Searching for Non-Monotonic Effects of Fiscal Policy: New Evidence

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Data revisions and the availability of a longer sample offer the opportunity to reconsider empirical findings that suggest that in the OECD countries national saving responds non-monotonically to fiscal policy. The paper confirms that the circumstance most likely to give rise to a non-monotonic response of national saving to a fiscal impulse is a "large and persistent impulse," defined as one in which the full employment surplus, as a percentage of potential output, changes by at least 1.5 percentage points per year over a two-year period. This particular circumstance remains the only statistically significant one even when we allow for non-monotonic responses to arise when public debt is growing rapidly or interest rate spreads are widening. We find that non-monotonic responses are similar for fiscal contractions and expansions. In particular, an increase in net taxes has no effect on national saving during large fiscal contractions or expansions. For government consumption there is a large, albeit in some specifications less than complete, offset during expansions or contractions.

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I. Introduction

The idea that fiscal consolidation can bear fruit in the short term is controversial. Some time ago, the *Financial Times* described the situation thus: "In one corner we have the political left, armed with a multiplier, and in the other we have the right, armed with a Laffer curve. The left insists that increased public spending boosts output via demand—the famous multiplier effect. The right asserts that tax cuts and curbs on public spending stimulate private sector effort via supply."

Commenting on this remark, Rodrigo de Rato, managing director of the International Monetary Fund (IMF), appropriately observed that the subject is much more complicated than that: "It is true that the starting point should be the standard Keynesian tenet that fiscal adjustment is contractionary. However, some years ago, in a surprising departure from this orthodox stance, Giavazzi and Pagano (1990) pointed to Denmark and Ireland in the 1980s as examples of expansionary fiscal contractions, as their respective fiscal adjustments were followed immediately by an increase in growth. The explanation of these cases was firmly rooted in the tradition of the turnaround in rational expectations. The argument is that a decisive policy for reducing both the fiscal deficit and high levels of indebtedness can shore up market confidence and create expectations among the public about future income."[1]

The possibility that fiscal contractions may be a source of economic growth immediately attracts those who doubt the effectiveness of traditional Keynesian fiscal policies, arguing that many empirical studies have shown the limited magnitude of fiscal multipliers, and these critics point to many instances, above all in the case of Japan, where the response of the economy to a fiscal expansion has been weak or nonexistent. But how common are expansionary fiscal contractions, or contractionary expansions, and when do they occur? This question has induced many institutions, including the IMF, the Organisation for Economic Co-operation and Development (OECD), and the European Commission (EC), to take a closer look at such episodes, in particular of expansionary fiscal contractions.

In previous work, we search for the circumstances in which the private-sector response to fiscal policy impulses is non-monotonic (Giavazzi and Pagano [1990, 1996], and Giavazzi, Jappelli, and Pagano [2000], hereafter referred to as GJP). The results drawn from the experience of OECD countries show that a non-monotonic response is more likely to arise when fiscal impulses are large and persistent and that non-monotonic effects are larger and more precisely estimated for changes in net taxes than for changes in public consumption.

In this paper we reconsider, extend, and update the evidence analyzed in GJP. There are several reasons to extend and update our previous results. First of all, the data used in GJP ended in 1996. Eight more years of data have since become available, and these include several new episodes of fiscal contractions and expansions: among them, the fiscal contractions in Europe to meet the Maastricht criteria in the run-up to European Monetary Union (EMU) and the subsequent fiscal expansions in 2000–02; the Japanese fiscal expansion that lasted through most of the 1990s; and

1. De Rato y Figaredo (2004).

the U.S. fiscal expansion during the first Bush administration. By extending the sample, we are able to increase from 109 to 128 the number of episodes characterized by a "large and persistent fiscal impulse," defined in GJP as one in which the full employment surplus, as a percentage of potential output, changes by at least 1.5 percentage points per year over a two-year period. The updated sample includes five more "large and persistent" contractions and 14 more "large and persistent" expansions with respect to GJP.

Secondly, since GJP was written, the OECD has revised backward some of the relevant series. In particular, its measure of the cyclically adjusted primary budget surplus has changed significantly, as the correlation between the old and the new series between 1970 to 1996, the original sample, is 0.874. Some of the episodes of large and persistent fiscal impulse considered in GJP have thus disappeared from the sample, replaced by a few new ones. The series for potential output has also been revised, as have other variables. It is thus interesting in itself to check whether the original results survive when using the revised data.

Finally, we check the robustness of the results by allowing for a different source of non-monotonic response of national savings to fiscal impulses: the possible role of the risk premium on government bonds—either exchange rate risk premium or default risk. This channel is suggested by the findings in Ardagna (2004) and by models of debt default (Blanchard [1990]) and liquidity (Caballero and Krishnamurty [2004]).

Our findings suggest, in a nutshell, that the hypothesis of a non-monotonic response of national savings to fiscal impulses is confirmed in the updated and revised OECD dataset. They also confirm that the circumstance most likely to give rise to a non-monotonic response of national saving is a "large and persistent fiscal impulse," defined as one in which the full employment surplus, as a percentage of potential output, changes by at least 1.5 percentage points per year over a two-year period. On the other hand, fiscal impulses that are "relatively small" tend to be associated with Keynesian effects.

The rest of the paper is organized as follows. We survey the relevant literature in Section II, present the data in Section III, report the econometric estimates in Section IV, and summarize the results in Section V.

II. What Have We Learned from the Analysis of Large Fiscal Adjustments?

A recent study by the EC covering 14 European Union (EU) countries in the period 1970–2002 finds that roughly half of the 49 episodes of fiscal consolidation they identify have been followed by an acceleration in growth (Giudice, Turrini, and in't Veld [2003]). This result is robust with respect to the criteria used to identify the consolidation episodes and to classify such episodes as expansionary. Using the EC's macroeconomic model (QUEST) to understand the mechanisms that could give rise to an output expansion, the study concludes that the source of non-Keynesian effects mostly lies in the response of private consumption to expected future income.

Similar work at the Central Bank of Poland (Rzonca and Cizkowicz [2005]) looks at seven episodes of strong fiscal adjustment (in this case, both expansions and contractions) that occurred since the mid-1990s in eight new EU member countries: the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, the Slovak Republic, and Slovenia. The paper finds evidence that discretionary fiscal consolidation "contributes substantially to the acceleration of output growth even in the short run," but is unable to identify unambiguously the channels through which non-Keynesian effects occur.

The IMF has also looked at the effects of large fiscal consolidations—measured as a cumulative primary fiscal adjustment of more than 5 percent of GDP—and concluded that they appear to be associated quite frequently with a positive macroeconomic response (Martinez [2004]). The study covers more than 160 countries in the last 30 years and identifies 155 episodes of large fiscal adjustment. About 40 percent of these episodes were linked to an upturn in short-term growth—although it is once again difficult to identify the likely sources of the observed output expansion.

Work at the OECD has examined the extent to which fiscal policy actions may be offset by simultaneous, anticipatory changes in private saving, as well as the determinants of that offset (de Mello, Kongsrud, and Price [2004]). Based on a sample of 21 OECD countries spanning the period 1970–2002, the study finds strong evidence of partial, yet substantial, offsetting movements in private saving. The overall offset is estimated at between about one-third and one-half, depending on model specification, and applies both to public consumption and to shifts in tax revenue. The magnitudes of these estimated offsets suggest that, in response to a fiscal tightening of approximately 5 percent of GDP—comparable to that of the OECD area as a whole during the upturn that occurred between 1993 and 2000—private saving is expected to fall by about 2.5 percent of GDP over the period. The effect on national saving of a fiscal easing of this magnitude is therefore of a rise of about 2.5 percent of GDP, other things equal. The saving offset appears to be greater over the longer term, with changes in fiscal stance being almost totally offset by changes in private saving, leaving national saving largely unaffected.

As for the conditions under which such effects are more likely to occur, the OECD paper finds that (1) private saving offsets appear to have been somewhat *lower* at *higher* levels of indebtedness; (2) the revenue/expenditure composition of the fiscal shift matters for the private saving offset; and (3) private saving appears to respond in relatively equal proportion to changes in current revenue and expenditure, but public investment does not elicit an offsetting response of saving.

Investigating the response of consumers to a fiscal impulse, Giavazzi and Pagano (1996) find that private consumption appears to respond in a non-monotonic way to fiscal impulses. Their results suggest that such non-monotonic effects tend to be associated with large and persistent fiscal impulses, and appear to be stronger for fiscal contractions than for fiscal expansions.[2] While in normal times an increase in net

2. There are several explanations for such non-monotonic effects. Spending cuts or tax increases can produce an increase in private consumption only if they raise the market value of nonhuman wealth or consumers' perception of their permanent income. Changes in permanent income depend on expectations. A spending cut that is sufficiently large and believed to be persistent can signal a future reduction in the tax burden, and therefore an increase in permanent disposable income (a point first made in Feldstein [1982]). But even small changes in

taxes (i.e., taxes net of government transfers) tends to depress consumption, during large fiscal contractions the effect on consumption of an increase in net taxes is not statistically different from zero. For government spending, they find that an increase (a cut) in public spending stimulates (lowers) consumption in normal times, but reduces (raises) it during a large fiscal impulse.

GJP also study the response of consumers to a fiscal impulse, but from a slightly different angle, that is, looking at the effects of fiscal policy on national savings. This allows them to interpret the results with reference to the predictions of Ricardian equivalence. As is well known, models with infinite horizons imply that (for a given level of government spending) taxes and transfers have no effect on national saving; in other words, the Ricardian proposition holds. On the other hand, the standard overlapping-generations model with finite horizon predicts that an increase in taxes (or a reduction in transfers) raises national saving. But the sign and size of the effect of fiscal policy on national saving might also depend on the level and sustainability of government debt, on the size and persistence of the fiscal impulse, or on the change in composition of the budget. In some situations (e.g., in the model described in Blanchard [1990]) an increase in net taxes may even produce a decline in national saving, by generating a boom in private consumption.

GJP search systematically for the circumstances in which national saving responds non-monotonically to fiscal policy impulses, using two datasets, of which one includes OECD countries and the other looks instead at emerging market economies. The results drawn from the OECD sample show that a non-monotonic response of national saving is more likely to arise when fiscal impulses are large and persistent and that non-monotonic effects on national saving are larger and more precisely estimated for changes in net taxes than for changes in public consumption. Furthermore, non-monotonic effects also appear to be asymmetric, and stronger and more precisely estimated for fiscal contractions than for fiscal expansions; in particular, during large fiscal contractions an increase in net taxes has little or no effect on national saving. They also find that a rapidly growing public debt is not, *per se*, a good predictor of non-monotonic responses.

These findings about the effect of the size and composition of the fiscal impulse are not entirely consistent with those found in other studies. Alesina and Perotti (1995, 1997) and Alesina and Ardagna (1998) find that the private-sector response is larger if the budget is cut by slashing public-sector wages and reducing social security benefits, rather than by raising taxes and cutting public investment. Along the same lines, Ardagna (2004) finds that fiscal adjustments effected by government spending cuts and generating a permanent and substantial decrease in government debt are associated with larger reductions in interest rates and increases in stock market prices—thus suggesting that the increase in the market value of nonhuman wealth is the channel through which such fiscal impulses raise output growth. Perotti (1999) finds that the outcome of a consolidation is more likely to be expansionary when public debt is high

public spending could produce large changes in private consumption in the opposite direction if they signal a change in regime or are sufficient to bring the economy over a critical threshold, as in the model of Bertola and Drazen (1993). See Giavazzi, Jappelli, and Pagano (2000) for a review of various models that produce these results.

or growing rapidly, but de Mello, Kongsrud, and Price (2004), as mentioned above, report findings that are more consistent with the results shown in GJP.

Finally, the presence of credit market imperfections constraining household borrowing may also affect the non-monotonic response of saving and consumption to fiscal policy. A non-monotonic response of private consumption and saving to a large and persistent fiscal contraction requires well-functioning household credit markets. Consider the case of a large increase in net taxes that leads households to update the estimate of their permanent income: if the household is prevented from borrowing against the expectation of higher future income, it will be unable to increase its actual spending. Ferraro (2005) tests whether the non-monotonic response of national saving depends on the ease with which households can tap financial markets and borrow. He splits the sample in two, distinguishing countries based the maximum "loan-to-value ratio"—the ratio between the maximum mortgage loan that a typical household could obtain, and the value of the house for which the loan was extended (reported in Jappelli and Pagano [1994, table 1]). He finds that non-monotonic effects are more likely to arise in countries characterized by a relatively high loan-to-value ratio, and are smaller and less precisely estimated in countries with a relatively lower loan-to-value ratio. This accords with the prediction that the functioning of the market for household credit affects the presence and magnitude of the non-monotonic effects of fiscal policy.

Summarizing, while episodes of contractionary fiscal expansions, and expansionary contractions, are a rather common finding, there is still disagreement on the conditions under which a fiscal consolidation can raise output growth—or a fiscal expansion can reduce it—and on the channels through which such effects might occur. Understanding these issues is obviously essential if one wishes to know which policies might improve the likelihood of non-Keynesian outcomes. One purpose of the present paper is to test the robustness of the findings which suggest that a prerequisite for such non-Keynesian outcomes is the magnitude and persistence of the fiscal impulse, and that the channel through which fiscal impulses affect the economy is private consumption.

III. A First Look at the Data

We start with a visual impression of our data. Figure 1 plots the distribution of the first differences in the full employment government surplus scaled by full employment output. Positive numbers correspond to fiscal contractions, and negative numbers to fiscal expansions. The sample and OECD countries considered are reported in the Appendix.

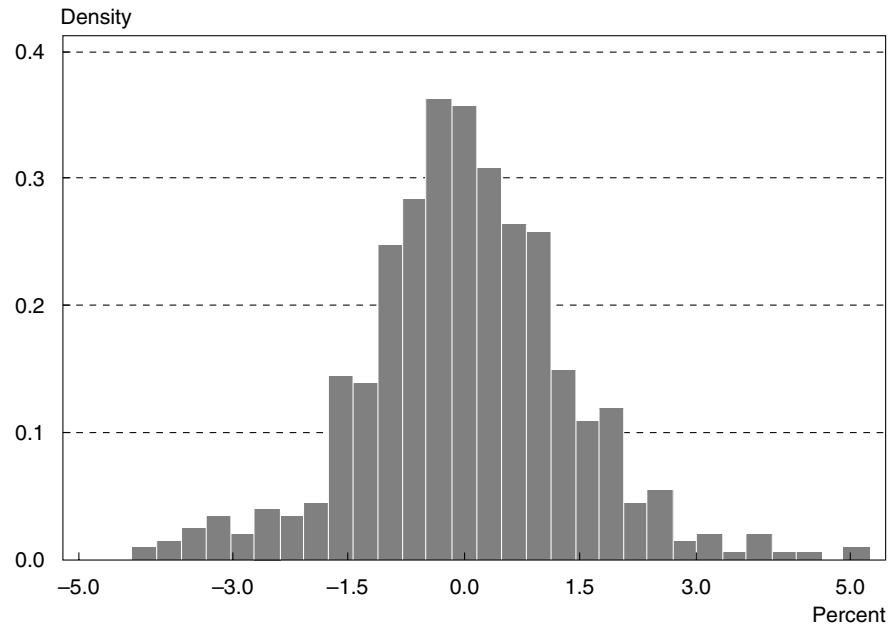
Most fiscal impulses—from one year to the next—are relatively small: contractions and expansions that do not exceed 1.5 percent of potential output. But there are many episodes outside this range, particularly in the case of fiscal expansions, and some contractions are as large as 5 percent of potential output in a single year.

Our definition of a "large and persistent" fiscal impulse uses a subset of the episodes in the tails of the distribution reported in Figure 1: those that lie outside

the –1.5/+1.5 percentage point range, and among them only those where a change of at least that magnitude has occurred for at least two subsequent years. This gives us 69 episodes of "large and persistent" fiscal contraction and 51 episodes of "large and persistent" fiscal expansion. The individual episodes are reported in the Appendix.

How were these "large and persistent" contractions and expansions implemented? By acting on revenues, government consumption, or public investment? Table 1 compares the growth rate of net taxes (taxes net of government transfers), of government consumption and of public investment (each defined as a fraction of potential

Figure 1 Year-to-Year Change in Full Employment Surplus



Note: The figures plot the change in the full employment budget surplus scaled by full employment output. Countries included are reported on the Appendix.

Table 1 Taxes and Spending during Fiscal Contractions and Expansions, Relative to Normal Times

	Fiscal contractions	Fiscal expansions
T/Y^*	+1.1 percentage points	–5.9 percentage points
G/Y^*	–1.2 percentage points	+0.6 percentage point
I/Y^*	–4.0 percentage points	+0.6 percentage point
Average length of a fiscal episode	2.95 years	2.98 years
Number of fiscal episodes	69	51

Note: The table reports the yearly percentage change in the ratios of taxes and spending to full employment output (T/Y^* and G/Y^*) and public investment (I/Y^*) during episodes of "large and persistent" fiscal contractions and expansions. The numbers are deviation from mean growth rate in "normal" times. The sample period is 1964–2003. Countries included in the estimation and variables' definitions are reported in the Appendix.

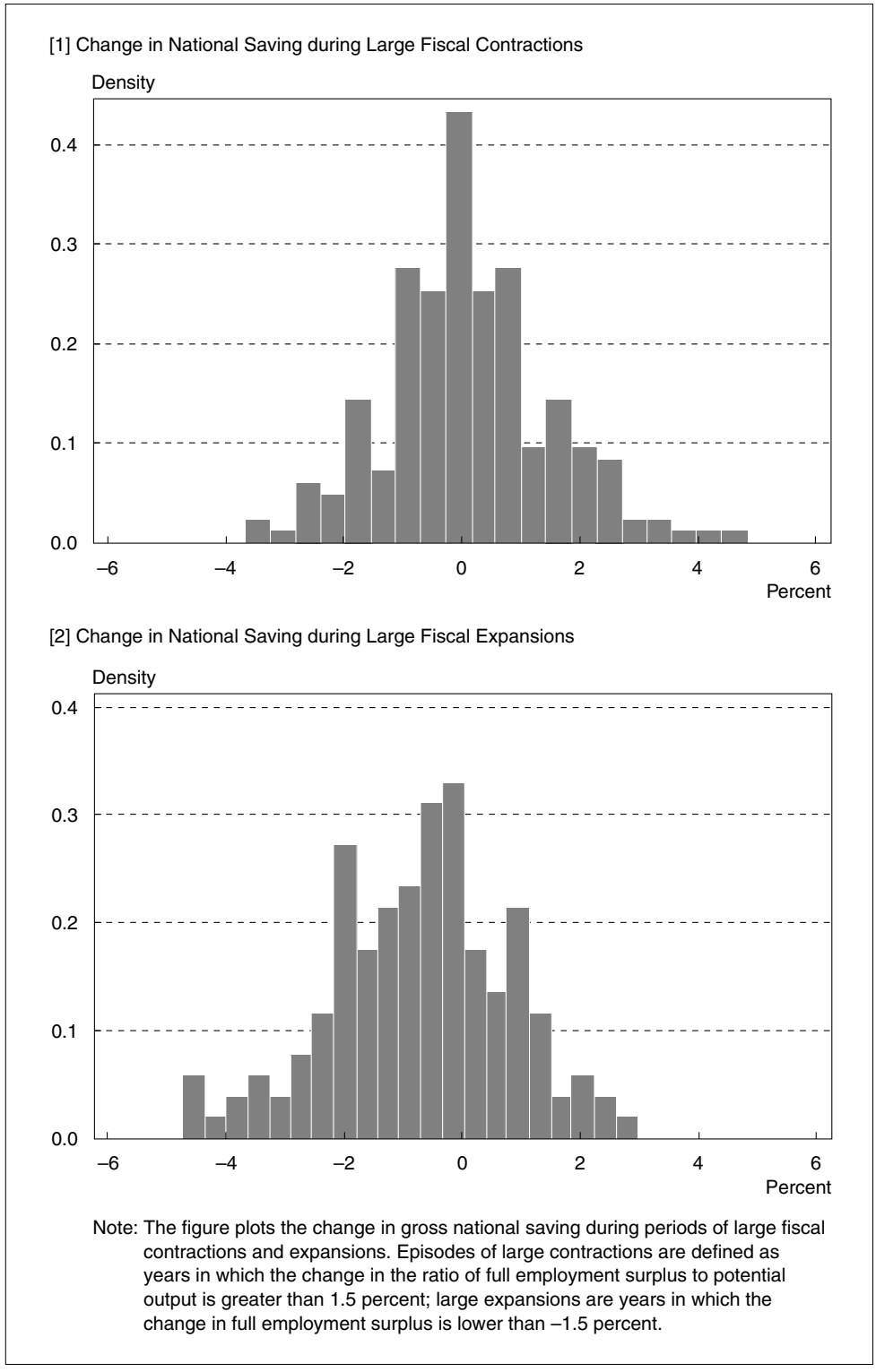
output) in years characterized by large contractions and large expansions, relative to "normal" times.

Fiscal expansions are typically driven by large reduction in taxes (relative to normal times) and by some growth in expenditure, both investment and consumption (also relative to normal times). But tax cuts account for the lion's share in a fiscal expansion. Instead, when governments slash the budget, they mostly do it by cutting public investment. During a typical contraction, the growth rate of public investment as a share of potential output is 4 percentage points lower than in normal times. Net taxes increase during a contraction and government consumption falls, but by a much smaller amount than public investment.

Figure 2 plots the change in national saving during episodes of "large and persistent" fiscal contraction (in the upper panel) and expansion (in the lower panel). The "normal" situation would be one in which a fiscal contraction raises national saving, and a fiscal expansion reduces it. Figure 2 shows that in the majority of episodes national saving is unaffected by the change in fiscal policy, indicating that private saving offsets the change in public saving one for one. But there are observations on both tails.

During episodes of large fiscal contraction (the upper panel), although the instances in which the fiscal contraction raises national saving (along the right-hand tail) are relatively more frequent, there is some mass in the left-hand tail, indicating episodes in which the offset is more than one for one. Symmetrically, during episodes of large fiscal expansion (the lower panel) there is some mass in the right-hand tail, which correspond to episodes where the private-sector offset was more than complete—although once again most of the fiscal expansions appear to lower national saving (the left tail). The bottom line is that the "normal" response to large fiscal impulses appears *prima facie* more frequent in the data, but there are also instances of non-monotonic responses.

Figure 2 National Saving during Large Fiscal Contractions and Expansions



Note: The figure plots the change in gross national saving during periods of large fiscal contractions and expansions. Episodes of large contractions are defined as years in which the change in the ratio of full employment surplus to potential output is greater than 1.5 percent; large expansions are years in which the change in full employment surplus is lower than –1.5 percent.

IV. Empirical Results

We estimate, as in GJP, the following reduced-form equation, whose dependent variable is the national saving rate as a ratio to potential output:[3]

$$\frac{S_t}{Y_t^*} = \alpha_0 + \alpha_1 \frac{S_{t-1}}{Y_{t-1}^*} + \alpha_2 \frac{Y_t - Y_t^*}{Y_t^*} + \alpha_3 r_t + \beta_1 \frac{T_t}{Y_t^*} + \beta_2 d_t \frac{T_t}{Y_t^*} + \gamma_1 \frac{G_t}{Y_t^*} + \gamma_2 d_t \frac{G_t}{Y_t^*}$$

$$+ \gamma_3 d_t + \epsilon_t. \qquad (1)$$

Y^* denotes potential output, $(Y_t - Y_t^*)/Y_t^*$ the output gap, r_t the real interest rate, T/Y^* net taxes (taxes net of government transfers) as a fraction of potential output, and G/Y^* government purchases, also as a fraction of potential output.[4]

The variable d_t is a dummy designed to capture the conditions that may give rise to a non-monotonic response of national saving to fiscal impulses. Since we want to discriminate across various conditions that may trigger such non-monotonic response, our specifications include a set of such dummies, each corresponding to one such condition, rather than a single dummy. One dummy variable is set equal to one during years characterized by a "large and persistent" fiscal impulse (as in GJP), and zero otherwise. A second dummy is equal to one only if the growth rate of the ratio of (cyclically adjusted) gross public debt to trend GDP exceeds 4 percent for two consecutive years (as in Perotti [1999]). A third dummy is one only if the change in the spread between the yield on long-term government bonds denominated in domestic currency and the yield on U.S. 10-year Treasuries exceeds the sample mean by more than 1.5 standard deviations. Note that each dummy d_t enters equation (1) both interacted with the two fiscal variables and by itself, to make sure that the interacted variable only captures the effect of the interaction rather than a possible independent effect of the dummy on national saving.

In equation (1), the lagged value of the national saving rate is expected to capture the dynamics of the dependent variable,[5] while the output gap should reflect the response of private saving and of the government surplus to transitory changes in income. The (*ex post*) real interest rate is the difference between the domestic short-term nominal rate and inflation, based on the private consumption deflator. It is introduced to control for the direct effect on saving of changes in wealth induced by

3. One advantage of using the national saving as a dependent variable is that it does not depend on the particular definition used for private saving. De Mello, Kongsrud, and Price (2004) find that the estimated offset of private saving depends on the definition used. For instance, they find lower offset coefficients when using household, rather than private, saving. The finding, however, is sensitive to the elimination of outliers, suggesting that measurement errors may be particularly pronounced. Measurement problems are due to the difficulty in accurately demarcating the household and corporate sectors, given extensive household ownership of businesses via equity and mutual/pension fund participation. These problems are avoided using national saving as the dependent variable. Furthermore, national saving is not affected by the redistribution between private and public saving that is induced by the effect of inflation on the public debt.
4. We scale national saving and fiscal variables by potential output to avoid problems of heteroskedasticity. Dividing by actual rather than potential output would introduce an endogeneity bias due to the correlation between the error term and the right-hand-side variables.
5. Equation (1) only allows for very simple dynamics of national saving. De Mello, Kongsrud, and Price (2004) use an error correction model and allow for more complex dynamics in the adjustment toward the long-term equilibrium.

monetary policy. Since the interest rate is potentially endogenous, we use its lagged value as an instrument.

Finally, following the specification in GJP, all regressions include a full set of country fixed effects, to account for the possibility that the response of national saving to a fiscal impulse might depend on the particular characteristics of a country, such as its size and degree of openness to trade. Understanding the effects of fiscal policy may be easier in a small country, and indeed some of the famous episodes of "non-Keynesian" effects have occurred in small countries, such as Ireland and Denmark. Countries that trade more, either in goods or in assets, may be more subject to capital market "discipline," in the sense that it may be easier for saving to exit the country when fiscal policy looks "bad."

One problem in estimating equation (1) is the potential endogeneity of the fiscal variables. Such endogeneity may arise from two different sources. Because of the automatic stabilizers built into the existing tax code, tax revenues and government transfers from and to the economy (which enter our definition of T) fluctuate with the business cycle, and are thus affected by the same shocks that affect national saving.[6] We deal with this first source of endogeneity by using the full employment government surplus net of interest payments, as estimated by the OECD, as an instrument for net taxes.

A second potential source of endogeneity arises from the possibility that the country's fiscal rules themselves respond to the business cycle, which in standard models is positively correlated with national saving. Our instrumental variables procedure does not handle this potential bias; GJP further discuss how the bias might affect the coefficient estimates.[7]

Our data are an unbalanced panel of OECD countries. The sample used in the estimation and the variables' definitions are reported in the Appendix. For each regression, we report results for three sample periods. "Up to 1996" defines the sample that is closest to our earlier results and thus comparable with the regressions reported in GJP, although several series have been revised. The other two samples—"up to 2000" and "up to 2003"—extend our previous results to the more recent period.

A. The Effect of Fiscal Policy during Large and Persistent Fiscal Episodes

We start by estimating a benchmark specification where we do not interact the fiscal variables with the dummy d, so that the effects of G/Y^* and T/Y^* are constrained to be linear. Which signs should we expect on the coefficients of the fiscal variables? Finite horizon models suggest that an increase in net taxes should raise national saving ($\beta_1 > 0$), whereas an increase in government consumption should reduce it ($\gamma_1 < 0$). In the benchmark infinite horizon model with lump-sum taxes, taxes have no effect on national saving ($\beta_1 = 0$): this is the Ricardian equivalence proposition.

6. Government consumption, on the contrary, is unlikely to fluctuate systematically with the business cycle: fluctuations in unemployment, for instance, affect government transfers, but are unlikely to be correlated with government purchases of goods and services.
7. There have been a few attempts at constructing measures of exogenous fiscal policy shocks that are not based on the full-employment surplus. Blanchard and Perotti (1999) identify tax and spending shocks in U.S. quarterly data by assuming that implementing fiscal policy changes requires at least one quarter and by relying on historical information on large discretionary changes in fiscal policy (such as the tax cut in the second quarter of 1975).

Also, in the infinite horizon model, for a given path of pretax income, Y, government consumption does not affect national saving either ($\gamma_1 = 0$).[8]

The results are in Table 2. Column (1) uses the sample that is closer to that originally used in GJP; columns (2) and (3) update the sample to 2000 and 2003, respectively. The coefficient of T/Y^* is positive and statistically different from zero at the 1 percent level in each regression. When we extend the sample to 2003, the point estimate becomes smaller, but so does the coefficient of the lagged national saving rate, so that the long-run effect of taxes—estimated as $\beta_1/(1 - \alpha_1)$—is similar in columns (1) and (3), 0.51 and 0.53, respectively.

Thus, when we constrain the effect of net taxes to be linear ($\beta_2 = 0$), the results run against the infinite horizon model and are consistent with the predictions of finite horizon models.

The coefficient of G/Y^* is negative and also statistically different from zero at the 1 percent level in both regressions. Contrary to the predictions of infinite horizon models with non-distortionary taxes, the fall in private consumption does not fully compensate the increase in government consumption, thereby reducing national saving. In this case too, the point estimate becomes smaller (in absolute value) when we extend the sample to 2003, but, as in the case of net taxes, the long-run response is similar.

The results in Table 2, however, are potentially biased because by omitting the interaction terms they impose that the response of national saving to fiscal variables is linear. Table 3 investigates this issue and contains the main results of the paper. Each

Table 2 National Saving, Taxes, and Government Spending: Baseline Specification

	Up to 1996	Up to 2000	Up to 2003
Lagged national saving rate	0.534 (0.036)***	0.649 (0.033)***	0.722 (0.029)***
Output gap	0.267 (0.039)***	0.269 (0.039)***	0.282 (0.035)***
Real interest rate	0.031 (0.029)	0.006 (0.031)	0.008 (0.030)
Net taxes (T/Y^*)	0.241 (0.066)***	0.206 (0.054)***	0.152 (0.048)***
Government consumption (G/Y^*)	–0.545 (0.059)***	–0.380 (0.056)***	–0.308 (0.052)***
Observations	425	501	556
Adjusted R-squared	0.94	0.94	0.94

Note: The dependent variable is the ratio of national saving to potential output. Instruments for net taxes and the real interest rate are the full employment government surplus net of interest payments (scaled by potential output) and the lagged real interest rate. Countries included in the estimation and variables' definitions are reported in the Appendix. All regressions are estimated with fixed effects. Standard errors are reported in parentheses. One, two, and three asterisks indicate significance levels at 10, 5, and 1 percent, respectively.

8. Recall that national saving is the difference between national income and total consumption (private plus public): $S = S_G + S_P = (T - G) + (Y - T - C) = Y - (C + G)$.

of the three regressions reported in Table 3 tests the hypothesis that fiscal policy coefficients are stable during periods of protracted and sizable fiscal impulse or during periods of rapid debt growth. Our hypothesis (supported by our previous findings) is that non-monotonic responses mostly appear during episodes of protracted and sizable fiscal impulse, as defined above.

Consider first the sample up to 1996. In this sample (column [1]) the effect of both T/Y^* and G/Y^* is highly non-monotonic. During "normal" times, the effect of net taxes on national saving is positive ($\beta_1 = 0.684$), but during sharp shifts in fiscal policy the response of private saving virtually cancels any effect of public saving on national saving: $\beta_1 + \beta_2 = 0.684 - 0.665 = 0.019$, and we cannot reject the hypothesis that $\beta_1 + \beta_2 = 0$ (the p-value of the F-test is 0.88). The same happens for fiscal impulses induced by changes in G/Y^*. During "normal" times, an increase in government spending reduces national saving ($\gamma_1 = -1.060$) but during sharp shifts in fiscal policy the response of private saving greatly dampens the fall in national saving: $\gamma_1 + \gamma_2 = -1.060 + 0.777 = -0.283$; in this case, the offset is less than

Table 3 National Saving, Taxes, and Government Spending: Distinguishing between "Normal Times," Episodes of "Large Change in Primary Fiscal Surplus," and Episodes of "Rapid Debt Growth"

	Up to 1996	Up to 2000	Up to 2003
Lagged national saving rate	0.576 (0.052)***	0.660 (0.042)***	0.737 (0.043)***
Output gap	0.213 (0.061)***	0.246 (0.047)***	0.268 (0.045)***
Real interest rate	0.042 (0.065)	–0.028 (0.052)	–0.036 (0.054)
Net taxes (T/Y^*)	0.684 (0.229)***	0.547 (0.170)***	0.527 (0.172)***
—and large change in surplus	–0.665 (0.281)**	–0.501 (0.228)**	–0.534 (0.221)**
—and rapid debt growth	0.434 (0.574)	–0.177 (0.511)	–0.489 (0.608)
Government consumption (G/Y^*)	–1.060 (0.223)***	–0.777 (0.185)***	–0.739 (0.179)***
—and large change in surplus	0.777 (0.289)***	0.613 (0.249)**	0.642 (0.241)***
—and rapid debt growth	–0.348 (0.492)	0.177 (0.440)	0.447 (0.524)
Dummy for large change in full employment surplus	–2.920 (1.073)***	–2.694 (0.905)***	–2.593 (0.906)***
Dummy for rapid debt growth	–1.051 (0.512)**	–1.071 (0.439)**	–1.092 (0.473)**
Observations	425	501	556
Adjusted R-squared	0.89	0.91	0.91

Note: The dependent variable is the ratio of national saving to potential output. Instruments for net taxes and the real interest rate are the full employment government surplus (scaled by potential output) and the lagged real interest rate. Countries included in the estimation and variables' definitions are reported in the Appendix. All regressions are estimated with fixed effects. Standard errors are reported in parentheses. One, two, and three asterisks indicate significance levels at 10, 5, and 1 percent, respectively.

complete, as the hypothesis $\gamma_1 + \gamma_2 = 0$ is rejected at the 5 percent confidence level (the *p*-value of the *F*-test is 0.027).

National saving does not appear to respond differently to fiscal impulses carried out by changing government consumption or net taxes: what matters is the size of the impulse, not its composition. When we tried to split the "net taxes" variable into "direct plus indirect taxes" and "transfers," the two coefficients were less precisely estimated, but the results suggest that most of the action in the "net tax" variables comes from shifts in transfers, confirming one of the findings in Alesina and Ardagna (1998)—namely, that what appears to matter most are changes in Social Security rules.

On the contrary, and confirming the findings in GJP, the coefficient of the dummy for rapid debt growth, interacted with net taxes or government consumption, is not statistically different from zero, suggesting that a non-monotonic response of fiscal policy is more likely to arise when the cyclically adjusted surplus changes significantly, rather than when public debt grows rapidly.

Columns (2) and (3), where we extend the sample to 2000 and 2003, respectively, show that the general pattern of these results survives in the more recent data. The evidence confirms the full offset of the tax coefficient during large fiscal episodes. In the more recent samples, contrary to the earlier one, we no longer reject the hypothesis of a full offset of the effect of government consumption during large fiscal episodes. Finally, the result that rapid debt growth is not a condition giving rise to a non-monotonic effect of fiscal impulses is confirmed: in none of these regressions are the interacted coefficients with the dummy for rapid debt statistically different from zero.

B. Are the Effects of Large Contractions Different from Those of Large Expansions?

The private sector's response to a fiscal impulse could differ depending on the sign of the impulse. To test for the possibility of asymmetric behavior, we interact T/Y^* and G/Y^* with two separate dummies, one for large fiscal expansions and one for large fiscal contractions (the two dummies are then also entered separately). In each regression, we keep the dummy for rapid debt growth interacted with net taxes or government consumption. The results in Table 4 report again estimates for three samples.

In column (1), up to 1996, during episodes of large swings in fiscal policy, private saving consistently offsets the effects of changes in public saving, independently of whether the fiscal impulse is an expansion or a contraction. During *contractions* induced by an increase in taxes the offset is complete, as we do not reject the hypothesis that taxes do not affect national saving: the sum between the tax coefficients ($0.640 - 0.723 = -0.083$) is not statistically different from zero (the *p*-value of the test is 0.68). In case of *fiscal expansions,* the point estimate of the sum of the tax coefficients is also close to zero and, again, we do not reject the hypothesis that during expansions the difference between the tax coefficients ($0.640 - 0.630 = 0.010$) is not statistically different from zero (the *p*-value of the test is 0.94). These results confirm the original GJP findings with the updated series.

During fiscal impulses induced by a change in government spending, we find a complete offset during large contractions: the sum of the two coefficients on

Table 4 National Saving, Taxes, and Government Spending: Distinguishing between "Normal Times," Large Fiscal Contractions, and Large Fiscal Expansions

	Up to 1996	Up to 2000	Up to 2003
Lagged national saving rate	0.606 (0.066)***	0.700 (0.050)***	0.765 (0.042)***
Output gap	0.226 (0.067)***	0.265 (0.052)***	0.279 (0.045)***
Real interest rate	0.051 (0.068)	–0.013 (0.055)	–0.032 (0.050)
Net taxes (T/Y^*)	0.640 (0.237)***	0.499 (0.179)***	0.478 (0.158)***
—and large increase in surplus	–0.723 (0.339)**	–0.620 (0.294)**	–0.559 (0.226)**
—and large decrease in surplus	–0.630 (0.290)**	–0.472 (0.238)**	–0.523 (0.219)**
—and rapid debt growth	0.620 (0.620)	0.204 (0.560)	–0.256 (0.546)
Government consumption (G/Y^*)	–1.071 (0.252)***	–0.792 (0.213)***	–0.697 (0.172)***
—and large increase in surplus	0.853 (0.358)**	0.785 (0.334)**	0.700 (0.256)***
—and large decrease in surplus	0.731 (0.297)**	0.524 (0.248)**	0.606 (0.236)**
—and rapid debt growth	–0.514 (0.531)	–0.166 (0.483)	0.237 (0.469)
Dummy for large fiscal contraction	–3.250 (1.285)**	–3.647 (1.267)***	–3.076 (1.086)***
Dummy for large fiscal expansion	–2.813 (1.546)*	–1.735 (1.213)	–2.366 (1.156)**
Dummy for rapid debt growth	–0.899 (0.547)	–0.820 (0.476)*	–0.935 (0.468)**
Observations	425	501	556
Adjusted R-squared	0.87	0.90	0.91

Note: The dependent variable is the ratio of national saving to potential output. Instruments for net taxes and the real interest rate are the full employment government surplus (scaled by potential output) and the lagged real interest rate. Countries included in the estimation and variables' definitions are reported in the Appendix. All regressions are estimated with fixed effects. Standard errors are reported in parentheses. One, two, and three asterisks indicate significance levels at 10, 5, and 1 percent, respectively.

government spending ($-1.071 + 0.853 = -0.218$) is not statistically different from zero. On the other hand, in the case of expansions induced by large increases in government spending, the offset is less than complete: the sum of the coefficients ($-1.071 + 0.731 = -0.340$) is statistically different from zero (the p-value of the test is 0.033).

Also when the sample is extended to 2000 and 2003 (columns [2] and [3]), we find that the response of national saving to large fiscal impulses is non-monotonic and symmetric for large expansions and large contractions. In the case of fiscal impulses induced by large changes in net taxes, the offset is complete both in the case of expansions and contractions; in the case of fiscal impulses induced by large changes in government spending, the offset is complete in the case of fiscal contractions and, in the sample extending to 2003, also in the case of fiscal expansions.

Finally, in this specification too the evidence is consistent with the hypothesis that "rapid debt growth" is not associated with a non-monotonic response of national saving to fiscal impulses.

C. Interest Rate Spreads

For spending cuts or tax increases to produce a fall in private saving (and vice versa) either the market value of nonhuman wealth must rise, or households must anticipate a higher permanent income. This could happen if fiscal impulses affected households' expectations. For instance, a spending cut that is sufficiently large and (believed to be) persistent could signal a future reduction in the tax burden, and therefore an increase in permanent disposable income (Feldstein [1982]).

Could there be other circumstances under which a fiscal impulse could induce a change in households' estimate of their permanent disposable income? One possibility (discussed in Blanchard [1990]) is that the response of consumers to fiscal impulses may differ depending on the perceived sustainability of the fiscal regime. For instance, when the debt-income ratio is growing rapidly, a fiscal contraction may provide the signal that a debt default will be avoided: it thus may induce a large revision in permanent disposable income.

The results in Tables 3 and 4 have shown that a rapidly growing debt-GDP ratio is not *per se* a condition for the response of national saving to fiscal impulses to be non-monotonic. One possibility is that consumers become concerned about rapid debt growth only when this starts inducing the risk of a default; in other words, only when interest rate spreads start widening.

In the regressions reported in Table 5, we allow for the possibility that the response of national saving to fiscal impulses becomes non-monotonic when there is a change in the spread between the yield on long-term government bonds denominated in domestic currency and the yield on U.S. 10-year Treasuries. Such yields reflect either expectations of currency depreciation or default premia: when the yield widens, it thus signals that the markets are becoming more concerned about fiscal sustainability.[9] In this case, our specification includes an additional dummy that equals one when the spread exceeds the sample mean by more than 1.5 standard deviations, and zero otherwise. The specifications also include the dummies that were already present in the regressions of Table 3, and therefore still allow for non-monotonic responses to be associated with the size and persistence of fiscal impulses and debt growth.

The results in Table 5 tend to confirm that the circumstance most likely to give rise to an offsetting response of private saving remains the size and persistence of the fiscal impulse. The offset coefficients, however, are much less precisely estimated, possibly because of collinearity between the dummy for rapid debt growth and the dummy for

9. Caballero and Krishnamurty (2004) suggests a different reason why the effects of a fiscal expansion might depend on the response of bond prices. In countries characterized by relatively thin financial markets, a fiscal expansion that is large enough to absorb significant amounts of liquidity will be accompanied by a sharp increase in interest rates and corresponding reductions in households' financial wealth. Ardagna (2004) finds that stock market prices surge around times of substantial fiscal tightening and plunge in periods of very loose fiscal policy—and that such effects are more likely to occur in country years with high levels of government deficits, and when consolidations are implemented by cutting government spending, and that generate a permanent and substantial decrease in government debt.

Table 5 National Saving during Large Fiscal Episodes, When Debt Is Growing Fast and When Spreads Widen

	Up to 1996	Up to 2000	Up to 2003
Lagged national saving rate	0.562 (0.075)***	0.641 (0.054)***	0.740 (0.058)***
Output gap	0.257 (0.083)***	0.277 (0.061)***	0.296 (0.055)***
Real interest rate	0.032 (0.090)	–0.023 (0.068)	–0.025 (0.067)
Net taxes (T/Y^*)	0.648 (0.327)**	0.549 (0.245)**	0.516 (0.215)**
—and large change in surplus	–0.953 (0.665)	–0.721 (0.516)	–0.707 (0.411)*
—and rapid debt growth	0.482 (0.874)	–0.091 (0.739)	–0.395 (0.786)
—and large change in spread	1.011 (0.978)	0.756 (0.804)	0.645 (0.693)
Government consumption (G/Y^*)	–1.017 (0.344)***	–0.787 (0.292)***	–0.715 (0.227)***
—and large change in surplus	1.083 (0.702)	0.861 (0.569)	0.845 (0.463)*
—and rapid debt growth	–0.423 (0.763)	0.080 (0.648)	0.347 (0.686)
—and large change in spread	–1.419 (1.213)	–1.129 (1.029)	–0.998 (0.916)
Dummy for large change in full employment surplus	–3.422 (1.828)*	–3.269 (1.532)**	–3.205 (1.470)**
Dummy for rapid debt growth	–1.794 (1.166)	–1.551 (0.895)*	–1.487 (0.832)*
Dummy for large change in spread	7.564 (5.098)	7.026 (4.818)	6.792 (4.801)
Observations	425	501	556
Adjusted R-squared	0.77	0.85	0.87

Note: The dependent variable is the ratio of national saving to potential output. Instruments for net taxes and the real interest rate are the full employment government surplus (scaled by potential output) and the lagged real interest rate. Countries included in the estimation and variables' definitions are reported in the Appendix. All regressions are estimated with fixed effects. Standard errors are reported in parentheses. One, two, and three asterisks indicate significance levels at 10, 5, and 1 percent, respectively.

the widening of spreads. The dummies set equal to one when debt is growing rapidly and when spreads widen are not statistically different from zero. Distinguishing between expansions and contractions (as in Table 4) does not change the results.

V. Conclusions

Data revisions and the availability of a longer sample offer the opportunity to reconsider the empirical findings which suggest that in the OECD countries the response of national saving to fiscal policy is non-monotonic. Our main results can be summarized as follows.

Before considering the possibility of a non-monotonic response—that is, when we constrain the effect of net taxes to be linear—we find, contrary to the predictions of infinite horizon models with non-distortionary taxes, that an increase in taxes raises national saving, and that an increase in government purchases lowers it.

The new data confirm that the circumstance most likely to give rise to a non-monotonic response of national saving to a fiscal impulse is a "large and persistent impulse," defined as one in which the full employment surplus, as a percentage of potential output, changes by at least 1.5 percentage points per year over a two-year period. This particular circumstance remains the only statistically significant one even when we allow for non-monotonic responses to arise when public debt is growing rapidly or interest rate spreads are widening.

We find that non-monotonic responses are similar for fiscal contractions and expansions. In particular, an increase in net taxes has no effect on national saving during large fiscal contractions or expansions. For government consumption, there is a large, albeit in some specifications less than complete, offset during expansions or contractions.

APPENDIX: DEFINITION OF THE VARIABLES USED IN THE REGRESSIONS

All variables are drawn from the March 2005 *OECD Economic Outlook* database. Public-sector data refer to general government. Definition of the variables and sample periods are as follows (Appendix Tables 1 and 2).

Appendix Table 1 Definition of the Variables

Definition	Name of the corresponding OECD series (*OECD Economic Outlook* database)
Private consumption	CP
Government consumption	CG
Government savings	SAVG
Government debt	GGFL
Gross national/domestic product	GDPV
Deflator for consumer expenditure	PCP
Taxes net of transfers	SAVG + CG
Government investment	IG
Social security contributions	SS
Social security benefits	SSPG
Full employment government surplus	NLQGA
Potential output	GDPVTR

Appendix Table 2 Countries, Sample Period Used in the Estimation, and Fiscal Episodes

Country	Sample used in the estimation	Episodes of "large and persistent" fiscal expansion	Episodes of "large and persistent" fiscal contraction
Australia	1971–2003	1974–76, 1990–93, 2000–01	1971–72, 1979–80, 1985–88, 1994–97
Austria	1973–2003	1982–83, 1993–95	1973–74, 1984–85, 1996–97, 2000–02
Belgium	1972–2003	1978–80	1972–73, 1981–87, 1992–94
Canada	1970–2003	1974–78, 1982–85, 2000–03	1970–71, 1980–81, 1986–88, 1994–98
Denmark	1972–2003	1974–76, 1981–82, 1990–91, 1994–95	1972–73, 1977–78, 1983–87, 1998–99
Finland	1977–2003	1979–80, 1982–83, 1986–87, 1990–91, 2001–03	1977–78, 1984–85, 1988–89, 1992–94, 1998–2000
France	1972–2003	1977–78, 1981–82, 1992–93, 2002–03	1972–73, 1979–80, 1995–97
Germany	1970–2003	1974–75, 1987–91, 2000–02	1969–70, 1976–77, 1981–83, 1992–94
Ireland	1986–2003	1990–91, 2000–02	1980–84, 1986–89, 1993–94
Italy	1964–2003	1971–73, 1978–81, 1984–85, 1998–2001	1964–65, 1976–77, 1982–83, 1990–93, 1995–97
Japan	1971–2003	1975–76, 1992–96, 1998–2000	1971–72, 1983–85
Netherlands	1980–2003	1986–90, 1994–95, 2001–02	1980–83, 1991–93, 1996–97
New Zealand	1987–2003	1996–97	1987–91
Norway	1979–2003	1989–92, 2001–03	1979–80, 1982–83, 1985–86, 1993–96, 1999–2000
Portugal	1977–2003	1989–90, 1993–94	1977–78, 1981–84, 1991–92, 2001–03
Spain	1980–2003		1980–81, 1986–87, 1991–92, 1995–97
Sweden	1980–2003	1990–93, 2001–03	1980–84, 1986–88, 1994–98
United Kingdom	1971–2001	1983–84, 1989–93, 2001–04	1971–72, 1974–75, 1979–82, 1995–99
United States	1966–2003	1970–71, 1974–75, 1982–83, 2001–03	1966–69, 1976–77, 1987–88, 1993–98

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Comment

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The authors' paper is the latest in a series of papers by Francesco Giavazzi and Marco Pagano and various co-authors that study episodes of expansionary "fiscal contractions," that is, episodes characterized by large reductions in fiscal budget deficits that have been associated with output expansions. The view that fiscal contractions can be expansionary has been characterized as the "German view" in light of statements by the German Council of Economic Experts in the early 1980s that fiscal contraction could set the foundation for an economic expansion in Europe. In the United States, this view sometimes has been labeled "Rubinomics" after Robert Rubin, who was U.S. Treasury Department Secretary in the mid-1990s when the United States reduced its fiscal deficits and simultaneously experienced lower interest rates and higher investment and growth.

The focus of the paper is on measuring the effects of fiscal policy on national saving. More specifically, it asks if and by how much does private saving decline in response to contractionary fiscal policy. The implied premise is that the more private saving declines, the more likely it is that fiscal consolidation has an expansionary effect on consumption and output.

I. Quick Review of Background Theory and Empirical Work

Let me begin my comments by providing a quick review of the relevant theory and empirical work. Some of this material is presented in other work by the authors but mostly omitted from the current paper. It is useful to have this material in mind to understand the logic of the results as well as to fit the paper into the larger literature of fiscal policy effectiveness.

In a basic Keynesian framework, a tighter fiscal policy contracts the economy as higher taxes and lower government spending both reduce aggregate demand. To the extent that the interest rate falls because of decreased government borrowing, this effect is offset somewhat by the crowding in of investment and other interest-sensitive demand. On balance, however, the effect of contractionary fiscal policy reduces output as long as money demand is interest elastic. But there are other so-called "non-Keynesian" channels through which fiscal policy can affect consumption, investment, and aggregate supply. For example, if fiscal consolidation lessens the future expected tax burden, permanent income and current consumption may rise. Lower interest rates can increase discounted income streams and wealth, which also boosts consumption. If fiscal consolidation leads to lower wages in the private sector and a lower tax burden on firms, expected profits may rise, fostering more investment, aggregate supply, and growth.

The authors argue that these non-Keynesian effects may be non-monotonic, that is, large fiscal changes have a proportionately greater impact on aggregate demand

than do small changes. One crude explanation is the existence of threshold effects to learning, implying that fiscal impulses must exceed some threshold to prompt revisions in expectations about future tax burdens and spending. Another story works off of tax distortions (Blanchard [1990]). If future taxes are inefficiently high, raising current taxes and lowering future taxes may greatly reduce the deadweight costs of future taxes. Still another story involves the path of government debt. If public debt is high and the fiscal deficit is perceived to be on an unsustainable path, then a large fiscal contraction may lead to a significant reduction of expected future spending and taxes.

The authors' paper is also part of a larger literature on the effectiveness of discretionary fiscal policy. One strand involves using vector autoregressions to estimate output multipliers of discretionary fiscal policy. For example, Blanchard and Perotti (2002) find that fiscal policy multipliers for the United States are positive though small, in other words, expansionary fiscal policy increases growth. Another strand uses real business cycle and structural models to simulate the effects of discretionary monetary policy (e.g., Ramey and Shapiro [1998]). A third strand involves individual case studies, including early work by Giavazzi and Pagano (1990) on budget adjustment in Denmark and Ireland, as well as cross-section and panel studies. The latter includes work by Alesina *et al.* (2004) that estimates investment effects and work by Giavazzi and Pagano (1996) and Giavazzi, Jappelli, and Pagano (2000) that has estimated consumption equations, or equivalently saving equations.

II. Overview of the Paper

The starting place of the authors' paper is a rearrangement of the national income accounting relation into the condition that national saving equals the sum of private saving and fiscal surplus, which in turn is equal to domestic investment plus foreign investment through the current account surplus. The authors then ask what is the effect of tax increases or government spending cuts on national saving, in other words, by how much does private saving offset changes in the budget surplus? This gives a direct test of Ricardian equivalence. Indeed, there is good evidence that private saving moves inversely to fiscal surpluses, dampening the change in national saving (Organisation for Economic Co-operation and Development [2004]). The authors emphasize that for large changes in fiscal policy the offsetting changes in private saving are even larger, implying a nonlinear relationship.

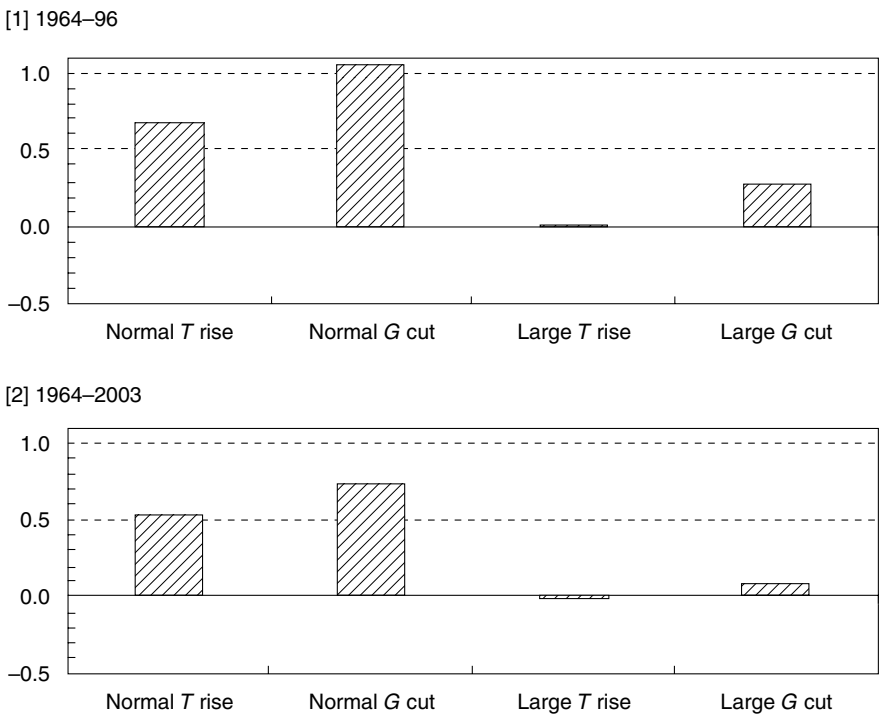
The empirical analysis involves estimation of a reduced-form saving equation by regressing the saving rate on taxes and spending, lagged saving to deal with dynamics and some control variables, including the output gap and the real interest rate. The effects of fiscal nonlinearities are tested by interacting dummies with taxes and spending. These dummies are defined in three ways: (1) large changes in the size of the surplus, that is, changes in the full employment budget surplus greater than 1.5 percentage points per year for a two-year period; (2) annual growth in the public debt-GDP ratio that exceeds 4 percent per year for two years; and (3) and deviations between the long-term interest rate for long-term government bonds and 10-year U.S. Treasuries that exceed the mean level by 1.5 standard deviation units. This empirical

analysis is essentially an update of earlier work by Giavazzi, Jappelli, and Pagano (2000) that extends the dataset for OECD countries from 1964–96 to 1964–2003.

Figure 1 shows their estimated short-run responses of national saving to "normal" and "large" tax rises and government spending cuts, based on the results in Table 3 of their paper, which also include dummies for rapid public debt growth (the long-run responses can be obtained by dividing by one minus the coefficient on lagged saving). The main points of the paper can be drawn from this figure:

- First, the magnitude of fiscal policy contraction matters. For "normal" tax increases and government spending cuts, national saving rises, implying fiscal policy is contractionary. For "large" fiscal contractions, saving rises less and may even fall, since offsetting private saving effects are larger. Thus, for large contractions, fiscal policy is less contractionary.
- Second, the composition of fiscal contraction matters. Tax increases raise saving by less than government spending cuts, since the offsetting fall in private saving is larger.
- Third, the sample period matters. The response is smaller when the sample is extended from 1964–96 to 1964–2003. I will come back to this particular result below.

Figure 1 Estimated Coefficient Responses of National Saving to "Normal" and "Large" Tax (*T*) Rises and Government Spending (*G*) Cuts



Source: Giavazzi *et al.* (2005, table 3).

III. Comments and Suggestions

I have several comments, questions, and suggestions for the authors. My intention in raising these points is not just to provide constructive suggestions to the authors for future work, but also to motivate other researchers in the field who seek to build upon their paper.

A. Measurement of Fiscal Policy Stance

Current taxes and government spending do not fully capture the stance of fiscal policy. First, fiscal policies may be backloaded, if, for example, tax increases are scheduled for later in a consolidation program. Second, fiscal reforms, for instance, pension reforms, may have a limited effect on the current budget balance position, but may have a large effect on the long-term government budget and on permanent income. Lastly, fiscal policy may stimulate current demand without any change in the current budget deficit by affecting aggregate demand through a change in relative prices, such as an investment tax credit. For these reasons, the measure of fiscal policy used in the paper may not totally reflect the effects of fiscal policy in some cases.

B. Are the Coefficient Estimates Consistent?

The estimated saving equation has a lagged saving rate as a right-hand-side variable. In a panel sample, coefficient estimates may be inconsistent in the presence of a lagged dependent variable and serially correlated errors. In an earlier version of the paper, the authors argue that this bias is likely to be small when the time dimension of the panel sufficiently exceeds the cross-section dimension. But this presumes that the estimated coefficients are homogenous across countries, for example, the coefficient on lagged saving is the same in the United States as in other OECD countries. Here I would suggest testing this homogeneity restriction formally. If the restriction is rejected, a dynamic panel estimator that gives consistent estimates, such as that developed by Arellano and Bond (1991), is in order.

C. Controlling for Endogeneity

Taxes and government spending are potentially endogenous for two reasons: (1) automatic stabilizers (taxes and spending depend directly on output and income levels); and (2) policy rules (discretionary changes in taxes and spending may depend on the output gap through a feedback policy followed by policymakers). In their paper, the authors address the first problem by instrumenting the tax variable with the full employment primary budget surplus, but they do not adjust government spending.

With respect to the second source of possible endogeneity, it might be argued that the bias due to a possible policy rule is small if the correlation of budget surplus changes and the output gap is zero for large fiscal changes. But the International Monetary Fund (2004) finds that discretionary budget surpluses in the European Union have been procyclical since the mid-1990s: the cyclically adjusted surplus rose with output in the late 1990s to meet Maastricht goals, while it fell with the output downturn in 2001–03. So the authors need to consider whether it is true that the bias due to endogeneity of taxes and spending through the output gap is small, particularly in the latter part of the sample.

D. Role of Other Factors Affecting Saving and Growth?

The authors make some effort to assess the role of other financial developments, such as public debt growth and interest differentials. But none of these factors when interacted with the fiscal variables is found to be significant. Moreover, there are lots of other factors that may also have affected consumption and output at the time of fiscal policy changes. Factors such as productivity growth, monetary policy, and exchange rate changes, among others, may all affect the link between fiscal consolidations and economic activity. For example, why did output grow after the Clinton administration fiscal consolidation of 1993–94? Certainly the productivity boom starting in the mid-1990s played a role in this output expansion. Why have the tax cuts and increased fiscal deficits of the George W. Bush administration apparently been expansionary, rather than contractionary, if non-Keynesian effects had been working strongly? Was it because of low interest rates engineered by Federal Reserve monetary policy? Lastly, what is the role of exchange rate policy? According to Lambertini and Tavares (2003), significant currency depreciation preceded some expansionary fiscal contraction episodes. For example, the currencies of Ireland and Denmark each depreciated before fiscal changes that created competitiveness advantages that contributed to the subsequent economic growth. Thus, the estimation needs to control for the roles of other factors, such as the effects of productivity, monetary policy, and exchange rate changes, to properly identify the effect of fiscal policy.

E. Why Have the Effects of Fiscal Policy Declined since 1996?

As noted above, the (short-run) coefficients of both normal and extreme fiscal policy changes decline with the addition of the 1997–2003 period observations. It would be useful to carry out a formal test of whether there has been a regime shift in fiscal policy multipliers between 1996 and 2003 using, for example, a Chow test or other test of parameter stability. In any event, the authors should ponder this result and provide some explanation for it. One possible reason is that membership in the European Monetary Union (EMU) has ended the ability of member countries in the sample to control their national exchange rates and national monetary policy. To the extent that the ability to devalue the exchange rate against major trading partners and engineer independent monetary policies was an important element in fostering successful fiscal adjustment, the locking in of exchange rates after 1993 within the EMU and the adoption of the euro may have reduced the effectiveness of fiscal policy in the latter part of the sample.

F. Do the Non-Keynesian Channels Work in the United States and Japan?

A final question has to do with the relative importance of the non-Keynesian channels in the cases of the United States and Japan, the two largest countries in the sample. Other work and casual observation suggest that these channels may not be very important for these countries. Specifically, Kuttner and Posen (2001) conclude in the case of Japan that, after properly controlling for the endogenous effect of income on tax receipts, fiscal policy has worked in a Keynesian way in the past decade. When taxes were increased in 1997 by over 2 percent of GDP, output fell (this episode does not appear in the authors' sample of large changes because it lasted only one year).

When government spending was increased in 1997–98, output rose. Similarly, in the case of U.S. fiscal policy, when taxes were cut and government spending raised in 2001–03 and the budget deficit rose, output rose as well. To see if Japan and the United States have behaved differently, it would be useful to formally test if the effects of fiscal policy in these two countries do indeed differ from those of the other countries in the sample.

Presuming that there is a difference, what is the explanation for the apparent absence of non-Keynesian effects for the United States and Japan? Evidently concern about future budget deficits does not appear to have a significant depressing effect on current spending in either of these countries, inasmuch as long-term interest rates have remained relatively low in both cases. Possibly the answer has to do with the credibility of policymaking institutions in the United States and Japan. This credibility has lessened the concern that the central bank will inflate away the value of the debt and/or the fiscal authorities will default on their liabilities. Consequently, non-Keynesian effects related to concerns about future tax liabilities have been muted.

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Comment

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The analysis in the session paper is a sequel to a number of other studies by a subgroup of the current authors. In particular, see Giavazzi, Jappelli, and Pagano (2000). I have some concerns about the structure of these analyses that I will discuss below.

The authors' analyses use panel datasets. The session paper's Appendix lists 19 countries with annual data spanning up to 40 years in one country. There is a substantial number of missing data points, resulting in unbalanced panels. Data for Italy and the United States start in the mid-1960s. The shortest samples are for New Zealand and Ireland, where the data start in 1987 and 1986, respectively. With the exception of the United Kingdom, the data extend through 2003.

The fundamental hypothesis in this analysis is that the saving ratio responds differently to large and persistent fiscal expansions or contractions than to more normal fiscal impulses. "Large and persistent" is defined in terms of fiscal expansions or contractions that exceed 1.5 percentage points in terms of the full employment surplus, and that persist for two years. In this analysis, the 1.5 percentage point threshold is taken as given, though in Giavazzi, Jappelli, and Pagano (2000) sensitivity analysis is undertaken for that particular threshold. In the sample used there, which is shorter than the sample in this analysis, the conclusions appear to be robust to the choice of the threshold.

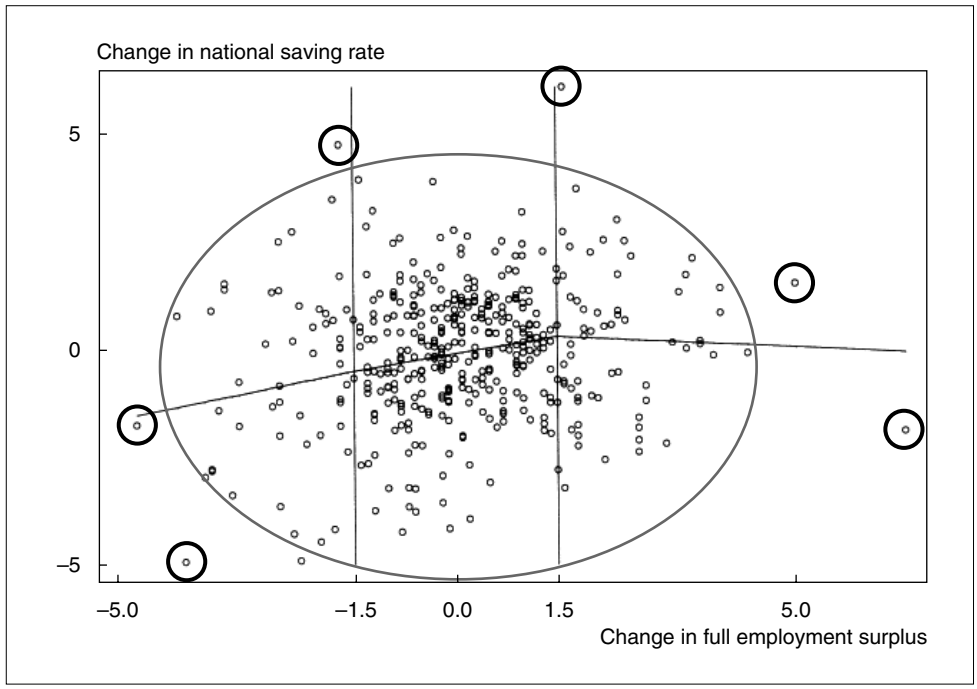
An interesting characteristic of the data in this sample, as well as in the sample of the previous analysis, is that a relatively large fraction of observations received the "treatment"; that is, they correspond to a large and persistent fiscal expansion or contraction. The observations that receive the treatment appear to be scattered fairly uniformly across the countries and across the sample (see the session paper's Appendix).

I confess that I am uncomfortable with the approach of gathering as much data as possible, and throwing it into the personal computer or statistical package to grind it up and see what comes out. I believe that there are better ways to approach the analysis, which I will come to a little later. I can cite two points as a rationale for my discomfort. First is the issue of outliers and how the results are affected by outliers. Second is the issue of specification and measurement errors that I believe are substantial here.

Consider Figure 1, which is reproduced from Giavazzi, Jappelli, and Pagano (2000). This figure has the data from a sample of OECD countries that ends in 1996.[11] The dependent variable on the vertical axis of this figure is the change in the national saving rate. The independent variable on the horizontal axis is the change in the full employment surplus. To be fair to the authors, this is not the regression they report in their various tables. However, three things are noteworthy. I can pick out six data points (circled in bold in Figure 1). These six points are outliers and, in

10. The opinions expressed here are mine and do not reflect the official position of the Federal Reserve System.
11. The data through 1996 employed in this analysis are revised from those previously available. Greece has been dropped, and New Zealand and Norway have been added to the dataset since the previous analysis. The range of the data has been changed for many of the remaining countries.

Figure 1 Year-to-Year Change in Full Employment Surplus



particular, one is way up at the top of the graph and two are way out to the right and below. These points have the tendency to pull the right end of the rightmost line segment down and, absent knots in the piecewise regression, would tend to pull the left end of the same segment up. Another outlier is in the upper left of the graph. This point will pull up the left end of the leftmost line segment. What happens if those outliers are ignored? I have drawn a large (gray) ellipse around the data points remaining after eliminating the outliers. The scatter within the ellipse looks pretty random. It is not clear that there is anything systematic about those points, either below the –1.5 percent threshold, between –1.5 percent and 1.5 percent, or above the 1.5 percent threshold.

My conclusion from this analysis is that the panel may include a few large outliers that substantially affect any estimated coefficients, particularly when regression lines are fitted in a piecewise fashion. Care should be taken to analyze such outliers, and all regression results should be tested to determine if they are robust to the inclusion of those observations.

The estimating equation that the authors use is

$$\frac{S_t}{Y_t^*} = \alpha_0 + \alpha_1 \frac{S_{t-1}}{Y_{t-1}} + \alpha_2 \frac{Y_t - Y_t^*}{Y_t^*} + \alpha_3 r_t + \beta_1 \frac{T_t}{Y_t^*} + \beta_2 d_t \frac{T_t}{Y_t^*} + \gamma_1 \frac{G_t}{Y_t^*} + \gamma_2 d_t \frac{G_t}{Y_t^*} + \gamma_3 d_t,$$

where S_t is national saving, Y^* is potential output, $(Y_t - Y_t^*)/Y_t^*$ is the output gap, r_t an *ex post* real interest rate, T_t/Y_t^* the ratio of taxes to potential output, and G_t/Y_t^* the ratio of government consumption to potential output. d_t represents a vector of dummy

variables that allow the slope and intercept of the regression line to change when the observation corresponds either to a large and persistent fiscal expansion or contraction, or to a high growth rate of the (cyclically adjusted) gross public debt, or a large spread between the long-term government bond yield and the 10-year U.S. Treasury yield.[12] The authors do not specify the model from which this equation is derived.

At first glance, the equation could be viewed as a reduced form, though technically it is not. The authors indicate that they have performed an instrumental variable regression with instruments for the interest rate and the tax ratio (see notes to the authors' Tables 2–5). In a true reduced-form equation, the variables on the right-hand side are exogenous—causality runs strictly from these variables to the dependent variable, and there is no need to instrument the regressors. Presumably within the model that the authors have in mind is a rule or a law that determines how the taxes are endogenously generated. Real interest rates presumably are also jointly determined with the saving ratio in that model.

It is not exactly clear how the output gap is treated in the instrumental variable regression. In the notes to the authors' Tables 2–5, there is no mention of the output gap being instrumented. However, in models that I am aware of, which include an output gap and a saving ratio, the two variables are jointly determined, so the problem of simultaneous equation bias exists here too. This leaves the government consumption ratio as the only exogenous variable in the specification. Variables such as oil shocks, changes in terms of trade, productivity shocks, and a long list of other variables that could reasonably be expected to be exogenous for at least some of the countries in the sample are conspicuously absent from the regression. It is likely that there are omitted variables which generate specification error.

The full employment primary surplus is indicated as the instrument for net taxes. That seems like a peculiar instrument. Neither government consumption nor government investment is instrumented. Would not the full employment primary surplus plus government consumption, or perhaps government consumption plus government investment, be a better instrument for net taxes? Certainly that measure can be easily constructed from the data used in the regressions.

The results of the empirical analysis are perhaps best understood by working from the most general specification in the authors' Table 5 to the most specific in Table 2. In Table 5, the estimated slope coefficients on the interaction of dummy variables for rapid debt growth and large changes in interest rate spreads with net taxes and government expenditures are not significantly different from zero. Nor are the estimated coefficients on these dummy variables by themselves significant. It would have been helpful if the authors had provided the result of an *F*-test that these 18 estimated parameters are simultaneously equal to zero. That information would support (or reject) the restricted specifications in the authors' Tables 3 and 4.

In the authors' Table 4, the authors report the result of omitting the dummy variable for large changes in interest rate spreads, both in interaction and stand-alone form. With these restrictions, the estimated coefficients on the interaction of the rapid debt growth dummy variable with the net taxes and government consumption variables

12. Presumably this dummy variable is always zero for the United States.

remain insignificant, though the estimated coefficient on the dummy variable itself is estimated with more precision and becomes significant as the sample size is extended from a terminal date of 1996 to a terminal date of 2003. The specification in this table retains the distinction between large increases and decreases in the cyclically adjusted surplus. The estimated coefficients on the interactions of large surpluses and large deficits with both net taxes and government consumption are similar in size. The point estimates of the coefficients of the fiscal expansion and contraction dummies are substantially different, but they are estimated with large standard errors. It would have been interesting had the authors provided an *F*-test of the equality of the estimated coefficients involving the two dummy variables in support of the restrictions that are implicit in going from the specification in the authors' Table 4 to that in Table 3. Since the adjusted R-squares in Table 3 are larger than those of the corresponding regressions in Table 4, the reader can probably safely infer that those restrictions are not rejected.

The authors' preferred set of results is that appearing in Table 3. This specification allows for interactions between both the large change in the surplus dummy and the rapid debt growth dummy and the fiscal variables. It also includes the two dummy variables by themselves. The authors note that the estimated coefficients of the interactions with the rapid debt growth dummy are not significantly different from zero. They also cite the significant estimated coefficients on the interactions of the change in surplus dummy with the fiscal variables as support for their hypothesis of a nonmonotonic effect of fiscal policy. They also note that for the longest sample period the changes in size of net taxes and government consumption relative to potential GDP have no effect on national saving in the regime of large changes in the surplus (the estimated coefficients on the fiscal variables are not significantly different from the estimated coefficients on the corresponding interaction terms). The reader should be careful to note that this result only implies that there is no marginal effect of the fiscal variables in the large surplus environment. The estimated coefficient on the dummy variable is significantly different from zero, indicating that on average national saving is lower in periods when there is a large change in the surplus.

The results reported in the authors' Table 2 are additional restrictions on the specification from those reported in Table 3. Relative to Table 3, the specification in Table 2 constrains the coefficients on all the terms involving the dummy variables to zero. In Table 3, the estimated coefficients on both dummy variables are reported to be significantly different from zero, as are those on the interaction terms with the fiscal variables. Yet a comparison of the adjusted R-squares reported in Table 2 with those in Table 3 indicates substantially higher values in Table 2. These results are difficult to reconcile, and a more detailed analysis of the restrictions is warranted.

What really is of interest is how private saving responds to fiscal actions. The dependent variable in the regression equation is the total saving ratio. But total saving is just government plus private, which is net taxes minus government purchases plus private saving. So the government saving can be subtracted from both sides of the regression, with the resulting coefficients of $\beta - 1$ on net taxes, and $\gamma + 1$ on government consumption.[13] These are the coefficients on the T ratio and

13. There would also be an additional term, government investment, on the right-hand side of the regression with a coefficient of 1.0.

the G ratio themselves, not the coefficients of the interaction terms. In the shorter samples, $\gamma + 1$ is not significantly different from zero. In the longest sample, $\gamma + 1$ approaches significance.

How can we understand what is going on here? I think a more interesting and informative way to look at this question would be to use a technique that rarely appears in econometrics texts, but is well known in other disciplines. This is replication. There are over 30 years of data from 11 countries. The observations that receive the large fiscal shock treatment are fairly uniformly spread across the countries and across time. It would be informative to separate the data into four roughly equal eight- to nine-year subsamples and replicate the analysis across the subsamples. Alternatively or additionally, replication could be used in the cross-section dimension of the dataset. Subpanels of three to four randomly selected countries could be created. Then the stability of the estimated coefficients across the replicates could be examined. Are the results robust in different subsamples/subpanels? Do they change in a systematic fashion? I believe that the replication technique would give better insight into the robustness or fragility of the evidence for the hypothesis than can be gleaned by putting all the data into one big regression as is done here.

Because of autocorrelation, such subsamples might not be truly independent as would be the case when two groups of people are selected and one group is given a drug and one group a placebo. But autocorrelation is likely quite low among subsamples, and the replication will give a better idea of robustness of these results than the standard errors from the basic regression program.

Finally, there is one other measurement issue. The saving defined here is the National Income and Product Account (NIPA) definition of saving. It is a well understood concept and standardized across countries. But it is not the only concept of saving available. In particular, Figure 2 gives three different measures of the personal saving ratio for the United States. This is the ratio of personal saving relative to GDP, it is not a national saving ratio. These measures come from the Federal Reserve's flow of funds statistics. One line is the ratio of NIPA personal saving to GDP. The other two lines are flow of funds personal saving ratios, one of which includes consumer durables and one of which excludes them. As far as the trends are concerned, they look very similar, although it is interesting that all of the lines have a small positive trend until the late 1980s or early 1990s and since then they show a significant negative trend. This break in trend may be unique to the United States, but it is not caught in the authors' regression equation. I am not sure that anybody has a good explanation for this break, but how it affects the results should be a concern.

Consider the changes in these three personal saving ratios, shown in Figure 3. These changes are quite volatile, and there are many instances where the flow of funds measures give large changes and the NIPA measure does not. The flow of funds measures include capital gains, the national income one does not. In cross-country analyses, the treatment of consumer durables could be important. A personal example: I managed to live in Tokyo for three months without ever driving a car. I cannot live in St. Louis for three hours without driving a car, which illustrates that transportation services are consumed in strikingly different ways across countries. This should be

Figure 2 Three Measures of Personal Saving to GDP: United States

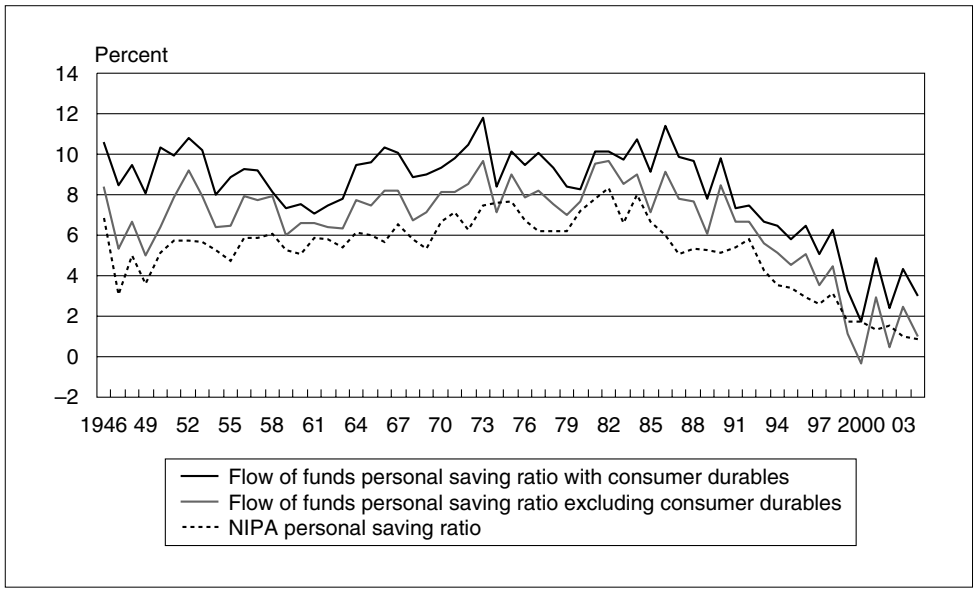
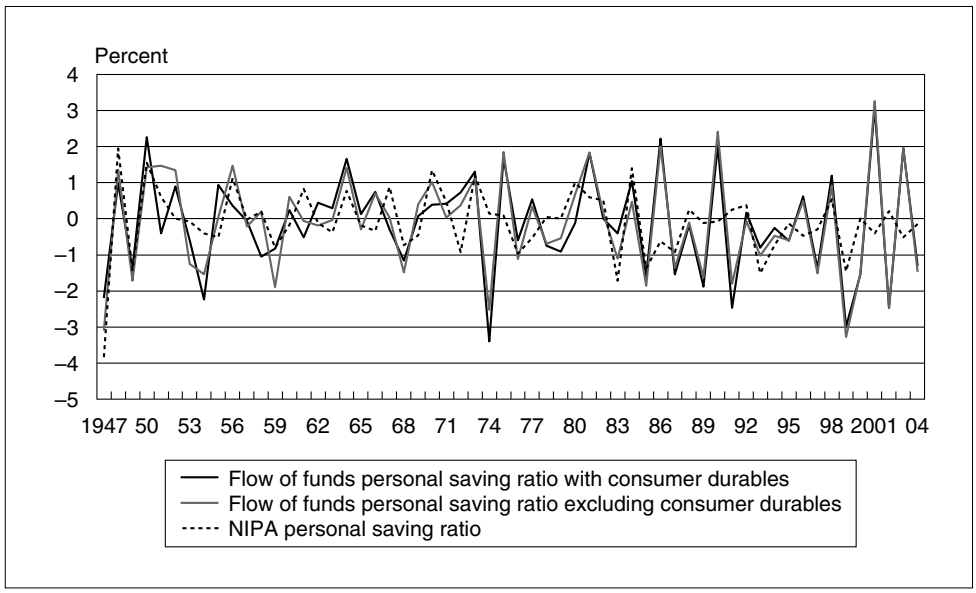


Figure 3 Three Measures of Changes in Personal Saving Ratio: United States



taken into consideration when thinking about how saving should be measured. Just to emphasize the point, correlations of the three measures of change in personal saving rates for the United States can be computed. The changes in the two flow of funds saving ratios are very highly correlated, 0.92. The correlation of the flow of funds with consumer durables measure against the NIPA measure is only 0.37. Even when consumer durables are excluded from both concepts of saving, the correlation is only

0.52. Again, measurement issues appear to be important. It is appropriate to consider what concept of saving is relevant to investigate the questions at hand.

Reference

Giavazzi, Francesco, Tullio Jappelli, and Marco Pagano, "Searching for Non-Linear Effects of Fiscal Policy: Evidence from Industrial and Developing Countries," *European Economic Review*, 44 (7), 2000, pp. 1259–1289.

General Discussion

Responding to comments made by the discussants, Francesco Giavazzi noted that the reduced-form estimation implicitly assumed a standard consumer behavior model and tried to control other conditions that could possibly produce a non-Keynesian effect. He agreed that the endogeneity problem might become more serious in the second part of the sample period, reflecting the tendency of governments, especially in the countries of the European Union (EU), to follow a fiscal policy rule more consistently than before.

Giavazzi agreed that exchange rate channel played an important role in stimulating economic activity, as observed in Denmark and Ireland in the 1980s and Italy in the mid-1990s. However, he added that such a mechanism was not expected to work within the euro area at present. He agreed that the inflow of foreign capital into the United States had contributed to the low interest rate, thereby enhancing the Keynesian effect in the United States. However, he added that Japan's experience seemed less favorable to the Keynesian effect as a large fiscal expansion through government investment did not seem to produce the traditional expansionary effect.

Following Giavazzi's response to two discussants, some participants emphasized the role of the private sector's expectations. Ulrich Kohli (Swiss National Bank) noted that changes in government expenditure should be divided into temporary and permanent components. He added that the negative coefficient on government expenditure did not necessarily contradict the Ricardian equivalence hypothesis, because if the increases in government expenditure were expected to be temporary, then nothing much would happen to private consumption and therefore national savings would fall. Janet L. Yellen (Federal Reserve Bank of San Francisco) noted the possibility that current changes and future expected changes in fiscal policy had opposite impacts on output. More precisely, tighter current fiscal policy should be contractionary, whereas expectations of tighter fiscal policy in the future could be expansionary because it would lower future expected short-term interest rates and thus current long-term interest rates. Yellen mentioned that "Rubinomics" could be most coherently explained by the scenario that a path of backloaded progressive fiscal tightening produces a current expansionary effect. José Luis Malo de Molina (Banco de España) suggested that the possible dominance of the Ricardian effect was mainly related to the private sector's perception of the constraints on public-sector solvency in the long run. He noted that the empirical model should capture the

initial position of the public sector in terms of the size of the deficit or surplus and the amount of debt, rather than the size or persistency of fiscal impulses.

Concerning the comment on temporary versus permanent changes in fiscal policy and backloaded adjustment, Giavazzi agreed with the importance of these points. He emphasized that because we do not know, when a policy is announced, whether it will be permanent, what mattered most was what the currently available information on the fiscal package told us about the future course of fiscal policy. He added that such information contents might be different for a large policy package than for a small one. To the comment on the size of debt, he responded that, although Olivier Blanchard's model clearly explained how changes in households' expectations caused contrasting effects when the amount of debt went beyond a certain threshold, he could not find empirical results supporting such a mechanism.

Shigenori Shiratsuka (Bank of Japan) suggested another interpretation of a non-Keynesian effect of fiscal policy based on a waiting-option motive for saving. When the fiscal situation was getting worse and uncertainty in the economy was growing, households tended to avoid making irreversible commitments and to postpone their expenditure decisions until a time when the uncertainties would be resolved. Giavazzi agreed that confidence and the option value of waiting were important.

Hiroshi Fujiki (Bank of Japan) proposed a variable for controlling the effects of a stricter implementation of fiscal rules in the 1990s than in the late 1980s: an index for a budgetary institution, as proposed by von Hagen (1992), to take care of the quality of fiscal rules.[14]

As for the Keynesian effect in the United States and Japan, Shiratsuka introduced a paper by Itoh and Watanabe (2004),[15] which found non-Keynesian effects in Japan by employing cross-prefecture panel data. In addition, Shiratsuka mentioned that there were many studies about the decline of the fiscal multiplier in the 1990s in Japan. Jean-Philippe Cotis (Organisation for Economic Co-operation and Development) provided a counterexample as recent research by the OECD using an error correction model, which enabled the clear separation of long-run and short-run dynamics, found no private savings offsets to changes in public deficits for the United States.

To conclude, Giavazzi emphasized that we should be careful in extracting policy implications from the empirical evidence shown in his paper, as well as in other related studies. He was concerned that policymakers tended to generalize about such evidence and to conclude that fiscal consolidations were always beneficial to the economy.

14. See Jürgen von Hagen, "Budgeting Procedures and Fiscal Performance in the European Communities," European Economy Economic Paper No. 96, Commission of the European Communities, 1992.
15. See Arata Itoh and Tsutomu Watanabe, "Zeiseiseisaku no Hi-Keinjian Koka (Non-Keynesian Effect of Fiscal Policy)," *Keizai Kenkyu* (Economic Review), 55 (4), Hitotsubashi University, 2004, pp. 313–327 (in Japanese).