

# Activist Fiscal Policy to Stabilize Economic Activity

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## **I. Introduction**

Facing the most severe recession since the 1930s, and probably the longest as well, the U.S. government has adopted an aggressive countercyclical fiscal policy stance, beginning with the “Economic Stimulus Act of 2008” in February of that year, shortly after the recession’s designated starting date, and followed one year later by the much larger “American Recovery and Reinvestment Tax Act of 2009.” Each of the bills, adopted under different presidents, contained temporary tax rebates for households and temporary investment incentives for firms, indicating at least limited bipartisan acceptance of these approaches to countercyclical stimulus. The 2009 act, amounting to 5.5 percent of gross domestic product (GDP), also included a variety of government spending provisions, most notably the funding of “shovel-ready” infrastructure projects and aid to state governments. And, even as signs are appearing that the recession’s end is near or already past, calls continue for the passage of yet another stimulus bill in 2009. Almost all Organization for Economic Cooperation and Development (OECD) countries have introduced stimulus measures, with the packages averaging 2.5 percent of GDP across the OECD.<sup>1</sup>

This fiscal policy activism is striking, given the consensus a decade ago against the use of discretionary fiscal policy as a stabilization tool.<sup>2</sup> In addition to traditional concerns about policy lags that seemed confirmed by certain unfortunate policy episodes, economists had provided various theoretical arguments and some evidence suggesting that multipliers might be small and that expectations could wreak havoc not only with the strength of policy effects but also with attempts at getting the timing right. The associated exclusive focus on automatic stabilizers and the use of monetary policy seems now to have come to an abrupt halt. While the depth and duration of the recession and the unusual challenges facing monetary policy, including hitting a zero nominal interest rate constraint for all practical purposes, undoubtedly contributed to this reliance on discretionary fiscal policy, the current policy environment is in other respects hostile to activist policy. In particular, adding to government liabilities carries obvious economic risks when the medium- and long-term budget outlooks appear to be increasingly unsustainable (Auerbach and Gale, 2009).

In this paper, we consider the evidence on the effects of discretionary fiscal policy, paying particular attention to the current context. We begin by considering how the practice of discretionary fiscal policy has changed over time and argue that the return to activist policy predated the current recession. We then turn to the evidence on the effects of discretionary policy on economic activity, considering the variety of approaches found in the literature, including direct econometric tests of the impact of stimulative policies on consumption and investment, as well as general equilibrium approaches to measuring the impact of taxes and government purchases. Finally, we look for lessons from the evidence from two important historical episodes: for the United States in the 1930s and Japan in the 1990s.

In addressing discretionary fiscal policy, we are explicitly excluding from our discussion the use of automatic stabilizers, which has been subject to much less controversy over the years than discretionary interventions. While automatic stabilizers may be an important

macroeconomic policy tool, recent policy activism suggests that they will, at least in some circumstances, be viewed by policymakers as insufficient on their own.

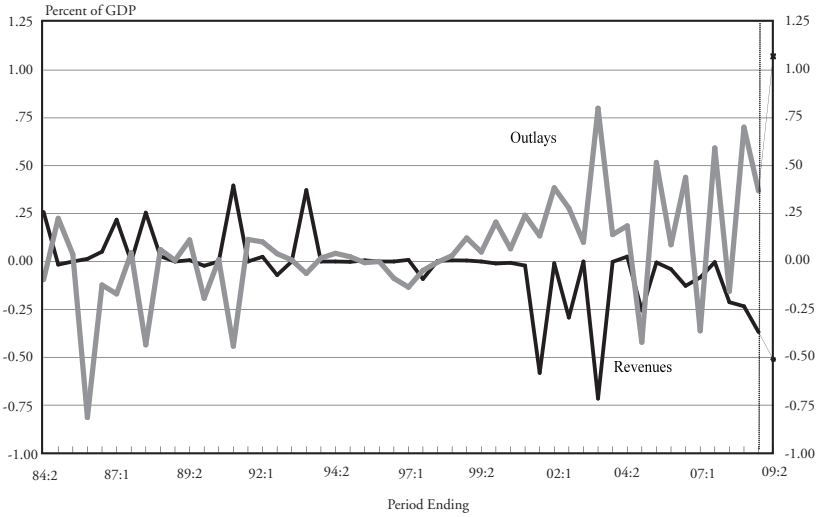
## **II. The Return of Activist Fiscal Policy**

In a paper written seven years ago for this conference (Auerbach, 2002), one of us argued that there was no evidence that the United States had increased its reliance on discretionary fiscal policy, contrary to what some had suggested to be happening in Europe. This conclusion was based on analysis of actual policy changes, as reported by the Congressional Budget Office (CBO), rather than on a more popularly used measure based on changes in the full-employment surplus. Because the latter measure incorporates changes in the surplus that are attributable neither to policy nor to the state of the economy<sup>3</sup> and that represent the phasing-in of policies adopted earlier, it provides a poor representation of the timing and magnitude of policy actions. Updating that paper's analysis using more recent evidence, however, provides a different picture.

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 ending in the winter of 2009, just before the passage of the 2009 stimulus bill.

For each observation, as before, we measure the policy change in question (revenue, outlays, or 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), with the five weights normalized to sum to 1.<sup>4</sup> The discount factor, as before, is 0.5, meaning that each

**Chart 1**  
**Policy Changes**



succeeding fiscal year's policy change is accorded half the weight of the previous one. Variables are scaled by potential GDP.

Chart 1 displays the resulting series for changes in revenues and outlays. Also included in the figure is a partial measure of the policy changes for the second observation in 2009 (winter 2009 to summer 2009), based on changes adopted through March 2009, including the February stimulus package—the largest changes in both revenues and outlays over the entire period. 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 very active again during the past decade. 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, again repeated from the 2002 paper. Explanatory variables are the projected annual budget surpluses over the same five-year budget period, weighted using the same discounting process<sup>5</sup>, and the lagged value of the full-employment GDP gap from the prior quarter, as calculated by CBO. The latter

variable is included to determine whether discretionary policy actually has been countercyclical in nature—which some observers have doubted—and the former is included to account for the likely response to budgetary conditions, with the predicted coefficients of both variables being negative in explaining revenues and the surplus, and positive in explaining outlays. The coefficients' absolute value measures the strength of the associated response.

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, with outlays accounting for a greater share of the overall response. The results indicate that policy changes adopted in a representative six-month period counter roughly one quarter of the GDP gap and offset a slightly higher fraction of the projected budget surplus.

Chart 2 repeats the series of policy changes shown in Chart 1, but now accompanied by the predicted policy changes based on the full-sample estimates for revenues and outlays. As the figure shows quite clearly, a large part of the changes in the level of activity, from active to quiet to active again, are attributable to changes in the underlying forces driving policy. That is, assuming no change in the policy reaction functions, the 1990s were a quiet period for policy because the reasons for intervention were absent. The intervention in early 2009 is not only the largest during the entire period, but also predicted to be.

But there is more to the recent resurgence of activity, as one sees by estimating the reaction functions separately for different time periods. The last three columns of Table 1 present evidence for the primary surplus for three sample periods of roughly equal length, corresponding respectively to the presidencies of Ronald Reagan and George H. W. Bush, Bill Clinton and George W. Bush.<sup>6</sup> As previously discussed, there is no evidence from a comparison of results for the first two sub-periods—roughly those analyzed in the 2002 paper<sup>7</sup>

**Table 1**  
**Determinants of Policy Changes**

Dependent Variable: Semiannual Legislated Change in Primary Surplus, Revenue, and Outlays Relative to Full-Employment GDP (standard errors in parentheses)

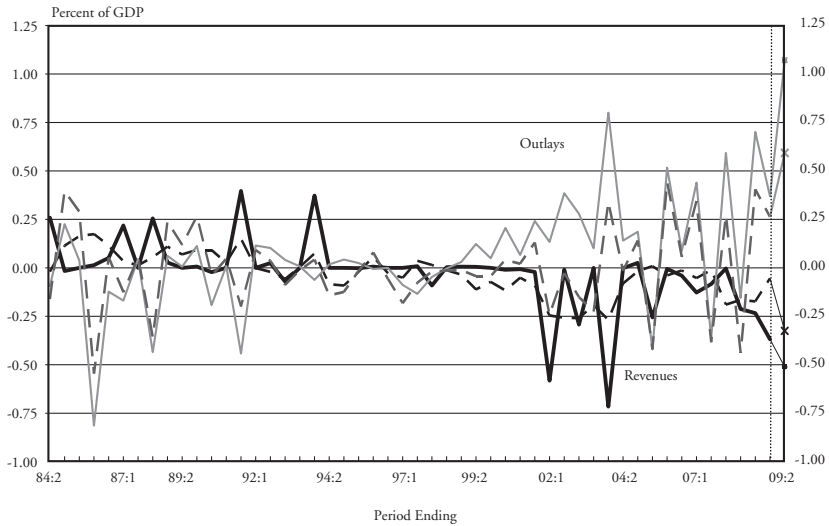
Period:	1984:2—2002:1	1984:2—1993:1	1993:2—2001:1	2001:2—2009:1		
Dependent Variable:	Revenues	Outlays	Primary Surplus			
Independent Variable:						
Constant	-0.002*** (0.0004)	0.003*** (0.001)	-0.006*** (0.001)	-0.005 (0.005)	-0.003*** (0.001)	-0.006*** (0.001)
GDP Gap (-1)	-0.091*** (0.024)	0.158*** (0.035)	-0.249*** (0.046)	-0.170 (0.106)	-0.116** (0.046)	-0.359** (0.144)
Projected Surplus (-1)	-0.103*** (0.021)	0.168*** (0.031)	-0.271*** (0.040)	-0.241 (0.154)	-0.162*** (0.045)	-0.301** (0.104)
$\bar{R}^2$	.315	.361	.471	.043	.649	.297
Number of Observations	50	50	50	18	16	16

\*\* - significant at the .05 level

\*\*\* - significant at the .01 level

Source: Congressional Budget Office and authors' calculations.

**Chart 2**  
**Actual and Predicted Policy Changes**



—for an increasing reliance on discretionary fiscal policy. That is, the reduced level of activity observed is attributable to changes in the perceived need for policy responses. However, the estimates for the last period tell a quite different story, as responses to fiscal conditions, and especially to economic conditions, have strengthened.

This increased countercyclical policy activism is nicely illustrated by the differences in policy responses during the last four recessions. In August 1982, after a year in a deep recession that 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 President Bush and Congressional leaders produced legislation aimed at reducing the deficit. 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. And early 2008 saw the first round of fiscal stimulus during the current recession, adopted well before the recession’s depth could be anticipated. It does appear, then, that the stage was set for the

2009 stimulus package, even though the extreme conditions at the time made a substantial intervention much more likely.

### III. Multipliers and Policy Lags

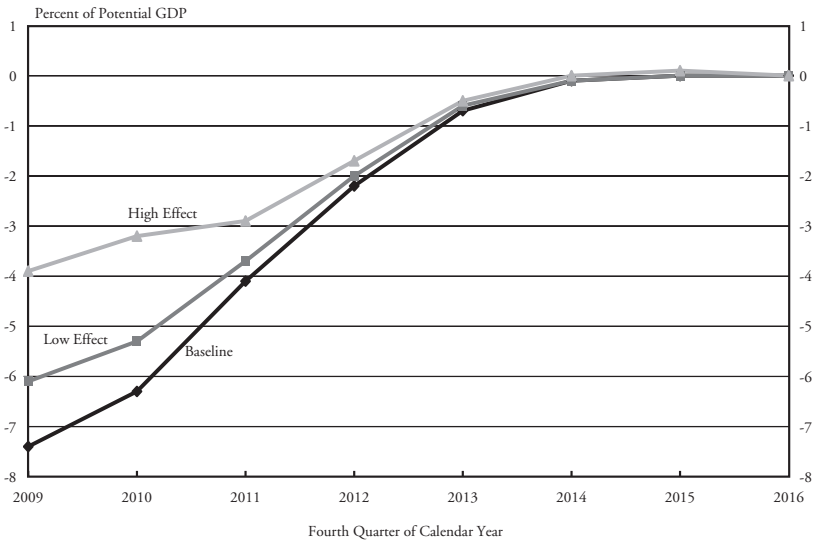
Despite being adopted during a period of very weak economic performance, the 2009 stimulus package encountered criticism on several fronts, which can be summarized by asking whether the package was sufficiently “timely, targeted, and temporary.”<sup>8</sup>

First, there was concern that the policies, although adopted in February 2009, would be implemented only gradually, with much of the impact coming after the recession was over and the recovery underway. While the concern about policy lags is one of the standard criticisms of countercyclical fiscal policy, the concern seems a little less relevant in the present context, if projections of a long and slow recovery are to be believed. Chart 3 shows the path for GDP relative to potential as projected in March by CBO, for the baseline without the February stimulus package and for two scenarios with the fiscal package, corresponding to CBO’s perceived range of multiplier estimates for the package’s different components. Under these projections, the economy would not reach its potential until 2014, and the stimulus package would simply speed the rate of approach, even under the most optimistic assumptions about multiplier size. Thus, while more rapid implementation might still have been preferred, the risk of destabilizing the economy by injecting stimulus in an overheating economy seems to be less of an issue.

The desire to keep the package temporary is motivated by concerns about the long-term budget outlook. As we have discussed elsewhere (Auerbach and Gale 2009), the contribution of the stimulus to the long-term U.S. fiscal problem is minimal, if one assumes that the provisions of the stimulus are temporary, as enacted. Indeed, the stimulus package contributes less to the current-year deficit than does the recession itself, through automatic stabilizers working primarily on the tax side. Of course, the Obama administration has already supported, through its budget proposals, a plan to extend an important part of the stimulus package—the Making Work Pay tax credit. But this proposal dates back to early in the presidential campaign, and so



**Chart 3**  
**Estimated Impact of 2009 Fiscal Stimulus**



Source: Congressional Budget Office.

has little if anything to do with the passage of the stimulus package. On the other hand, the inclusion in the stimulus package of a provision designed originally without the recession in mind does highlight the third set of criticisms of the stimulus package: that it was not well-targeted to provide the strongest fiscal stimulus per dollar of revenue loss or spending increase. Some focused on the composition of the package, questioning whether projects that were “shovel-ready” were likely to be of high value to society and whether the particular tax cuts adopted were the right ones from a longer-term perspective. But most concerns related to the policy’s macroeconomic effects.

How well-targeted the package was, and the size of the resulting policy multipliers, is a key area of controversy. Even before the stimulus package was adopted, the Obama administration released a document written by two of its economists (Romer and Bernstein, 2009) estimating the impact 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 Fed’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 a 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 impact reached after two years. These multipliers are consistent with those assumed by CBO (2009a, 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. But many economists outside of government (e.g., Barro, 2009; Cogan, et al. 2009; Leeper, et al. 2009) have recently challenged these or similar estimates, arguing that the assumed multipliers are too large.

How should one calculate government policy multipliers? The literature has not settled on a single preferred method, so the evidence comes from a variety of approaches. For some policies, such as tax cuts provided to households, we have ample data at the individual level, and can use these data to estimate household responses to tax changes, such as the rebates introduced in 2008 and the tax credits of 2009. Similar approaches have been applied in estimating the impact of tax incentives on investment, although this line of research has proved more challenging because of the greater complexity of the likely effects. We review the estimates from both of these literatures in some detail below. These approaches, however, look only at direct responses to tax changes and not the impact on economic activity overall, which could be smaller or larger than the direct effects on consumption or investment. As a result, models are needed that take account of the various additional channels through which tax cuts, as well as increases in government purchases or transfer payments, affect GDP and its components. There have been various approaches to the development of such models, and we review these developments and their predictions below, after discussing the results regarding the direct effects of tax policy.

### ***III.A. Evidence from Household Responses to Tax Cuts***

Personal consumption expenditures on nondurables, durables, and services currently account for about 70 percent of GDP, rising from between 60 and 65 percent during much of the post-war period. As

a result, tax cuts to stimulate consumption have long been a staple of government efforts to stimulate the economy. These efforts, in turn, have generated a substantial literature that offers several “stylized” results about the marginal propensity to consume (MPC) out of tax cuts.

First, consistent with standard life-cycle and permanent-income models, most of the evidence suggests that household consumption responds more vigorously to tax changes that are plausibly expected to be longer-lasting (“permanent”) than to changes that are plausibly expected to be shorter-lasting (“temporary”). Using aggregate consumption data, Blinder (1981), Blinder and Deaton (1985), and Poterba (1988) examine the effects of two temporary policies—the 1975 income tax rebate and the 1968 surtax. Blinder finds that the current-period MPC is smaller for a temporary tax cut than a permanent cut, but that some of the difference is made up in subsequent periods. Blinder and Deaton find almost no contemporaneous consumption out of temporary income tax cuts. Poterba finds that between 12 percent and 24 percent of the 1975 rebate was spent in the month the rebate is received.

Microeconomic studies of the longer-lasting shifts in tax policy enacted in 1981 and 2001 have produced larger estimated MPCs. Souleles (2002) uses Consumer Expenditure Survey (CEX) data to show that the MPC out of the 1981 tax cuts was as high as 0.9. Johnson, Parker and Souleles (JPS 2006) use CEX data to measure the effect of the 2001 tax “rebate” on consumption, exploiting the time variation across households regarding when the rebates were received.<sup>9</sup> Their central finding is that households spent 20 to 40 percent of the funds on nondurable goods during the first three-month period after receipt of the rebate, and a total of 69 percent over the first two quarters after receipt. These results are sensitive to specification, however. Hamilton (2008) replicates the JPS findings and shows that the estimated MPC falls to 45 percent if just 20 outliers, out of a sample of 13,000, are removed. The outliers are households with the 10 most positive and 10 most negative changes in quarterly consumption. Each change exceeds \$13,000 in absolute value and could not possibly have been generated by the rebates, which were capped at \$600.

Additional trimming of the sample reduces the coefficient further, and the MPC estimated from a median regression is about 30 percent.

A second stylized finding, in addition to differences in MPCs out of temporary versus permanent tax cuts, is that household responses to a given tax cut are heterogeneous, with borrowing constraints materially affecting the MPC. In theory, borrowing-constrained households should have a larger MPC out of tax cuts than other households do, and low- and middle-income households are more likely to be constrained than upper-income households. As a result, tax cuts targeted toward lower- and moderate-income households should have a larger “bang for the buck.” In practice, most of the evidence from the best studies is consistent with this view. JPS find that households with low income and few liquid assets have a higher MPC out of the 2001 tax cuts than those with high income or a greater amount of liquid assets. Broda and Parker (2008) obtain similar results for the 2008 tax cut. Perhaps the most compelling evidence on the role of borrowing constraints comes from Agarwal, et al. (2004), who examine monthly data on credit card accounts in conjunction with the 2001 tax cut. They show that households with lower credit card limits and/or those who have balances above 90 percent of their credit limit have a significantly stronger MPC (looking at credit-card financed purchases only) out of the 2001 tax cut than do other households. Bertrand and Morse (2009) find that pay-day loan holders, who are likely to be borrowing-constrained, have a very low propensity to use their rebates to pay down existing debt, with the implication that the marginal propensity to consume the rebate is quite high among this group.<sup>10</sup>

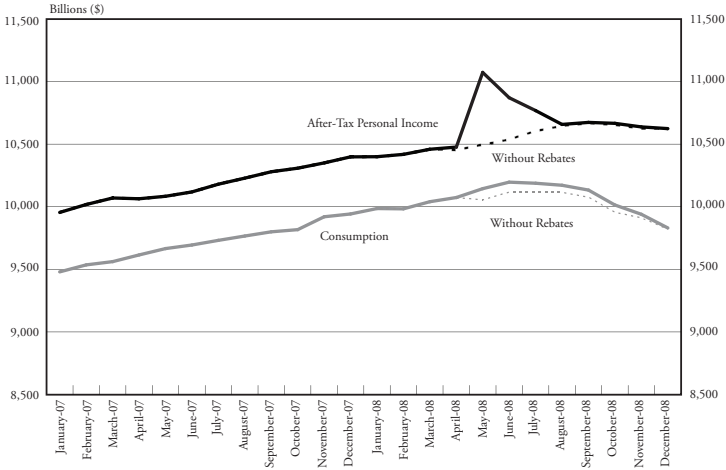
Comparing estimated MPCs for the 2001 and the 2008 tax cuts provides interesting perspectives on the two issues noted above—the role of tax cut permanence and of heterogeneous responses. The 2001 rebate was clearly—even at the time of enactment—part of a longer-lasting tax cut, whereas the 2008 rebate was very explicitly a one-time event. On the other hand, the 2001 rebate went to all income groups and was not refundable, whereas the 2008 rebate was limited to low- and middle-income households and was refundable. The first difference should raise the MPC out of the 2001 rebate relative to 2008, the second difference should reduce it. In fact,

estimated MPCs are not significantly different for the two tax cuts. For example, Broda and Parker (2008) examine micro data on household purchases and find that households consumed about 20 percent of the rebate in the first month after receiving it, a rate of consumption that is consistent with the MPC out of the 2001 tax cuts reported in JPS. Shapiro and Slemrod (2003, 2009) report the results of asking respondents in phone surveys how they intend to use the 2001 and 2008 tax cuts, respectively, and report a remarkable similarity in overall responses for the two tax cuts. For example, 21.8 percent of households say they would mostly spend the 2001 tax cuts, compared to 19.9 percent for the 2008 tax cut.

Finally, aggregate data show personal saving spiking and personal consumption relatively smooth during the months when rebates were paid in both 2001 and 2008 (Feldstein, 2009; Shapiro and Slemrod, 2009; and Taylor, 2009). The last finding is consistent with the responses to the 2001 and 2008 rebates being similar, but does not necessarily imply that the MPC out of each was negligible. Chart 4 replicates a figure from CBO (2009b) that shows that if the two-quarter MPC out of the 2008 tax cut were 40 percent, and the spending occurred over a six-month period, the resulting time path for consumption looks remarkably smooth relative to income.

A third important finding from the literature is that the impact of tax changes on consumer spending tends to occur when the policy change is implemented, not when it is enacted or credibly announced (in contrast to investment behavior, summarized below). There is both general and tax-specific evidence that the timing of income matters for consumption. Campbell and Mankiw (1989) show that aggregate consumption responds to current disposable income. Likewise, Wilcox (1989) shows that households raise their aggregate spending upon receipt of Social Security benefit increases. Using micro data, Parker (1999) shows that households' expenditures on nondurable goods rise in the months of the year after they have met their Social Security payroll tax cap and their take-home pay rises. Souleles (1999) shows that household consumption rises upon receipt of a tax refund, but not earlier, even though the value of the refund can presumably be known in advance.

**Chart 4**  
**Consumption and Income With and Without Rebates Provided**  
**Under the Economic Stimulus Act of 2008**



Source: Congressional Budget Office (2009b).

Note: The cumulative area between lines showing consumption (personal spending) with and without the effects of rebates is 40 percent of the area between the lines showing income with and without the rebates. In the chart, it is assumed that the 40 percent of rebates is spent over six months, according to this pattern: 15 percentage points in the first month and 5 percentage points in each subsequent month. On the basis of those assumptions, CBO estimates that the rebates added 2.3 percent (at an annual rate) to the growth of consumption in the second quarter of 2008 and 0.2 percent in the third quarter but—because of those effects—reduced the growth of consumption by 1.0 percent in the fourth quarter.

The evidence from tax cuts is equally strong. Poterba (1988) shows that aggregate consumer spending did not respond in the months surrounding the announcement of several major tax cuts. Souleles (2002) shows that the MPC out of the Reagan tax cuts, which eventually reached 0.9 as noted above, rose during the second and third phases of the tax cuts, which were pre-announced. Blinder and Deaton (1985) find similar results in the aggregate data. Johnson, Parker, and Souleles (2006) and Broda and Parker (2008) exploit the fact that the 2001 and 2008 tax cuts were received by different households at different times to show that consumption only rose after the tax cut had been received.<sup>11</sup>

The findings summarized above are generally consistent with standard optimizing behavior, with some households facing borrowing constraints (though the findings for the timing of the consumption response are also consistent with mental accounting or myopia). Other results suggest the importance of an additional set of factors, namely

the way tax cuts are described and delivered, holding budget constraints constant. These results are consistent with a growing literature indicating that framing, presentation, and other factors such as default specifications have a significant influence on saving behavior, and therefore are relevant for the simple reason that saving and consumption choices are closely linked.<sup>12</sup>

For example, there is some evidence that adjustments to withholding that do *not* represent tax cuts can affect consumption. Shapiro and Slemrod (1995) report the results of a phone survey asking respondents how they reacted to the 1992 changes in withholding, which changed the timing of tax payments but not overall tax liability. The respondents were surveyed one to two months after their changes in withholding occurred. One question summarized the policy change, specifying that respondents' overall tax burden would not change, and then asked whether they planned to mostly save, mostly spend, or mostly pay down debt with their increased take-home pay. More than 40 percent said they planned to spend the extra cash. There are, of course, obvious reliability questions relating to asking people what they did rather than measuring their actions.<sup>13</sup> Nevertheless, it seems striking that so many respondents would indicate plans to spend the extra, temporary cash, and it is consistent with the evidence above on how household spending responds to disposable income.<sup>14</sup>

Although there is little evidence on the subject, it is interesting to speculate on how the magnitude of the tax cut affects the propensity to consume the funds. Hsieh (2003) shows that the annual payments that Alaskan residents receive under the Alaska Permanent Fund (as dividends from the state's oil royalties) have little effect on their immediate consumption. These payments are typically large relative to the rebates and tax cuts discussed above. After examining several factors, Hsieh concludes that the effects of fiscal policy on consumption may in fact depend on the size of the transfer or tax cut, as well as the transparency of the policy. Similar results occur in analysis of households' disposition of pre-retirement lump-sum distributions (LSDs): larger distributions are more likely to be saved (Burman, Coe and Gale, 2001). In addition, respondents in at least one experiment report that they would be more likely to spend tax cuts given as

high-frequency, small increases in after-tax earnings than the same amount given as a one-time lump-sum (Chambers and Spencer, 2008). In all of these cases, the results suggest households may be more disposed to respond consistently with life-cycle planning when the amount at stake is significant, and may otherwise exhibit higher spending propensities than one would expect among households who are not liquidity-constrained.

In light of these considerations, two aspects of the tax cuts enacted in the American Recovery and Reinvestment Act of 2009 are noteworthy. First, the cuts are part of a longer-term agenda for tax change. The Obama administration has already made clear its intent to make permanent the Make Work Pay credit. Second, the tax cuts are being delivered through changes in withholding, rather than one-time, lump-sum payments. On the basis of the analysis above, both features should serve to increase the MPC out of the stimulus package.

### ***III.B. Evidence from Firm Responses to Investment Incentives***

Gross private domestic investment is a smaller share of GDP than consumption (typically averaging about 20 percent of output) but is far more sensitive to cyclical conditions and so takes on elevated significance as a potential way to stimulate the economy. Just as one can estimate the household consumption responses to temporary tax rebates, one can use changes in investment incentives to estimate the responsiveness of business fixed investment. But estimating investment responses is a considerably more challenging exercise, for at least two reasons.

First, we have few natural experiments to analyze on the investment front. As noted in Auerbach (2009), the use of investment incentives for countercyclical policy disappeared from the U.S. landscape in the 1970s and reappeared only in 2002. While there were other changes in tax policy that affected investment, notably in 1981 and 1986, these were intended as long-run changes and so would be expected to have had different impacts on investment, just as permanent tax cuts would have a different influence on household consumption than temporary ones (although with a different ordering here, temporary incentives



having a larger predicted impact than permanent ones because of the incentive to accelerate investment to qualify for incentives).

Second, specifying the behavior of investment is more difficult, both because of the interaction of different tax provisions (notably those that affect financial policy and that limit the ability of firms to utilize tax deductions) and because modeling the behavior of this very volatile component of output has proved more challenging than modeling consumption.

A series of studies has focused on the effects of tax changes on the composition of business fixed investment, primarily using panel data on firms, industries or asset categories. These studies (for example, Auerbach and Hassett, 1991, and Cummins, Hassett and Hubbard, 1994) relate changes in investment to changes in the Hall-Jorgenson user cost of capital, which incorporates changes in tax policy variables. The studies provide ample evidence that changes in the user cost of capital do influence the pattern of investment, i.e., that the mix of investment is responsive to relative changes in the user cost of capital. This literature suggests an elasticity of equipment investment with respect to the user cost ranging between -0.5 and -1.0 (Hassett and Hubbard, 2002). However, there are several issues that make the use of such estimates difficult to translate into predicted effects for the recent U.S. investment incentives.

First, the estimates are based primarily on variations from tax *reforms*, which by their nature typically involve changes to more tax parameters than do stimulus policies (making inference more difficult), are not necessarily undertaken during periods of recession, and are typically expected to be of longer duration (making inference from these events less relevant) than the various rounds of bonus depreciation. Second, the relevance of cash flow as a factor explaining investment remains unresolved, and the importance of this factor might easily be much greater during a recession, particularly the most recent one. Third, the cyclical sensitivity of net operating losses among firms, which appears to have become more significant in the past decade (Altshuler, et al. 2009), means that tax incentives provided through an acceleration of deductions against taxable income may have weaker effects in recession than in normal times. Finally, these empirical estimates are informative

primarily about the composition of investment, rather than the level of investment, with responsiveness identified in relation to differential changes in investment incentives. While one can use these estimated elasticities to make inferences about aggregate responses, there are variety of reasons why the two might differ.

There have been some attempts at estimating the actual responses of investment to the bonus depreciations incentives of 2002-2004. Perhaps the most careful study is that of House and Shapiro (2008). Using a methodology related to that already discussed, they find that the composition of investment did shift from non-qualifying investment (in this instance, most structures) to qualifying investment (equipment investment plus some shorter-lived structures). As discussed, this methodology is not designed to measure aggregate responses, although doing so would have been particularly difficult in the case of bonus depreciation, given that the incentive effects were rather small overall and hence the predicted increases in aggregate investment difficult to observe (Desai and Goolsbee, 2004).

One interesting result in the House-Shapiro analysis is that responses of investment to the 2002 introduction of bonus depreciation appeared to begin during the last quarter of 2001 and the first quarter of 2002, a period ultimately covered retroactively by the 2002 legislation. Thus, firms expected that investment incentives would be enacted and that investment undertaken during this interval would be covered. This is not entirely surprising, since proposals for investment incentives began circulating shortly after the terrorist attacks of September 11, 2001 (the date ultimately used to mark the beginning of the retroactive application period). But the announcement effect could just as easily have been perverse—it could have reduced investment in the time between 9/11 and the enactment of the law—had investor anticipations of a tax incentive not included expectations of retroactive application, for then firms would have been encouraged to await actual implementation (or at least clarification with respect to dates) before investing.

The predictability of investment incentives, then, should be of much greater concern than the predictability of individual tax rebates. While the expectation of tax rebates can help spur demand immediately, the

expectation of investment incentives can have the opposite effect. And, historically, the introduction of investment incentives is to a considerable extent predictable. This is illustrated in Table 2 and Chart 5, reproduced from Auerbach (2009). The table provides estimates of an ordered probit model explaining changes in the user cost of capital using annual data from 1962 through 2008. The dependent variable takes on three possible values, depending on whether tax policy adopted in that year increased, decreased, or neither increased nor decreased the user cost by at least 0.5 percentage points. Explanatory variables are the lagged GDP gap, the lagged federal budget surplus, and the lagged change in equipment investment, scaled by potential GDP. Chart 5 plots the actual series of policy changes against predicted changes, as measured by the difference between the probabilities of an increase and of a decrease. The table suggests that government policy changes can be predicted to a considerable extent, although there does appear to have been unusual forbearance during the period from 1987 through 2001. The years since have seen stronger reasons for policy changes but also changes that were very predictable, suggesting again that incentives to invest in the years just prior to 2002 and 2008 could have been compromised considerably had investors not expected retroactive implementation.

In summary, tax incentives affect investment, with the compositional effects more easily identified than the aggregate effects. But too little attention has been given to the announcement effects of policy and the fact that conditions governing investment in recession (particularly cash-flow constraints and tax losses) may produce quite different investment responses to temporary tax cuts than would be predicted using models based on responses to long-term tax reforms adopted under more normal circumstances.

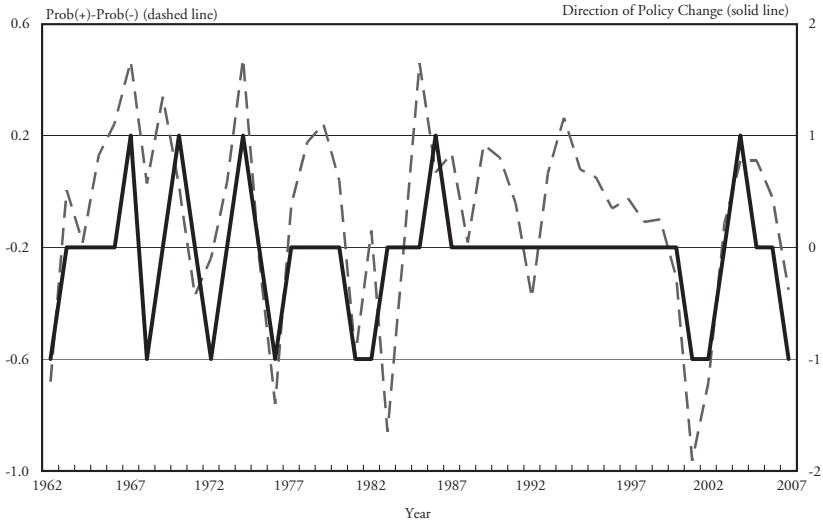
### ***III.C. Evidence from Economy-Wide Models***

Although the effects on individual components of output are of interest, perhaps the most important question regarding the effectiveness of fiscal stimulus is the effect of different policies on overall output, taking into account not only the direct effects, but also the indirect effects and private sector responses. Generally, three types of models have been used for such analysis, with differing strengths and weaknesses.

**Table 2**  
**Ordered Probit Analysis of Changes in the**  
**User Cost of Capital, 1962-2008**  
 (standard errors in parentheses)

Independent Variable	Coefficient
Constant	0.654 (0.367)
GDP Gap (-1)	-27.073 (11.303)
Surplus (-1)	-31.800 (15.212)
$\Delta$ Equipment Investment (-1)	182.978 (61.372)
Scaled $R^2$	.423
Number of Observations	47

**Chart 5**  
**Investment Policy Changes: Actual and Predicted**



*III.C.i. Three Approaches*

Large-scale macroeconomic models encompass several equations accounting for relevant prices and quantities in different sectors of the economy and relating these prices and quantities to each other and to government policy variables. Many government-reported multiplier estimates, including those of Romer and Bernstein (2009) and CBO (2009a) mentioned above, are based on such models, and they remain a workhorse for analysis within government and private industry. Many prominent large-scale models predict that the stimulus package passed in early 2009 materially affected economic growth in second and third quarters of 2009 (Council of Economic Advisers, 2009, Table 7). While the large-scale models provide considerable detail regarding the channels through which policy can operate, their theoretical grounding has been challenged.

A second approach is to estimate models of a more reduced-form nature, relating changes in output to changes in taxes<sup>15</sup> and government purchases, without specifying the channels through which the effects occur. Some of this work uses single-equation methods, but the more standard framework for this approach is the structural vector autoregression (SVAR), with the structure being provided in the form of assumptions that make possible the identification of fiscal policy shocks and their effects. Analyses using SVARs tend to find that changes in taxes and government purchases can significantly influence output, after accounting for economy-wide interactions and responses to the initial stimulus. But the implications for the current downturn are not as clear, because, as reduced-form models, SVARs cannot address the effects of policies that were not observed in-sample, nor can they address how the economic response to a given policy varies with economic conditions that were not observed in-sample.

A third approach specifies dynamic equilibrium models, such as Baxter and King (1993). More recent analyses in this vein typically include stochastic elements and are referred to as DSGE (dynamic stochastic general equilibrium) models, including recent contributions by Cogan, et al. (2009), and Leeper, et al. (2009). The hallmark of this approach is 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 DSGE models specify the full economic structure, they are able to analyze a wide range of policies and policy environments that is not limited by historical experience. As a result, there is an enormous variety of estimates of the impact of fiscal policy on economic activity. The DSGE approach, however, leans heavily on modeling assumptions that may or may not be valid regarding the stickiness of wages and prices, the prevalence of liquidity constraints, the rationality of agents, and so forth.

Because of these differences in approach and in strengths and weaknesses, it is useful to compare the predictions obtained from different approaches. Where predictions are similar, we can have more confidence in them, and when predictions differ, we can look further into the sources of such differences to help us decide where the strength of the evidence lies. In our discussion below, we focus on the estimates of some key policy issues from the SVAR literature and relate these to findings from analyses that employ DSGE methods.

### *III.C.ii. Effects of Taxes and Purchases on Output*

The SVAR augments the simple VAR methodology of regressing output and other aggregates on lagged values of themselves and policy variables of interest with some method of identifying policy shocks, changes in current policy variables that are attributable to actual changes in policy rather than to endogenous responses to economic conditions. Thus, the literature has focused on both the SVAR specification and the choice of identifying assumption in attempting to obtain more convincing multiplier estimates.

An important early contribution in the 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 because of 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 a year.

As noted above, a central concern with SVARs is the identification of policy shocks. For example, a change in taxes or spending identified by the Blanchard-Perotti methodology as a policy shock might have been anticipated by individuals (even if not by the economic model), or it might not have been a policy change at all (for example, because it might represent the phase-in of policy enacted earlier). 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 buildups as an important source of variation in government purchases, Ramey and Shapiro (1997) are able to estimate the impact of these buildups on GDP and its other components. More recently, Ramey (2008) 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.

In a recent review of this and other recent papers in the literature, Hall (2009) concludes that the GDP multiplier of government purchases is probably at least 0.5 when identification is based on military buildups, and that a range of 0.5 to 1.0 is consistent with the evidence from the literature that uses the alternative identification approaches discussed above. After reviewing the alternative DSGE modeling approaches that might be reasonable to adopt, he concludes that plausible DSGE models can generate results within this range. One important implication of this conclusion of a

multiplier below 1.0 is that other components of GDP fall in response to the increase in government purchases.

On the tax side, the narrative approach to identifying policy shocks has been introduced by Romer and Romer (2007), who used the same approach 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 after four quarters but rising to 3 after 10 quarters. This very large multiplier is associated with an enormous impact on investment. While the result is striking, it is one that merits further investigation.<sup>16</sup>

### *III.C.iii. Limitations of SVARs*

Although the narrative approach probably yields 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 the model—especially if the goal is to apply historical estimates to the current downturn. 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. The effects of both types of policies are “baked in” to the coefficient estimates themselves in some unspecified manner.

Second, SVARs can measure only the multipliers of activist 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—with respect to the firms or households targeted, or with a different mix of income and substitution effects—from those examined in-sample could well have different multipliers than those estimated. This concern is



especially important 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 the Korean War, when taxes were raised and defense spending rose permanently as the Cold War began (Hall, 2009). As a result, they do not necessarily apply very well to the recently-enacted temporary increases in non-defense spending, which were accompanied by reductions in taxes and took place in a severe economic downturn.

Third, SVARs can only estimate the effects of policy interventions under the economic conditions prevailing within the sample, and it is worth emphasizing again that 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 impact during periods in which liquidity constraints bind more tightly. Government spending might have larger multipliers during periods, like the present, when the zero-interest rate bound is binding, as suggested by recent analyses based on DSGE models (Eggertsson, 2008a; Christiano, et al. 2009).<sup>17</sup> But fiscal expansions may have smaller impacts during periods, again like the present, when long-term budget shortfalls are salient. The upshot of these concerns is that relying on truly exogenous changes in government purchases and taxes, as in the narrative approach, limits our ability to estimate such state-dependent multipliers, because it rules out estimating the effects of policies adopted explicitly as countercyclical measures.

### *III.C.iv. Short-Term Stimulus with Long-Term Deficits*

Although reviewing the vast literature on fiscal impacts is beyond the scope of this paper, in this section we highlight an issue that is particularly relevant in the current economic situation—how the presence of long-term deficits alters the efficacy of fiscal stimulus. There are many reasons to think fiscal policies would have different effects if they are adopted during a period of fiscal stress than they would otherwise. There is an extensive literature, consisting of both theoretical and empirical contributions, arguing 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. Indeed, this notion was a cornerstone of what has come to be called "Rubinomics" during the Clinton administration in support of a sustained program of fiscal discipline.

The empirical evidence, based on panel data for OECD countries, does suggest that fiscal consolidations have less contractionary effects 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 indicates 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). One channel through which the differing effects of fiscal policy under different initial conditions may occur is through expectations of how the deficit resulting from a stimulus will be closed in the future. Several recent papers utilizing the DSGE modeling approach address this issue with mixed results (Corsetti, et al. 2009; Davig and Leeper, 2009; Leeper, et al. 2009).

Thus far, this literature identifies fiscal shocks without reference to the type of auxiliary information utilized in the narrative approaches of Romer and Romer (2007) and Ramey (2008), and so the interpretation of the resulting multipliers is subject to the same caveats that apply to those from the basic SVAR literature. That is, we do not know what information these fiscal shocks provided to agents in the economy. Moreover, we have little information about situations like the United States now faces, in which the current debt-GDP ratio is not extraordinarily high by international standards, and the most serious fiscal problem involves very large unfunded liabilities that will not show up in government spending for several years.

#### **IV. Two Case Studies: The Great Depression and the Lost Decade**

Most of the evidence discussed above comes from what might be deemed typical downturns. At the risk of oversimplifying, we can

say that the typical U.S. post-war recession was induced largely by Federal Reserve tightening, was relatively short in duration, and did not involve the collapse of financial intermediation, the presence of nominal interest rates near zero, or deflation. The experiences of the U.S. economy in the 1930s and the Japanese economy in the 1990s were quite different, however. In each case, the bursting of an asset bubble followed by weakening in the financial intermediation system (among other things) led to deflation and a prolonged decline in output, punctuated by nominal interest rates approaching zero. As we have emphasized above, the size and timing of multiplier effects are likely to be sensitive to the structure and state of the economy, other contemporaneous policies, expectations, and other factors. As a result, analysis of the effects of fiscal policy in the Great Depression and the Lost Decade can provide additional evidence beyond the consumption, investment, and SVAR literatures surveyed above. Lessons from these two historical episodes may be particularly timely given their structural similarities to the current downturn.

#### ***IV.A. The Great Depression***

The Great Depression actually consisted of two sharp downturns with recoveries after each. From 1929 to 1933, the economy contracted strongly: Output fell by 35 percent and unemployment rose by 22 percentage points to 25 percent. Starting in 1933, a strong recovery ensued; real GNP grew by 33 percent through 1937, at which time unemployment had fallen to 14 percent. In 1938, the economy fell back in recession, with output dropping by 5 percent and unemployment rising to 19 percent. Beginning in 1939, a spectacularly strong recovery occurred, with output growing by 49 percent through 1942, at which time the unemployment rate had fallen to less than 5 percent, the economy had returned to full employment, and the massive U.S. mobilization for World War II was well underway. Looking at the period as a whole, output did not return to its 1929 level until 1937, after which it dropped again, and did not attain its pre-1929 trend level until 1942.<sup>18</sup>

Sorting out the role of fiscal policy in these remarkable economic gyrations is not a simple task, in part because so many other things, including the state of the economy and the stance of monetary policy,

were changing over time as well. To control for the state of the economy, Brown's (1956) classic analysis focuses on the role of federal and state government fiscal policy in stimulating aggregate demand.<sup>19</sup> He finds that in only two years in the 1930s—1931 and 1936—was total (federal, state, and local) government's net contribution to aggregate demand more than 0.6 percent of potential GDP larger than it had been in 1929. Each of these episodes coincided with one-time payments to veterans, not to systematic efforts to boost aggregate demand. Moreover, each of those two years was followed by severe fiscal tightening. Between 1931 and 1933, the contribution of fiscal policy to aggregate demand decreased by over 3 percent of potential GNP. The federal government raised income tax rates in the Revenue Act of 1932, while state and local governments cut purchases and raised taxes further. Between 1936 and 1937, fiscal policy's net impact on demand decreased by 2.5 percent of potential GNP, mainly due to federal tax increases—for Social Security and on undistributed profits—and the end of the veterans' payments enacted in 1936. By the end of the decade, even with output well below potential and the unemployment rate at 17 percent, the contribution of fiscal policy to aggregate demand in 1939 was just 0.6 percentage points larger than in 1929.

One important result from Brown's work is the small magnitude of the fiscal expansions, relative to the drop in output. As noted, output fell by 35 percent from 1929 to 1933. Hence, even if fiscal policy expansions were assigned a large multiplier of 2 (or 4), the economy would have needed deficits equal to an additional 18 (or 9) percent of GNP just to restore 1929 GNP levels during the depths of the Depression. These factors led to Brown's oft-cited conclusion that "Fiscal policy, then, seems to have been an unsuccessful recovery device in the 'thirties—not because it did not work, but because it was not tried." A second, perhaps even more striking result is that fiscal tightening occurred during the two recessions in the 1930s and thus likely contributed to those downturns, especially the 1936-38 episode.

Both of these findings—the second in particular—suggest the importance of continuing concerns about the fiscal outlook as a constraint on stimulus policies. Indeed, even after the sharp downturn in

1929 and 1930, President Hoover continued to try (albeit unsuccessfully) to balance the budget, and Franklin Roosevelt ran for president in 1932 on a balanced budget platform, which was later abandoned after he took office (DeLong, 1996). Congress approved veterans' payments in 1931 and 1936 over strong opposition from Presidents Hoover and Roosevelt. The tax increases were partially efforts to restore fiscal discipline and likely drove the economy back into recession.<sup>20</sup> Although overall government spending did in fact rise more or less continuously and significantly over the decade, from 9.1 percent of GNP in 1929 to 14.5 percent in 1939, the reason fiscal stimulus on net was so small and was sometimes negative was that taxes rose as well, from 8.3 percent of GNP in 1929 to 11.4 percent in 1939, with particularly large federal tax increases in 1932 and 1937.

While most of the research literature has subsequently focused on the New Deal and other federal initiatives, Brown's work also highlights the important status of state and local governments in the conduct of aggregate fiscal policy. Subfederal governments provided the majority of government spending during the 1930s, but not only did they not help recovery, they likely hurt the economy. Between 1931 and 1933, the impact of state and local government budgets on the economy declined by 0.5 percent of GDP, which probably helped to exacerbate the already steep downturn. State and local governments ran essentially balanced budgets the rest of the decade, providing almost no net stimulus during the 1933-1942 period. Federal spending accounted for almost all of the increase in spending over the period, rising from less than 2 percent of GNP in 1929 to 6.5 percent in 1939.<sup>21</sup>

While there is no controversy regarding the numbers in Brown's (1956) study, there are at least three prevailing views of the role fiscal policy played in ending the Depression. Romer (1992) attributes virtually no role to fiscal policy. She finds that the recovery from 1933 to 1942 can be explained almost completely by an increase in the money supply (due to gold inflows and other factors) that helped refuel aggregate demand. Her estimates suggest that fiscal policy (as measured by the ratio of the real federal surplus to real GNP) was

not very expansionary and that the stimulative effect of fiscal policy changes was small.

Vernon (1994) provides evidence for an intermediate role for fiscal policy—namely, that while fiscal policy did little to help the economy through 1939, the massive fiscal mobilization involved in the buildup to the Second World War played a key role in bringing the economy out of the clutches of the Depression. He notes that less than half of the overall recovery in output from 1933 to 1942 had been accomplished by 1940 and that fiscal policy—in particular, a pre-war run-up in defense spending—explains the lion's share of output growth from 1940 to 1942.

Eggertsson (2008b) offers a significantly more sanguine view of fiscal policy, operating through an expectational channel, dating to the beginning of the recovery in 1933. He argues that in addition to implementing a variety of policies, Roosevelt also effectively changed people's view of the underlying policy paradigm to allow for deficit spending, inflation, and abolition of the gold standard. In Eggertsson's model, this change in the policy paradigm was seen as credible (the rise in the deficit made future monetary expansion credible, for example) and hence made people more likely to spend their funds or invest productively rather than hoarding cash. This effect occurs above and beyond any direct effects of tax cuts and spending increases. He estimates a multiplier of 3.4 for government spending, given the change in monetary policy and credible shifts in the underlying policy paradigm. As a result, even the relatively small changes in fiscal policy as noted by Brown could have significant effects. This work is consistent with other analyses that find that, when the interest rate is near zero, the government spending multiplier can be large (Eggertsson, 2006, 2008a; Christiano, et al. 2009).<sup>22</sup>

#### ***IV.B. The Lost Decade***<sup>23</sup>

After growing at real annual rates exceeding 4 percent from 1970 to 1991, spurred by high rates of saving and investment, the Japanese economy fell into a lengthy downturn that began during the second half of 1991 and continued through 1996. As in the Great Depression, a decline in aggregate demand was fueled by collapsing markets

for housing and stocks and a poorly functioning financial sector. In addition, Japan experienced overcapacity in key industries and an excess of saving in a traditionally relatively closed economy. As a result, credit contracted, investment collapsed, and nominal interest rates approached zero. Output growth averaged less than 1 percent per year, and Japan suffered the first sustained deflation in an industrial economy in the post-war period. By 1996, with the recession seemingly ending and the economy enjoying a 3.6 percent growth rate, the government raised taxes. The economy then turned down again, due in part to the fiscal tightening, and roughly coincident with the onset of the Asian financial crisis. This led to stimulus packages in 1998 and 1999 and a short-lived recovery. A significant recovery did not begin in earnest in 2003, however, after real output had grown at just 1.4 percent per year during the previous decade.

Kuttner and Posen (2001, 2002) and Posen (1998, 2004) argue that strong, sustained fiscal stimulus would have been especially appropriate in Japan during the 1990s, which was characterized by low interest rates, excess capacity, an abundance of cash and saving, and little demand for new investment. In these circumstances, monetary policy may well prove ineffective, and the impact of fiscal policy on aggregate demand could be substantial, while the drawbacks—the potential crowd-out of private investment—are small.

Although there was significant potential for expansionary fiscal policy, and although the conventional wisdom is that Japan stimulated aggressively, Posen (2004) notes that “Japanese fiscal policy remains widely, and occasionally wildly, mischaracterized.” The first issue, then, is characterizing Japanese fiscal policy during this period. Despite the conventional wisdom, Japanese fiscal expansion efforts were inconsistent, smaller than commonly thought, and undercut by a variety of factors.

In response to the initial and continuing downturn, Japan enacted a series of stimulus packages, including public works packages in 1992 and 1993, and a temporary income tax reduction in 1994. However, the stimulus packages were simply not that large—amounting to a combined 2 percent of GDP—and the 1994 income tax reduction was introduced in a way that would likely substantially reduce its



impact—it was explicitly described as a temporary tax cut that would not only expire in 1997 but would be followed by tax increases.

The income tax reduction did in fact end in 1997, accompanied by a 2 percentage point increase in the value-added tax (VAT) rate (from 3 percent to 5 percent), an increase in contribution rates for Social Security, and increases in medical copayments. The 1997 tax increases were explicitly efforts to control the medium- and long-term fiscal outlook, were based on an optimistic economic outlook, and were further justified by notions of expansionary fiscal consolidation, discussed above, and perhaps motivated in part by the American experience of raising taxes and cutting spending in 1993. The total fiscal tightening amounted to about 2 percent of GDP. However, the VAT rate increase on July 1, 1997, induced significant intertemporal substitution of consumption into the second quarter of 1997 and then “the economy fell off a cliff” (Fischer, 2001). This led to the introduction of two relatively large fiscal packages in 1998 that included both increases in spending on public works and permanent tax reductions for corporations and high-income individuals. In November 1999, another public works package was enacted.

Fiscal policy efforts were marked not only by the inconsistent patterns noted above, but by several other factors as well. First, Japan’s *announced* public spending on stimulus was much larger than what was actually implemented.<sup>24</sup> The 1992-93 public works projects, for example, were announced to be 5 percent of GDP but turned out to be only 2 percent. Implementation and coordination issues in the relationship between central and local governments in Japan are partly to blame; in particular, many of the public works spending projects required partial local government funding that did not come through (Ishii and Wada, 1998). Coordination issues at the central government level also played a role, with Japanese bureaucrats playing an important role in blocking the government’s intentions.<sup>25</sup>

Second, many of the provisions that were announced would not be expected to stimulate demand, or were already in the budget, and therefore not new. For example, the two 1998 fiscal packages are officially reported as totaling 10 percent of GDP, but experts indicate that only about 4 percent of GDP was “really” new stimulus. These



differences are an artifact of a complex and opaque budgeting system in Japan.<sup>26</sup> Third, although it is difficult to find hard data, conventional wisdom suggests that the public investment projects undertaken with stimulus funding were notoriously wasteful and hence less productive than they might otherwise be.<sup>27</sup>

In light of the analysis above, one may wonder about the source of the conventional wisdom that Japan aggressively stimulated. Kuttner and Posen (1998) attribute the confusion to the rapid and substantial rise in deficits in the early 1990s, which they report was interpreted as evidence of fiscal expansion by several prominent observers. They show, however, that the increase in deficits is due almost entirely to the automatic decline in revenue caused by the reduced economic output, not to aggressive discretionary stimulus.

Although fiscal policy was not tried on a consistent basis, it does appear to have had stimulative effects when employed, at least over the longer historical period for which estimates have been produced. Kuttner and Posen (2001, 2002) use a SVAR to assess the impact of fiscal policy on economic growth in Japan. They find that both tax cuts and spending increases are expansionary, with multipliers for Japan that are about as large as the multipliers estimated by Blanchard and Perotti (2002) for the United States, when calculated in the same manner, and with multipliers for tax cuts somewhat higher than for spending increases. Bayoumi (2001) also presents SVAR results for Japan, finding positive but short-lived effects of spending expansions, and positive, small, but longer-lasting effects of tax cuts.<sup>28</sup>

Thus, the main conclusions from the Lost Decade are that fiscal policy in Japan may have been effective as stimulus when it was tried, but it was not tried in a strong and consistent manner—in part due to concerns about the fiscal outlook and in part because of confounding effects that played out at the level of subnational governments or within the central government. The parallels with the conclusions in Brown (1956) about the 1930s are noteworthy.

## **V. Conclusions**

In response to the recent, sharp downturn in economic activity, the U.S. federal government—as well as other governments around the

world—enacted substantial fiscal stimulus. These policies continue a recent trend toward activist federal interventions, at least in the United States. Both the recent trend and the stimulus policies enacted this year highlight a number of key issues that the economics profession has addressed to varying degrees for decades.

Our most fundamental conclusion is that, despite the large literature on these topics, the state of theory and evidence is not as “shovel-ready” as some of the stimulus projects were said to be. But much progress has been made. We have presented evidence that countercyclical policy has in fact adjusted within a time frame that is relevant for stabilization purposes, and that stimulus policies can manipulate household consumption expenditures and business investment in ways that have significant macroeconomic impact on a timely basis. While it is true that these impacts will vary according to the nature of the policies and the state of the economy, the general finding that well-designed tax cuts can boost consumption and investment seems robust.

While knowing the direct effects of taxes (or spending) on output is helpful, a critical but much less clear set of information surrounds the indirect effects, taking into account economy-wide expectations, reactions, and interactions. Results from the SVAR literature imply that tax and spending changes have significant impact on the economy, albeit with some crowding out of other economic activity. The implications of these reduced-form results, however, are unclear and difficult to apply to the exceptional events and the new policies enacted during the last year. In contrast, DSGE models can be adapted to address the current economic environment and current policies, but in so doing, the models generate an enormously wide range of multipliers, from the essentially zero estimate provided by Cogan, et al. (2009) to estimates in the range of 3 to 4 provided by Christiano, et al. (2009). More generally, relatively small changes in parameter specification—within ranges that can not be ruled out by the empirical evidence—are capable of producing substantial shifts in estimated multipliers in the DSGE approach (Hall, 2009).

An alternative source of information is the fiscal policy experience in the Great Depression and the Lost Decade. The remarkable fact is that sustained fiscal policy expansion was not attempted in either

episode. This could have been due to many factors, including putting concerns about the status of the budget ahead of concerns about the status of the economy, or believing that the best way for the government to help would be to keep the fiscal house in order. If so, at the very least, one conclusion would be that attempts to balance the budget during those episodes did not succeed either in balancing the budget or in avoiding a prolonged, severe downturn. There is some evidence that fiscal policy in Japan was successful, when it was tried, but it is subject to the same caveats as those for the SVAR literature in general. Another interesting result from the two episodes is the extent to which subnational governments undid some of the effects of whatever federal stimulus did exist. All of these results have implications for current stimulus policies.

With these findings in place, what can be said about the American Recovery and Reinvestment Tax Act (ARRA) of 2009? We divide this issue into two parts: Was a fiscal stimulus appropriate, and was ARRA the right fiscal stimulus? In addressing these questions, we note that the stimulus package has already been and will continue to be the subject of extensive research. While one should be able to offer clearer conclusions with the benefit of more hindsight, policymakers need to reach conclusions on such questions even when—in fact, especially when—the economics literature has not provided the final word on the topic. With that caveat, our provisional answers are “yes” and “sort of.”

It is fair to say that if a fiscal stimulus were ever to be considered appropriate, the beginning of 2009 was such a time. Well over a year into the longest recession since the Depression, with several millions jobs lost, nominal interest rates at zero, fears of deflation, no signs of life in the major components of GDP, and the rest of the world in recession and flooding the United States with capital, a fiscal expansion carried much smaller risks than the lack of one would have.

As to the structure of the package, a large, diversified, phased-in stimulus was the right approach: large because the economy was in dire straits, diversified because there is uncertainty about the size of the multipliers attached to different parts of the package, phased-in because it is hard to implement everything all at once and there is a long way to go to get back to full employment in any case.

Measured against the standard of “timely, targeted, and temporary,” ARRA gets reasonably good marks for being timely. About three-quarters of the effects take place in the first 18 months, a period during which the economy is expected to remain far below full employment. While a faster implementation might have been desirable, the biggest avoidable delay probably was at the enactment stage, in which a lame-duck president and Congress deferred action for months even after the likelihood of intervention became high.

The stimulus package was certainly not as well-targeted as it could have been, but there was some logic to its structure. The package was approximately one-third tax cuts, one-third aid to states and individuals, and one-third government investments. The tax cuts will stimulate aggregate demand, but could have been designed more effectively. The aid to individuals was based on humanitarian needs. The aid to states was well-advised. Because essentially all states adhere to some form of balanced-budget rule, economic declines that reduce state revenues force cuts in state spending. From the perspective of macroeconomic stabilization, reducing public spending during a sharp downturn is counterproductive. The aid provided should offset some of the state and local spending cuts that would otherwise have occurred. The fact that state and local government spending and employment rose in the second quarter of 2009 is consistent with the view that the transfers have helped. Government investments were part of a longer-term Obama administration agenda and are probably not best evaluated solely as stimulus.

Aside from the particular provisions of the Act, though, there is a more general manner in which the stimulus may well have helped. Specifically, while the economy was in “free fall” in late 2008 and early 2009, the Obama administration and the Federal Reserve Board offered clear and strong statements that they would not stand by idly while the economy collapsed. This concerted and consistent display of intention may well have created favorable expectations among households and firms, giving them more confidence to spend and invest than they otherwise would have.

Even if the stimulus has been successful, and especially if it has not, policymakers will face a series of difficult fiscal policy choices in the near term. The deficit is expected to decline from about 11 percent of GDP in fiscal year 2009 to 4.5 percent in 2013 under President Obama's budget (Auerbach and Gale, 2009). The decline is due to the ending of the stimulus package, the ending of the financial bailout, and the overall recovery of the economy. However, if the economy recovers more slowly than anticipated, deficits could stay very high for a very long time, and even if the economy recovers as CBO forecasts, the deficit will rise to almost 6 percent in 2019, even after four years of full employment, and will continue to rise thereafter as a share of the economy.

As a result, policymakers will need to decide when to cut off stimulus and start imposing fiscal discipline. Cutting off stimulus too soon could plunge the economy into a new downturn, as happened to the United States in 1937 and Japan in 1997. Letting stimulus run for too long could ignite investors' fears and create a "hard landing" scenario, a scenario considered plausible by Ball and Mankiw (1995) and Rubin, et al. (2004) under much more sanguine conditions than exist today.

It is unclear whether the stimulus package will remain temporary. As legislated, almost all of the provisions expire by 2011, but there is likely to be significant political pressure to extend certain features, in particular, aid to the states. In addition, some of the tax cuts and government investments were explicitly designed to be part of the Obama administration's longer-term agenda.

Is another stimulus needed? In light of the history from the Great Depression and the Lost Decade, it seems likely that policymakers will under-provide fiscal stimulus in a major, prolonged crisis. But this conclusion is based on ex post analysis, once the full severity of these crises was known. As this paper is written, the U.S. economy is showing signs of recovery, and the addition of another stimulus package at this point could turn out to be poorly timed.

Whether additional action is taken in the coming months, it seems likely that discretionary fiscal actions will play an enhanced role in

policy discussions and research activities in the future, given the last decade's increase in fiscal activism and continuing concerns about the state of the economy and the pace of any eventual recovery. There is certainly much more work to be done to understand the key issues, including how to conduct fiscal expansions while addressing long-run fiscal imbalances. Another key issue is the potential use of behavioral or other non-standard incentives in designing effective stimulus. The recent popularity of the "Cash for Clunkers" program, which as a constrained, in-kind payment might have been expected to be significantly less popular than a cash transfer, suggests the role that creative thinking can play in designing more-effective stimulus policies.

## Endnotes

<sup>1</sup>Automatic stabilizers are substantially smaller in the U.S. than in most other OECD countries. As a result, while the U.S. had the largest discretionary stimulus package, the combined effects of its automatic and discretionary policies on the government's budget for 2008-10 as a share of GDP are the sixth largest in the OECD (OECD, 2009).

<sup>2</sup>See, for example, Eichenbaum (1997) and Taylor (2000). For more recent cautious statements of an opposing view, see Blinder (2006) and Blanchard (2006).

<sup>3</sup>Examples include a change in revenues due to a stock market decline or a change in the income distribution.

<sup>4</sup>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.

<sup>5</sup>The 2002 paper included the lagged budget surplus instead of the projected surplus, but subsequent evidence (Auerbach, 2003) suggested that the latter provides a better fit.

<sup>6</sup>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.

<sup>7</sup>The 2002 paper included the first two observations under George W. Bush along with those for the Clinton period.

<sup>8</sup>These criteria were put forward by Summers (2007), among others.

<sup>9</sup>Technically, the 2001 lump-sum payments were advance credits against 2001 tax liabilities rather than rebates of past tax payments.

<sup>10</sup>The one recent study that does not find evidence of liquidity constraints creating differential MPCs is Souleles' (2002) analysis of the Reagan tax cuts. One caveat in making this interpretation, though, is that the estimated MPC is so high that differences between borrowing-constrained and other households may simply not be very large in this particular example.

<sup>11</sup>Evidence from Japan is consistent the first and third stylized facts (Watanabe, et al. 2001).

<sup>12</sup>See Congdon, et al. (2009) for a general discussion of behavioral factors and tax policy. For evidence on how saving responds to cues or presentation issues, see Madrian and Shea (2001), Saez (2009), Thaler and Sunstein (2008), and Tufano and Schneider (2009).

<sup>13</sup>Broda and Parker (2008) report that those respondents in their survey who reported that they did spend most of the rebate spent about twice as much out of the rebate as those who said they mostly saved the rebate or used it to pay down debt. This is not quite the same thing as reporting consistency between what people say they will do with the funds and what they actually do, but it provides some reassurance. In addition, Shapiro and Slemrod (2009) report that there were no significant differences in answers provided in the period before the rebate was mailed and the period when it was mailed, providing an additional robustness check.

<sup>14</sup>Shapiro and Slemrod (2003, 2004, and 2009) have used similar methodology to ask respondents about the disposition of their 2001, 2003, and 2008 tax cuts, respectively. The results in each case are that around 20 percent of households say they will consume the rebate. Typically, paying down debt is the modal response, followed in frequency by saving the money and then spending the funds. None of the results show a pattern by income class that suggests that borrowing constraints are affecting the results. Coronado, et al. (2005) undertake a similar study of the 2003 tax cut.

<sup>15</sup>Typically in this literature, taxes are defined on a net basis, as revenue less transfer payments.

<sup>16</sup>Indeed, one recent paper (Favero and Giavazzi, 2009) suggests that the multipliers for the tax shocks identified by Romer and Romer are considerably smaller if one models the shocks as explanatory variables in a multivariate model rather than simply regressing output on the tax shocks. The source of this difference is not clear, although the authors suggest that their results reject the assumptions by Romer and Romer that such shocks are independent of other explanatory variables.

<sup>17</sup>Although these models are more sophisticated, they echo the logic from simpler Keynesian models regarding the effectiveness of expansionary fiscal policy in a liquidity trap. Eggertson also argues that a tax cut would be less expansionary in the zero-bound case, in fact having a negative effect on output, because their positive supply-side effects could have deflationary consequences. But this conclusion would not apply to most of the tax cuts adopted in the 2009 stimulus bill, which were largely inframarginal in their effects.

<sup>18</sup>We rely on Romer (1992, 1993) for historical GDP growth rates and Lebergott (1964) for historical unemployment rates.

<sup>19</sup>DeLong (1996) offers cautionary comments about the reliability of the calculations involved in developing full-employment budgets when the economy is as far away from full employment as it was throughout most of the 1930s.

<sup>20</sup>Romer (2009) states: "The results of the fiscal and monetary double whammy in the precarious environment were disastrous.....Policymakers soon reversed course and the strong recovery resumed, but taking the wrong turn in 1937 effectively added two years to the Depression." Eggertsson and Pugsley (2006) disagree, arguing that the 1937 downturn was instead due to confusing policy communication by policymakers.



<sup>21</sup>Throughout this section, federal and subfederal budget estimates are based on the authors' calculations of findings presented in Brown (1956).

<sup>22</sup>Another implication of the model is that the National Industrial Recovery Act (NIRA) of 1932, which suspended antitrust laws and gave unions increased bargaining power, helped the recovery by reinforcing the expectation that prices and wages would rise (Eggertsson, 2008c). In contrast, neoclassical analysis of NIRA implies that, by reducing competition on the price and wage margins, the Act retarded the pace of recovery. See, for example, Cole and Ohanian (2004).

<sup>23</sup>See Bayoumi (2001), Horioka (2006), Krugman (1998), and especially Posen (1998) and Kuttner and Posen (2001) for analysis of the Japanese economy and policy responses in the 1990s.

<sup>24</sup>McKibbin (1997) argues that this pattern reduces the net effect of the package because forward-looking financial markets will respond to the expected pressure on interest rates and exchange rates based on the announcement, but spending will only respond to the actual, eventual stimulus that is provided. Alternatively, one could argue that the large announcement (again, if credible) increases confidence and hence spurs increases in spending in and of itself, along the lines elaborated by Eggertsson (2008b).

<sup>25</sup>For example, as Patterson and Beason (2001, page 501) note: "In April 1993... when the Miyazawa government announced its second economic stimulus package, MOF vice minister Ozaki Mamoru stated that '[t]he Ministry of Finance will resist to the end' going into heavy debt again."

<sup>26</sup>See McKibbin (1997) and Patterson and Beason (2001). Due to the peculiarities of the Japanese budget system, it is notoriously difficult to determine the magnitude and stimulative impact of Japan's fiscal stimulus packages. This difficulty has led to the frequent use of the term *mamizu*, or "clear water," to describe the proportion of stimulus spending that is actually stimulative in nature.

<sup>27</sup>For example, Posen and Kuttner (2001, page 129) report that "As many observers have stressed, traditional public works in Japan more closely approximate the building of pyramids in hinterlands, famous to macroeconomic undergraduates, than do those in any other OECD country. Some have indicated that they would expect the multiplier on such wasteful expenditures to be less than one."

<sup>28</sup>Concerns about overly tight Japanese fiscal policy continued after the end of the Lost Decade, even as the economy was recovering in 2003 and 2004. See Posen (2004) and Iwamura, Kudo, and Watanabe (2005).

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