and third rows of the table show how the mean multiplier values are affected by the initial debt-GDP ratio, obtained from

estimates of a model in which the government spending shocks are interacted with the debt-GDP ratio.

As the Table 2 shows, the multiplier in recession is strengthened when the debt-GDP ratio is zero, and becomes roughly zero when the debt-GDP ratio is 1. While this finding does not suggest that fiscal contractions are *expansionary*, it does indicate that attempts at fiscal stimulus may be relatively ineffective for countries facing budget stress. Of course, results for OECD countries, as most of the preceding studies are, do not necessarily apply to the United States, still a provider of a reserve currency and perceived as a relatively safe haven in turbulent world economic times.

In summary, there is some evidence suggesting that fiscal stimulus may be more difficult to achieve, even in recession, for a country in a challenging fiscal environment, as measured by its debt-GDP ratio. But, while fiscal consolidation may have clear long-run benefits, its short-run desirability in recession remains to be proved for countries that, like the United States, do not face imminent financial crises or reduced access to credit markets.

V. Fiscal Policy and its Impact during the Great Recession

The biggest element of stimulus adopted by the United States during the Great Recession was the American Recovery and Restoration Act of 2009 (ARRA). We have argued above that it can be viewed as the continuation of a series of activist fiscal policy interventions dating back to 2001, but ARRA was of a much larger scale than previous efforts. The direct cost of the bill (excluding interest payments on accumulated debt) was eventually estimated to cost \$862 billion (CBO, 2010). The policies were to be phased in over time, with \$200 billion occurring in fiscal year 2009, \$404 billion occurring in fiscal year 2010, and the remainder occurring in fiscal year 2011 or afterwards. A substantial portion of ARRA provided aid to individuals and transfers to

states, mainly through Medicaid and other programs. Also, a primary objective of the stimulus package was to increase funding for public infrastructure programs, with a stated emphasis on “shovel ready” projects that could be quickly undertaken.

How well-targeted the package was, and the size of the resulting policy multipliers, remains an area of controversy. Even before the stimulus package was adopted in February 2009, the Obama administration released a document written by Bernstein and Romer (2009) estimating the effect of a potential stimulus plan on employment. These projections were based on estimates of multipliers for government purchases and tax cuts averaged over those from the Federal Reserve’s FRB/US model and a private forecasting model. The resulting multiplier for a permanent change in government purchases was about 1.5, reached after about one year; the corresponding multiplier for tax cuts (other than investment incentives) was about 1.0, with about three-fourths of the impact reached after one year and the full effect reached after two years. These multipliers are consistent with those assumed by the Congressional Budget Office (2009, Table 1) in making its projections, in that both the government-spending multiplier and the tax-cut multiplier fall roughly midway between the upper and lower bounds CBO lists for its high-multiplier and low-multiplier scenarios.

The similarity in multiplier assumptions by CEA and CBO is reflected in similar estimates of the aggregate impact of the stimulus package. CBO (2010) estimated that, in the first quarter of 2010, the stimulus package raised the level of GDP by between 1.7 percent and 4.2 percent and raised the level of employment by 1.2 million to 2.8 million. Subsequent evaluations, also based on large-scale macroeconomic models, yielded similar results. However, two points should be made about such analyses. First, these large-scale models incorporate traditional Keynesian features that can generate relatively large multipliers when the economy is

far from full employment, as was the case in 2009. As discussed above, such multipliers are less easily generated using alternative modeling techniques. Second, these studies are *predictions* of the effects of ARRA – they do not in any way confirm that such effects occurred. For that, one would need to know the counterfactual path of the economy. Indeed, an alternative approach to evaluating the effects of ARRA, looking at trends in behavior at the state and local government level (Cogan and Taylor, 2011), concluded that state and local governments offset the increase in federal transfers to them by reducing outstanding debt and increasing transfer payments, rather than increasing government purchases. As a consequence, Cogan and Taylor argue that ARRA had virtually no impact on overall government purchases as of mid-2010, and as a consequence was unlikely to have had a strong stimulus effect even if the government purchases multiplier were large.

More optimistic evaluations of ARRA come from the cross-state analyses of Chodorow-Reich et al. (2012) and Wilson (2012). Both papers use the formulas that ARRA used to distribute funds to different states as sources of exogenous cross-state variation in funds, with Chodorow-Reich et al. looking exclusively at a portion of Medicaid funding and Wilson considering a broader range of programs. While both studies estimate effects on employment rather than on output, it is possible to translate their findings into multiplier effects. Doing so, Chodorow-Reich et al. estimate an output multiplier of around 2. Wilson does not provide such a direct translation, but he does estimate that, if one aggregates state-level effects to the federal level, the overall employment effects are comparable to the large effects predicted using large-scale macroeconomic models, discussed above. However, these results must be viewed with some reservation. First, other cross-state evaluations of ARRA, using somewhat different approaches, find smaller effects (Conley and Dupor, 2011, and Feyrer and Sacerdote, 2011).

Second, cross-state effects are not directly comparable to national multipliers. Some differences, such as positive spillovers to other states caused by increased demand for their products, would tend to make the national multiplier larger when one aggregates the individual state effects, while others, such as the anticipated burden of future taxation associated with spending increases, would tend to make national multipliers smaller, since most of the taxes that finance any one state's increased spending out of federal funds would be financed by those in other states.

In summary, it is impossible to pin down the aggregate effects of ARRA because, as discussed above, the Great Recession is a brief period that cannot be evaluated using direct time series methods. Some studies interpreted as showing that ARRA had large effects are essentially predictions, not ex post evaluations. An examination of trends, based on the assumption that such trends would continue, suggests that ARRA may have been undercut by offsetting state and local reactions, although some cross-state analyses seem inconsistent with this conclusion. The recovery from the Great Recession has been weak in the United States, but this recession is by far the worst since the Great Depression, and caused by quite different factors than previous postwar recessions, so it is hard to know how much evidence against ARRA this weak recovery provides. Nevertheless, uneasiness about the growing level of national debt, combined with skepticism about the effects of ARRA, appear to have contributed to a reduced enthusiasm for fiscal activism in the United States, and one has seen a similar evolution in other countries, notably in Europe where many countries have adopted policies of fiscal contraction even as unemployment has remained high and output growth is still slow.

Although ARRA was by far the largest fiscal stimulus measure adopted during the Great Recession, there were smaller pieces of legislation that followed. One, already discussed above,

is the payroll tax cut adopted in late 2010. To date, there is little evidence about the impact of this legislation. Another policy, the so-called “cash for clunkers” program, subsidized the disposal of high-emission automobiles. The argument for this program was that it would combine a stimulus to new car purchases (to replace the junked clunkers, which under the terms of the law had to be rendered unusable upon being turned in) with a pro-environmental impact of replacing low-efficiency, highly polluting vehicles with more modern ones. The size of the second benefit is questionable, because one must take into account the energy used in new car production and the possible impact on miles driven when a new car replaces an old one, but the stimulus effect appears to have been minimal, with a very small shift in timing and very little overall impact on car purchases (Mian and Sufi 2010).

VI. Conclusions

At least in the United States, fiscal policy became more active and more responsive to the state of the economic cycle in recent years, even before the onset of the Great Recession. There are many potential explanations for this increase in activism, but at least one source may be a renewed sense that government policy actions may be effective at strengthening the economy when it is in recession. However, the large fiscal stimulus adopted in and around 2009 has not resolved the question of policy effectiveness, even though it has already resulted in a number of empirical analyses.

In one key way ARRA illustrates a long-known difficulty of fiscal stimulus, the lags associated with policy adoption and action. Even though the recession had been in place for more than a year when ARRA was passed, its provisions were phased in over a period of years. Also, its structure was not necessarily ideal from the perspective of economic stimulus, in terms of generating increases in overall government purchases. Also, its contribution to the rapid debt

accumulation that was already in place, to a large extent because of the depth of the recession but also because of policy choices earlier in the decade, heightened concerns about US fiscal balance and, in turn, whether further stimulus was desirable or potentially effective. Concerns about long-term fiscal balance are clearly appropriate for the United States, but the extent to which these concerns should limit policy even in the very short run is far less clear, given the mixed evidence on the effects of fiscal consolidations and the fact that the recent rise in the US debt-GDP ratio, though rapid, has had a relatively small impact on the country's long-term fiscal imbalance.

The shift in focus of US fiscal policy from cyclical to budget concerns has been intermittent so far, with fiscal tightening (such as the summer, 2011 budget agreement) seemingly alternating with further stimulus measures, notably the adoption in 2010 and eventual renewal in late 2011 and early 2012 of a payroll tax cut. As the economy continues to recover and the fiscal position continues to be a source of concern, it seems likely that addressing the budget will eventually become a steadier determinant of fiscal decisions. But whether recent policy activism has taken root, or whether this will be undone by the skepticism about the effect of ARRA and other policies and remaining concerns about the budget, will not fully be known until the United States faces its next opportunity to respond, or not to respond, to recession.

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Notes

¹ For example, U.S. income tax revenues surged in the late '90s, as the incomes of high-income individuals and the stock market boomed. They fell sharply in 2001 after the dot-com bubble burst, far more than could be explained by the weakening of the economy.

² Because policy revisions between the winter and summer take effect starting midway through the current fiscal year, we reduce the weight on the current fiscal year by one-half and increase weights on subsequent years correspondingly.

³ This discount factor is the one that provided the best fit in the initial versions of these estimates in Auerbach (2002), and so we continue to use it here. However, reasonable variations in the discount factor do not appear to have an important impact on the results.

⁴ One might also consider other variations on the equations estimated in Table 1, such as to allow nonlinear effects of the independent variables. For example, the output gap might matter more when it is relatively large, or there might be policy inaction ranges for small deviations from full employment. Unfortunately, it is difficult to evaluate such alternatives given the limited sample size available, and some attempts to do so did not yield interesting or significant results.

⁵ We focus on the results for the primary surplus because the results for revenues and outlays are less significant for the subperiods due to the small sample sizes. However, the coefficients themselves are all of the same signs as for the sample as a whole.

⁶ For a review of this literature, see Auerbach and Gale (2009a) or Auerbach, Gale, and Harris (2010).

⁷ Table 1 of Ramey's paper lists the results from several studies based on US time series data, including large-scale macroeconomic models, standard SVAR models and models using the narrative approach to identify spending shocks.

⁸ The responses are expressed in terms of the percent of GDP, so to translate into a standard multiplier one needs to multiply these coefficients by the ratio of GDP to government purchases, or around 4 in the sample of countries.

Table 1. Estimated Policy Functions
(standard errors in parentheses)

Sample Period:	84:2-12:1	84:2-12:1	84:2-12:1	84:2-12:1	84:2-93:1	93:2-01:1	01:2-09:2	01:2-12:1
Dependent Variable:	Revenues	Non-Interest Outlays	Primary Surplus					
Constant	-0.002 (0.001)	0.003 (0.001)	-0.004 (0.001)	-0.014 (0.005)	-0.008 (0.005)	-0.001 (0.000)	-0.008 (0.002)	-0.007 (0.002)
Output Gap (-1)	-0.083 (0.029)	0.096 (0.032)	-0.179 (0.049)	-0.237 (0.055)	-0.179 (0.096)	-0.097 (0.050)	-0.364 (0.091)	-0.158 (0.097)
Projected Surpluses (-1)	-0.068 (0.030)	0.102 (0.032)	-0.169 (0.049)	-0.189 (0.049)	-0.286 (0.157)	-0.128 (0.041)	-0.165 (0.077)	-0.163 (0.113)
Debt (-1)				0.024 (0.012)				
\bar{R}^2	.097	.132	.179	.228	.091	.595	.472	.029
Observations	56	56	56	56	18	16	17	22

Table 2. Percent Response of Real GDP to an Unanticipated One-Percent Government Spending Shock

	Mean Response			Maximum Response		
	Recession	Expansion	Linear	Recession	Expansion	Linear
	0.46*	-0.20	0.14	0.68**	0.04	0.19*
	(0.26)	(0.22)	(0.10)	(0.34)	(0.09)	(0.11)
Government Debt-GDP Ratio = 0	0.84***	-0.58	0.22			
	(0.32)	(0.38)	(0.17)			
Government Debt-GDP Ratio = 100%	0.05	0.26	0.04			
	(0.35)	(0.36)	(0.16)			

Notes: Mean and maximum responses are calculated over three years. Robust standard errors are reported in parentheses. *, **, *** indicate statistical significance at 10, 5, and 1 percent levels.

Source: Auerbach and Gorodnichenko (2012b)

Figure 1. Policy Changes

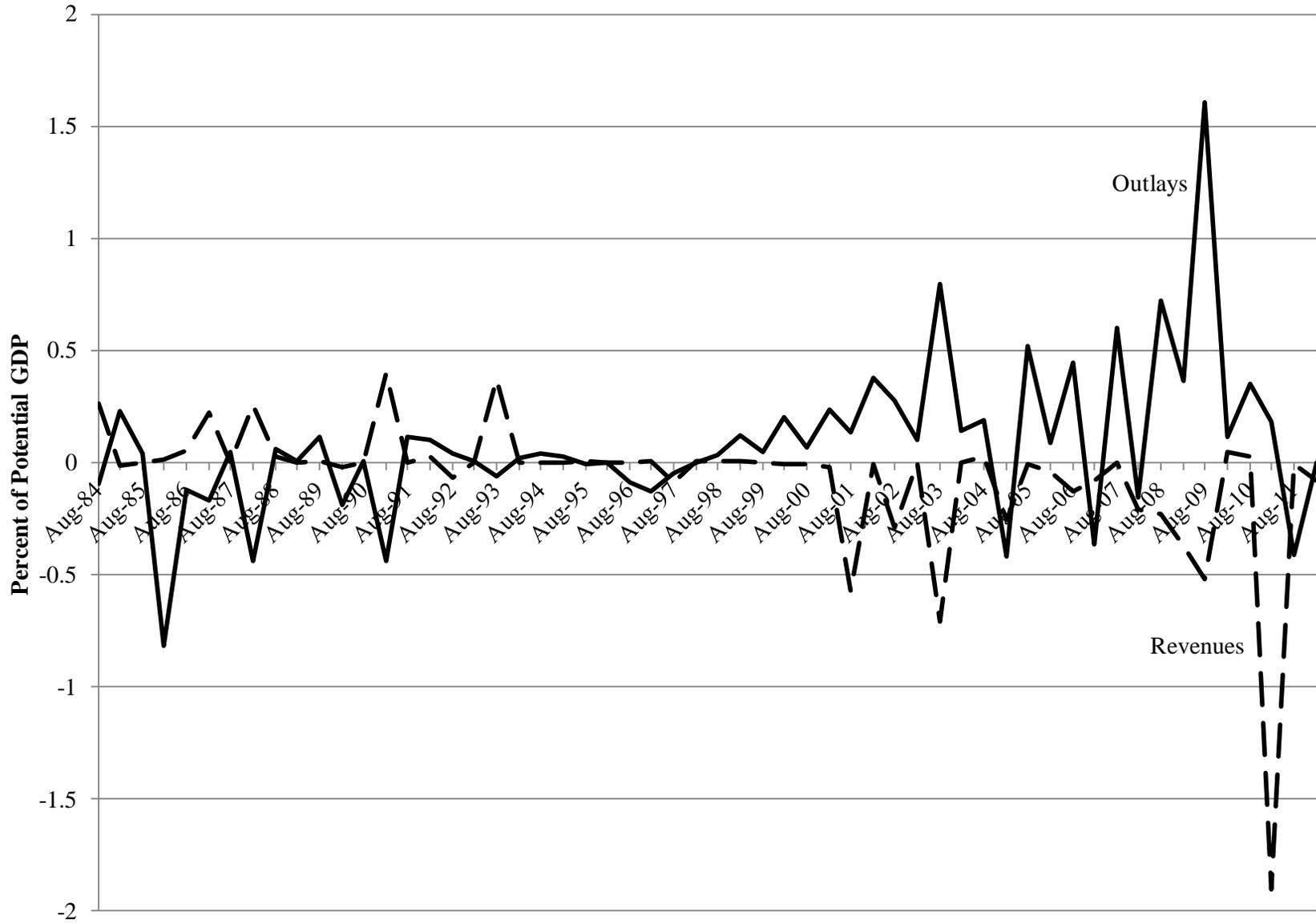


Figure 2. Actual and Predicted Policy Changes

