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ABSTRACT

We contribute to the intense debate on the real effects of fiscal stimuli by showing that the impact of government expenditure shocks depends crucially on key country characteristics, such as the level of development, exchange rate regime, openness to trade, and public indebtedness. Based on a novel quarterly dataset of government expenditure in 44 countries, we find that (i) the output effect of an increase in government consumption is larger in industrial than in developing countries, (ii) the fiscal multiplier is relatively large in economies operating under predetermined exchange rate but zero in economies operating under flexible exchange rates; (iii) fiscal multipliers in open economies are lower than in closed economies and (iv) fiscal multipliers in high-debt countries are also zero.

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As fiscal stimulus packages were hastily put together around the world in early 2009, one could not have been blamed for thinking that there must be some broad agreement in the profession regarding the size of the fiscal multipliers. Far from it. In a January 2009 *Wall Street Journal* op-ed piece, Robert Barro argued that peacetime fiscal multipliers were essentially zero. At the other extreme, Christina Romer, Chair of President Obama's Council of Economic Advisers at the time, used multipliers as high as 1.6 in estimating the job gains that would be generated by the $787 billion stimulus package approved by Congress in February 2009. The difference between Romer's and Barro's views of the world amounts to a staggering 3.7 million jobs by the end of 2010. If anything, the uncertainty regarding the size of fiscal multipliers in developing and emerging markets is even greater. Data are more scarce and often of dubious quality. A history of fiscal profligacy and spotty debt repayments calls into question the sustainability of any fiscal expansion.

How does financial fragility affect the size of fiscal multipliers? Does the exchange regime matter? What about the degree of openness? There is currently little empirical evidence to shed light on these critical policy questions. In this paper we aim to fill this gap by conducting a detailed empirical analysis that establishes the relevance of key country characteristics in predicting whether fiscal stimulus is effective or ineffective.

A big hurdle in obtaining precise estimates of fiscal multipliers has been data availability. Most studies have relied on annual data, which makes it difficult to obtain precise estimates. To address this shortcoming, we have put together a novel quarterly dataset for 44 countries (20 high-income and 24 developing). The coverage, which varies across countries, spans from as early as 1960:1 to as late as 2007:4. We have gone to great lengths to ensure that only data originally collected on a quarterly basis is included (as opposed to interpolated based on annual data). Using this unique database – and sorting out countries based on various key characteristics – we have estimated fiscal multipliers for different groups of countries in our sample. The paper's main results are summarized as follows:

1. In *developing countries*, the response of output to increases in government consumption is negative on impact. It is smaller by a statistically significant margin from both zero and the response estimated for high-income countries. In developing countries, output increases in response to a shock in government consumption only with a lag (of 2 to 4 quarters) and the cumulative response of output is not statistically different

from zero. Fiscal policy differs in developing countries not only in its effect, but also in its execution, as increases in government consumption are far more transient (dying out after approximately 6 quarters), in contrast to highly persistent government consumption shocks in high-income countries.

2. The degree of *exchange rate flexibility* is a critical determinant of the size of fiscal multipliers. Economies operating under predetermined exchange rate regimes have long-run multipliers that are larger than one in some specifications, but economies with flexible exchange rate regimes have essentially zero multipliers. The fiscal multiplier in countries with predetermined exchange rates is statistically different from zero and from the multiplier in countries with flexible exchange arrangements at any forecast horizon. We find that the main difference between the response to government consumption in countries with different exchange rate regimes is in the degree of monetary accommodation to fiscal shocks. Our evidence thus supports the notion that the response of central banks to fiscal shocks is crucial in assessing the size of fiscal multipliers.

3. *Openness to trade* is another critical determinant. Economies that are relatively closed (whether due to trade barriers or larger internal markets) have long-run multipliers of around 1.3 to 1.4, but relatively open economies have negative multipliers. In economies with large proportions of trade to GDP the multiplier is statistically different from zero and from the multiplier in open economies at any forecast horizon. The multiplier in open economies is negative and significantly lower than zero both on impact and in the long run.

4. During episodes where the outstanding *debt* of the central government was high (exceeding 60 percent of GDP) the fiscal multiplier was not statistically different from zero on impact and was negative (and statistically different from zero) in the long run. Experimentation with a range of sovereign debt ratios indicated that the 60% of GDP threshold, used for example by the Eurozone as part of the Maastricht criteria, is indeed a critical value above which fiscal stimulus may have a negative, rather than a positive impact on output in the long run.

5. We do not find that the multiplier on government investment is significantly higher than that of government consumption in most country groupings. An exception is in

developing countries, where the multiplier on government investment is positive, close to 1 in the medium term, and statistically different from the multiplier on government consumption at forecast horizons of up to two years. This indicates that the composition of expenditure may play an important role in assessing the effect of fiscal stimulus in developing countries. Our point estimate of the fiscal multiplier on government investment is larger than that of government consumption in high-income countries as well, but this difference is small and not statistically significant.

Given increasing trade integration and the adoption of flexible exchange rate arrangements – particularly the adoption of inflation targeting regimes – our results cast doubt on the effectiveness of fiscal stimuli. Moreover, fiscal stimuli are likely to become even weaker, and potentially yield even negative multipliers, in the near future, because a large number of countries are now carrying very high public debt ratios. At the same time, our findings provide new evidence on the importance of fiscal-monetary interactions as a crucial determinant of the effects of fiscal policy on GDP.

The paper proceeds as follows: Section 1 discusses the empirical methodology. Section 2 describes the new dataset used in this study. Section 3 conducts the econometric analysis and reports the results. Section 5 concludes.

1 Methodology

1.1 Identification of Fiscal Shocks

In addition to the existing debate on the size of the fiscal multipliers, there is substantial disagreement in the profession regarding how one should go about identifying fiscal shocks. This identification problem arises because there are two possible directions of causation: (i) government spending could affect output or (ii) output could affect government spending (through, say, automatic stabilizers and implicit or explicit policy rules). How can we make sure that we are isolating the first channel and not the second?

Two main approaches have been used to address this identification problem: (i) the structural vector autoregression approach (SVAR), first used for the study of fiscal policy by Blanchard and Perotti (2002) and (ii) the "natural experiment" of large military buildups first suggested by Barro (1981) and further developed by Ramey and Shapiro (1998). Rather

than using military buildups per se to identify fiscal shocks, Ramey and Shapiro (1998) use news of impending military buildups (through reporting in Business Week) as the shock variable.

The basic assumption behind the SVAR approach is that fiscal policy requires some time (which is assumed to be at least one-quarter) to respond to news about the state of the economy. After using a VAR to eliminate predictable responses of the two variables to one another, it is assumed that any remaining correlation between the unpredicted components of government spending and output is due to the impact of government spending on output. The possible objection is that these identified shocks, while unpredicted by the econometrician, may have been known to private agents. We will revisit the question of the predictability of SVAR residuals in our data in section 2.1.

The natural experiment approach relies on the fact that it is very unlikely that military buildups may be caused by the state of the business cycle, and thus are truly exogenous fiscal shocks. The objections to this approach are (i) military buildups occur during or in advance of wars, which might have a macroeconomic impact of their own and (ii) in the United States, two military buildups (WWII and the Korean war) dwarf all other military spending, so that in practice, this instrument may be viewed as consisting of only two observations (see Hall (2009)).

In the few OECD countries that have been studied so far, the existing range of estimates in the SVAR literature varies considerably. Specifically, Blanchard and Perotti (2002) find a multiplier of close to 1 in the United States for government purchases. Perotti (2004a, 2007), however, shows that estimates vary greatly across (five OECD) countries and across time, with a range of -2.3 to 3.7. Other estimates for the United States – using slight variations of the standard SVAR identifying assumption – yield values of 0.65 on impact but -1 in the long run (Mountford and Uhlig (2008)) and larger than one (Fatas and Mihov (2001)).

In the "natural experiment" literature, Ramey (2009) recently extended and refined the Ramey and Shapiro (1998) study using richer narrative data on news of military buildups and finds a multiplier of close to 1. She also shows that SVAR shocks are predicted by professional forecasts and Granger-caused by military buildups, a critique of the SVAR approach. Using a similar approach, Barro and Redlick (2009) find multipliers on military spending of around 0.5. Fisher and Peters (2009), on the other hand, address possible anticipation effects using stock prices of military suppliers as an instrument for military spending, and find a multiplier

of 1.5.

In this paper, we employ the SVAR approach as in Blanchard and Perotti (2002) and elsewhere. In our case the choice is forced because the military buildup approach is not practical for our purposes. While U.S. wars have been fought primarily on foreign soil and have not involved significant direct losses of productive capital, this is certainly not the case in developing or smaller developed countries. While the main cause for military buildups are wars or the anticipation of wars, in most countries wars have had devastating direct macroeconomic effects. Identifying government consumption through military purchases would risk conflating the effects of government consumption on output with those of war, risking significant misestimation of fiscal multipliers in developing countries. However, as we discuss in section 2.1, we also believe that the volatility of government consumption in developing countries makes it less predictable to the private sector, which makes the SVAR approach less immune to the standard critique mentioned above.

1.2 Estimation Methodology

Following Blanchard and Perotti (2002), our objective is to estimate the following system of equations:

$$AY_{n,t} = \sum_{k=1}^{K} C_k Y_{n,t-k} + Bu_{n,t}, \tag{1}$$

where $Y_{n,t}$ is a vector of variables comprising government expenditure variables (e.g. government consumption and/or investment), GDP, and other endogenous variables (the current account, the real exchange rate, and the policy interest rate set by the central bank) for a given quarter t and country n. C_k is a matrix of the own- and cross-effects of the k^{th} lag of the the variables on their current observations. The matrix B is diagonal, so that the vector u_t is a vector of orthogonal, i.i.d. shocks to government consumption and output such that $Eu_{n,t} = 0$ and $E\left[u_{n,t}u'_{n,t}\right]$ is an identity matrix. Finally, the matrix A allows for the possibility of simultaneous effects between the endogenous variables $Y_{n,t}$. We assume that the matrices A, B, and C_k are invariant across time and countries. In additional regressions (not reported), we have allowed for variability across countries to ensure that our results are robust to assuming heterogeneity in autoregressive processes across countries.[1] Results

[1]Formally, we used the Mean Group estimator of Pesaran and Smith (1995) and obtained similar results to the ones reported here, although the power of the regressions for inference purposes was significantly

are also robust to an "international VAR" specification, where the endogenous variables of large countries in the sample (either the U.S. or all G7 countries, excluding Japan, which is not part of our sample) are used as exogenous inputs to the estimating equations of other countries.

In our standard specification, the system (1) can be estimated by panel OLS regression.[2] OLS provides us with estimates of the matrices $A^{-1}C_k$. As is usual in SVAR estimation of this system, additional identification assumptions are required to estimate the coefficients in A and B. In our benchmark regressions, which are bivariate regressions given by $Y_{n,t} = \begin{pmatrix} g_{n,t} \\ y_{n,t} \end{pmatrix}$, where g_t and y_t are government consumption and output, respectively, we follow Blanchard and Perotti (2002) in assuming that changes in government consumption require at least one quarter to respond to innovations in output. This is equivalent to a Cholesky decomposition with g_t ordered before y_t or the assumption that A takes the form $A = \begin{pmatrix} 1 & 0 \\ a_{21} & 1 \end{pmatrix}$.

We choose to pool the data across countries rather than provide estimates on a country-by-country basis. As we discuss in Section 2, with the exception of a handful of countries, the sample for a typical country is of approximately ten years, yielding around forty observations. We therefore exploit the larger sample size – almost always exceeding one thousand observations – delivered from pooling the data. We divide the sample into a number of country–observation groupings: high-income versus developing, predetermined versus flexible exchange arrangements, and open versus closed, among others. We then estimate and compare the fiscal multiplier across categories.

1.3 Fiscal Multipliers: Definitions

As there are several ways to measure the fiscal multiplier, a few definitions are useful. In general, the definition of the fiscal multiplier is the change in real GDP or other measure of output caused by a one-unit increase in a fiscal variable. For example, if a one dollar increase in government consumption in the United States caused a fifty cent increase in U.S. GDP, then the government consumption multiplier is 0.5.

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diminished.

[2] Formally, we use an OLS regression with fixed effects. All results are robust to using a GLS estimator allowing for different cross-sectional weights.

Multipliers may differ greatly across forecast horizons. We therefore focus on two specific fiscal multipliers. The *Impact Multiplier* defined as:

$$\text{Impact Multiplier} = \frac{\Delta y_0}{\Delta g_0},$$

measures the ratio of the change in output to a change in government expenditure at the time in which the impulse to government expenditure occurs. In order to assess the effect of fiscal policy at longer forecast horizons, we also report the *Cumulative Multiplier* at time T, defined as

$$\text{Cumulative Multiplier}\,(T) = \frac{\sum_{t=0}^{T} \Delta y_t}{\sum_{t=0}^{T} \Delta g_t},$$

which measures the cumulative change in output per unit of additional government expenditure, from the time of the impulse to government expenditure to the reported horizon. A cumulative multiplier that is of specific interest is the *Long-Run Multiplier* defined as the cumulative multiplier as $T \to \infty$.

1.4 Lag Structure

In choosing K, the number of lags included in system (1), we conducted a number of specification tests (the results are summarized in Table 1). As is often the case, and as evident from Table 1, the optimal number of lags varies greatly across country-groups and tests, ranging from 1 to 8. For simplicity, and for comparability across regressions, we set $K = 4$ in all reported results.

In VAR analyses, results often change significantly depending on the number of lags chosen in the VAR. It is reassuring that all the paper's results, including the five main results reported in the introduction, are robust to choosing any alternative number of lags from 1 to 8. The results are strengthened (meaning the difference between country-groupings is more significant) as the number of lags used in the regressions are increased. As a likelihood ratio test for the exclusion of lags 5 to 8 can be rejected at the 95% confidence level, it may well be the case that the reported results (using only 4 lags) understate the paper's results.

2 Data

To the best of our knowledge, this paper involves the first attempt to catalogue available quarterly data on government consumption in a broad set of countries. Until recently, only a handful of countries (Australia, Canada, the U.K. and the U.S.) collected government expenditure data at quarterly frequency and classified data into functional categories such as government consumption and government investment.

The use of quarterly data that is collected at a quarterly frequency is of essence for the validity of the identifying assumptions used in a Structural Vector Autoregression (SVAR), as we do in this paper. SVAR analysis assumes that fiscal authorities require at least one period to respond to new economic data with discretionary policy. But while it is reasonable to assume that fiscal authorities require a quarter to respond to output shocks, it is unrealistic to assume that an entire year is necessary. For example, many countries, including developing countries, responded with discretionary measures as early as the first quarter of 2009 to the economic fallout following the collapse of Lehman Brothers and AIG at the end of the third quarter of 2008. While in this particular instance the shock and response occurred in different calendar years, it clearly suggests that assuming that governments require an entire year to respond to the state of the economy cannot be generally valid.

In addition, data *reported* at a quarterly frequency but *collected* at annual frequency may lead to spurious regression results. One common method of interpolating government expenditure data that was collected at annual frequency is to use the quarterly seasonal pattern of revenue collection as a proxy for the quarterly seasonal pattern of government expenditure (data on tax revenues are more commonly collected at quarterly and even monthly frequency).[3] As tax revenues are highly procyclical, this method of interpolation creates a strong correlation between government expenditure and output by construction. Using an SVAR to identify fiscal shocks with data constructed in such a manner would clearly yield economically meaningless results.

This paper exploits the fact that a larger number of countries have begun to collect fiscal data at a quarterly frequency. Two recent changes made high-frequency fiscal data available for a broader set of countries. First, the adoption in 1996 of a common statistical standard in the European Monetary Union, the ESA95, encouraged Eurozone countries, and countries

[3] We have learned this from personal conversations with officials at numerous national statistical agencies.

aspiring to enter the Eurozone, to collect and classify fiscal data at quarterly frequency.[4] In its 2006 *Manual on Non-Financial Accounts for General Government,* Eurostat reports that all Eurozone countries comply with the ESA95, with quarterly data based on direct information available from basic sources, that represents at least 90% of the amount in each expenditure category.[5]

Second, the International Monetary Fund adopted the Special Data Dissemination Standard (SDDS) in 1996. Subscribers to this standard are required to collect and report central government expenditure data at annual frequency, with quarterly frequency recommended. A number of SDDS subscribers have begun collecting fiscal data at quarterly frequency and classifying expenditure data in to functional categories at that frequency.

With these institutional changes, a decade or more of quarterly data is now available for a cross-section of 44 countries, of which 24 are developing countries (based on World Bank income classifications). While ten years (40 observations) of data are hardly enough to estimate the effect of fiscal policy on output for an individual country, the pooled data contains more than 2,500 observations – an order of magnitude greater than used in VAR studies of fiscal policy to date.[6]

Table 2 provides some summary statistics for the main new variable in the dataset: quarterly government consumption. The table provides information about the proportion of government consumption to GDP, the autocorrelation of (detrended) government consumption, and the variance of (detrended) government consumption relative to the variance of GDP. These statistics are calculated for a number of country groupings, which will be used in the empirical analysis of the following sections. The proportion of GDP devoted to government consumption varies from 9.6 percent in El Salvador to 27.4 percent in Sweden during the sample period. This reflects the larger government size (with government consumption averaging 20.8 percent) in high income countries than in developing countries (15.6 percent).

There is also a difference between high-income and developing countries in the persistence of government consumption. The cyclical component (deviations from quadratic trend) of government consumption has an autocorrelation coefficient of 0.75 in high income countries, compared with 0.54 in developing countries.

[4] See http://circa.europa.eu/irc/dsis/nfaccount/info/data/ESA95/en/een00000.htm for more details.

[5] Austria was an exception with a coverage of 89.6% and is not included in our sample.

[6] We ended the dataset with the fourth quarter of 2007 as data from 2008-9 may still be subject to significant revisions.

With respect to volatility, the greatest difference appears again in comparing developing to high-income countries. In both groups of countries, government consumption is more variable than GDP. However, while in high-income countries government consumption is less than twice as volatile as GDP, in developing countries it is more volatile by a factor of 8.

A country-by-country description of data sources is available in the data appendix. Here we address the use of the data in the empirical analysis that follows. The main specification includes real government consumption and GDP. Other specifications include real government investment, the ratio of the current account to GDP, the real effective exchange rate, and the policy short-term interest rate targeted by the central bank. Nominal data was deflated using the corresponding deflator, when available, and using the CPI index when such a deflator was not available; using a GDP deflator instead of CPI for those countries where both were available left the paper's results unchanged. We took natural logarithms of all government expenditure and GDP data and the real effective exchange rate.

The data show strong seasonal patterns. Our selected de-seasonalization method was the SEATS algorithm (see Gómez and Maravall (2000)). In an earlier version of this study we used the X-11 algorithm and obtained similar results. All variables were non-stationary, with the exception of the central bank interest rate and the ratio of the current account to GDP. The data used in the reported regressions are deviations of the non-stationary variables from their quadratic trend. Using a linear trend yielded similar results. The current account and the policy interest rate were included in levels, while the real exchange rate was included in first differences. After detrending the data, the series were stationary, with unit roots rejected at the 99 percent confidence level for all variables in both an Augmented Dickey–Fuller test and the Im, Pesaran and Shin (2003) test.

2.1 Are innovations to government consumption foreseen?

We now plot the data for a number of countries in the sample. A full chartbook of figures for all countries in our sample will be made available in a companion paper. In Figure 1 we display examples for two countries, illustrating that major fiscal events are captured at quarterly frequency in our dataset. The top panel shows data for Botswana; Uruguay is in the lower panel. The solid line shows government consumption (in real local currency units in Botswana, and as an index in Uruguay, both on a logarithmic scale). Recessions, defined

as two consecutive quarters of negative GDP growth are shaded in grey.

In Botswana – a major diamond exporter – fiscal cycles are strongly influenced by world diamond prices. The green shaded areas in Botswana's figure are years when world prices of precious gems increased by more than 30 percent. While government consumption is clearly very volatile in Botswana, a clear pattern emerges. In fact all large positive quarterly shocks to government consumption were during or following booms in the prices of precious gems. This is further evidence of the procyclicality of government expenditure in developing countries, as documented extensively elsewhere.[7] This phenomenon is not unique to Botswana. Figure 2 shows government consumption (circles, left-hand scale) and oil or gold prices (dots, right-hand scale) for Ecuador (top) and South Africa (bottom) respectively. Clearly, much of the movement in government consumption is attributable to commodity prices in these commodity-exporting countries. It is noteworthy, however, that government consumption typically reacts with a lag of a quarter or more to these shocks (which are likely to have an immediate impact on GDP) reinforcing the assumption that government consumption reacts with a lag of one quarter or more to business cycle shocks.

Turning to Uruguay in the lower panel of Figure 1, the most striking feature is the enormous impact of the recessions of the late-1990s and the financial crisis of the early 2000s on government consumption. Here too, fiscal policy appears to be procyclical, declining substantially during the financial crisis. The figure demonstrates again the difficulty in identifying exogenous changes in fiscal policy. Clearly, the main driver of government consumption in Uruguay is the state of the business cycle, indicating a strong causal effect in the opposite direction of that which we are attempting to identify in this paper. However, even within these longer cycles of declining government consumption during recessions and increasing government consumption during recoveries there is variability in government consumption, particularly when studying the data at quarterly frequency. Government consumption does not decline uniformly in recessions nor does it rebound uniformly in recoveries. The two most prominent examples in Uruguay are large increases in government consumption in 2000 and 2001. The first follows the International Monetary Fund's (IMF) approval of a Stand By Arrangement for Uruguay, in which the IMF loosened restrictions on Uruguay's deficit limit for the year. This is indicated in the leftmost vertical line in the figure. The second vertical line indicates the global foot and mouth disease scare, which spurred a significant (and

[7] See Kaminsky, Reinhart and Vegh (2004) and Ilzetzki and Vegh (2008).

costly) response by the Uruguayan government in the form of emergency livestock health inspections later in the year. (The livestock inspections were partially financed by a World Bank loan. In addition, large IMF disbursements began at the beginning of the year.)

Could these large fiscal shocks have been anticipated? Ramey (2009) has shown that fiscal shocks identified through VAR residuals are predicted by private forecasts in the United States. A similar exercise is difficult to conduct in the case of developing countries because there is little documentation of private sector expectations of fiscal policy. Nevertheless, we can provide suggestive evidence that these shocks could not have been foreseen. We do so by using data revisions by a number of central banks, for which (very short) time series of vintage government consumption data are available. These are shown in Figures 3a and 3b for Bulgaria, Ecuador, and Uruguay. The dotted markers indicate the error in the central bank's preliminary estimate of government consumption in a given quarter. This is calculated as the difference (in percent) between the final published data by the central bank and the first published official estimate (typically the quarter following the data point). The circle markers are the residuals from the government consumption equation in the VAR (for developing countries). While the availability of vintage data is limited, the short time-series available show a very clear correlation between the central bank's estimation error and the VAR residuals. This suggests that VAR residuals are a fairly good measure of unexpected innovations in government consumption. It is extremely unlikely that the information set of the private sector *prior* to shocks to government consumption was better than that of the central bank *after* the shock. But in developing countries, fiscal policy is sufficiently erratic that even ex-post estimates are subject to significant revision in following years. We find this evidence suggestive of the fact that, at least in developing countries, VAR residuals do capture a significant portion of unanticipated shocks to government consumption.

3 Results

3.1 High-income and developing countries

To exploit the largest possible sample of our government consumption data, we begin with a simple specification of a bivariate Panel VAR of the form $Y_{n,t} = \begin{pmatrix} g_{n,t} \\ y_{n,t} \end{pmatrix}$, where $g_{n,t}$ is

real government consumption and $y_{n,t}$ is real GDP. As a first cut at the data, we divided the sample into high-income and developing countries.[8] Figures 4 and 5 show the impulse responses to a 1 percent shock to government consumption at time 0 in the first column, and to output in the second column. Figure 4 gives responses for high-income countries and Figure 5 for developing countries.

The response of output to government consumption is in the lower left-hand panel of each figure. Two differences stand out between the impulse responses. First, the impact response of output to government spending is positive in high-income countries (0.08 percent), but is negative in developing countries (-0.03 percent). Both are statistically significant from zero and from each other at the 99% confidence level.[9] Second, the output response to a shock in government consumption is significantly less persistent than that of high-income countries. Indeed, while the output response for high-income countries remains significantly positive for the 20 quarters covered in the plot, it becomes zero (statistically speaking) for developing countries after only six quarters.

Based on the impulse responses depicted in Figures 4 and 5, we can compute the corresponding fiscal multipliers, using the definitions of Section 1.3. The impact multiplier for high-income countries is 0.37. In other words, an additional dollar of government spending will deliver only 37 cents of additional output in the quarter in which it is implemented. This effect of government consumption, while small, is statistically significant. For developing countries, the impact multiplier is negative at -0.21 and also statistically significant. The difference between the impact multiplier in the two groups of countries is statistically significant at the 99 percent confidence level.

Focusing on the impact multiplier, however, may be misleading because fiscal stimulus packages can only be implemented over time and there may be lags in the economy's response. To account for these factors, Figure 6 shows the cumulative multipliers for both high-income and developing countries at forecast horizons ranging from 0 to 20 quarters. For example, a value of 0.5 in quarter 3 would indicate that, after 3 quarters, the cumulative increase in

[8] We use the World Bank classification of high income countries in 2000, and include all other countries in the category "developing". The marginal countries are the Czech Republic, defined as developing in 2000, but high-income in 2006; and Slovenia, categorized as high-income in 2000, but as "upper-middle income" (and thus developing by our typology) before 1997. Excluding or reclassifying these two countries does not alter the results. Israel is classified as high income, based on this definition, but was categorized as an "emerging market" in J.P. Morgan's EMBI index. Excluding or reclassifying Israel does not alter the results.

[9] Displayed error bands in figures reflect 90% confidence intervals throughout the paper.

output, in dollar terms, is half the size of the cumulative increase in government consumption. The plots also report the value of the impact and long-run cumulative multipliers. Dashed lines give the 90 percent confidence intervals, based on Monte Carlo estimated standard errors, with 500 repetitions.

We can see that the cumulative multiplier for high-income countries rises from an initial value of 0.37 (the impact effect) to a long-run value of 0.80. Hence, even after the full impact of a fiscal expansion is accounted for, output has risen less than the cumulative increase in government consumption, implying some crowding out of output by government consumption at every time horizon. The multiplier is statistically different from zero at every horizon. On the other hand, the cumulative long-run multiplier for developing countries is only 0.18. In other words, in the long run, over four fifths of the increase in government consumption is crowded out by some other component of GDP (investment, consumption, or net exports).

3.2 Exchange rate regimes

As a second cut at the data, we divided our sample of 44 countries into episodes of predetermined exchange rates and those with more flexible exchange rate regimes. We use the de facto classification of Ilzetzki, Reinhart, and Rogoff (2008) to determine the exchange rate regime of each country in each quarter. Table A3 lists for each country the episodes in which the exchange arrangement was classified as fixed or flexible.[10]

The cumulative multipliers, shown in Figure 7, suggest that the exchange rate regime matters a great deal. Under predetermined exchange rates, the impact multiplier is 0.09 (and statistically significantly different from zero) and rises to 1.5 in the long-run. Under flexible exchange rate regimes, however, the multiplier is negative and statistically significant on impact, and statistically indistinguishable from zero in the long-run. The difference between the two results is statistically significant at every forecast horizon. The results are robust to dividing the sample by country, with each country classified based on the exchange rate regime it maintained for the majority of the period.

[10]We divided the sample into country-episodes of predetermined exchange rates. For each country we took any 8 continuous quarters when the country had a fixed exchange rate as a "fixed" episode and any 8 continuous quarters or more when the country had flexible exchange rates as "flex". As fixed we included countries with no legal tender, hard pegs, crawling pegs,and de facto or pre-anounced bands or crawling bands with margins of no larger than +/- 2%. All other episodes were classified as flexible. Based on this definition, Eurozone countries are included as having fixed exchange rates.

These results are, in principle, consistent with the Mundell-Fleming model, which would predict that fiscal policy is effective in raising output under predetermined exchange rate but ineffective under flexible exchange rates. In the textbook version, the initial effect of a fiscal expansion is to increase output, raise interest rates, and induce an inflow of foreign capital, which creates pressure to appreciate the domestic currency. Under predetermined exchange rates, the monetary authority expands the money supply to prevent this appreciation. Such monetary policy accommodation serves to accommodate the rise in output. Under flexible exchange rates, however, the monetary authority keeps a lid on the money supply and thus allows the real exchange rate appreciation to reduce net exports. Output does not change because the increase in government spending is exactly offset by the fall in net exports.

The broader monetary context of the fiscal stimuli is explored in Figure 8. This figure reports impulse responses to a 1 percent shock to government consumption in a VAR that now includes the ratio of the current account to GDP, the real exchange rate, and the short-term interest rate set by the central bank, in addition to government consumption and GDP.[11]

The first row of Figure 8a presents government consumption shocks in episodes of fixed and flexible exchange rates. The second row presents the response of GDP to these shocks. Although the impulses to government consumption are similar in both cases, the increase in GDP is positive, of a larger magnitude and much more persistent when exchange rates are fixed than under flexible exchange rates. The difference between the two is no longer statistically significant because of a substantial loss of observations due to the availability of policy interest rate data for only a subset of the sample.[12]

Figure 8b explores the traditional Mundell-Fleming channel. It shows the response of the real exchange rate (first row) and the current account (second row). We find only weak evidence for the traditional channel in this figure. As expected, the real exchange rate appreciates on impact under flexible exchange rates, but does not (in fact it depreciates) under

[11]The variables are Cholesky-ordered as follows: government consumption, the central bank's interest rate, GDP, the current account, and the real exchange rate. A discussion of this ordering is discussed in section 3.6, where full results from multivariate VARs are presented. The ordering of the fiscal variable before the central bank's instrument follows from the assumption that the monetary authority can respond more rapidly to news than can fiscal decision-makers can. Results are virtually unchaged if the policy interest rate is ranked lower in the Cholesky ordering. However, the response of the policy interest rates is significantly weakened if the ordering of the fiscal and monetary variables is reversed.

[12]More than one third of the sample is lost in this specification.

fixed exchange rates. However, this result is not robust in a multivariate regression excluding the policy interest rate (which includes a larger sample size), where a real appreciation is seen under both fixed and flexible exchange rates, following a government consumption shock. (The response of the real exchange rate is lagged in countries with fixed exchange rates, however.) Moreover, this does *not* translate into a larger decline in the current account in episodes where the exchange rate was flexible, as the Mundell-Fleming model would predict. The differences across exchange rate regimes are, moreover, not statistically significant.

On the other hand, we find strong evidence for the "monetary accommodation" channel, as shown in Figure 8c. Monetary authorities operating under predetermined exchange rates lower the policy interest rate by a statistically significant margin, with the short-term nominal interest rate declining by a cumulative 125 basis points in the two years following a government consumption shock of 1% of GDP. In contrast, central banks operating under flexible exchange rates increase the policy interest rate by a statistically significant margin, with interest rates increasing an average of 60 basis points within the two years following a fiscal shock of similar magnitude.

More generally, our results are related to the notion that monetary accommodation plays an important role in determining the expansionary effect of fiscal policy. Davig and Leeper (2009), for example, show in a DSGE model with nominal rigidities that the effect of fiscal policy differs greatly depending on whether monetary policy is active or passive. Coenen *et al* (2010) show that monetary accommodation is an important determinant of the size of fiscal multipliers in seven different structural models used in policymaking institutions. This result also relates indirectly to the theoretical studies of Christiano, Eichenbaum, and Rebelo (2009) and Erceg and Lindé (2010) showing that fiscal multipliers are larger when the central bank's policy interest rate is at the zero lower bound.

We thus find that differences in monetary accommodation are the main cause for differences in the magnitude of fiscal multipliers across exchange rate regimes. But the lack of evidence on differences in the response of the current account raises the question as to which components of GDP differ in their response across monetary regimes. With the current account deteriorating in response to a government consumption shock under both fixed and flexible exchange arrangements, the simple GDP accounting identity implies that either consumption or investment must differ in its response to government consumption shocks across these regimes. Figure 9 explores this question. In a new set of regressions, we replaced

GDP with two variables: private consumption and private investment.[13] Data availability restricted our attention to OECD countries and a small number of Latin American countries. Nevertheless, Figure 9 shows that there is a marked difference across exchange rate regimes between the response of private consumption and investment to government consumption shocks. The response of investment (in the first row of the figure) is similar under either predetermined or flexible exchange rate regimes. In both cases, the response of investment is erratic and investment declines by a statistically significant margin on impact, followed by additional dips in the future. The response of private consumption, on the other hand, differs greatly across exchange rate regimes. Under fixed exchange rates, consumption responds positively on impact and by a statistically significant margin to a shock in government consumption. Although the response under flexible exchange rates is not statistically significant from zero, our point estimates show a negative response of government consumption in both the short and long run.

This result is, in turn, related to the debate on the response of consumption to government consumption shocks. Perotti (2004a, 2007), using a VAR framework similar to ours, finds a positive response of private consumption to government consumption. On the other hand, Ramey (2009) finds that private consumption declines in response to a military expenditure shocks. While the focus in this debate has been on how to identify shocks to public expenditure, our results point to an additional potential explanation of these contrasting findings. Both approaches have ignored the interaction between fiscal and monetary policy. Once monetary policy is controlled for, we find that consumption does respond positively to government consumption shocks, but only when the central bank accommodates to the fiscal shock. Further exploration of fiscal-monetary interactions might help shed more light on the response of macroeconomic variables to government expenditure shocks.

3.3 Openness to trade

Next, we divide our sample of 44 countries into countries for which trade is a significant portion of GDP. We classified countries based on the ratio of trade (imports plus exports)

[13] Consistent with our earlier identifying assumption, we do not allow for a contemporaneous response of government consumption to unpredicted shocks to private consumption or private investment. The ordering of the latter two variables among the other variables in the VAR system did not affect the results reported here.

to GDP. As shorthand, we lablel an economy as "open" if this ratio exceeded 60 percent. If foreign trade is less than 60 percent of GDP, we defined the country as "closed". [A list of "open" and "closed" economies by this classification is shown in Appendix Table A5 @@@] Minor variations of this threshold did not significantly affect our results. Using this criterion, 28 countries are classified as "open", having high ratios of trade to GDP, and the remaining 16 are classified as "closed", with approximately half of the sample in either category.

The cumulative responses, shown in Figure @@, indicate the volume of trade as a proportion of GDP is a critical determinant of the size of the fiscal multiplier. For economies with high trade-GDP ratios, the impact response is 0.11 and the long-run multiplier is 1.4. For the economies with low trade volumes as a proportion of GDP, the impact response is negative and the long-run response is *negative* and statistically significant from zero. The difference between the two categories is statistically significant at every forecast horizon.

It should be apparent that this definition of trade openness conflates two main factors that affect the proportion of trade in a country's GDP. A country may have a low ratio of trade to GDP because it truly closed to trade in the sense that it has high tariffs or other barriers to trade, or a country may be have a low ratio of to trade to because it is a large economy with a relatively large internal market. We find however, that both factors affect the magnitude of the fiscal multiplier independently.

In defining openness based on legal restrictions to trade, we divided the sample into country-episodes where the weighted mean of tariffs across all products exceeded 4 percent and those where it was lower than 4 percent, according to the World Bank World Development Inidcators. 4 percent was roughly the median of this average tariff rate in our sample. To gain a sense of the magnitudes involved, in 2008 this figure ranged from 0.42 percent in Norway to 8.73 percent in Colombia among the countries in our sample. The figure for the U.S. was 1.49 percent. ["Open" and "closed" economy episodes in our sample based on this definition summarized in Table A6]. When defining openess to trade based on this criterion, we found a statistically significant difference between the multiplier in countries open and closed to trade at any forecast horizon, with multipliers of -0.28 on impact and -0.75 in the long run for open economies and 0.02 in impact and 1.29 in the long run for closed economies. [Results shown in Appendix Figure @@]

We then divided our sample into the ten largest economies (in terms of their total GNP

in U.S. dollars) on one hand and the remaining countries on the other.[14] We find that the fiscal multiplier is larger in large economies relative to small, with an impact multiplier of 0.02 in the former and -0.19 in the latter and a long-run multiplier of 1.2 in the former and -0.47 in the latter. This difference is statistically significant on impact, but not at longer horizons. [Results show in Appendix Figure @@]

As before, this result is, in principle, consistent with the texbook Mundell-Fleming model. In such a model, the fiscal multiplier would be lower in a more open economy (i.e., an economy with a higher marginal propensity to import) because part of the increase in aggregate demand would be met by a reduction in net exports rather than by an increase in domestic production.

3.4 Financial fragility

With debt burdens rapidly accumulating during the current round of global fiscal stimuli and several countries teetering on the verge of default, it is natural to ask how the level of sovereign debt affects the impact of government consumption stimulus on GDP. To this effect, we built a sample of country-episodes where the ratio of the total (including domestic and external debt of any currency) debt of the central government exceeded 60 percent of GDP. Any period of three (or more) consecutive years where this debt ratio exceeded 60 percent was included in this subsample. A list of "high-debt" episodes is provided in Table A6.

Figure 11 shows the resulting cumulative multiplier during periods of high debt burden. While the error bands are admittedly broad, our point estimates are in general consistent with the notion that attempts at fiscal stimulus in highly indebted countries may be actually counter-productive and their effects very uncertain. Our estimate for the impact multiplier is very close to zero, and we estimate a long run multiplier of -2.3. We are reassured that this result is not spurious by the fact that this long run multiplier remains negative when the threshold is set to 60 or 70 percent of GDP, while it becomes positive for debt-to-GDP ratios of 30 or 40 percent. But experimenting with different thresholds indicated that the 60 percent threshold was a meaningful cutoff, above which fiscal stimulus appears ineffective. In the

[14] Based on this threshold, countries with GNPs greater than or equal to that of Australia were considered "large." The Netherlands was the largest economy classified as "small."

lower panel of the same figure, we compare the government consumption multiplier during episodes when the debt-to-GDP ratio exceeded 60 percent in the lower line to those when the debt-to-GDP ratio was lower than 60 percent in high-income and developing countries in the top and middle lines, respectively. We find that the fiscal multiplier is lower when debt burdens are high, particularly in the long run.[15]

These results are consistent with the idea that debt sustainability may be an important factor in determining the output effect of government purchases. When debt levels are high, increases in government expenditures may act as a signal that fiscal tightening will be required in the near future. Moreover, as recent events in southern Europe illustrate, these adjustments may need to be sudden and large. The anticipation of such adjustments (in effect, a contraction in fiscal policy, possibly involving both a reduction in fiscal spending and higher taxes) should have a contractionary effect that would tend to offset whatever short-term expansionary impact government consumption may have. Under these conditions, fiscal stimulus may therefore be counter-productive.

3.5 Government Investment

While our focus so far has been on government consumption – due in part to limited availability of government investment data – it is nevertheless interesting to see whether the effects of government investment differ from those of government consumption. To explore this question, we estimate (1), this time with $Y_{n,t} = \begin{bmatrix} g^I_{n,t} \\ g_{n,t} \\ y_t \end{bmatrix}$, where $g^I_{n,t}$ is real government investment, and $g_{n,t}$ and y_t are real government consumption and real GDP as before. We follow Perotti (2004b) in ordering government investment before government consumption in the Cholesky decomposition, although results are not affected if the ordering is reversed. The number of countries in the sample declines when including government investment, but the results for government consumption reported in section 3 hold roughly for this sub-sample

[15] The lower panel of this figure omits (admittedly wide) error margins to avoid cluttering. They would show that the multiplier is larger by a statistically significant margin in the low-debt high-income country sub-sample relative to the high-debt subsample. Error bands are unfortunately too wide for any further inference. It is difficult to split the highly-indebted sample into high-income and developing countries, as the number of observations would be extremely low (lower than 100 for developing countries). We separate low-debt countries into high-income and developing as resposes are very heterogeneous in the two groups and pooling these two groups leads to results with very wide error margins as well.

as well.

Figure 12 shows the cumulative government investment multiplier for high-income countries in a simple bivariate regression, including only government investment and GDP. The smaller sample size yields estimates that are significantly less accurate. The estimated impact and long-run government investment multipliers are substantially higher than those on government consumption. However, the results in Figure 12 may be somewhat misleading, due to the exclusion of government consumption. As Figure 13 shows, government consumption responds strongly to government investment, so that the multiplier calculated in Figure 12 is attributing the entire increase in output to the increase in government investment, while ignoring the increase in government consumption.[16]

To address this issue, we estimate the multiplier to "pure" government investment multipliers, as suggested by Perotti (2004b). This is done by estimating the full system with the three endogenous variables, but setting all values of $g_t = 0$ in our forecasts of g_t^I and y_t. The resulting cumulative multipliers for high-income countries and developing countries are presented in Figure 14. The estimates of the government investment multiplier remain highly uncertain in high-income countries, in the upper panel of this figure. But their point estimates at all horizons are similar to the government consumption multipliers presented in Figure 6. We thus have no robust evidence that government investment is more productive in its simulative effect on output in high-income countries. This is consistent with the findings of Perotti (2004b).

In developing countries, in contrast, the lower panel of Figure 14 shows the impact multiplier of government investment is 0.6 and statistically significant. While our estimates have little power to predict the long-run effects of a shock to government investment in developing countries, we can reject (at the 95% confidence level) the hypothesis that the effect of government investment is no higher than that of government consumption in short horizons (the first two years). It appears that the composition of government purchases is an important determinant of the impact of government spending shocks on output in developing countries.

When comparing between predetermiend and flexible exchange rates, open and closed

[16] This is true of the response of government investment to government consumption as well. However, the omission of the latter from the regressions of section 3 does not have a significant impact on the estimate of government consumption multipliers. This is because, in all countries in our sample, government investment is small relative to government consumption.

economies, and countries with high debt-to-GDP ratios, we find similar results for the pure government investment multiplier as those in regressions with government consumption. We find a government investment multiplier is 0.36 on impact and 1.42 in the long run for predetermined exchange rates and 0.46 on impact and 0.16 in the long run for flexible exchange rates. The diffference between the two is, however, not statistically significant due to the smaller sample size. In economies with high trade to output ratios, we find a government investment multiplier of 0.51 on impact and -0.23 in the long run as comapred with 0.46 on impact and 0.70 in the long run in economies with low trade to output ratios. This difference is statistically significant at horizons ranging from 1 to 14 quarters. [See figures @ and @ in the Appendix for the actual results.]

3.6 Multivariate Regressions

We have so far primarily focused on bivariate panel VARs with real government consumption and real GDP as the endogenous variables. This section shows that the results reported above are robust to an expanded VAR system that includes the real effective exchange rate and the ratio of the current account balance to GDP. [17] Our rationale for adding these two variables is that, as discussed above, theory suggests that both the real exchange rate and net exports should play an important role in the economy's adjustment to government consumption shocks. We expect higher government consumption to lead to a real appreciation of the currency which, in turn, should affect the current account. It thus seems important to check the robustness of our results in this expanded, four-variable VAR.

As before, our identifying assumption calls for ordering government consumption before GDP. As for the ordering of the newly added variables, we follow Kim and Roubini (2008) and numerous other studies in ordering the remaining variables after GDP and ordering the current account balance before the real effective exchange rate.

The results are presented in Figure @@@, comparing the cumulative multiplier on government consumption in high-income versus developing countries, predetermined versus flexible exchange rates, and economies with high and low ratios of trade to GDP, respectively. Error bands are wider relative to Figures 6, 7, and 10, due to the loss of observations caused by

[17] Results are similar when including the policy interest rate as in section 3.2. However, the power of our analysis diminishes significantly as few countries in our sample used short-term interest rates as monetary instuments before the mid-2000s.

the inclusion of additional variable. Nevertheless, point estimates are similar to those found in bivariate regressions. In Figure @@@, we illustrate the fact that the negative multiplier in highly indebted economies is a result that is not only robust, but even strengthened when additional controls are included.

4 Conclusions

This paper is an empirical exploration of one of the central questions in macroeconomic policy in the past few years: what is the effect of government purchases on economic activity? We use panel SVAR methods and a novel dataset to explore this question. Our most robust results point to the fact that the size of fiscal multipliers critically depends on key characteristics of the economy studied.

We have found that the effect of government consumption is very small on impact, with estimates clustered close to zero. This supports the notion that fiscal policy (particularly on the expenditure side) may be rather slow in impacting economic activity, which raises questions as to the usefulness of discretionary fiscal policy for short-run stabilization purposes. The medium- to long-run effects of increases in government consumption vary considerably. In particular, in economies closed to trade or operating under fixed exchange rates we find a substantial long-run effect of government consumption on economic activity. In contrast, in economies open to trade or operating under flexible exchange rates, a fiscal expansion leads to no significant output gains. Further, fiscal stimulus may be counterproductive in highly-indebted countries; in countries with debt levels as low as 60 percent of GDP, government consumption shocks may have strong negative effects on output.

Finally, the composition of government expenditure does appear to impact its stimulative effect, particularly in developing countries. While increases in government consumption decrease output on impact in developing countries, increases in government investment cause an increase in GDP.

With the increasing importance of international trade in economic activity, and with many economies moving towards greater exchange rate flexibility (typically in the context of inflation targeting regimes), our results suggest that seeking the Holy Grail of fiscal stimulus could be counterproductive, with little benefit in terms of output and potential long-run costs due to larger stocks of public debt. Moreover, fiscal stimuli are likely to become even

weaker, and potentially yield even negative multipliers, in the near future, because of the high debt ratios observed in countries, particularly in the industrialized world.

On the other hand, emerging countries – particularly large economies with some degree of "fear of floating" – would be well served if they stopped pursuing procyclical fiscal policies. Indeed, emerging countries have typically increased government consumption in good times and reduced it in bad times, thus amplifying the underlying business cycle – what Kaminsky, Reinhart, and Végh (2004) have dubbed the "when it rains, it pours" phenomenon. The inability to save in good times greatly increases the probability that bad times will turn into a full-fledged fiscal crisis. Given this less-than-stellar record in fiscal policy, an a-cyclical fiscal policy – whereby government consumption and tax rates do not respond to the business cycle – would represent a major improvement in macroeconomic policy. While occasional rain may be unavoidable for emerging countries, significant downpours would be relegated to the past.

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Data Appendix

The greatest challenge of this high-frequency study of fiscal policy in a large number of countries was collecting and vetting the data. We have gone to great lengths to ensure that data included in the dataset was originally collected at a quarterly frequency, covered a large proportion of the government expenditure category studied. Tables A1 and A2 summarize the sources, time frame, and definitions of the two main government expenditure categories studied. In all cases, the integrity of the data was confirmed directly through correspondence or conversation with national statistical agencies, central banks, or fiscal authorities. While some of the time series extend to 2008 or 2009, data from these last two years were not used in the empirical analyses in this paper, as recent data may still be subject to significant revisions.

One inconsistency across countries in the data set is in the level of government included. In most cases, data for the general government was available, but in some cases, only the expenditures of the central government were available. The exclusion of regional government consumption risks biasing our results, as fiscal multipliers may be overstated or understated, depending on whether the excluded expenditures are positively or negatively correlated with central government consumption, and whether they have similar effects on economic activity. We opted to keep coverage as broad as possible and included all countries in the sample, regardless of the level of government for which data was available. The results reported in this paper did not change when only countries for whom general government consumption data was available.

Another possible concern is that in some cases data was deflated directly by the local statistical agencies. In other cases, we deflated the data using a CPI deflator. This both creates an inconsistency across countries, but moreover raises the question of whether consumer prices are the appropriate measure of the ratio between the nominal value of government purchases and their real value. Finding an appropriate government consumption deflator is not a simple task, but we were reassured by the fact that in countries where more than one deflator was available (e.g. GDP deflator, CPI, or a government consumption deflator) the correlation of the series when deflated using different price indexes was close to 1. Other variables are as follows.

Gross Domestic Product Whenever possible, GDP data are taken from the same data source as the government expenditure data. Elsewhere, IFS series 99B was used for developing countries and real GDP from OECD Statistics was used for high-income countries.

Consumer Price Index IFS series 64.

Current Account OECD Statistics, when available, and IFS series 75 elsewhere. Current account to GDP ratio was created by dividing this series using nominal GDP from OECD statistics or from IFS series 99B converted to US dollars using the average interest rate for that year.

Central Bank Discount Rate Central bank discount rates were taken from IFS series 60 whenever available. Series 60P (repurchase agreement rate) was used for France, series 60A (rate on advances) was used for the Netherlands, and 60A for Romania (National Bank of Romania Structural Credit rate). Central bank data was used to obtain the policy rates for Australia, El Salvador, the Eurozone, Estonia, Mexico, and the United Kingdom. Series for Eurozone countries were created by splicing interbank rates of the national central banks until the later of 1998 or the date of Euro adoption, with the ECB's deposit facility rate thereafter. In Estonia, the average between the one-month Talibid and Talibor rates was used (bid and ask rates), as the overnight rate was used as a policy tool starting only in 2007. For Mexico, the 30-day interbank rate (TIIE) was used.

Debt to GDP Debt of the central government (external only for developing countries) as a proportion of GDP from the OECD, Eurostat, the Joint External Debt Hub, and the Federal Reserve Bank of St. Louis and ONS for the United States and United Kingdom, respectively.

Real Effective Exchange Rate A CPI-based real exchange rate was used. Where ever available, the narrow real exchange rate index of the Bank for International Settlements was used. Otherwise, the broad index was used. Elsewhere, IFS series RECZF was used.

Trade to GDP The ratio of the sum of imports (IFS series 70) and exports (IFS series 71) to GDP (IFS series 99).

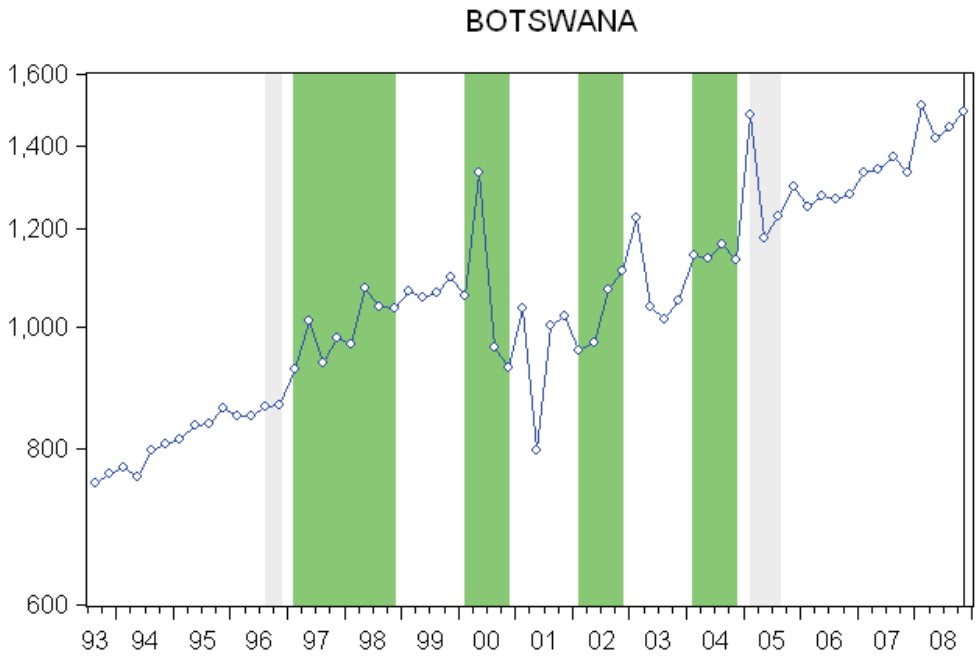
Table 1: Optimal Number of Lags Based on Specification Tests

	Model						
Criteria	High Income	Developing	Fixed	Flex	Open	Closed	Highly Indebted
Akaike	8	8	8	8	8	8	4
Schwartz	4	2	4	2	2	3	1
Hannan Quinn	4	8	8	7	8	4	3

Table 2: Summary Statistics on Quarterly Government Consumption Data

Summary Statistics on Quarterly Government Consumption Data			
	Gc/GDP	**Autocorrelation**	**Var(Gc)/Var(GDP)**
Full Sample	17.97%	0.64	5.31
	(4.76%)	(0.32)	(10.26)
High income	20.77%	0.75	1.79
	(3.39%)	(0.28)	(2.16)
Developing Countries	15.63%	0.54	8.24
	(4.52%)	(0.33)	(13.16)
Fixed exchange rates	17.56%	0.66	4.29
	(4.71%)	(0.32)	(7.51)
Flexible exchange rates	18.10%	0.63	7.15
	(4.77%)	(0.30)	(12.88)
Open economies	20.05%	0.68	5.11
	(3.50%)	(0.29)	(9.77)
Closed economies	14.07%	0.56	6.08
	(4.46%)	(0.38)	(11.83)
Debt>60%GDP	18.43%	0.57	2.60
	(5.12%)	(0.44)	(3.18)
Group averages. One standard deviations in parentheses.			

Figure 1: Quarterly government consumption



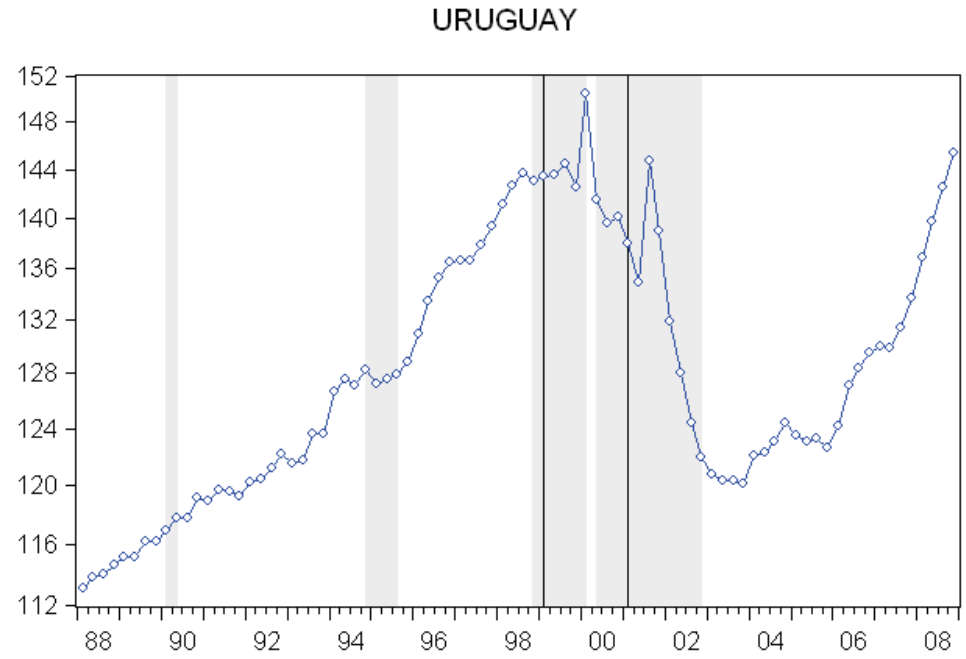


Figure 2: Quarterly government consumption and commodity prices

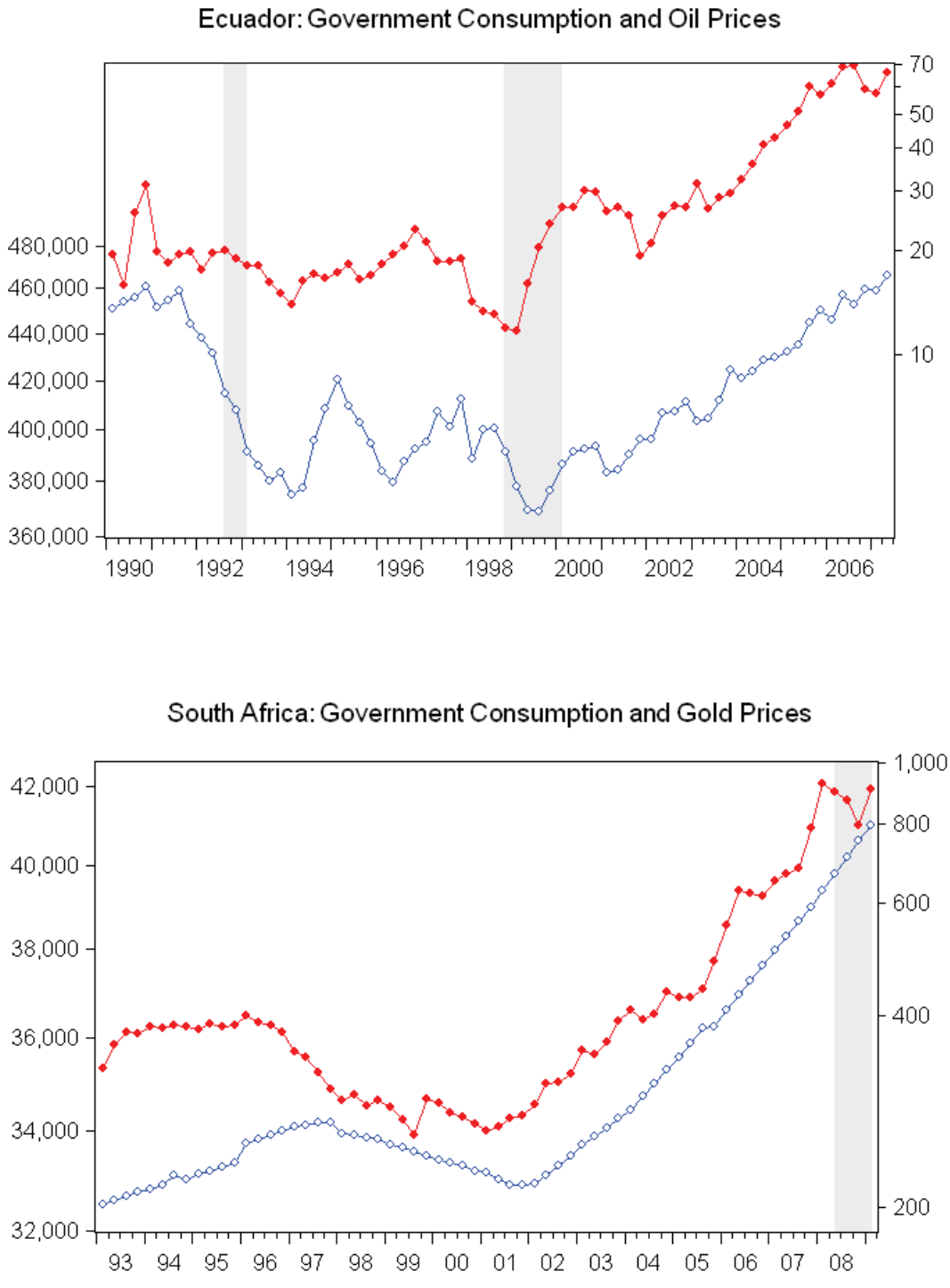
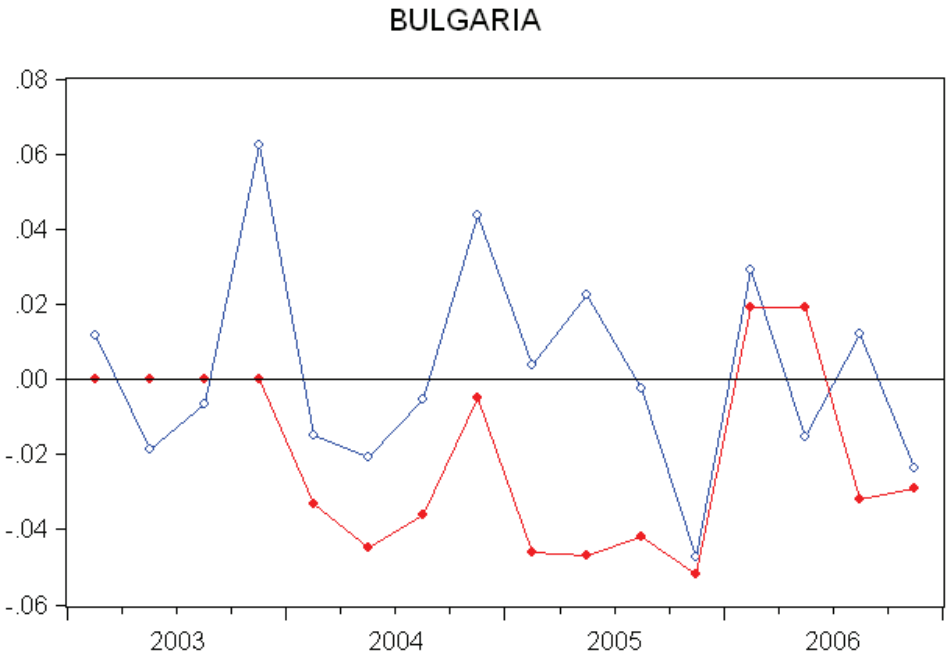


Figure 3a: Central bank estimation errors and VAR residuals



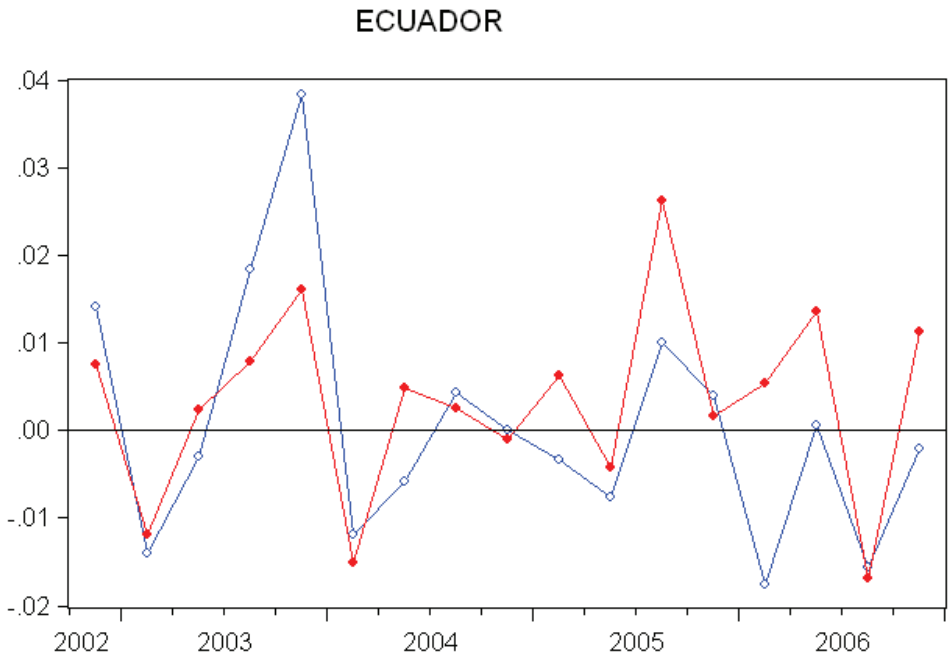


Figure 3b: Central bank estimation errors and VAR residuals

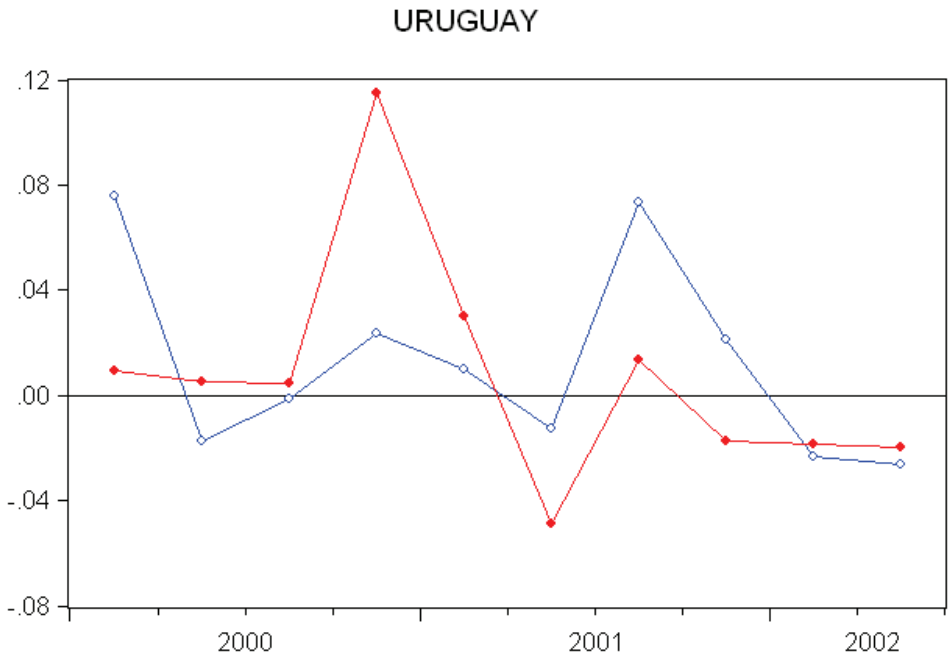


Figure 4: Impulse responses in high-income countries

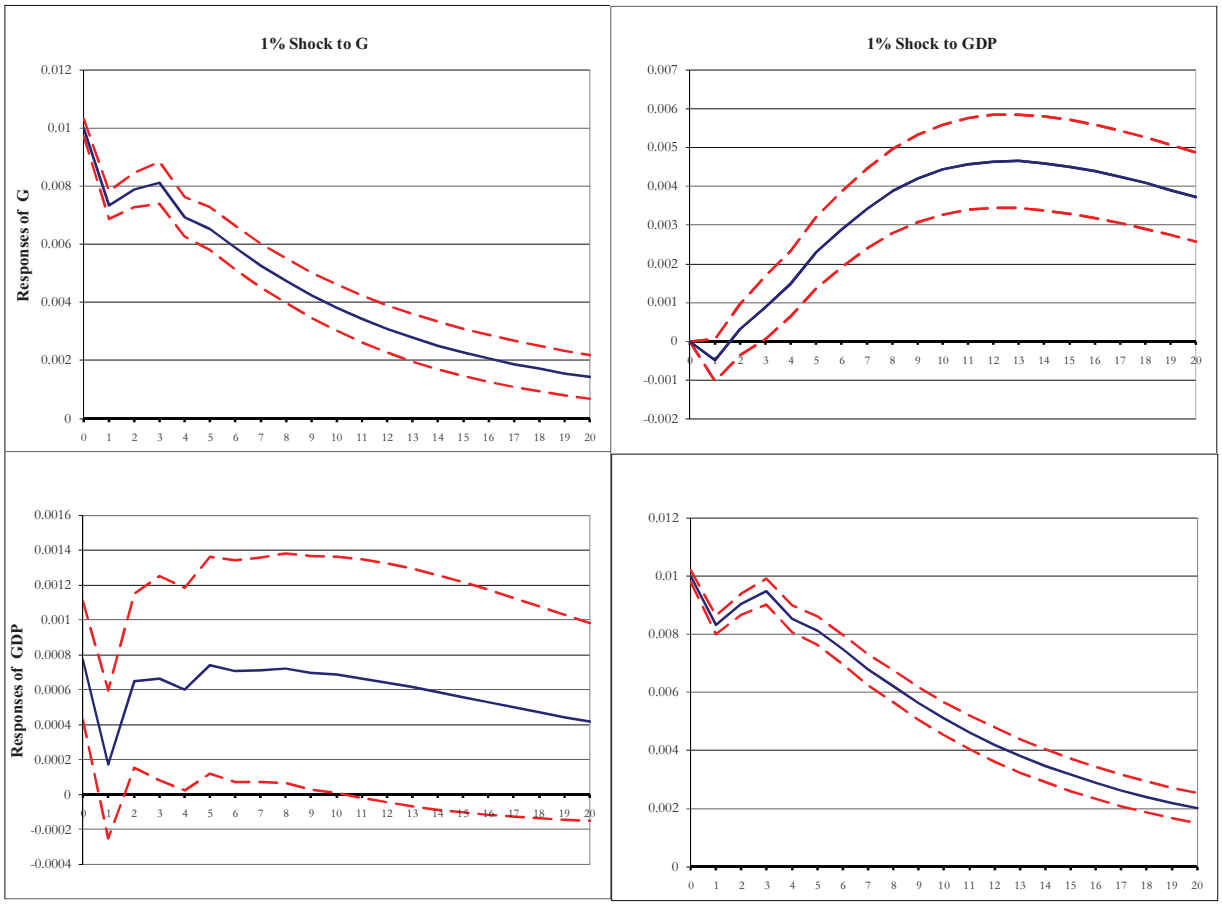


Figure 5: Impulse responses in developing countries

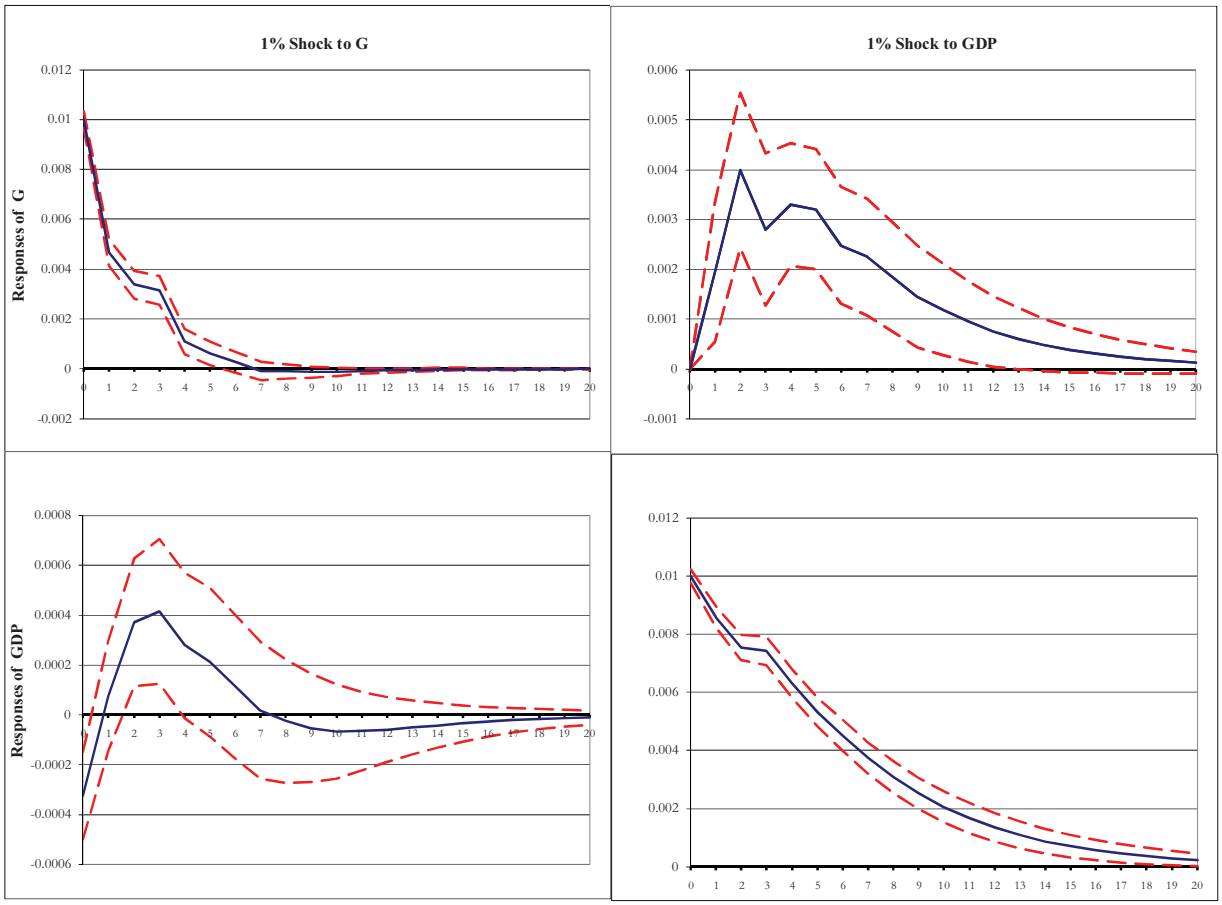
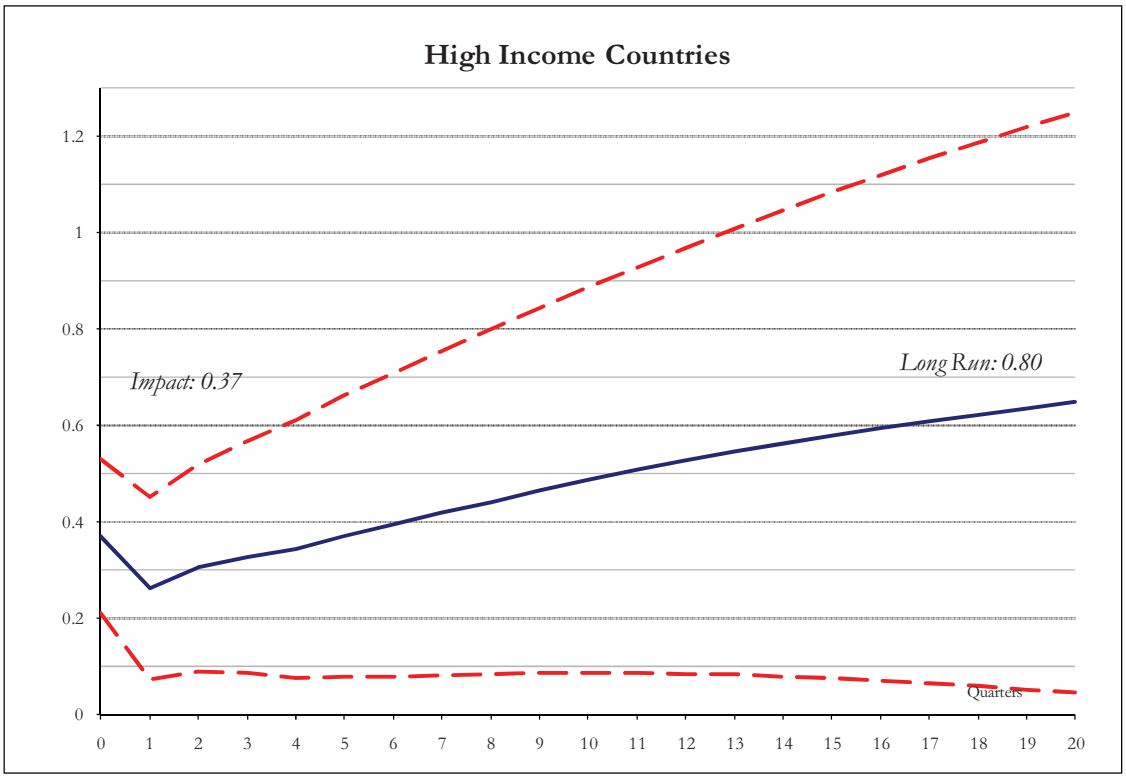


Figure 6: Cumulative multiplier—high income and developing countries



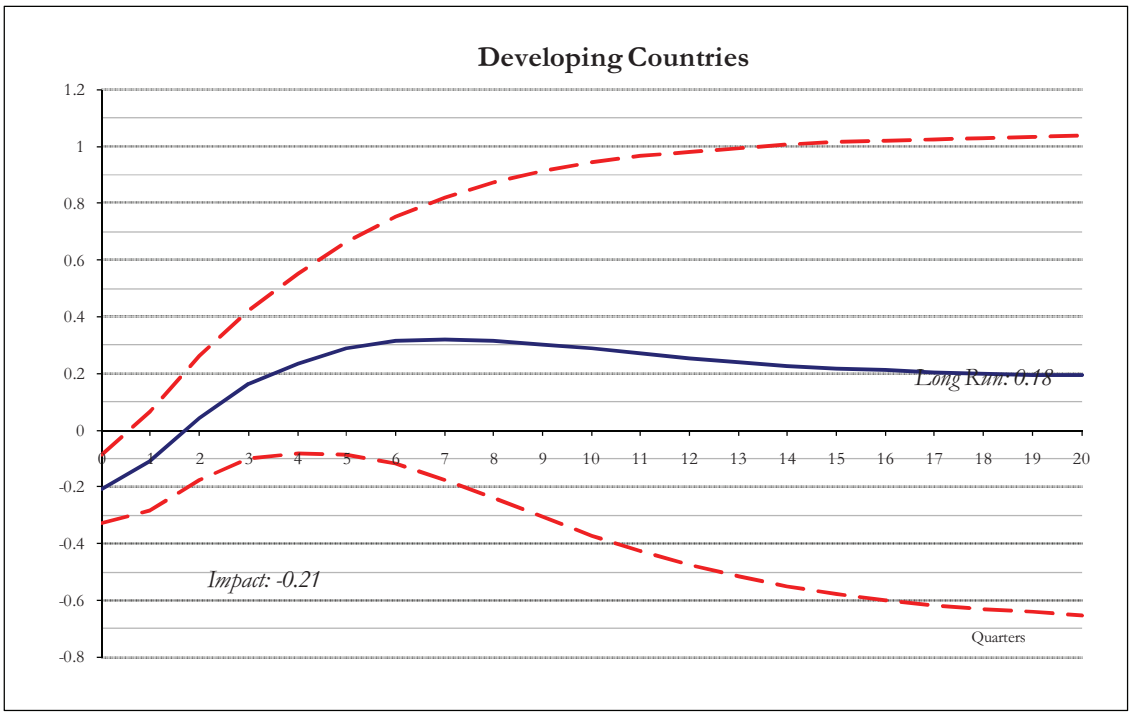


Figure 7: Cumulative multiplier—predetermined (fixed) and flexible (flex) exchange arrangements

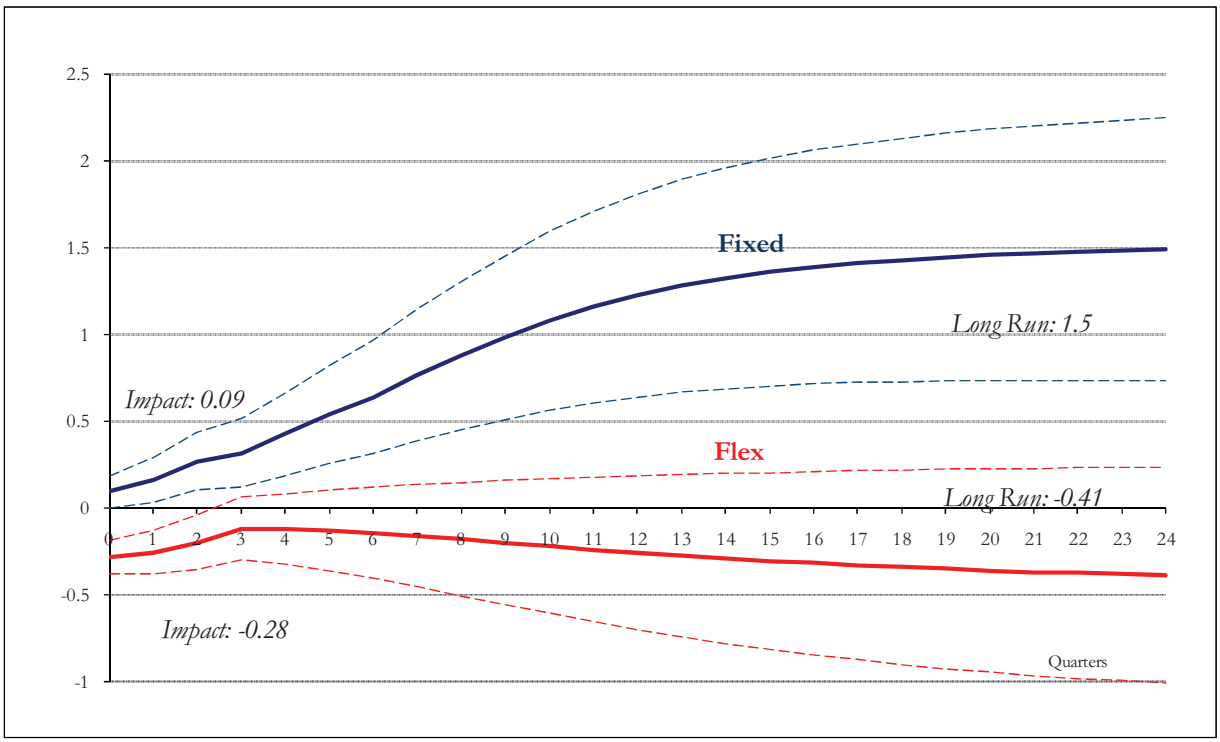


Figure 8a: Responses to a 1% shock to government consumption

Fixed Flex

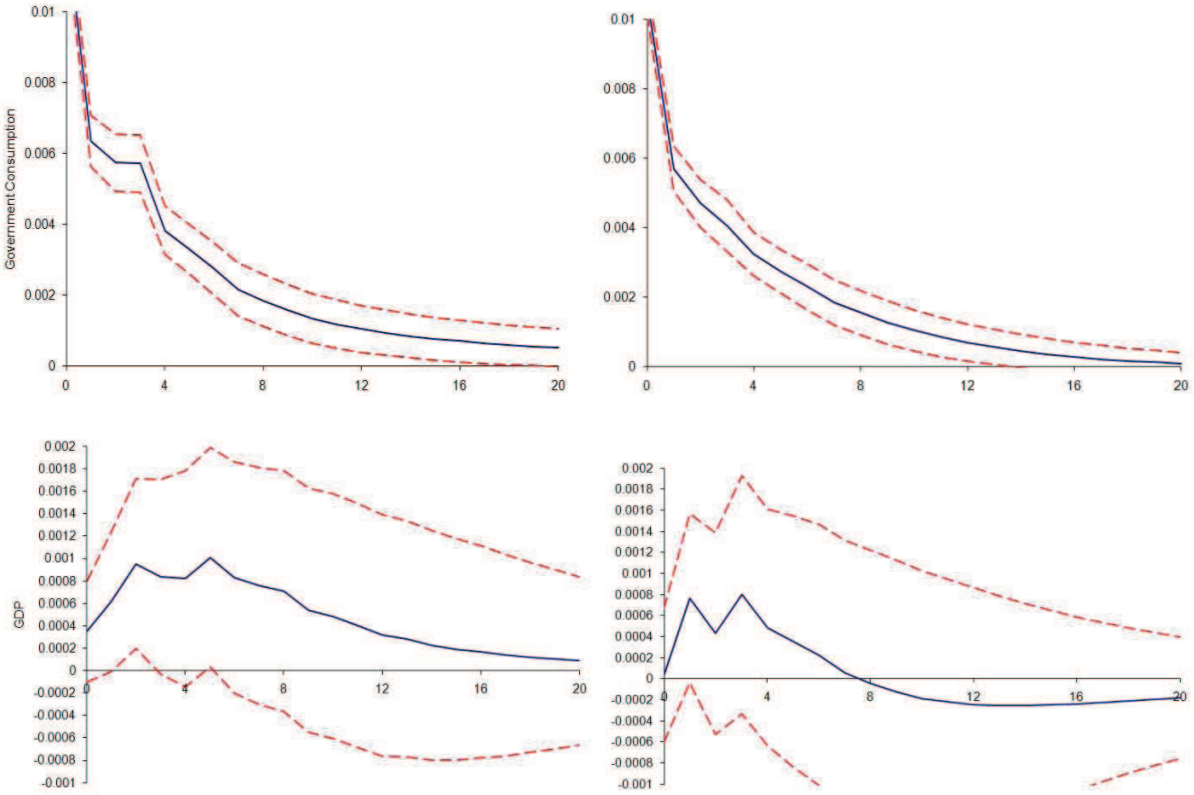


Figure 8b: Responses to a 1% shock to government consumption

Fixed Flex

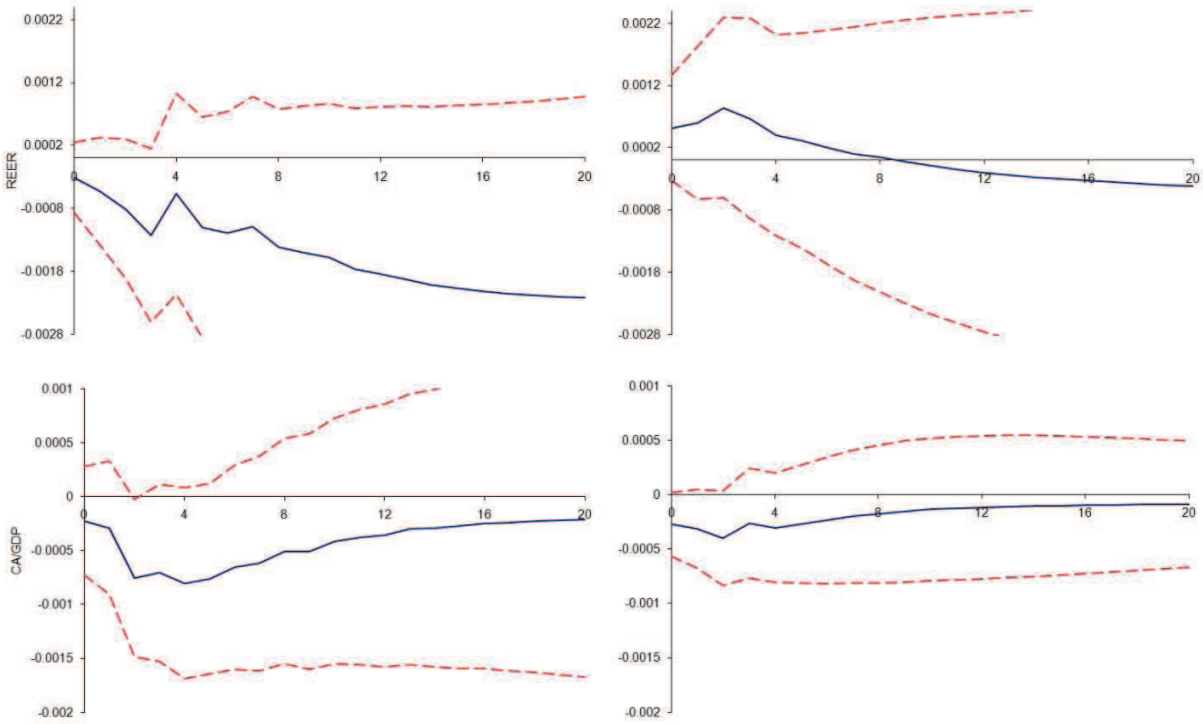
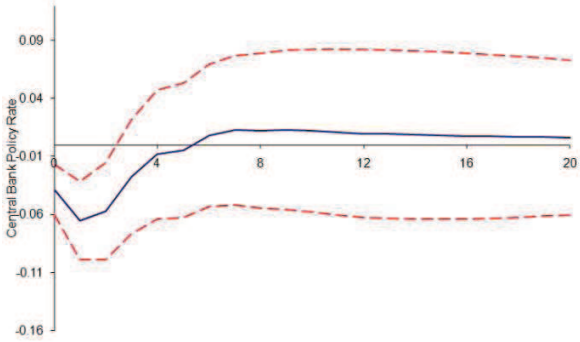


Figure 8c: Responses of the policy interest rate to a 1% shock to government consumption

Fixed



Flex

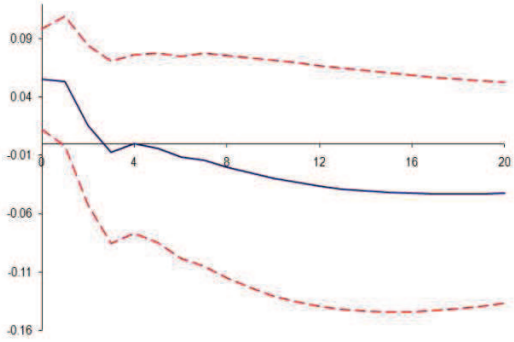


Figure 9: Responses of private investment and consumption to a 1% shock to government consumption

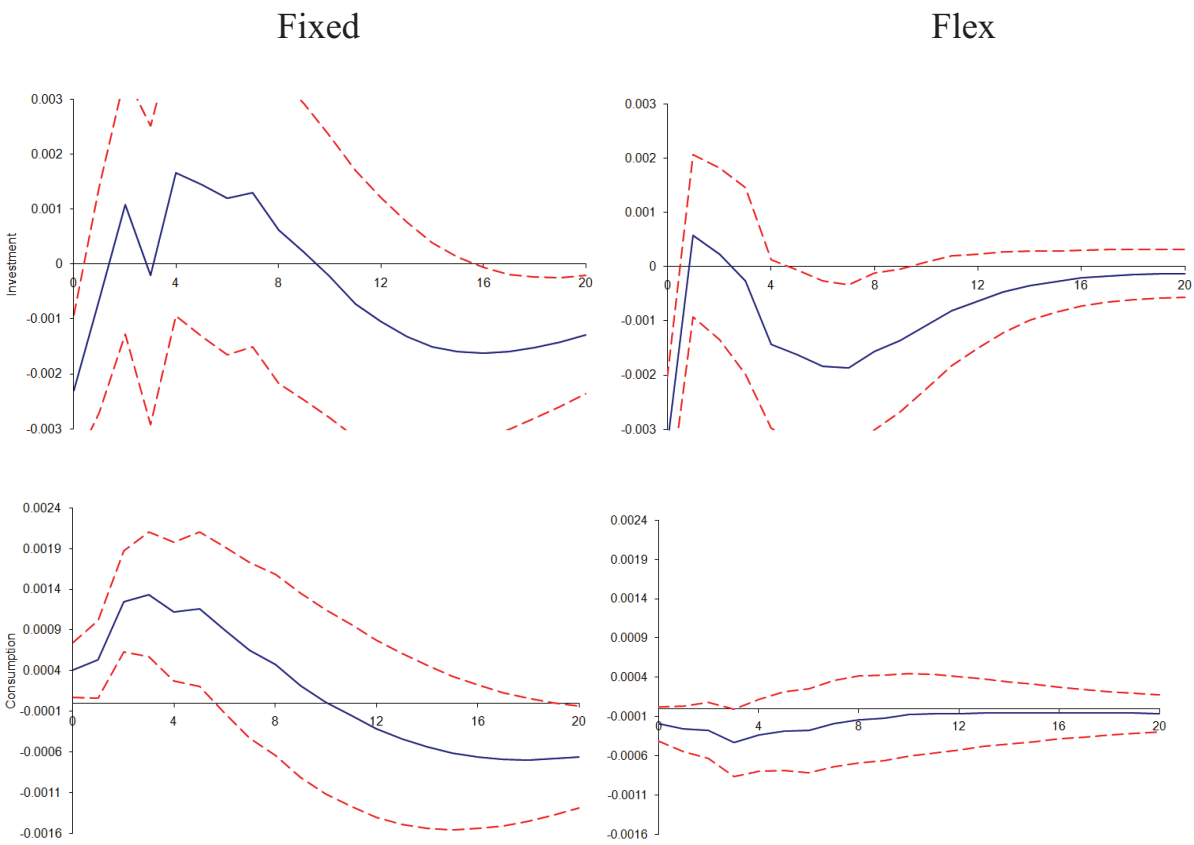


Figure 10a: Cumulative multiplier—open and closed economies

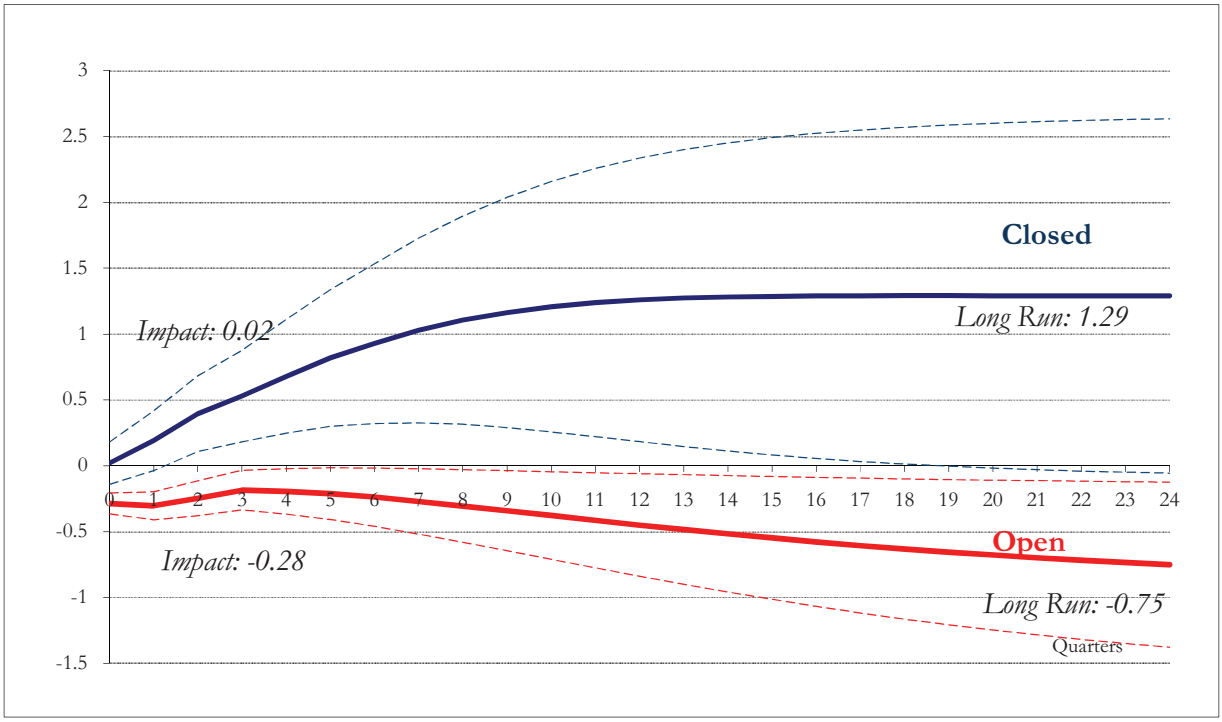


Figure 10b: Cumulative multiplier—the effect of total trade to GDP

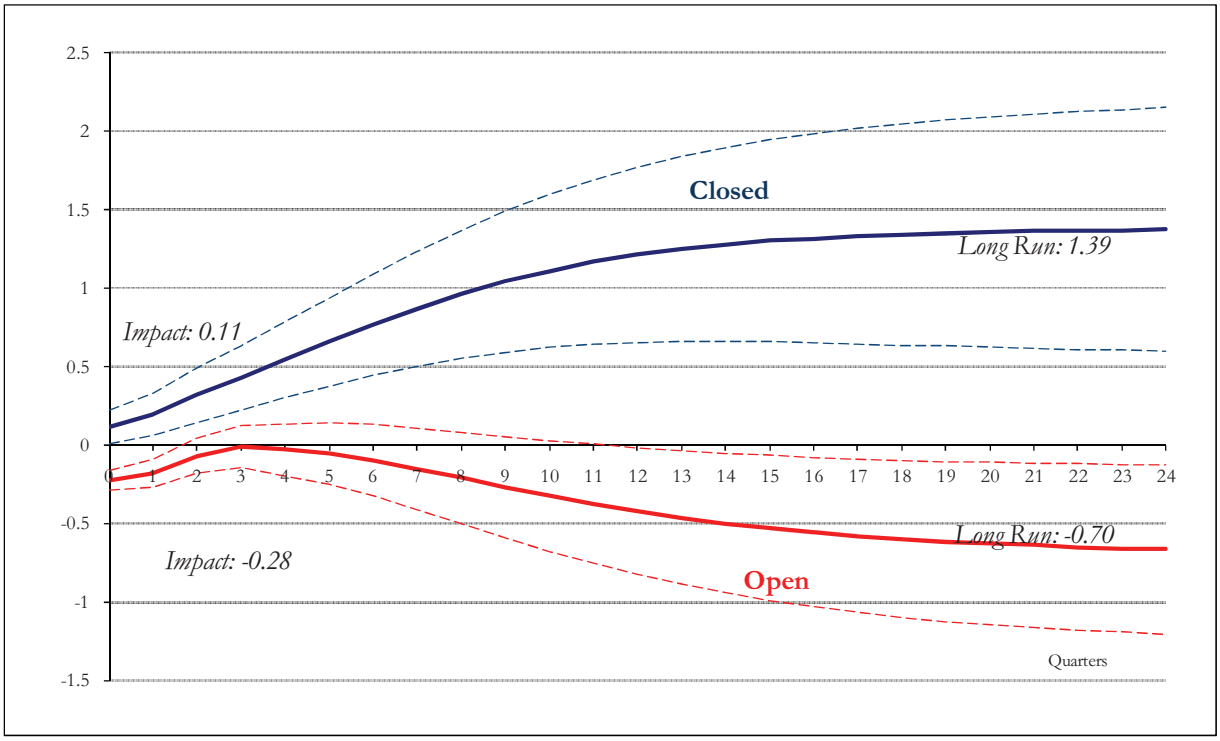
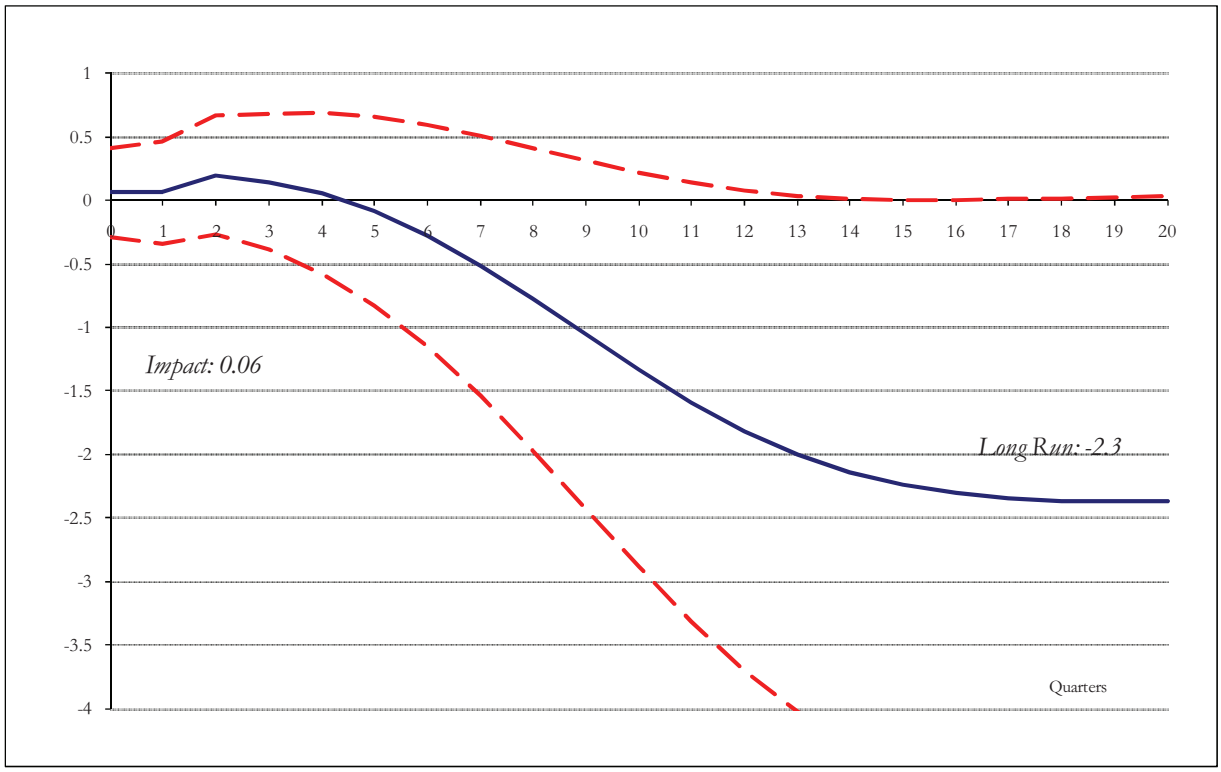


Figure 11: Cumulative multiplier: Highly indebted countries



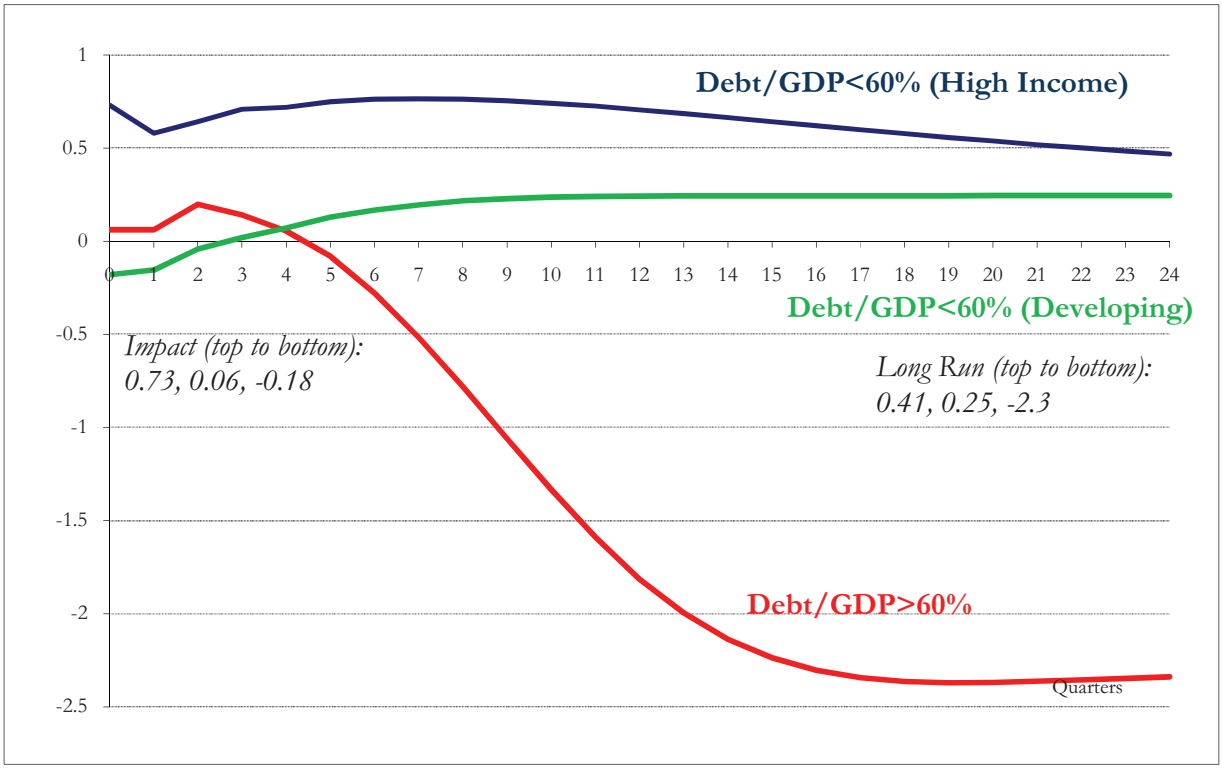


Figure 12: Cumulative government investment multiplier

High-income countries; includes indirect effects of government consumption

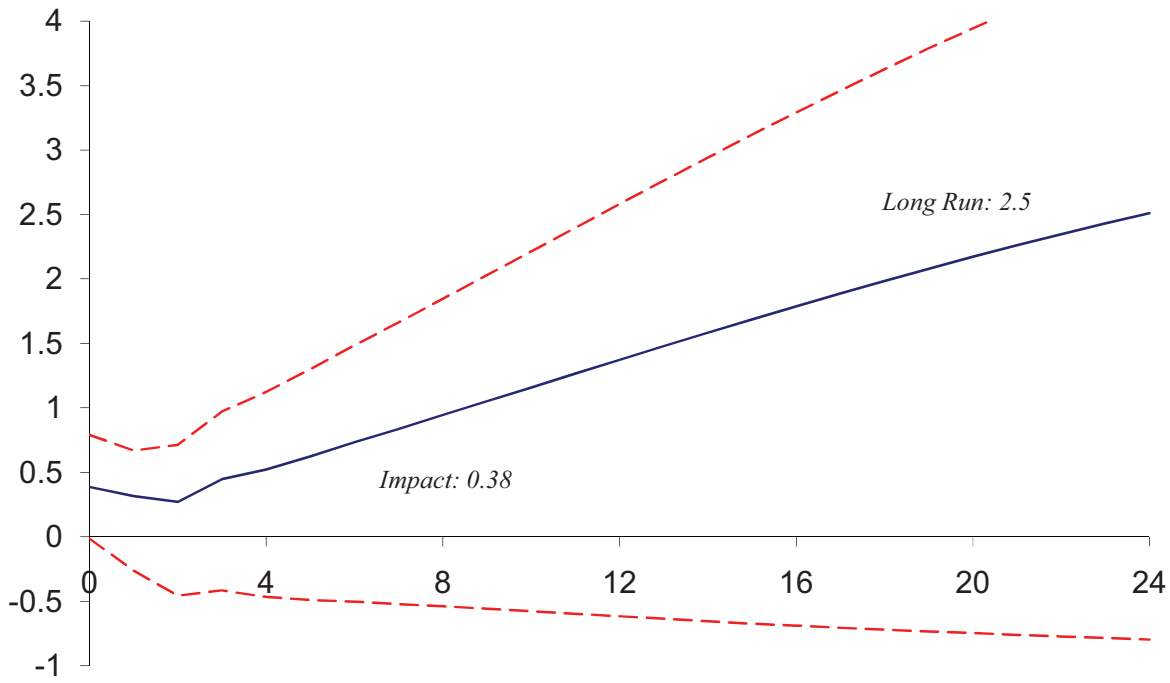
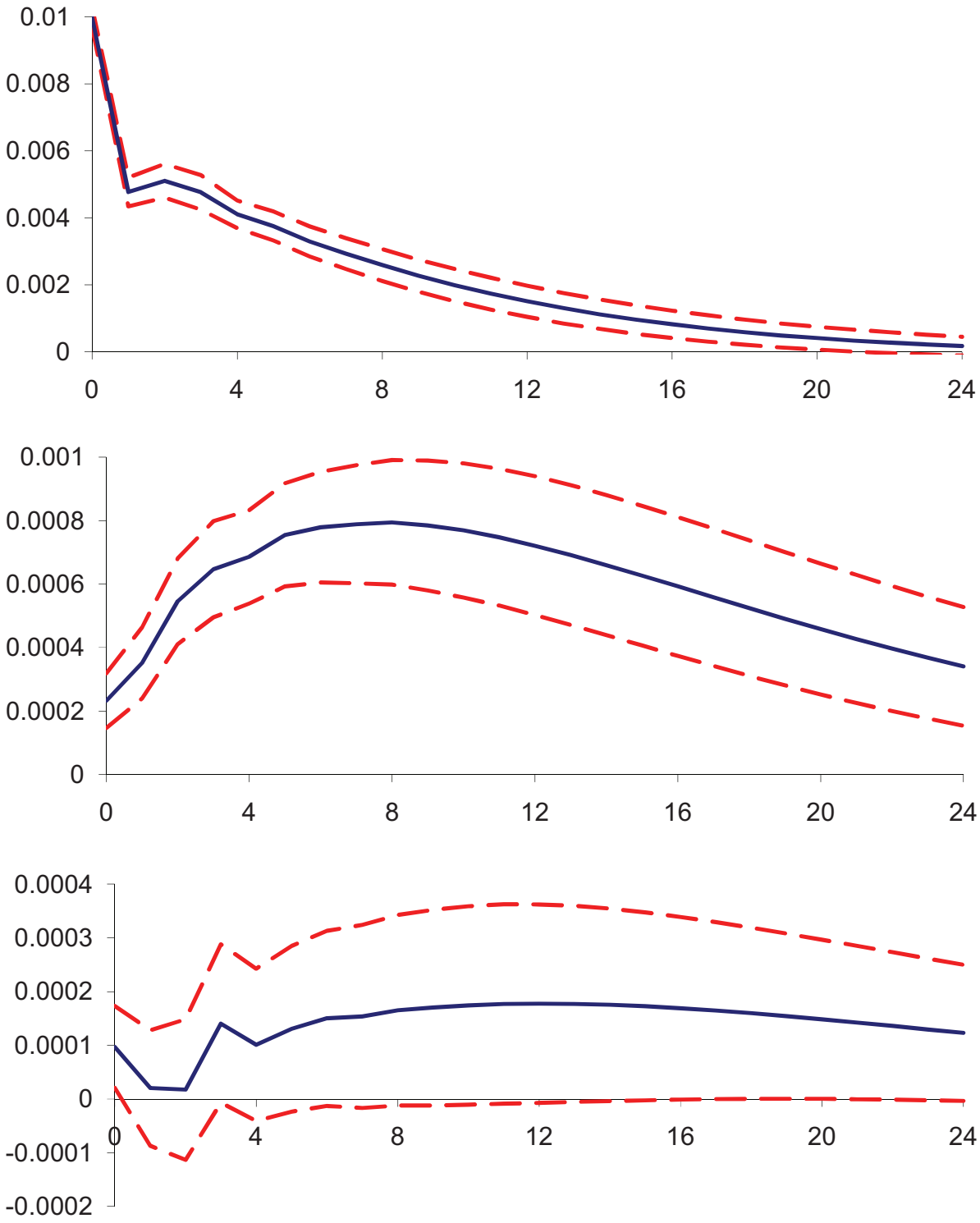
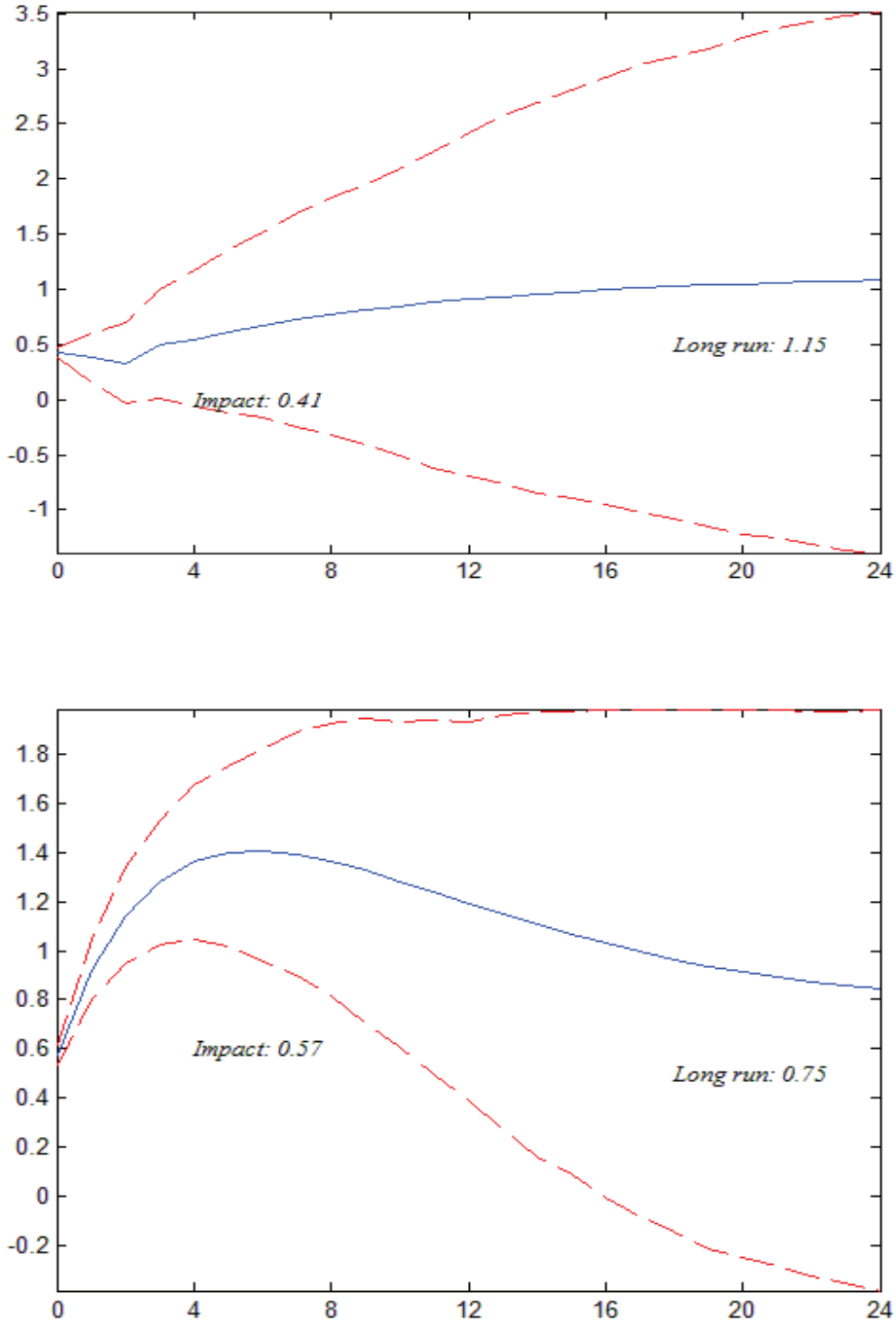


Figure 13: Responses to a 1% government investment shock



Responses from top to bottom: government investment, government consumption, and GDP. 90% confidence intervals in dashed lines.

Figure 14: Cumulative multiplier to a "pure" government investment shock: high-income and developing countries



High income countries in top panel, developing countries in lower panel. 90% confidence intervals in dashed lines.

Figure 15: Cumulative multiplier to a "pure" government investment shock: predetermined (fixed) and flexible (flex) exchange arrangements

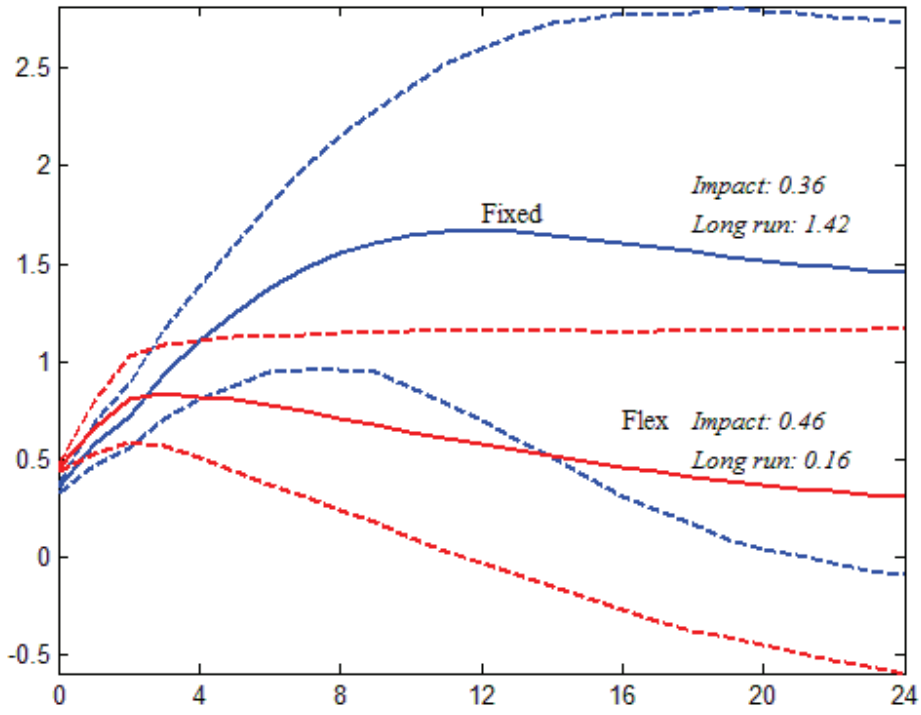


Figure 16: Cumulative multiplier to a "pure" government investment shock: open and closed economies

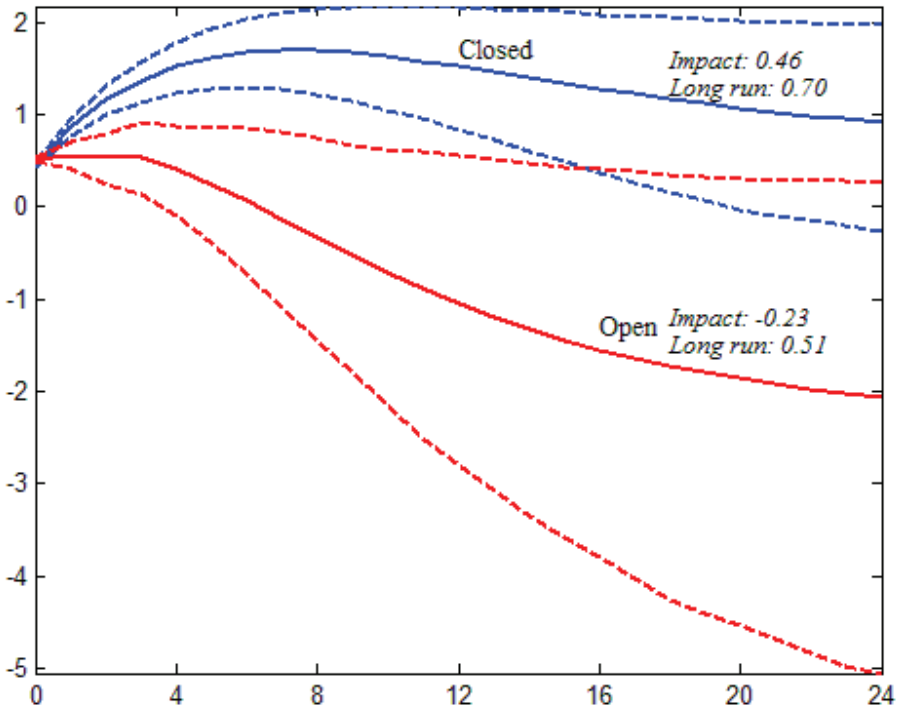
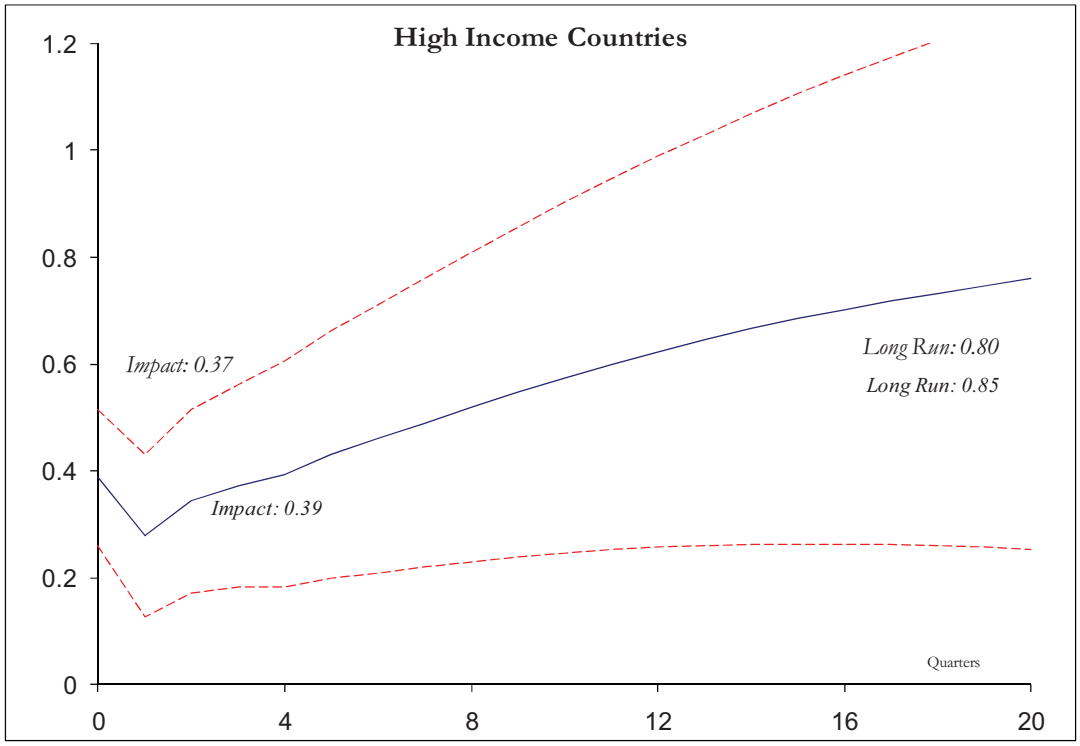


Figure 17: Cumulative multiplier—high income and developing countries

Multivariate Regression



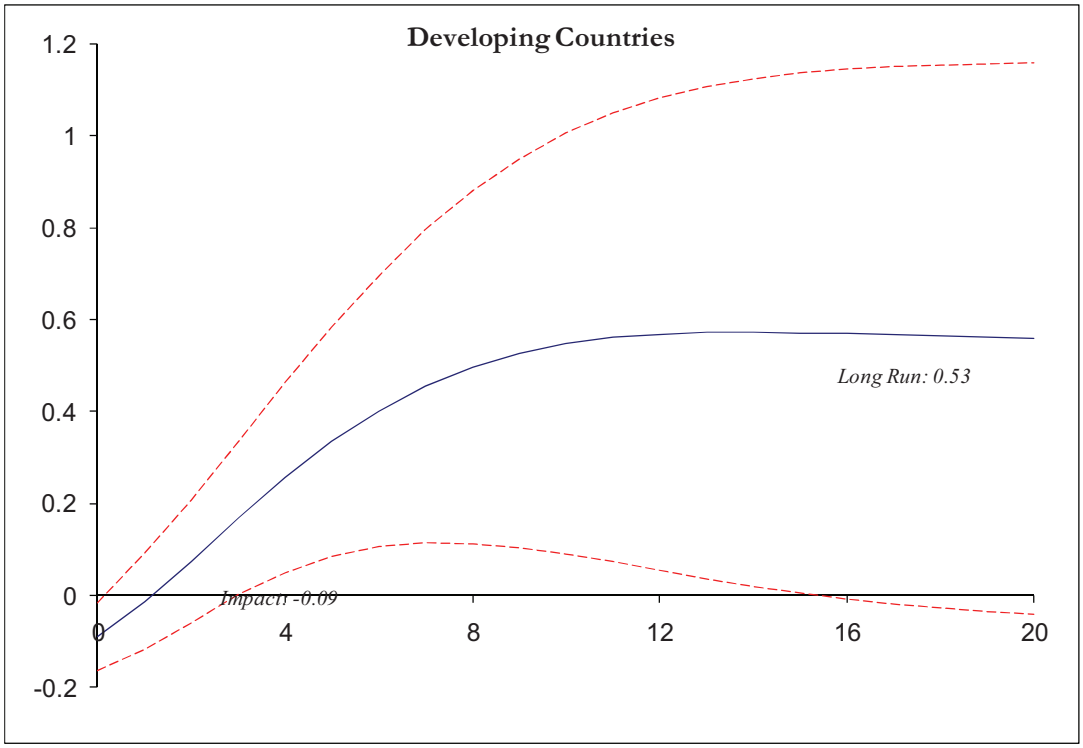


Figure 18: Cumulative multiplier—predetermined (fixed) and flexible (flex) exchange rates

Multivariate Regression

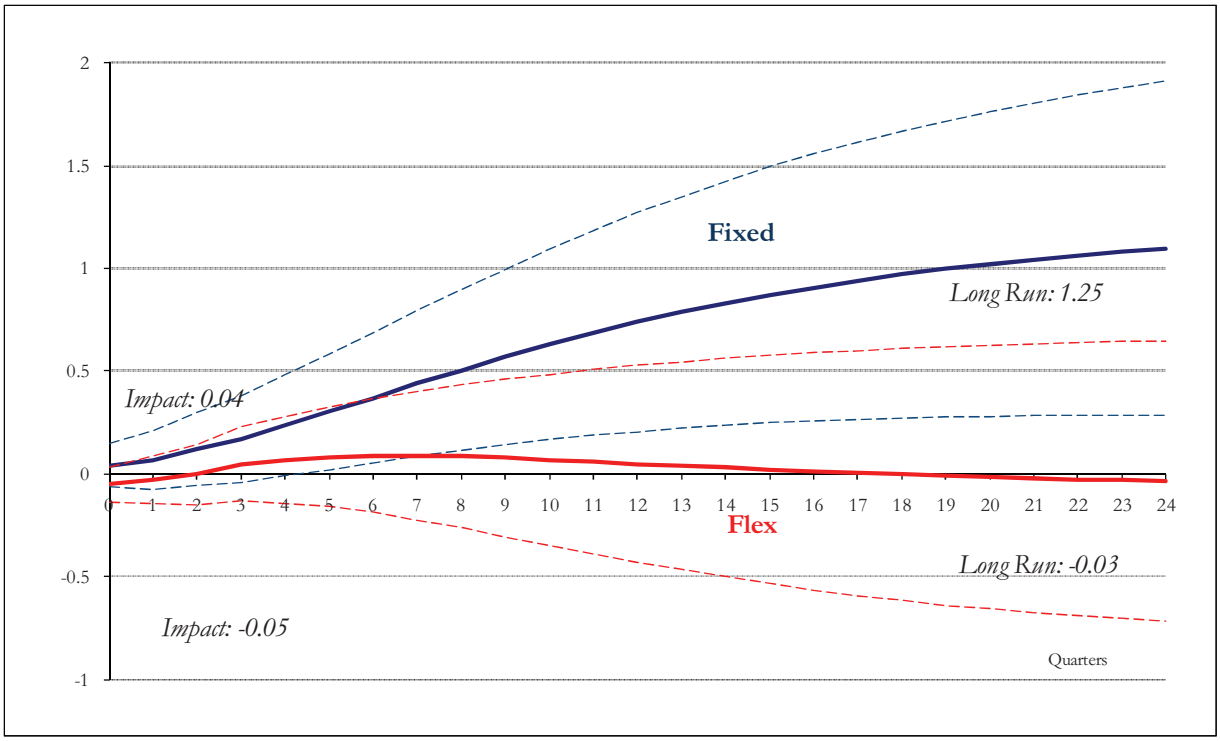


Figure 19: Cumulative multiplier—open and closed economies

Multivariate Regression

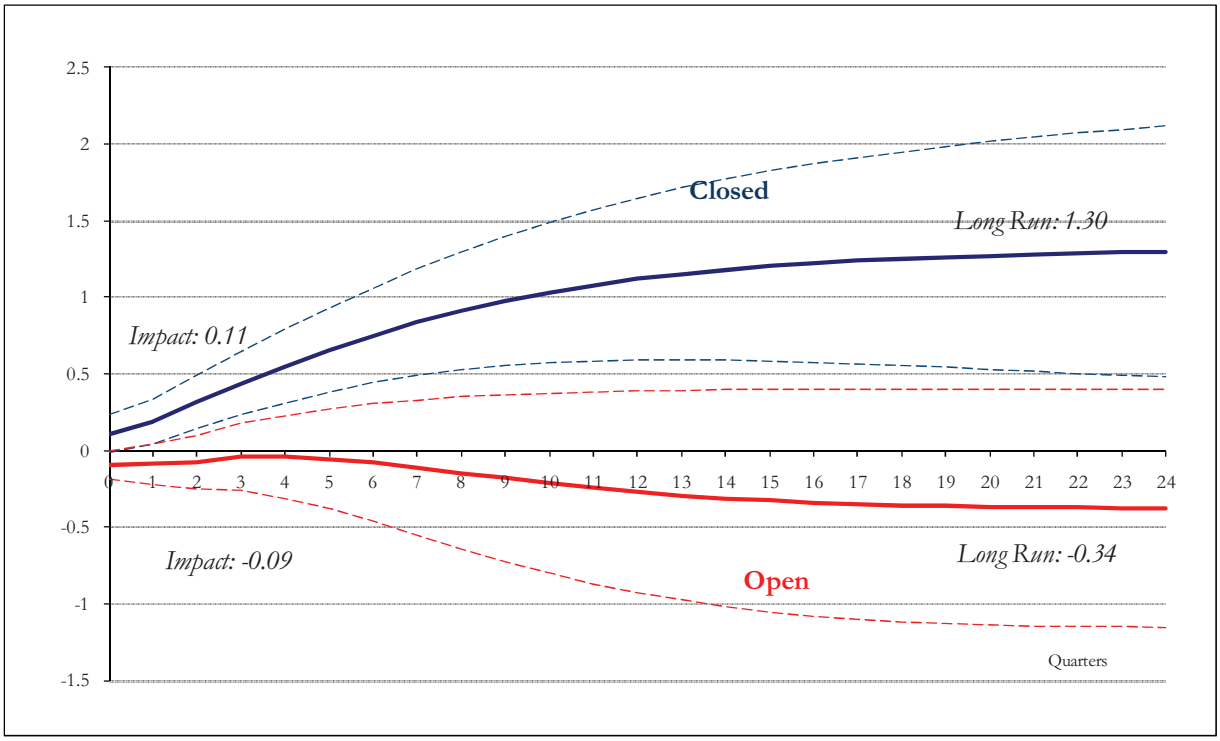


Figure 20: Cumulative multiplier: Highly indebted countries

Multivariate Regression

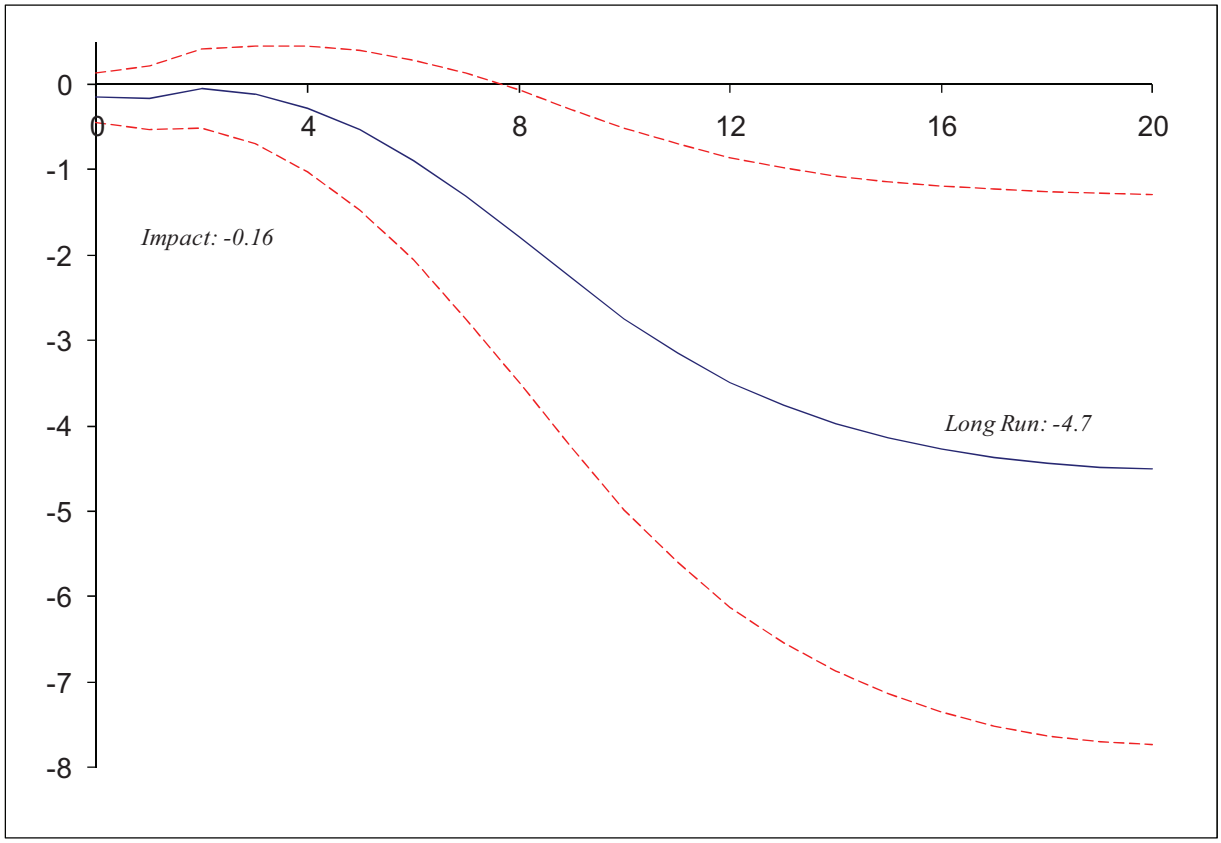


Table A1: Government Consumption

Country	Start	End	Source	Series and Comments
Argentina	1993q1	2006q4	MECON	Real General Government Consumption. Seasonally Adjusted and deflatedby MECON
Australia	1960q1	2008q4	OECD	General Government Consumption
Belgium	1991q1	2008q4	National Bank of Belgium	Real General Government Consumption, in chained 2008 Euros, seasonalized at th source using the TRAMO-SEATS method
Botswana	1993q3	2008q4	Statistical Agency	General Government Consumption (Constant Prices)
Brazil	1995q1	2008q4	IBGE	Real General Government Consumption
Bulgaria	1999q1	2008q2	Finance Ministry	General Government Consumption . Calculated as the sum of compensation of public employees, social benefits provided in kind that were purchased from the private sector, and final consumption of the public sector (both indivudual and collective)
Canada	1961q1	2008q4	OECD	General Government Consumption expenditure, chained volume estimates, Seasonally adusted
Chile	1989q1	2009q1	Hacienda	Real Central Government Consumption Expenditure
Colombia	2000q1	2008q4	Central Bank	Real General Government Consumption Expenditure
Croatia	2000q1	2009q4	Eurostat	General Government Consumption
Czech Republic	1999q1	2009q1	Eurostat	General Government Consumption
Denmark	1999q1	2009q1	Eurostat	General Government Consumption
Ecuador	1990q1	2007q2	Central Bank	General Government Consumption (Constant Prices)

El Salvador	1994q1	2007q4	Ministry of Finance	Central Government Consuption. Caclulated as the sum of public renumeration and purchases and goods and services.
Estonia	2000q1	2009q1	Eurostat	General Government Consumption
Finland	1998q1	2008q4	Statistics Finland	General Government Consumption Expenditure
France	1978q1	2008q4	Eurostat	General Government Consumption Expenditure, seasonally adjusted using the X-11 method in the source.
Germany	1991q1	2008q4	OECD	Real General Government Consumption Expenditure, Chain-Valued
Greece	2000q1	2009q1	Eurostat	General Government Consumption Expenditure
Hungary	1995q1	2009q1	Eurostat	General Government Consumption Expenditure
Iceland	1997q1	2007q4	Eurostat	General Government Consumption Expenditure
Ireland	1999q1	2009q1	Eurostat	General Government Consumption Expenditure
Israel	1999q1	2008q4	Central Bank	Real General Government Consumption
Italy	1999q1	2009q1	Eurostat	General Government Consumption Expenditure
Latvia	1999q1	2009q1	Eurostat	General Government Consumption Expenditure
Lithuania	1995q1	2008q4	Statistics Lithuania	General Government Consumption
Malaysia	1999q1	2008q1	Central Bank	Central Government Consumption. Calculated the sum of compensation of public employees (including both emoluments and pensions); and supplies and services of the central government.

Mexico	1991q1	2008q4	Finance Ministry	Central Government Consumption. Calculated as the sum of wages and sallaries and acquisitions.
Netherlands	1988q1	2009q1	Statistical Agency	Real General Government Consumption. Public compensation and purchases of goods and services are deflated separately and seasonally adjusted by the statistical agency.
Norway	1996q1	2009q1	Eurostat	General Government Consumption
Peru	1995q1	2008q2	Ministry of Finance	Central Government Consumption. Calculated as public renumeration and final consumption of goods and services
Poland	1999q1	2009q1	Eurostat	General Government Consumption
Portugal	1995q1	2009q1	Eurostat	General Government Consumption
Romania	1998q1	2008q4	Eurostat	General Government Consumption
Slovak Republic	1999q1	2008q2	International Monetary Fund	Central Government Consuption. Calculated as the sum of compensation of public employees and purchases and goods and services.
Slovenia	1995q1	2008q4	Eurostat	General Government Consumption
South Africa	1993q1	2009q1	Statistics South Africa	Real General Government Consumption
Spain	1995q1	2009q1	Eurostat	General Government Consumption
Sweden	1993q1	2008q4	Statistics Sweden	Central Government Consumption
Thailand	1993q1	2006q4	Central Bank	Real General Government Consumption. Central government data collected at quarterly frequency; some regional statistics are estimates.
Turkey	1998q1	2009q1	Central Bank	General Government Consumption
United Kingdom	1960q1	2008q4	OECD	Real General Government Consumption, Chain Valued, Seasonally Adjusted
United States	1960q1	2008q4	OECD	Real General Government Consumption, Chain Valued, Seasonally Adjusted
Uruguay	1988q1	2008q4	Central Bank	Real General Government Consumption

Table A2: Government Investment

Country	Start	End	Source	Series and Comments
Argentina	1993q1	2006q4	INDEC	Central government investment.
Australia	1959q3	2009q3	OECD	Real general government investment. Seasonally adjusted.
Belgium	1991q1	2009q2	Eurostat	General government gross fixed capital formation.
Canada	1961q1	2009q3	OECD	Real general government investment. Seasonally adjusted.
Chile	1990q1	2008q1	Central Bank	Central governmentL Inversión pública + Transferencias de capital.
Colombia	1994q1	2007q2	DANE	Obras civiles. Real and seasonally adjusted in original series.
Czech Republic	1999q1	2009q2	Eurostat	General government gross fixed capital formation.
Denmark	1999q1	2009q2	Eurostat	General government gross fixed capital formation.
El Salvador	1994q1	2007q4	Central Bank	Central government gross investment.
Estonia	1995q1	2009q2	Eurostat	General government gross fixed capital formation.
Finland	1998q1	2009q3	Statistics Finland	Gross fixed capital formation
France	1991q1	2008q4	Eurostat	General government gross fixed capital formation.
Germany	1999q1	2008q4	Eurostat	General government gross fixed capital formation.
Hungary	1999q1	2009q2	Eurostat	General government gross fixed capital formation.
Ireland	1999q1	2009q1	Eurostat	General government gross fixed capital formation.
Italy	1999q1	2009q2	Eurostat	General government gross fixed capital formation.
Latvia	1999q1	2009q2	Eurostat	General government gross fixed capital formation.

Lithuania	1999q1	2009q3	Statistics Lithuania	General government capital investments.
Mexico	1991q1	2009q1	Secretaría de Hacienda y Crédito Público	Central government investment.
Netherlands	1988q1	2009q1	Central Bureau of Statistics - Netherlands	Real fixed capital formation by general government.
Norway	1996q1	2009q2	Eurostat	General government gross fixed capital formation.
Romania	1995q1	2009q2	Eurostat	General government gross fixed capital formation.
South Africa	1993q1	2009q1	Statistics South Africa	Real General Government Consumption
Sweden	1993q1	2009q2	Statistics Sweden	Central government investment
Turkey	1987q1	2006q4	Central Bank	Public Sector Investment (index)
United Kingdom	1991q1	2009q3	Eurostat	General government gross fixed capital formation.
United States	1960q1	2009q3	OECD	Real general government investment. Current prices. Seasonally adjusted. Millions of national currency.
Uruguay	1992q1	2009q3	Central Bank	Real general government investment.

Table A3: Episodes of De-Facto Fixed and Flexible Exchange Rates

Country	Fixed	Flex
Argentina	1993q1:2001q4; 2003q2:2006q4	-
Australia	1960q1:1982q4	1983q1:2007q4
Belgium	1991q1:2007q4 (Euro since 1999)	-
Botswana	1993q3:2001q4	2002q1:2007q4
Brazil	-	2000q1:2007q4
Bulgaria	1999q1:2007q4	-
Canada	1961q1:2002q2	2002q3:2007q4
Chile	-	1992q2:1998q2, 1999q4:2007q4
Colombia	-	2000q1:2007q4
Croatia	-	2000q1:2007q4
Czech Republic	1999q1:2001q4	2003q1:2007q4
Denmark	1999q1:2007q4	-
Ecuador	2000q2:2007q4	1990q1:2000q1
El Salvador	1994q1:2007q4	-
Estonia	2000q1:2007q4	-
Finland	1998q1:2007q4 (Eurozone from 1999)	-
France	1978q1:2007q4 (Eurozone from 1999)	-
Germany	1999q1:2007q4 (Eurozone)	1991q1:1998q4
Greece	2000q1:2007q4 (Eurozone from 2000)	-
Hungary	1995q1:2007q4	-
Iceland	1997q1:2000q3	2000q4:2007q1
Ireland	1999q1:2007q4 (Eurozone)	-
Israel	-	1999q1:2007q4
Italy	1999q1:2007q4 (Eurozone)	-
Latvia	2001q4:2007q4	1999q1:2001q3

Lithuania	1995q2:2007q4	-
Malaysia	1999q1:2007q4	-
Mexico	-	1991q1:2007q4
Netherlands	1988q1:2007q4 (Eurozone from 1999)	-
Norway	-	1996q1:2007q4
Peru	1995q1:2007q4	-
Poland	-	1999q1:2007q4
Portugal	1995q1:2007q4 (Eurozone)	-
Romania	-	1998q1:2007q4
Slovak Republic	1999q1:2007q4	-
Slovenia	1995q1:2007q4	-
South Africa	-	1993q1:2007q4
Spain	2000q1:2007q4 (Eurozone)	-
Sweden	-	1993q1:2007q4
Thailand	1993q1:1997q2	1997q3:2007q4
Turkey	-	1998q1:2007q4
United Kingdom	1960q1:1972q2	1972q3:2007q4
United States	1960q1:1971q3	1971q3:2007q4
Uruguay	1995q4:2001q4; 2005q3:2007q4	1988q1:1990q4; 1992q1:1995q3; 2002q1:2005q2
Total Observations	1501	1119

Table A4: Open and Closed Economies

Average tariff rates greater than or smaller than 4%

Country	Open	Closed
Argentina	-	1992-2008
Australia	2002-2008	1991-2001
Belgium	1988-1989, 1997-2008	1990-1996
Botswana	2001	2004-2008
Brazil	-	1989-2008
Bulgaria	1988-1989, 1997-2008	1990-1996
Canada	1993, 1997-2008	1989, 1995-1996
Chile	2004-2008	1992-2002
Colombia	-	1991-2008
Croatia	2004-2008	2001
Czech Republic	1988-1989, 1997-2008	1990-1996
Denmark	1988-1989, 1997-2008	1990-1996
Ecuador	-	1993-2008
El Salvador	2008	1995-2007
Estonia	1988-1989, 1997-2008	1990-1996
Finland	1988-1989, 1997-2008	1990-1996
France	1988-1989, 1997-2008	1990-1996
Germany	1988-1989, 1997-2008	1990-1996
Greece	1988-1989, 1997-2008	1990-1996
Hungary	1988-1989, 1997-2008	1990-1996
Iceland	1993,1996, 2001, 2003, 2006-2008	-
Ireland	1988-1989, 1997-2008	1990-1996
Israel	1993, 2004-2008	-
Italy	1988-1989, 1997-2008	1990-1996
Latvia	1988-1989, 1997-2008	1990-1996
Lithuania	1988-1989, 1997-2008	1990-1996

Malaysia	2006-2007	1988, 1991, 1993, 1996-1997,2001-2005
Mexico	2004-2008	1991, 1995, 1997-2003
Netherlands	1988-1989, 1997-2008	1990-1996
Norway	1988, 1993, 1996-2008	1995
Peru	2008	1993-2000, 2004-2007
Poland	1988-1989, 1997-2008	1990-1996
Portugal	1988-1989, 1997-2008	1990-1996
Romania	1988-1989, 1997-2008	1990-1996
Slovak Republic	1988-1989, 1997-2008	1990-1996
Slovenia	1988-1989, 1997-2008	1990-1996
South Africa	-	1988-1993, 1996-2001, 2004-2008
Spain	1988-1989, 1997-2008	1990-1996
Sweden	1988-1989, 1997-2008	1990-1996
Thailand	-	1989-1995, 2000-2006
Turkey	2003-2008	1993-1999
United Kingdom	1988-1989, 1997-2008	1990-1996
United States	1989-2008	-
Uruguay	2005-2008	1992, 1995-2004

Table A5: Open and Closed Economies

(De-facto: Economies with a ratio of exports+imports to GDP greater than or less than 60%

Open	Trade/GDP	Closed	Trade/GDP
Belgium	108.9%	Argentina	19.4%
Bulgaria	112.3%	Australia	33.2%
Botswana	100.0%	Brazil	18.6%
Chile	64.2%	Canada	54.8%
Croatia	87.4%	Colombia	31.9%
Czech Republic	125.0%	Ecuador	46.5%
Denmark	69.9%	Finland	56.3%
El Salvador	63.3%	France	40.8%
Estonia	154.7%	Germany	59.9%
Hungary	79.4%	Greece	42.0%
Iceland	74.4%	Italy	41.3%
Ireland	118.0%	Mexico	34.5%
Israel	80.8%	Peru	36.3%
Latvia	98.6%	Poland	52.9%
Lithuania	111.6%	Romania	58.3%
Malaysia	132.5%	South Africa	52.2%
Netherlands	217.1%	Spain	37.8%
Norway	76.9%	Turkey	42.7%
Portugal	60.1%	United States	18.2%
Slovak Republic	141.5%	Uruguay	37.9%
Slovenia	118.4%		
Sweden	63.4%		
Thailand	63.4%		
United Kingdom	60.1%		
Total Observations	1366		1234

Table A6: Episodes of High Debt
(Gross Debt of Central Government Exceeding 60% of GDP)

Country	High Debt Episodes
Argentina	2002-2006
Belgium	1981-2007
Brazil	1991-1993, 1999-2005
Bulgaria	1991-2001
Denmark	1983-1999
Ecuador	1990-1994, 1999-2000
Finland	1995-1997
Greece	1993-2007
Ireland	1981-1996
Israel	1996-2007
Italy	1982-2007
Malaysia	1990-1992
Netherlands	1992-1993
Poland	1992-1994
Portugal	1994-1996, 2003-2007
Sweden	1993-1999
Uruguay	2002-2006
Total Observations	1501