Trade Spill-overs of Fiscal Policy in the European Union: A Panel Analysis*

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ABSTRACT

We explore the international spill-overs from fiscal policy shocks via trade in Europe. A fiscal expansion stimulates domestic activity, which leads to more foreign exports and, hence, higher foreign output. To quantify this, we combine a panel VAR model in government spending, net taxes and GDP with a panel trade model. We find statistically significant spill-overs. Our baseline estimates imply that, on average, over the first two years a German public spending increase equal to 1% of GDP implies an average foreign exports gain of 2.2% of its annual level. The corresponding figure for an equal-size net tax reduction is 0.8%. As far as the direct effect of enhanced exports on foreign activity is concerned, the corresponding average gains from the two shocks are, respectively, 0.13% and 0.07% of annual GDP. Of course, these numbers are (substantially) smaller when the fiscal shock originates from smaller EU countries.

Keywords: Fiscal shocks, trade spillovers, European Union, impulse responses.
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1. Introduction

Cross-border integration of national economies is progressing steadily in the European Union (EU). As a result, policy changes in one country may have potentially important effects on other countries in the EU. In this paper, we shall explore the relevance of international trade for the cross-border transmission of fiscal shocks within the EU. The importance of such spill-overs helps to determine the interdependence of national macroeconomic policies and the interest that governments might have in each others' policy stances. To the best of our knowledge, the empirical literature has hardly given any attention to fiscal spill-overs and, hence, it is not clear to what extent policymakers should be concerned with foreign fiscal policy changes. One reason might be that in the past it has proved hard to find statistically significant effects of such spill-overs (e.g., see McKibbin, 1997). Therefore, some authors, such as Gros and Hobza (2001) and In 't Veld (2004), simulate calibrated models to assess the trade spill-overs from fiscal policy.

Our empirical analysis is based on the conjecture that a fiscal expansion stimulates domestic activity, which leads to more domestic imports and thus more exports by other countries. This, in turn, boosts foreign income. Our quantification of these effects is innovative. In particular, we combine a panel vector auto-regression (PVAR) model with a panel trade model to calculate the "full" effect of a fiscal impulse on bilateral foreign exports. The PVAR model allows us to identify the fiscal shocks and compute the output responses to these shocks, while the panel trade model, which is closely related to the well-known gravity model, enables us to calculate the effect of output on bilateral foreign exports. This allows us to (1) combine two models with different dimensionality, that is, a country PVAR model and a country-pair trade model, and (2) disentangle the contributions of the various steps leading to the overall trade spill-over of fiscal policy and (3) exploit the similarities between the EU countries in the data set. Nevertheless, the approach also has potential disadvantages. By splitting the calculation of the fiscal spill-over in steps, we may forego the (presumably) greater flexibility of a fully integrated approach in terms of capturing the cross-border transmission of the fiscal shocks. In addition, our panel analysis imposes homogeneity restrictions on a number of model parameters. However, our sensitivity analysis shows that the results are quite robust to extensions aimed at capturing heterogeneity.

The panel VAR model shows that both an increase in government spending (i.e. government consumption plus investment) and a reduction in net taxes (i.e. revenues minus transfers) give a significant boost to domestic economic activity, which persists for some years. The panel trade estimates reveal a significantly positive impact effect of domestic output on foreign exports. Combining the baseline estimates of the two models, we find that, on average, over the first two years a German public spending increase equal to 1% of GDP implies a foreign exports gain of 2.2% of its annual level. The corresponding figure for an equal-size net tax reduction is 0.8%. Both estimates are statistically significant. As far as the direct effect of enhanced exports on average foreign activity is concerned, the corresponding gains from the two shocks are, respectively, 0.13% and 0.07% of annual GDP. Obviously, these numbers become smaller when the fiscal shock originates from other EU countries. The smallest effect is found for a fiscal stimulus in Greece, which raises average foreign GDP by only 0.01% in the case of a spending increase and 0.005% in the case of a net tax reduction. The numbers reported here provide only a first attempt at quantification and are intended to show that fiscal policy spill-overs via trade are potentially relevant, at least when they originate from large trading partners.

By focussing on the trade spill-overs of fiscal policy, we thus largely ignore other potential spillovers, in particular those that take place through changes in the interest rate.[1] There are several reasons to motivate this choice. First, the possibility of positive spillovers of fiscal expansions via trade has received much less attention in the discussions surrounding the European fiscal framework than the negative spillovers via a rise in the common interest rate. Second, especially in the case of Europe, the trade channel may well be more important than the interest rate channel. One reason is that the spillovers via trade are mostly confined to Europe, because intra-European trade is substantially larger than trade with the rest of the world. Further, to the extent that the European capital market is integrated with the capital market in the rest of the world, the interest rate increase following a debt-financed fiscal expansion in some European country is diluted.[2]

[1] Theoretically, a fiscal expansion can generate a spill-over both via the common short-run and the common long-run interest rate. If the expansion puts upward pressure on inflation, the ECB is forced to raise the short interest rate. If public debt rises, then the long-run interest rate may go up.

[2] If the rise in the interest rate is caused by an increase in the likelihood of debt default, then there is a priori no reason why the rise should be transmitted to other countries. However, if part of the rise is explained by crowding out because the additional debt generates an excessive demand for funds at the

We believe that our findings may provide policymakers with potentially useful insights. First, better information on the quantitative importance and time profile of the effects of fiscal expansions on the national economies should enable policymakers to design better-targeted measures for stabilising their own economy. Further, the EU Ministers of Finance meet regularly in a formal capacity in the ECOFIN and informally in the Euro-group (the Euro-area Finance ministers only). Insight into the national and cross-border effects of fiscal policy should help them form a judgment on each other's fiscal plans and evaluate the consequences for their own economy. A recent example is the stimulation package designed by the new German government under Merkel. Our results could help understand the consequences of this package for Germany and its trading partners. Even though the European fiscal authorities do not coordinate their macroeconomic policies, peer pressure and regular interaction in combination with better information on the spill-over effects should be conducive to a better alignment of national fiscal policies. Finally, more accurate information on the distribution of the costs and benefits of fiscal stimulation should create a better understanding of each other's policy positions.

Our analysis could also shed some light on the suitability of the existing macroeconomic framework in Europe and, in particular, on the role of the Stability and Growth Pact (SGP). Our estimates can help policymakers to assess the short-run effects of a tight enforcement of the Pact's deficit limit, both for the economy for which the limit is binding but also for the economies of trading partners. Of course, a more comprehensive assessment of the functioning of the SGP requires taking into account not only these short-run effects but also the longer-run effects of reduced public debt under enforcement. Ironically, our results suggest that the positive trade benefits of a laxer fiscal policy of some EU countries could make it easier for others to obey the Pact's limits.

The remainder of the paper is structured as follows. Section 2 discusses in more detail the empirical strategy. Section 3 presents the baseline estimates (as well as a number of robustness checks) for the effects of fiscal policy in the PVAR model, while Section 4 does the same for the panel trade model. Then, Section 5 combines the results from the previous two sections to produce estimates of domestic fiscal impulses on foreign trade and foreign output. Section 6 discusses some policy experiments based on

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initial interest rate, then one might expect also an upward pressure on the foreign interest rate as funds are attracted from abroad.

the estimated models. Finally, Section 7 concludes the main body of this paper. Details on the data, procedures and some technicalities are contained in the Appendices. Some further details and results are found in the Additional Appendices that are not for publication, but can be found on our websites.

2. Motivation for the empirical strategy and sample choice

We obtain the overall effect of domestic fiscal impulses on exports by trading partners in Europe in two steps. In the first step, we estimate the link between a domestic fiscal impulse and domestic output. To fix terminology, we will refer to this step as the *fiscal block*. In the second step, we link foreign exports to domestic GDP. We shall refer to this step as the *trade block*. By combining the links established in the two blocks, we can quantify the overall effect of a domestic fiscal impulse on foreign exports. In an extra step, we can then translate the movements in exports into changes in foreign output.

Because we are most interested in the short- and medium-run economic effects of the fiscal impulses, the fiscal and trade blocks will be specified as dynamic rather than static. For the fiscal block, we estimate a panel vector auto-regression (PVAR) model in which the responses of output to the discretionary fiscal shocks are traced out. For the other block, we set up a panel trade model based on the gravity approach and estimate the dynamic responses of bilateral exports by the EU trading partners to domestic output.

By estimating the fiscal and trade blocks separately and then combining the respective outcomes, we circumvent the difficulty that the two models have different dimensions, because the fiscal block concerns the domestic economy only, whereas the trade block deals with the interactions between pairs of countries. An additional advantage of estimating these models separately is that this allows us to disentangle the sizes of both steps going from the fiscal impulse via domestic output to foreign exports.[3] Such an approach buys extra transparency and, more importantly, the estimates of both blocks contribute to the respective fields of research and can be compared with what is found there.

[3] In an earlier experiment, we combined the fiscal and trade blocks in a single, huge PVAR. The responses of exports to fiscal policy shocks were very similar to those described below. This provides another reason to keep the two blocks separate.

With the fiscal block, we can thus assess the relevance of Keynesian effects of fiscal shocks. Focussing only on the US, Fatás and Mihov (2001) and Galí *et al*. (2003) find that private consumption goes up in response to a rise in public spending. Related studies by Blanchard and Perotti (2002), and to a lesser extent Perotti (2005), confirm the short-run stimulus resulting not only from spending increases, but also from net tax reductions.[4] Mountford and Uhlig (2002) also find that net tax cuts stimulate economic activity. The existing results are mostly obtained in the context of single-country VARs rather than panel VARs. However, these findings are roughly in line with what we obtain below. As for the comparison with the trade block, several papers estimate the effect of income on trade (for a recent survey, see Barrell and Te Velde, 2002). We shall see that here our results are also broadly in line with the literature.

Instead of separating the fiscal block from the trade block, one could link fiscal policy to trade directly, as some papers do. For example, Lane and Perotti (1998) regress the trade balance, exports and imports directly on movements in different components of the budget. Lane and Perotti (2003) investigate the implications of fiscal expansions for the traded sector. Clarida and Prendergast (1999), Kim and Roubini (2003) and Müller (2004) model fiscal policy in the G3, or the US only, in a VAR, together with the current account or the trade balance and the exchange rate. Marcellino (2002) estimates VARs that include the German output gap and German fiscal policy together with the corresponding variables for other large European countries. He finds only small and insignificant effects of German fiscal shocks on the other economies. Canzoneri et al. (2003) use a VAR to explore the effects of changes in US fiscal policy on output in France, Italy and the United Kingdom. They find that the spill-over effect of a government spending shock is quite large for all three countries.

Our choice of countries is led by the fact that we want to explore the trade spill-overs of fiscal shocks within the EU. An advantage of focusing on EU countries only is that this helps to limit the potential heterogeneity, as the economies share many similarities. This is important, because the use of panels requires us to impose certain homogeneity restrictions. In principle, we could have considered including other

[4] Other relevant contributions to this expanding literature include Canova and Pappa (2002), Favero (2003), Burnside *et al*. (2000), Van Aarle *et al.* (2003), Claeys (2004) and Muscatelli *et al.* (2004). It should be noted that these contributions mostly look at the effects of fiscal impulses under "normal" circumstances. Periods of fiscal distress may well show non-Keynesian effects of fiscal changes. In particular, Giavazzi and Pagano (1990) – followed by a large number of other papers – suggest that fiscal contractions can actually be expansionary in such a situation.

European countries, such as Switzerland and Norway, but the availability of the fiscal data was too limited for these countries. Data limitations also exclude Luxemburg for the fiscal block as well as the recent EU entrants. Including this latter group would in any case not be recommendable in view of the still substantial economic differences with the fourteen current EU members that are now left in the sample.

As regards to the time dimension of our panels, our sample period is 1965-2004. However, for some countries it is shorter (see the Appendix A). In view of the potential remaining differences across the countries and the sub-periods in our sample, we allow for extensive deterministic heterogeneity across the observations. Moreover, we shall explore how our findings change with specific sub-samples of countries and sub-periods.

While many recent analyses of the effects of fiscal impulses rely either on relatively small samples or on quarterly fiscal data,[5] in this paper we use panels over a relatively long period of time. This provides us with enough observations to use annual data for our fiscal block. Although with annual data we may miss potentially interesting dynamics and some of our identification assumptions become stronger, the use of annual rather than quarterly data also has advantages. One is statistical. With annual data, there is no (or less) need to be concerned with seasonal effects in the data or potential quarter dependence in the coefficients of the lagged variables in our regressions. Second, details about the institutional setting become more important with quarterly fiscal data and it is hard to properly capture these in an empirical model. Examples are tax collection lags and differences in payment methods across countries and over time (see Perotti, 2005, for a detailed description). Third, the economic interpretation of fiscal shocks may be difficult at the quarterly level. While budget revisions are generally possible within the fiscal year, their frequency is often less than quarterly and their size and scope are likely to be more modest than changes associated with the budget presented for the entire fiscal year. Fourth, the quality of quarterly fiscal series is often not clear, because only little may be known about the construction of the data and in many cases the quarterly time series are interpolated from annual or

[5] For example, Giuliodori and Beetsma (2005) use quarterly data to assess the trade spillovers from fiscal impulses in large EU countries. Major differences with the current paper are that here we disentangle the effects of fiscal policy on output and of output on foreign trade, use panel models, and take annual instead of quarterly data.

semi-annual data. An exception is the dataset constructed by Perotti (2005), which, however, is available only for a limited number of countries.

Our choice of the data frequency for the trade block is led by our choice for the fiscal block. In addition, as far as the trade block is concerned, it is useful to note that with the use of bilateral trade data we avoid potential biases caused by the aggregation of trade flows over countries.

3. The fiscal block

3.1. Model set-up and baseline estimates

The baseline fiscal block (see Appendix B for technical details) is modelled as a panel vector auto-regression (PVAR) that explains as follows the dynamics of real public spending (g_{it}, i.e. government consumption plus government investment), cyclically adjusted real net taxes (nt_{it}^{CA}, i.e. revenues minus transfers), and real total output (y_{it}), all in natural logarithms:

$$\begin{pmatrix} 1 & 0 & 0 \\ -\alpha_{tg} & 1 & 0 \\ -\alpha_{yg} & -\alpha_{yt} & 1 \end{pmatrix} \begin{bmatrix} g_{it} \\ nt_{it}^{CA} \\ y_{it} \end{bmatrix} = A(L) \begin{bmatrix} g_{i,t-1} \\ nt_{i,t-1}^{CA} \\ y_{i,t-1} \end{bmatrix} + \begin{bmatrix} e_{it}^{g} \\ e_{it}^{nt,CA} \\ e_{it}^{y} \end{bmatrix}, \qquad (1)$$

where subscripts "*i*" and "*t*" index the country and the year, respectively and the matrix on the left-hand side captures the contemporaneous relations between the variables. Further, *A(L)* is a matrix polynomial in the lag operator L, which captures the relation between the current values of the variables and their lags. Finally, e_{it}^{g}, $e_{it}^{nt,CA}$, and e_{it}^{y} are mutually uncorrelated structural shocks that we want to recover. The first two are discretionary shocks to government spending and cyclically adjusted net taxes, respectively. Our specification follows Blanchard and Perotti (2002). We set the lag length of the system to two and estimate it for 14 EU countries over the period 1965-2004.[6]

[6] We de-mean and de-trend each variable included in the PVAR to control for country-fixed effects and country-specific linear time trend effects. Additionally, fixed time effects are included. With a panel that is large enough in the time dimension, ordinary least-square (OLS) estimation yields consistent

Equation (1) shows that public spending is allowed to depend on lagged public spending, lagged (cyclically adjusted) net taxes, lagged output and a discretionary shock. However, it does not depend on contemporaneous values of net taxes and income. In contrast, net taxes may depend on contemporaneous spending, but not on current income, and income is allowed to depend on the contemporaneous values of both other variables. Hence, the model restricts three contemporaneous effects to zero. These assumptions are needed to estimate the model and to identify the structural shocks.

To justify the restriction that cyclically adjusted net taxes do not respond to output, we follow Blanchard and Perotti (2002) and Perotti (2005) in using elasticities of the various components of net taxes with respect to output to purge each of these components of its cyclical part. These elasticities are available on a country-by-country basis from the OECD (2005). Adding up the cyclically adjusted components then yields the cyclically adjusted net taxes (see the Appendix A for more details). This variable thus takes account of the fact that the tax elasticities differ across countries. By including cyclically adjusted rather than non-adjusted net taxes we can thus impose that the contemporaneous reaction of this variable to output is zero.

Another advantage of making these *country-specific* cyclical adjustments to net taxes is that this should enable us to more accurately identify the country-specific discretionary tax shocks. If the elasticities between net taxes and output indeed differ across countries, then imposing homogeneity on the reaction of output to unadjusted net taxes would lead us to identify as the net tax shocks combinations of the true shock and the country-specific part of the cyclical reaction of net taxes to output.

The other two restrictions implicit in (1) are based on the assumption that public spending does not react to contemporaneous changes in (cyclically adjusted) net taxes and output. These assumptions seem reasonable given that spending plans are usually determined in a government budget that is presented before the new fiscal year starts. (Note that while government transfers, in particular, unemployment benefits, may be sensitive to the cycle, these are not included in our measure of government expenditure). An alternative to the former restriction would be to assume that cyclically

estimates. Alesina *et al.* (2002) and Ardagna *et al.* (2004) estimate panel VARs for similar country and period samples.

adjusted net taxes do not react to government spending.[7] However, as in Alesina *et al*. (2002), our results show that the specific restriction on the contemporaneous relation between the two fiscal variables has no bearing on the results.[8] Therefore, in what follows, we will always restrict the effect of cyclically adjusted net taxes on public spending to zero.

Column 1 of Table 1 reports the maximum likelihood estimates of the unrestricted left-hand-side coefficients in (1) over the full sample period 1965-2004. We take this as the baseline case for the panel VAR. The results indicate that government spending has no clear contemporaneous effect on cyclically adjusted net taxes. Further, a government spending increase and a net tax cut exert a highly significant positive effect on current output. We depict the impulse responses in Figure 1, where the size of both fiscal shocks is set to 1% of GDP. The impulse responses trace out the time path of the effect of the shock on output and the other variables contained in the VAR. In both instances, output rises significantly upon impact and the increase remains significant for three years after the net tax shock and even for longer after the spending impulse.

3.2. Putting identification under further scrutiny

A priori, the assumption that fiscal variables are not affected by output within the year could be contentious. Government spending and net taxes may automatically react to business cycle movements that take place after the budget has been set for the fiscal year. Such automatic reaction is generally thought to be small for government spending, while for net taxes we have tried to correct for it with the cyclical adjustment described above. However, fiscal variables could also be subject to a discretionary reaction to output movements within the year. Such reaction can for example be embedded in a midterm revision of the budget. While within-year adjustments of tax

[7] This ordering would thus allow government spending to react to net taxes. Such a reaction could for example be imagined when tax revenues are unexpectedly low and the SGP is considered as strictly binding, in which case a government would be forced to cut spending. However, an unexpected fall in taxes is usually caused by an unexpected slowdown of the economy, so that correct identification would point to a negative output shock as the source of the spending contraction. Moreover, the SGP applies only to a relative small part of our sample, while it is unclear to what extent it is actually viewed as binding. As mentioned, there is no evidence of a contemporaneous reaction of government spending to either net taxes or output.

[8] Indeed, the correlation between the reduced-form residuals of the government spending and the net tax equations turns out to be insignificant, while the impulse responses are very similar to those for the baseline.

rates, tax bases and transfer programs are likely to be limited due to time it takes to prepare such measures and transfer them through parliament, the renewal of public sector wage contracts could take into account changes in business cycle conditions that occur after the yearly budget has been set.

To assess the potential importance of the within-year reaction of government spending to output movements, we exploit the fact that within-period reactions become less likely the shorter the period is. Following Perotti (2005), we assume that the reaction is zero within one quarter. Therefore, we estimate the baseline VAR for quarterly data for Germany (see Additional Appendix A),[9] which is the only country for which we are confident to have all variables non-interpolated at the quarterly frequency. We check that seasonal patterns in the data are absent. Using the procedure described in Additional Appendix B, we then transform the estimates of the quarterly model into respective estimates α_{gy}, α_{gt} and α_{ty} for the yearly VAR of the contemporaneous reactions of public spending to an output shock and a cyclically adjusted net tax shock and of the latter to an output shock. Although theory (see McCrorie and Chambers, 2003) indicates that if the true quarterly model is a VAR, then the annual model is a vector-autoregressive moving average (VARMA) model, our procedure should provide a reasonable compromise between accuracy and complexity. The procedure also yields 90% confidence bands on α_{gy}, α_{gt} and α_{ty}, which are (-0.22, 0.69), (-0.13, 0.23) and (-3.07, 3.76), respectively. Our baseline identifying restriction that all parameters are zero is thus not rejected. However, particularly the interval on α_{gy} does suggest that its true value may be positive. Using our a priori arguments discussed earlier, we will thus examine α_{gy} further, whereas we believe that the zero restrictions on α_{gt} and α_{ty} are acceptable.

The confidence band on α_{gy} is quite wide. This is mainly due to the limited number of observations. We can enhance the number of observations by excluding net taxes from the VAR, as quarterly data on government spending and output are available for seven countries (Finland, France, Germany, Italy, the Netherlands, Sweden and the UK). For this sub-sample and on the basis of inspection of the series, we are quite confident that government spending (or at least government consumption) is not

[9] To construct cyclically adjusted net taxes at the quarterly frequency, we use the elasticity of net taxes with respect to output found by Perotti (2005) for West Germany (0.90).

interpolated. Again, we order government spending first and impose that its within-quarter reaction to output shocks is absent. The resulting 90% confidence band on α_{gy} is now (–0.082, 0.168) and has become substantially narrower, while a zero within-year reaction of government spending to output is still contained in the band. To obtain an idea of the consequences of the uncertainty around α_{gy}, we re-estimate the yearly PVAR (1) on the entire sample, but with α_{gy} set at the bounds 0.168 or –0.082. The impact reactions of output to a 1% of GDP spending increase are 0.8% and 1.2%, respectively. These values are contained by the confidence interval around the baseline, which is roughly the average of the other two impact reactions. Hence, we believe that the baseline annual identification scheme will give reasonable approximations to the effects of interest for this paper.

3.3. Comparison with the literature

The scope for comparison with the literature of the size of the output responses to fiscal impulses is limited. Most of the literature focuses on the US rather than Europe. The IMF (2004) provides an overview of some studies for the US and shows that the magnitudes of the output responses to fiscal shocks vary quite widely. We find a peak response of output to a spending increase in the EU that is larger (but not much) than what Blanchard and Perotti (2002) find on the basis of full-sample estimation for the U.S. and substantially larger than the (positive) effects found by Mountford and Uhlig (2002). The output response to a net tax reduction that we obtain for Europe lies between the positive responses obtained by Mountford and Uhlig (2002) and Blanchard and Perotti (2002) for the US. Perotti (2005) conducts a quarterly analysis on 5 countries, only two of which are also in our sample (Germany and the UK). He generally finds weak effects of fiscal impulses on output. Quite interestingly, for the two-variable PVAR in government spending and output estimated on our 7-country sub-sample for both quarterly and annual data (see previous subsection), we find that the former case yields substantially weaker and less persistent effects of a spending shock on output than the latter. For the latter case, the results were very close to what we obtained for the three-variable VAR for the full sample of countries. Although this is speculation, a possible source of difference may be that what the model identifies as

quarterly shocks consists of not only discretionary policy shocks but possibly also other shocks,[10] in which case we should not a priori expect economic activity to react.

3.4. Variations and robustness

In order to assess the robustness of the baseline model, we have also estimated (1) in first differences. In addition, we have estimated a number of plausible variants on (1). Basically, all cases yield the same conclusions. This subsection describes a number of these sensitivity checks.

In one variant, we replace output (GDP) in the PVAR with (the log of) real private output, that is, GDP minus government expenditures. The objective is to see if a spending impulse causes significant multiplier effects on output. We realise that this approach is not entirely accurate, because we subtract from output also the component of government spending that falls on foreign products. However, this component is generally small, so that our measure of private output should be close to the conceptually correct one. We report the estimates in column 2 of Table 1 and the impulse responses in Figure 2. Not surprisingly, the response of private output to a spending increase is weaker than the response of total output, though it is still becomes significant after one period. Hence, this indicates the presence of multiplier effects.

In next variation on the baseline, we have estimated the model only for the second half of the sample period (i.e., for 1985-2004). Galí and Perotti (2003) and Perotti (2005) find that fiscal policies and their effects might have changed over time. Table 1, column 3, reports the results for this case, while Figure 3 shows the impulse responses. However, both the effect of a government spending increase and that of a net tax reduction on output are basically unchanged.

In a further investigation of the robustness of the results, we have extended the baseline model with the (log of the) price level, the short-run interest rate and the (log of the) real multilateral exchange rate. One cannot a priori exclude the possibility that fiscal shocks influence the dynamics of economic activity by affecting these variables. In particular, it could be important to include an exchange rate, because in a large part of the sample exchange rates were not fixed. We assume that price level is contemporaneously unaffected by any of the other variables in the system. This

[10] For example, depending on the specific accounting practices used, a cash disbursement could be recorded in a different quarter than when it takes place.

restriction is motivated by the fact that prices tend to be sticky and budgets are generally set in nominal terms. A price increase would reduce real government spending within the same year (and could affect real tax revenues through bracket creep). We allow for the real exchange rate to be contemporaneously affected by all other variables in the system, because (through the nominal exchange rate) it is expected to react instantaneously to changes in other macro-economic variables. The interest rate is potentially contemporaneously affected by all other variables except for the exchange rate.[11] The impulse responses of output (and the fiscal variables) are very similar to the baseline responses (compare Figure 4 to Figure 1), which supports our choice to proceed with the baseline specification in the remainder of the analysis. The price rise is followed by a lagged increase in the interest rate that can presumably be explained by the desire of the central banks to counteract inflationary pressures. However, the increase in the interest rate is barely statistically significant. The real exchange rate is unaffected after the spending shock, but depreciates (i.e., increases) slightly for a brief period after the net tax reduction. We also estimated a version of the model with a long-run interest rate replacing the short-run interest rate and we estimated a version in which both the short and the long interest rate were included together. The impulse responses are virtually unchanged, while the long interest rate behaves in much the same way as the short interest rate. This is not surprising given the strong empirical correlation that is usually found between the short and long end of the term structure.

In studying fiscal spill-overs, we limit ourselves to an assessment of the spill-overs through the trade channel. However, the panel VAR set up for the fiscal block is very suitable for studying potential spill-overs via the interest rate. Rather than exploring the effects of a fiscal impulse on the domestic interest rate as we did above, we should investigate the effect on foreign interest rates. Therefore, we augment our baseline VAR for each country with the (bilateral trade-weighted) average long-term interest rate of all *other* countries in the sample. This variable is ordered last. To save space, we only report the relevant impulse responses in Additional Appendix C. The impact increase in the average foreign interest rate is on the border of significance, but after that the change becomes insignificant. The output response is very similar to that under the baseline. We maintain the baseline set up, but observe that a further

[11] We have also experimented with a switch in the position of the interest rate and the exchange rate in the system. This left the results unchanged, however.

investigation of interest rate spill-overs along the lines suggested here would be desirable in future research.

The impulse response analysis conducted so far implicitly assumed that the composition of government spending is irrelevant. Figures 5a and 5b check whether this assumption is warranted. We return to the baseline specification, but replace total government spending with one of its components, government consumption (Figure 5a) or government investment (Figure 5b). For comparability, in each case, we depict the responses to a shock equal to one percent of GDP. With average government investment of roughly 3% of GDP, this implies a rise in investment close to 35%. While the output stimulus from the investment shock is slightly larger and more persistent than the stimulus produced by the government consumption shock, the differences are modest and from a statistical point of view the baseline model, which treats government consumption and investment symmetrically, is acceptable.

Another baseline assumption is that we force impulse responses to be the same for all countries. Although all countries in our sample are members of the EU, which ensures that their economies are similar in some basic respects, and our panel VAR allows for substantial deterministic heterogeneity (country and time effects and country-specific time trends), it is important to put the homogeneity restriction under further scrutiny.

Heterogeneity in the responses to shocks could in principle arise for many reasons. For example, differences in the size and operation of the welfare system affect the strength with which transfers and thus net taxes react to movements in the economy. Further, the response of government consumption to the performance of the economy may vary with differences in the wage setting process for civil servants. Differences in the political system could affect the speed with which decisions can be taken in response to economic developments. While our data are too limited to test these possibilities within our panel VAR setting, we shall now split the country sample in some ways to focus on possible sources of heterogeneity that we can indeed test for with our data. Later on (Section 5), we shall explore the variation in the estimates of the fiscal block on individual countries.

A potential major source of heterogeneity that we can explore with our empirical framework concerns the differences in the size of the economies in our sample. Larger economies are generally more closed, suggesting that fiscal expansions are more effective, as less of the stimulus leaks abroad. To investigate the relevance of

the country size for the impulse responses, we estimate the baseline model on a sub-sample with the five largest economies (Germany, France, UK, Italy and Spain) and a sub-sample with the nine remaining countries. The impulse responses are shown in Figures 6a and 6b, respectively. The output responses to the shocks are of a similar order of magnitude. The main difference concerns the net tax response after the initial spending impulse. The large countries react with a somewhat lagged tax increase after a spending increase, while the small countries react with a (barely) significant net tax cut on impact. We shall not speculate on an explanation for this difference, which may require a detailed look at differences in the budgetary processes in the various countries. Our main focus is on the output effects, in particular because the size of the trade spillovers, assessed later on, is to a large extent linked to the output responses. Given that these are rather similar, we continue to impose homogeneity on our PVAR system.

As a further check for possible heterogeneity, we estimate the baseline model on the "core" set of countries (France, Germany, Italy, Belgium and the Netherlands) that have been members of the European Union (previously Community) from the start onwards. Therefore, trade linkages between these countries may be more intense than on average in the sample and a fiscal impulse originating in a core country may have a smaller effect on its economy, as a larger part of the shock leaks away. However, the stimulating effects of a spending increase or a net tax reduction on output hardly differ from those for the entire sample and we only report the impulse responses in Additional Appendix C.

4. The trade block

4.1. Model set-up and baseline estimates

The second step in our procedure is to estimate the trade block. We are interested in the effect of domestic output on foreign exports to the home country. These exports are also affected by the exchange rate, for which we want to control. A popular model that includes both income and the exchange rate as export determinants is the gravity model of trade – see Bergstrand (1989) for a description and theoretical motivation. Our model is closely related to that model. Nevertheless, to simplify the model, we leave

out some typical gravity variables, such as the exporter's income and income per capita, because they turn out to be unimportant here – see Subsection 4.3.

Because we are ultimately interested in the short- and medium-run effects of fiscal impulses on foreign exports, we have to extend the standard gravity model by allowing for dynamics in the form of lagged responses of exports to income, the exchange rate and past exports. Therefore, we use the following autoregressive distributed lag (ADL) specification with n=2 lags:

$$x_{ji,t} = \sum_{s=1}^{n} \beta_{1s} x_{ji,t-s} + \sum_{s=0}^{n} \beta_{2s} y_{i,t-s} + \sum_{s=0}^{n} \beta_{3s} rer_{ji,t-s} + \varepsilon_{ji,t}, \qquad (2)$$

where $x_{ji,t}$ is (the log of) bilateral real exports at time t from country j (the foreign country) to i (the home country), $y_{i,t-s}$ is (the log of) real output in the home country, and $rer_{ji,t-s}$ is the (log of the) bilateral real exchange rate between country j and country i. It is defined such that, if $rer_{ji,t-s}$ rises, then the currency of country j (the exporting country) depreciates in real terms against the currency of county i. The model also contains – though not shown in equation (2) – fixed effects for country-pair ji, which capture the impact of all time-invariant determinants of trade (such as distance, a common border, a common language, etc., as in the gravity model). Further, we include fixed time effects to control for, among other things, the general state of the European economy in a specific year, and country-pair specific linear time trends representing potentially omitted country-pair variation in trending determinants of exports (such as transportation costs and trade liberalization),[12] as motivated in Bun and Klaassen (2003) – see also below. Following standard practice in the trade literature (see, for example, Rose, 2000, and Glick and Rose, 2002), we add the dummies $EU_{ji,t}$ and $FTA_{ji,t}$. The former dummy scores one if at time t both j and i are members of the European Union (or the European Community, before the ratification of the Maastricht Treaty), and zero otherwise. Similarly, $FTA_{ji,t}$ is a dummy equal to one if there is a free trade agreement between j and i at time t. For both dummies only contemporaneous values are included, because their lags turn out to be irrelevant. Finally, $\varepsilon_{ji,t}$ is a zero-

[12] Barrell and Dées (2005) show that the growth of foreign direct investments may also be a potentially important determinant of the trend in imports (hence, exports).

mean random variable that may be heteroskedastic (over time and country pairs), but is assumed to be uncorrelated over time and country pairs.

We estimate the model by OLS (see Panopoulou and Pittis, 2004, for theoretical and empirical support). Again, the data set covers the period 1965-2004. It accounts for all bilateral trade relationships between the 14 EU countries used in the fiscal block, providing us with 182 country pairs (see Appendix A for further details).

Table 2, column 1, presents the estimates for the parameters of interest in (2). Bilateral exports are highly correlated with bilateral exports one year earlier. A real depreciation of the exporting country's currency (i.e. a rise in $rer_{ji,t}$) has a strong positive effect on bilateral exports from *j* to *i*; the long-run effect (i.e. sum of the contemporaneous and all lagged effects) is 0.704 with a standard error of 0.070 (not reported in the table).[13] Similarly, an increase in real GDP of country *i*, the importing country, exerts a strong positive effect (the long-run effect is 1.133 with standard error 0.149). Finally, in the long run, membership of a free trade area leads to 20% (standard error 3.5%) more trade and membership of the European Union stimulates trade also by an additional 20% (standard error 2.3%). Obviously, these effects are substantial.

To see how the long-run effects materialise over time, we plot the impulse responses of bilateral exports from the foreign to the domestic country to shocks in bilateral exports, the real exchange rate and domestic output (see Figure 7). The size of the shock is a one percent increase in the variable under consideration. As expected, the effect of an increase in domestic output on bilateral exports is positive and statistically highly significant on impact. A one percent real depreciation of the foreign currency leads to a statistically significant 0.5% increase in foreign exports on impact. However, these effects die out quickly. The effect of the real exchange rate impulse has disappeared after two years, while the effect of the output shock vanishes already after one year.[14] Note that the accumulation of the impulse responses over time gives the long-run estimates presented above.

4.2. Relation with the literature

[13] This long-run effect is computed as $(\beta_{30} + \beta_{31} + \beta_{32})/(1 - \beta_{11} - \beta_{12})$.

[14] Notice that, although the estimated coefficient of one-year lagged domestic output is significantly negative, the impulse response to the shock is close to zero after one year. The reason is that the impulse

We can compare our baseline estimates for the trade block with the literature. Barrell and Te Velde (2002) provide a recent survey of (long-run) income and price elasticities for European countries. Typically, the income elasticity is clearly positive and lies between one and two. Regarding the exchange rate effect on trade (the price elasticity), one usually reports significant estimates with values between zero and one. Our results are thus in line with what the literature finds. However, we should note that in contrast to the literature, we include country-pair specific time trends. In particular, if we leave these out, the country-pair specific time trends, the long-run income elasticity rises, although it remains well within the range from one to two.

As the inclusion of country-pair specific time trends is not standard for gravity-type models such as (2), we shall now clarify their relevance in more detail. Suppose that we estimate model (2) without country-pair trends and also leave out any dynamics (i.e., n=0 in (2)). Further, let us divide all country pairs into two groups, namely those that have the Euro now and the other pairs. The left graph of panel A1 in Figure 8 shows that the averaged residual series for each of the two groups (the solid line for the 110 Euro-country pairs and the dotted line for the 72 remaining pairs) exhibits strongly diverging trends, thereby invalidating standard inference.[15]

If we add the dynamics to the model, while still excluding the country-pair trends, we obtain the left graph of panel A2, which no longer exhibits trends. In fact, this is entirely due to the presence of lagged dependent variables.[16] However, the underlying estimates for lagged exports are substantially and significantly higher than in our benchmark model with trends: 0.746 and 0.145 instead of 0.610 and 0.065, respectively. As the latter model generalizes the former, the estimated coefficients of lagged exports are distorted if there is no correction for potential omitted trending trade determinants. Proper inference thus requires adjusting the model.[17]

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response captures the total effect of the one-year output lag on current foreign exports. Hence, it captures also the positive effect via lagged foreign exports.

[15] The residuals capture a deviation from a general time trend. For each year, aggregated over all country-pairs they sum to zero, implying that whenever the solid line has positive slope, the other line has a negative (and steeper, due to the difference in group size) slope.

[16] Leaving these out, while including lagged income and real exchange rate terms, results in a graph similar to panel A1.

[17] The residual trends may also be partly due to neglected country-pair heterogeneity of the parameters in specification (2). A model with fully heterogeneous regression parameters but no country-pair trends indeed shows no trends in the residuals. However, such a model does not correct for potential omitted trending trade determinants, so it would still be unclear to what extent the estimates would be biased. Hence, we prefer solving the trends problem first, and address the heterogeneity issue later on as another robustness check.

There are two possible approaches in this regard. First, one could try to find regressors that fully explain the omitted determinants of exports. Although above we discussed some potential determinants, it is difficult, if not impossible, to find the complete set of determinants. Second, as an approximate solution one can include deterministic trends to correct for the main signal of the omitted trending variables. As the left graph of panel B1 in Figure 8 shows, the problem of residual trends disappears for the static variant. In the dynamic variant, which is our baseline model (2), the lagged export coefficients then more cleanly represent the true export dynamics. Thus improved inference motivates our use of country-pair trends.

4.3. Variations on baseline trade block

Column 2 in Table 2 reports the estimates when we add as explanatory variables in the panel trade model the domestic discretionary government spending shocks that we identified from the fiscal block. It is conceivable that the government buys part of a spending increase directly from abroad, in which case the discretionary government spending shock should enter with a positive coefficient in the trade equation. However, the estimates show that the discretionary spending impulse exerts no direct effect on bilateral exports from the foreign country. This is not surprising, because generally by far most of government spending falls on domestic goods and services. Incidentally, we note that the government spending shock is a generated regressor in the trade model. Hence, if correctly computed, the standard errors would exceed those reported in Table 2, which would reinforce our finding that the government spending shock has no significant direct effect on foreign exports.

We have estimated a number of additional variations on the baseline, motivated by trade model specifications elsewhere in the literature. First, we add the log of real GDP of the exporting country *j*. While this variable includes the bilateral export that is on the left-hand of the regression equation, on average this bilateral export makes up only a small fraction of total exporter's income. Hence, any potential endogenous regressor problem is likely to be unimportant. Column 3 of Table 2 shows the estimates. We conclude that there is no indication that foreign output matters for foreign exports, which motivates us to retain our baseline specification. Second, we have also estimated specifications in which we added to the baseline the log of real GDP *per capita* of country *i* or country *j* (to control for the effect of the welfare level)

or a measure of bilateral real exchange rate volatility. The estimates are not reported here, but none of these variables came out significantly. This result differs somewhat from what is usually found in the empirical trade literature. The reason is that we use a dynamic instead of a static model. Indeed, if we follow the usual procedure of estimating static models and not adjusting the standard errors for serial correlation in the residuals, then all the variables just mentioned become statistically significant. Because we find clear evidence of dynamics (see Table 2), again we prefer to stick to our baseline specification.

We also explore a variation on the baseline in which we add a dummy for joint membership of the Euro area. This extension is motivated by the work of Rose and co-authors. They find evidence that, ceteris paribus, countries trade substantially more with each other when they share a common currency.[18] Although this conclusion is based on datasets in which the country-pairs forming a currency union tend to be very different from the Euro-area pairs in our data (the former pairs mostly involve smaller and poorer countries, while many of them are former colonies of industrialised countries), Micco *et al.* (2003) confirm the trade-enhancing impact of currency unions for the Euro-zone. Our estimates, however, show that the Euro dummy is insignificant and close to zero. As Bun and Klaassen (2003) explain, this is due to the fact that, in contrast to existing papers, we include country-pair specific time trends to control for omitted trending variables.

To show this more explicitly, recall the top left graph in Figure 8 based on the estimation of (2) in the absence of country-pair specific trends and dynamics. This resembles the specification typically used in the currency union trade literature. The diverging trends between the two groups of country pairs lead to a large difference in the residuals at the end of the sample period. If we then add the Euro dummy and plot the new average residual series, we obtain the top right panel. It shows that the Euro dummy, as it equals unity only towards the end of the sample, simply explains the difference in the residuals towards the end of the sample. Not surprisingly, the Euro

[18] See Rose (2000, 2001), Rose and Van Wincoop (2001), Frankel and Rose (2002) and Glick and Rose (2002). A positive effect of monetary unification on trade is also found by Barro and Tenreyro (2004) who use an instrumental variables approach to control for the effect that trade intensity itself might have on the choice of forming a currency union. However, Persson (2001) argues that the characteristics of country-pairs in a union may systematically differ from the entire sample of country pairs. Moreover, he points to the relevance of potential non-linearities. He indeed finds much smaller, though still substantial, effects of a currency union on trade. However, the effects he estimates are statistically insignificant or only weakly significant.

effect estimated using this standard specification is 30% (on the log of exports), and this is thus driven by the diverging trends that started long before the Euro came into existence – see Bun and Klaassen (2003) for further analysis. As we have seen earlier, adding dynamics (but leaving out country-pair trends) removes the trends in the average residual series. However, it still leaves a distorted (long-run) Euro effect of 31% (standard error 2%), while our more general approach with heterogeneous trends included yields an estimate of 0% (standard error 3%). Hence, such trends are important for inference on the Euro effect.

Nevertheless, one should realise that our insignificant estimate of the Euro effect does not necessarily contradict the work by Rose and others. For instance, the trade-magnifying effect of the formation of a currency union may take time to materialise. The Euro has existed only for 5 years and that may be insufficient for finding strong trade increments. However, our result motivates us not to include a Euro dummy in our baseline model. In addition, interactions of joint Euro membership with year dummies, to allow for time-varying effects of the Euro, and interactions with the other variables in the regression were mostly insignificant. We shall thus leave them out.

Our next step is to explore whether the results differ over sub-samples of countries or time, as we did for the fiscal block. Again, this helps us to assess the homogeneity assumption. As before, we split the sample into the five large (EU5) and nine small (EU9) countries. The parameter estimates reported in columns 4 and 5 of Table 2 are reasonably similar, though there are some small differences. Column 6 of the table reports the corresponding estimates when the sample is limited to the EU5-core (Germany, Italy, France, Belgium and the Netherlands). Again, the differences with the baseline are limited. Given our focus on the income elasticity, we retain the baseline model. This is also confirmed by the impulse responses (not reported here).

How do the parameter estimates vary over time? We repeat the baseline regression, where we now allow the coefficients on lagged trade flows, home country income, the real exchange rate and EU membership to depend on the specific sub-period, 1965-84 or 1985-2004. (Because in the second sub-period all country pairs had free trade, the parameter for the FTA dummy over the second period is not identified, so we do not make that dummy time varying). Table 3 shows the results. While the sign and significance is the same for all parameters for both sub-periods, the size of the impact effect of income is larger for the first sub-period: 2.13 versus 1.43. Not

surprisingly, the impact effect of the baseline model, 1.75, is in between these numbers. Given the standard errors of the three estimates, and the fact that the long-run effects are very close for the two sub-periods (1.10 in both cases), we stick to the baseline model as a reasonable compromise. Nevertheless, because the income elasticity is important for our overall estimate of the effect of fiscal policy on foreign exports, we will further examine the consequence of the short-run sub-sample sensitivity of the income elasticity on this overall estimate in Subsection 5.1.

5. The effects of a fiscal impulse on foreign exports and income

5.1. The overall effect on foreign exports

By combining the estimates for the trade block with those for the fiscal block, we can compute the overall effect of a domestic fiscal impulse on foreign bilateral exports to the home country. In principle, the effect can operate both through output and through the real exchange rate. In our baseline, we set the effect via this second channel to zero. The reason is that we want to assess the spill-overs of a fiscal impulse under the current regime of a monetary union in Europe. With a common currency and sticky prices, we can expect only limited short-run movements in real exchange rates.

Appendix C shows that, to obtain the impulse responses of foreign bilateral exports to a fiscal shock, we can simply multiply the impulse response function estimated for the fiscal block with the estimated distributed lag on home output for the trade block. Figure 9 shows the impulse responses obtained by combining the baseline estimates of the fiscal and the trade block. The effect of a domestic spending shock is significantly positive for several years and reaches its maximum after one year. A net-tax cut also produces a significant increase in net exports. However, the effect is smaller and shorter-lived than that of an equal-sized spending increase.

Table 4 reports the corresponding cumulative effects on foreign exports of the domestic fiscal shock. A relevant question concerns the appropriate horizon for assessing the effects of the shock. As we explained earlier, we are primarily interested in the short- and medium-run consequences of fiscal impulses. Therefore, Table 4 reports the impact effects and cumulative responses after two and after five years. For a one percent of GDP spending increase, which amounts to an increase in public spending of slightly over 4 per cent, the cumulative export effect after two years is a

statistically significant gain of 6.8 per cent of annual exports. For an equally-sized net tax reduction, the (significant) gain in exports amounts to 1.9 per cent.

A complication in assessing the gains from a fiscal impulse is that, after the shock, fiscal policy continues to deviate from its original value for some time (recall Figure 1). It can be reasonably argued that these subsequent deviations should be added to the initial spending shock in order to obtain the total budgetary cost of the cumulative export gain. Table 4 also reports the "normalised" response, which divides the cumulative gain in exports by the cumulative deviation of government spending from its original level. Over a two-year horizon there is an export gain of 2.2% of annual exports for each one percent of GDP additional spending. The corresponding figure for a net tax reduction is 0.8%. Both figures are statistically significant.

To examine the robustness of these estimates, we also compute the responses of foreign exports to domestic fiscal shocks for some major variants of the baseline model. In particular, we consider the combination of (1) the extended panel VAR with the baseline trade model when we shut off the effect on trade via movements in the real exchange rate, (2) idem, when we also include the effect on trade via changes in the real exchange rate (for details, see Appendix C), (3) the panel VAR and the trade model both for the most relevant sub-period, 1985-2004, (4) the panel VAR and the trade model each for the group of the five largest EU countries, (5) the panel VAR and the trade model each for the group of the nine smallest EU countries and (6) the panel VAR and the trade model each for the EU-5 core (Germany, France, Italy, Belgium and the Netherlands). The numbers are again found in Table 4. In most instances the responses remain reasonably close to those for the baseline combination, while in a few instances the responses are outside the confidence band for the baseline combination. In virtually all cases we consider, the effect of the fiscal shock on trade is significant and quite sizable, with the effects for government spending being larger than for the net tax cut. The exception is case (2) when the fiscal shock is a net tax reduction. As Figure 4 already showed, a net tax reduction leads to a real exchange rate depreciation for the initiating country, which exerts a negative effect on exports to this country. Indeed, the trade spill-over now loses its significance after one year.

While the above variations on the baseline already address to some extent the potential importance of cross-country heterogeneity, we explore the issue further by estimating both the fiscal and the trade block for each country separately and combining the results into a country-specific impulse response of foreign exports to the

country implementing the fiscal shock (i.e., a country-specific version of Figure 9). The estimation of each individual trade block is based on observations of bilateral trade between the country and each of its 13 partners over the sample period. Figure 10 depicts the median of the responses of foreign exports to the individual countries initiating the fiscal shock, as well as the boundaries on the ten individual responses closest to the median. The figure confirms a tendency towards a fiscal spill-over in the first periods after the shock. Indeed, with one exception, whenever the effect of the fiscal expansion is significant (in, respectively, 4 and 8 cases for government spending and net tax shocks), it is positive. The figure also suggests non-negligible differences in the individual responses. However, in this connection, it is important to realize that there is substantial inaccuracy in the estimates of the individual countries' fiscal block, likely leading to an exaggeration of the heterogeneity suggested by Figure 10. To support this argument, Figure 11 shows the median over the individual impulse responses obtained by combining the fiscal and trade block estimates on the individual countries, as well as the medians of the upper and lower-bounds of the 90% confidence intervals around the individual impulse responses. Clearly, "on average" the width of the individual confidence band is substantial, thus motivating the use of panel data in order to enhance accuracy.[19]

5.2. The effect of a domestic fiscal impulse on foreign output

By combining the cumulative and normalised responses of the bilateral foreign exports with the actual shares of bilateral foreign exports in foreign output, we can calculate the effects of domestic spending and net tax shocks on foreign output. We calculate only the *direct* contemporaneous contributions of higher exports to output and ignore contemporaneous multiplier effects of exports on foreign economic activity, future effects of current activity increases and further feedback effects among the economies. The numbers are only intended to give a rough sense of what could be the possible order of magnitude and, therefore, the economic significance of the fiscal spill-overs. Computation of the total effects on foreign output is complicated and beyond the scope of the present paper.

[19] The summary responses of output to fiscal shocks reported in Additional Appendix D show that it is the inaccuracy of the individual fiscal block estimates that causes the confidence bands to be so wide.

Obviously, there is a large variation in the size of the spill-over effects, depending on the country of origin of the fiscal shock and on the intensity of trade between the partner and origin country. Table 5 reports the normalised responses of EU countries' output to a spending increase or a net tax reduction in the two largest economies in the Euro area (Germany and France). The computation of these responses is analogous to those for the net exports responses in Table 4. A German fiscal expansion (especially a spending shock) has quite strong effects on its small neighbours. An increase in public spending (a decrease in net taxes) by 1% of GDP in Germany leads to a more than 0.4% (0.13%) normalised increase in GDP of Austria, Belgium-Luxemburg and the Netherlands after two years. In accordance with the gravity model, the effects on these countries are larger than the effects on other small countries that are further away and do not share a common border with Germany. For example, the corresponding number for Greece is only a 0.06% increase in GDP. Based on a GDP-weighted average across all partner countries, the normalised effect of a German fiscal stimulus is 0.13% (0.07%) of foreign GDP. Naturally, the spill-over effects of a French fiscal impulse are smaller (on average, roughly half of the size of the spill-overs from the German shock), but they are still non-negligible. Of course, fiscal shocks originating in the smaller economies tend to lead to smaller spill-over effects. The smallest effect is found for a fiscal stimulus in Greece, which raises normalised weighted-average foreign GDP after two years by only 0.01% in the case of a spending increase and 0.005% in the case of a net tax reduction.

The reported effect of a domestic fiscal impulse in Germany on foreign output seems rather large compared to what the limited empirical literature, in particular Marcellino (2002), finds. His model includes German and foreign (French, Italian or Spanish) variables in one VAR. A German output shock has a significant positive effect on partner countries, an effect that is consistent with our finding for the panel trade model. However, a German fiscal shock has no contemporaneous effect on German output (in contrast to what we find) and only small and insignificant effects on the other economies. A major difference with our work is that we have more observations because we use a panel VAR approach for our fiscal block, while Marcellino estimates a VAR only for Germany. Moreover, owing to some specification differences and the fact that Marcellino's setup does not distinguish spill-overs via trade from other spill-overs, a detailed direct compassion between his and our results is not possible.

6. Policy experiments and counterfactuals

6.1. A "Portugal-style" budget squeeze

The deficit of Portugal, as reported by Eurostat (2006), was 2.8% of GDP in 2002, down from 4.2% in 2001. An interesting question is what would have been the economic consequences had Germany, say, brought its deficit back from its actual level of 3.8% to 2.8% in 2002. To "simulate" such a budget squeeze, we compute the size of the discretionary fiscal shocks (public spending or cyclically-adjusted net taxes) needed to produce a contemporaneous deficit reduction of one percent of GDP. We can then also compute the implications for domestic and foreign GDP and the foreign public deficit of such a budget squeeze. Obviously, our computations are only meant to give a sense of the size of the potential effects.

In the case of a government spending reduction, the computation is complicated by the fact that both output and cyclically adjusted net taxes respond within the same period and, moreover, that we need to transform the change in cyclically-adjusted net taxes into a change in actual net taxes. Appendix D shows the details of the computation. While a one-percent of GDP reduction in government spending is equivalent to a reduction of 4.2% in the variable itself, to produce an improvement in the deficit of 1% of GDP, government spending has to be contracted by 6.8%. The difference is mainly accounted for by the large fall in output by 1.8% and the substantial effect on the cyclical component of net taxes. The same deficit reduction is attained with an increase in cyclically adjusted net taxes by 5.5% and results in an output fall of 0.40%. Obviously, on the basis of our estimates, Germany would have had to implement substantial budgetary contractions with serious short-run output consequences for the deficit to stay below the limit set by the SGP.

For France, the consequences would be even larger. According to Eurostat (2006), the French deficit peaked at 4.2% of GDP in 2003. A "Portugal-style" budget squeeze to bring down the French deficit to 2.8% of GDP would have required a spending contraction of 9.5% and led to an output fall of 2.5%, or, alternatively, it would have required a net tax reduction by 7.7%, which, in turn, would have caused output to fall by 0.56%.

A deficit reduction in Germany also affects EU partners via the trade spill-over. In fact, the government spending shock needed to produce a one-percent reduction in

the German deficit to GDP ratio equals 6.8/4.2 = 1.62 times the shock on which the numbers in Table 5 are based and thus leads to a contemporaneous average fall in partners' GDP by 0.21%. For the net tax increase the corresponding number is a 0.043% fall in GDP.

Finally, with these numbers we can calculate the effects of the German fiscal contraction on partners' public finances. We do this by feeding the change in partners' GDP as an output shock into the approximation expression for the deficit change in Appendix D. On average, a German deficit reduction of 1% of GDP attained with a spending contraction raises partners' current deficit/GDP ratio by 0.10%, while the corresponding number for a net tax increase is 0.021%. Hence, forcing Germany (or another country) to remain within the deficit limit set by the SGP may make it more difficult in the short run for its partners to obey the limits imposed by the SGP.

6.2. Consequences of ignoring trade spill-overs

This subsection shows how the estimates of the two blocks can be used to assess the consequences of mistakenly ignoring the spill-overs from fiscal policy via trade. We emphasise that the purpose of this experiment is to illustrate the methodology and to give an impression of the potential order of magnitude of the effects of these policy failures.

To simplify the illustration, suppose that Europe only consists of France and Germany. We consider two scenarios. In scenario I, both countries set their policy instruments with the intention of stimulating their economy by 1%, for instance, in response to a symmetric negative shock. In scenario II, France, say, aims at stimulating its output by 1%, whereas Germany aims at setting its instrument to reduce output by 1%, for example, to offset an asymmetric shock. Under both scenarios we compare the setting of the policy instruments (government spending and cyclically adjusted net taxes) and the realized output effects for the case where governments account for spill-overs and for the case where they ignore spill-overs. We note that, irrespective of the behaviour of the governments, spill-overs are always assumed to exist.

If the governments use government spending (e_i^g) to affect their economies, then the (percentage) increase in output of country i (i = F(rance) or G(ermany)) is given by:

$$dy_i = \left(\alpha_{tg}\alpha_{yt} + \alpha_{yg}\right)e_i^g + \left(X_{ij}/Y_i\right)\beta_{20}dy_j, \tag{3}$$

where the first term on the right-hand-side is the policy effect (its coefficient follows from inverting the structural VAR in equation (1)) and the last term is the spill-over from abroad, where X_{ij} is exports in levels from country i to country j and Y_i is GDP of country i. We are only interested in contemporaneous effects. As in the earlier computations, we only account for the direct effect of exports on output, implying that the latter rises one-for-one with an increase in exports. An expression similar to (3) holds for also cyclically adjusted net taxes as the instrument. In our computations below, we take for the α parameters the estimates in the first column of Table 1 and use β_{20} from column 1 in Table 2. For exports and GDP we take the 2003 figures. Hence the only unknowns are the two output changes and the two instrument settings.

In scenario I, France and Germany want to stimulate their economies by 1%. If they are aware of spill-overs and of each other's objective and instrument setting, the governments choose their fiscal policy stances such that each one of them reaches the goal. Taking spending as instrument, each government i chooses $e_i^g = \left(1 - \left(X_{ij}/Y_i\right)\beta_{20}\right)/\left(\alpha_{tg}\alpha_{yt} + \alpha_{yg}\right)$, which is 3.51% (3.53%) for France (Germany) and, obviously, $dy_i = 1\%$ (see Table 6) – details on the computations are found in Additional Appendix D. If both governments ignore spill-overs, they erroneously ignore the final term in (3), so that $e_i^g = 1/\left(\alpha_{tg}\alpha_{yt} + \alpha_{yg}\right) = 3.79\%$ and the resulting output change is 1.08% (1.07%) for France (Germany). Therefore, ignoring spill-overs leads to excess government spending and overshooting of the GDP target. However, the deviation from the goal is not large. Likewise, if governments use cyclically-adjusted net taxes as their instruments, incorporating spill-overs in policy leads to a net tax cut of 12.87% (12.93%) and both countries meet the 1% output stimulus. Ignoring the spill-over, however, leads to a larger tax cut (namely, 13.89% for both countries) and output is stimulated by too much.

The overall result is that in case of a symmetric stimulus, once governments are aware of trade spill-overs, the need for expansionary fiscal policy is lower and the output target is perceived to be achieved more easily. This suggests that in case of a European-wide recession, governments find it easier to fulfil the SGP. In addition, note

that we have assumed throughout that spill-overs exist. If they did not exist, then a more expansionary fiscal policy would be needed to reach the 1% target. Hence, from this perspective, the mere existence of trade spill-overs makes the SGP less binding.

In scenario II, France (Germany) wants to stimulate (contract) its economy by 1%. If they use spending and are aware of the spill-overs, France should increase spending by 4.07%, whereas Germany reduce it by 4.05%, with both countries meeting their target (see Table 6). However, if spill-overs are ignored, France (Germany) increases (reduces) public spending by 3.79%. Therefore, France expands too little as it ignores the negative spill-over from the Germany contraction, whereas the German fiscal contraction is too small to hit the output target. For cyclically adjusted net taxes Table 6 reports similar results. Hence, if the French expansion was to offset a negative shock and the German contraction was to offset a positive shock, erroneously neglecting spill-overs leads to insufficient smoothing of the business-cycle.

7. Conclusion

In this paper, we have explored the empirical relevance of trade spillovers from discretionary fiscal policy shocks in the European Union. Our estimates indicate that these spill-overs may be economically important, although their size varies quite substantially with the distance between trading partners and the size of the country where a policy shock originates. The presence of such spill-overs may have a number of consequences for policymakers. First, stronger spill-overs imply that a larger share of a national fiscal stimulus leaks away, thus reducing its effectiveness. Second, they make clear that policies of individual countries are a matter of common concern, thereby underlining the need for informal and formal meetings of policy-makers in the Euro-group and ECOFIN. Yet, given the variation in the size of the spill-overs, policymakers will differ in the importance they attach to reaching some consensus on a broad European policy stance. Our analysis can provide some rough indication on the quantitative consequences of neglecting policy spill-overs via trade. More accurate information on the distribution of the costs and benefits of fiscal impulses also promotes a better understanding among countries of each other's policy preferences. A third consequence is that the trade spill-overs may help to rationalise the transfers to the new EU member states via the Cohesion and Structural Funds, if they lead to more public spending and higher incomes there and thus to more imports from the "old" EU

members. Finally, exploiting the estimates of the separate steps that lead to the spill-over effect (that is, the fiscal and trade blocks), one can assess the consequences of a tight enforcement of the SGP, both for the domestic economy and the trading partners. Our back-of-the envelope calculation suggested that these may be quite substantial in the short run.

Our baseline estimates suggest that, measured over a two-year horizon, a German fiscal expansion equal to 1 per cent of GDP implies a gain of 2.2 per cent of annual bilateral exports by EU trading partners for a public spending shock and 0.8 per cent for a net tax shock. Averaged across EU partners, the corresponding direct output gain of a fiscal stimulus in Germany is 0.13 percent in case of a spending increase and 0.07 percent for a net tax cut. Of course, the spill-overs on output are (substantially) smaller when the fiscal shock originates in another country. The purpose of this first attempt at quantification is to provide an indication of the potential economic significance of the spill-overs via trade. An all-inclusive calculation of such spill-overs on foreign output would have to take into account many other effects, such as contemporaneous multiplier effects of exports, lagged effects on future foreign output, feedback effects between economies and, even beyond the scope of the current model, issues such as possible shifts in the activity of multinationals to the country that initiates the fiscal expansion. This might be especially relevant in the case of well-targeted tax reductions.

While the paper detects potentially important fiscal spill-overs via trade, its analysis is obviously subject to a substantial number of limitations. The scope for exploring how the model parameters and spill-overs have changed in response to all major institutional changes during the sample period (for example, reductions in trade barriers and changes in the scope and size of the public sector) was only limited. We were also limited in our investigation of the cross-country homogeneity restrictions in our panels. In future work, both issues might be addressed with a more sophisticated model of the sources of time and cross-sectional variation of the parameters and using data on variables that are at the source of this heterogeneity.

Obviously, there are also limitations regarding the conclusions that we can draw from our work. In particular, the scope of the analysis is too limited to take a stand on

the potential desirability of more formal fiscal coordination.[20] The magnitude of the spill-overs is still too uncertain while other potentially relevant transmission channels have, on purpose, not been taken into account here. Indeed, as empirical analysis in Darvas et al. (2005) suggests, discretionary fiscal policy, possibly resulting in less fiscal discipline, may actually be a source of divergence in business cycles.

As the preceding discussion suggests, there are many directions for further extension of analysis. One extension would be to come to a more complete assessment of the overall spill-overs of fiscal policy, not only quantifying the trade channel, but also other channels (for example, via the foreign interest rate – see Faini, 2005), and comparing the sizes of these channels.

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[20] The issue is studied, for example, in a report of the German Federal Ministry of Finance (2002), which advises negatively on such co-ordination. In contrast, Jacquet and Pisani-Ferry (2001) argue strongly in favour of enhanced fiscal coordination in Europe.

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APPENDIX A. Data sources and procedures

Data sources are the Economic Outlook (EO) of the OECD Statistical Compendium; the International Financial Statistics (IFS) of the International Monetary Fund (IMF) Database; and the Direction of Trade Statistics (DOTS).

Fiscal variables

The EO provides time series at *annual* frequency for the following variables:

CGAA	=	Government Consumption
IGAA	=	Fixed Investment, Government
PCG	=	Deflator, Public Consumption (base year 1995 =100)
PIG	=	Deflator, Government Fixed Investment (base year 1995 =100)
TIND	=	Indirect Taxes
TSUB	=	Subsidies
TY	=	Direct Taxes
SSPG	=	Social Benefits Paid by Government
TRPG	=	Other Current Transfers Paid by Government
SSRG	=	Social Security Contributions Received by Government
TRRG	=	Other Current Transfers Received by Government

Additional variables

GDP	=	Gross Domestic Product (Market Prices), Value
PGDP	=	Deflator for *GDP* at Market Prices (base year 1995 =100)
IRS	=	Short term interest rate

From the above series, we construct the following variables:

Y	=	Real GDP = *GDP**100/*PGDP*
G	=	Real Public Spending = *CGAA**100/*PCG* + *IGAA**100/*PIG*
PY	=	Real Private GDP = *Y* – *G*
REVENUES	=	*TY* + *TIND* + *SSRG* + *TRRG*
TRANFERS	=	*TSUB* + *SSPG* + *TRPG*
NT	=	Real Net Taxes = (*REVENUES* – *TRANFERS*)*100/*PGDP*

Note that due to short data availability, for Ireland and the Netherlands *TRPG* and *TRRG* are not included in the calculation of *REVENUES* and *TRANFERS*.

In order to cyclically adjust net taxes, we follow Alesina *et al.* (2002) and for each component of revenues and transfers at time *t* we compute:

$$R_{it}^{CA} = R_{it}^{NCA}(Y_t^{TR} / Y_t)^{\xi_i},$$

where superscripts *CA*, *NCA* and *TR* denote, respectively, "cyclically adjusted", "non cyclically adjusted" and "trend", and ξ is the elasticity of component i with respect to real output. Elasticities are provided by Van den Noord (2000) and the OECD (2005). However, the OECD does not provide the transfers elasticity. Therefore, as in Alesina *et al.* (2002), we use the total primary expenditure elasticity and scale it up by the ratio of transfers to total primary spending. Additionally, we calculate trend GDP separately for each country by regressing log real GDP on a constant and a linear and a quadratic time trend.

Trade variables

The real bilateral export flows X_{ji} from country *j* to country *i* in a given year are taken from Bun and Klaassen (2003) updated with the years 2003 and 2004. They are constructed as the sum of the monthly real exports, where the latter is the nominal value of exports in exporter's currency divided by the exporter's price index. The nominal value of exports in exporter's currency is obtained by converting the original dollar denominated export values of the DOTS. The real bilateral exchange rate RER_{ji} is the average of the monthly real rates computed using nominal rates and the same exporter's price indices as used above. The real effective exchange rate *rer* (used in the sensitivity analysis for the fiscal block) is the weighted average of the log of RER_{ji} in index form, using export shares as weights. The trade integration dummies $EU_{ji,t}$ and $FTA_{ji,t}$ are based on the dating of the membership of the EU or a free trade agreement used in Bun and Klaassen (2003).

Variables used in the panel estimation

p	=	log($PGDP$)
y	=	log(Y)
py	=	log(PY)
i	=	IRS
rer	=	log(RER)
g	=	log(G)
nt^{CA}	=	log(NT^{CA})
x_{ji}	=	log(X_{ji})
rer_{ji}	=	log(RER_{ji})

Country and data samples:

The "fiscal block" is estimated for 14 EU countries: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Portugal, Spain, Sweden and the United Kingdom. The estimation sample is 1965-2004. The only exceptions are Belgium (1970), Denmark(1971), Ireland (1977), the Netherlands (1969) and Portugal (1977). The "trade block" is estimated over the same period and accounts for all the bilateral trade relationships between the 14 countries above, leading to 182 country pairs. The trade variables for Belgium also include trade flows of Luxembourg.

APPENDIX B. Model specification for the PVAR

The PVAR specification in its structural form is:

$$A_0 Z_{it} = A(L) Z_{it-1} + e_{it},$$

where Z_{it} is the $(m \times 1)$ vector of endogenous variables and A_0 is an $(m \times m)$ matrix with 1's on the diagonal. It contains the structural parameters that capture the contemporaneous relations among the endogenous variables. Further, e_{it} is the vector with the structural shocks. For the baseline model, $Z_{it} = \left[g_{it}, nt_{it}^{CA}, y_{it} \right]'$ and $e_{it} = \left[e_{it}^{g}, e_{it}^{nt,CA}, e_{it}^{y} \right]'$. By pre-multiplying the above equation by A_0^{-1}, we obtain its reduced form:

$$Z_{it} = B(L)Z_{it-1} + u_{it},$$

where $B(L) = A_0^{-1}A(L)$ and $u_{it} = A_0^{-1}e_{it}$ is the reduced-form residual vector. We can write out $A_0 u_{it} = e_{it}$ as:

$$\begin{pmatrix} 1 & -\alpha_{gt} & -\alpha_{gy} \\ -\alpha_{tg} & 1 & -\alpha_{ty} \\ -\alpha_{yg} & -\alpha_{yt} & 1 \end{pmatrix} \begin{bmatrix} u_{it}^{g} \\ u_{it}^{nt,CA} \\ u_{it}^{y} \end{bmatrix} = \begin{bmatrix} e_{it}^{g} \\ e_{it}^{nt,CA} \\ e_{it}^{y} \end{bmatrix},$$

where u_{it}^{g}, $u_{it}^{nt,CA}$ and u_{it}^{y} are the reduced-form residuals. The three identifying restrictions thus amount to imposing $\alpha_{gt} = \alpha_{gy} = \alpha_{ty} = 0$.

APPENDIX C. Combination of the responses for the two blocks

We take the extended fiscal block, which provides the most general case. For this block, we can write the impulse response functions for domestic output and the real effective exchange rate as:

$$y_{it} = \Psi(L)e_{it}^{f}, \quad reer_{it} = \Phi(L)e_{it}^{f},$$

where $\Psi(L)$ and $\Phi(L)$ are lag polynomials and e_{it}^{f} is the discretionary fiscal shock ($e_{it}^{f} = e_{it}^{g}$ or $e_{it}^{f} = e_{it}^{nt,CA}$). The coefficients of $\Psi(L)$ and $\Phi(L)$ are functions of the parameters from the fiscal block. They show how a fiscal shock affects the two variables over time.

The trade block provides the link between domestic output and bilateral exports of the foreign country, where the response function is the distributed-lag function:

$$x_{ji,t} = D(L)y_{it} + C(L)rer_{jit},$$

where the coefficients in the lag polynomials $D(L)$ and $C(L)$ are functions of the parameters of the trade model.

Combining the previous expressions and assuming that $\partial rer_{jit} / \partial reer_{it} = -1$,[21] we calculate the effects of the discretionary fiscal shock in country *i* on bilateral exports from country *j* to country *i* as:

$$x_{ji,t} = \left[D(L)\Psi(L) - C(L)\Phi(L) \right] e_{it}^{f}.$$

A change in exports translates into a one-for-one direct change in country *j*'s GDP, so that the log increase in country *j*'s GDP is computed as:

$$y_{jt} = \frac{X_{jit}}{X_{jt}} \frac{X_{jt}}{Y_{jt}} \left[D(L)\Psi(L) - C(L)\Phi(L) \right] e_{it}^{f}.$$

where X_{jit} / X_{jt} is the exports to *i* as a share of *j*'s total exports and X_{jt} / Y_{jt} is *j*'s exports as a share of its total GDP. Of course, when the exchange rate channel is closed, as in our baseline case, then $C(L)\Phi(L) = 0$.

APPENDIX D. A budget squeeze

We want to compute the percent government spending reduction or net tax revenue increase that reduces the government deficit by 1% of GDP. We abstract from changes in interest payments on the public debt. These are likely to be of an order of magnitude smaller than the changes in spending or net taxes.[22] The percentage-point change in the government deficit to GDP ratio is then given by the percentage-point change in $(G_t - NT)_t / Y_t$, which is in turn approximated by

[21] This is the case when nominal exchange rate movements against third countries resulting from a domestic (policy) shock are roughly equal in percentage terms and domestic shocks have no or identical effects on third country inflation rates. In addition, the numbers we report are based on the estimated "average" response in the panel of exports to the bilateral real exchange rate, which may be close to the response of exports to a country to its real effective exchange rate.

[22] A one-percent of GDP deficit reduction leads to a one-percent of GDP debt reduction at the end of the period in which the fiscal contraction was implemented. On average during the period, the debt-to-GDP ratio would be roughly lower by half a percent of GDP, an amount that needs to be multiplied by the annual interest rate to calculate its effect on the spending reduction or net tax increase.

$$\left(G_t / Y_t\right)\left[\widehat{G}_t - \widehat{Y}_t\right] - \left(NT_t^{NCA} / Y_t\right)\left[\widehat{NT}_t^{NCA} - \widehat{Y}_t\right] =$$
$$\left(G_t / Y_t\right)\left[\widehat{G}_t - \widehat{Y}_t\right] - \left(NT_t^{NCA} / Y_t\right)\left[\left(\widehat{NT}_t^{CA} + \xi \widehat{Y}_t\right) - \widehat{Y}_t\right]$$

where a hat denotes the percent deviation from the initial value. The move from the first to the second line makes use of the relation between cyclically and non-cyclically adjusted variables presented in Appendix A. With a discretionary government spending shock, $\widehat{G}_t$ is equal to the shock and $\widehat{Y}_t$ and $\widehat{NT}_t^{CA}$ are the impulse responses that we computed earlier. With a discretionary net tax shock, $\widehat{NT}_t^{CA}$ is equal to the shock and $\widehat{Y}_t$ and $\widehat{G}_t$ are the impulse responses that we computed earlier. To compute the instrument settings in Subsection 6.1 for a deficit reduction of 1% of GDP, we equate the second line of the above expression to –1, substitute into this line for G_t / Y_t and NT_t^{CA} / Y_t the averages of the ratios of government spending and cyclically-adjusted net taxes to GDP over all observations (0.241 and 0.217, respectively), and for ξ the average elasticity of net taxes to deviations of income from trend (2.10).[23] We also substitute a one-percent of GDP change in the policy instrument as well as the corresponding impulse responses in the other two variables, all expressed in percent of the variables themselves and all multiplied by the same scaling factor. We can then solve for the scaling factor and, thereby, for the instrument change and the changes in the other variables.

[23] Preferably, when computing the effects of a German fiscal contraction, we would use the (latest) values of the corresponding German variables. However, the impulse responses that we feed into the computation are based on the estimates of the fiscal block on all observations and, for consistency, we thus stick with the numbers chosen here. This inevitably leads to inaccuracies, but the outcomes are primarily intended to give a sense of their order of magnitude.

TABLES

Table 1: Estimates of the contemporaneous coefficients of the fiscal block

	1	*2*	*3*
	Baseline	Baseline with private output	Baseline over 1985-2004
α_{tg}	-0.152 (0.100)	-0.157 (0.099)	-0.122 (0.132)
α_{yg}	0.253*** (0.027)	0.037 (0.036)	0.252*** (0.032)
α_{yt}	-0.072*** (0.012)	-0.095*** (0.015)	-0.052*** (0.015)
Sample period	1965-2004	1965-2004	1985-2004
Panel size	14	14	14
Observations	493	493	278

Notes: the table shows the coefficients estimates (and their standard errors in parentheses) of the contemporaneous effects between the variables. Here, α_{tg} is the effect of government spending on net taxes, α_{yg} is the effect of government spending on output, while α_{yt} is the effect of net taxes on output. Finally, superscripts " * ", " ** " and " *** " indicate statistical significance at the 10%, 5% and 1% level, respectively.

Table 2: Estimates of the bilateral export panel model

	1	***2***	***3***	***4***	***5***	***6***
	Baseline	Adding spending shocks	Adding foreign output	EU5 (five largest countries)	EU9 (nine smallest countries)	EU5-core
$x_{ji,t-1}$	0.610*** (0.033)	0.598*** (0.035)	0.566*** (0.035)	0.729*** (0.067)	0.584*** (0.052)	0.746*** (0.046)
$x_{ji,t-2}$	0.065** (0.030)	0.070** (0.032)	0.102*** (0.033)	-0.0052 (0.055)	0.065 (0.047)	-0.0011 (0.040)
$rer_{ji,t}$	0.502*** (0.036)	0.510*** (0.038)	0.486*** (0.035)	0.359*** (0.054)	0.600*** (0.081)	0.435*** (0.056)
$rer_{ji,t-1}$	-0.187** (0.052)	-0.174*** (0.053)	-0.134*** (0.050)	-0.127 (0.081)	-0.404*** (0.107)	-0.195** 0.077
$rer_{ji,t-2}$	-0.087** (0.036)	-0.099*** (0.037)	-0.153*** (0.035)	-0.083 (0.054)	0.059 (0.078)	-0.130** (0.054)
$y_{i,t}$	1.747*** (0.137)	1.687*** (0.146)	1.723*** (0.131)	1.980*** (0.269)	1.644*** (0.252)	1.419*** (0.172)
$y_{i,t-1}$	-1.300*** (0.219)	-1.167*** (0.233)	-1.191*** (0.206)	-1.876*** (0.414)	-1.221*** (0.416)	-1.815*** (0.243)
$y_{i,t-2}$	-0.077 (0.142)	-0.155 (0.154)	-0.219* (0.132)	0.147 (0.256)	0.107 (0.283)	0.589*** (0.160)
e^g_{it}		0.089 (0.095)				
$e^g_{i,t-1}$		-0.113 (0.099)				
$e^g_{i,t-2}$		0.102 (0.086)				
$y_{j,t}$			0.097 (0.142)			
$y_{j,t-1}$			-0.171 (0.220)			
$y_{j,t-2}$			0.064 (0.131)			
EU	0.064*** (0.0082)	0.063*** (0.0087)	0.063*** (0.0084)	0.108*** (0.023)	0.060*** (0.019)	
FTA	0.064*** (0.011)	0.069*** (0.011)	0.051*** (0.011)	0.0042 (0.021)	0.071*** (0.023)	
Panel size	182	182	182	20	72	20
Observations	6409	6045	6012	760	2424	724

Notes: the table shows the estimates of the coefficients (and their respective standard errors in parentheses) in alternative specifications of equation (2). (*), (**) and (***) indicate statistical significance at the 10%, 5% and 1% level, respectively. Each model is estimated with time fixed effects, country-pair fixed effects and country-pair time trends. Observe that column 6 does not present estimates of the EU and FTA dummies. These are not identified, because the EU5-core are all EU (or European Community) members and had free trade among themselves over the whole sample period.

Table 3: Bilateral export panel model, with period-dependent coefficients

	Sub-period	
	1965-84	**1985-2004**
$x_{ji,t-1}$	0.593*** (0.048)	0.620*** (0.037)
$x_{ji,t-2}$	0.073* (0.044)	0.048 (0.037)
$rer_{ji,t}$	0.559*** (0.053)	0.472*** (0.040)
$rer_{ji,t-1}$	-0.258*** (0.081)	-0.117** (0.057)
$rer_{ji,t-2}$	-0.061 (0.054)	-0.112*** (0.039)
$y_{i,t}$	2.127*** (0.216)	1.428*** (0.141)
$y_{i,t-1}$	-1.553*** (0.327)	-0.937*** (0.224)
$y_{i,t-2}$	-0.204 (0.236)	-0.124 (0.141)
EU	0.111*** (0.013)	0.049*** (0.0081)
FTA	0.044*** (0.012)	
Panel size	182	
Observations	6409	

Notes: the table shows the estimates of the coefficients (and their respective standard errors in parentheses) in alternative specifications of equation (2). (*), (**) and (***) indicate statistical significance at the 10%, 5% and 1% level, respectively. Each model is estimated with time fixed effects, country-pair fixed effects and country-pair time trends. These are held constant over the two sub-periods. We thus conduct a single estimation for the full sample period, interacting some of the coefficients with specific sub-periods.

Table 4: Responses of foreign exports to domestic fiscal shocks – cumulative and normalised

Panel A: Spending increase (1% of GDP)

		Cumulative			***Normalised***		
		Impact Effect	After 2 years	After 5 years	Impact Effect	After 2 years	After 5 years
Baseline	Point estimate	1.94	6.82	10.66	1.94	2.21	2.07
	Lower (5%)	1.49	5.20	7.26	1.49	1.64	1.35
	Upper (95%)	2.42	8.47	14.38	2.42	2.76	2.83
Extended VAR – *rer* channel shut	Point estimate	2.16	7.41	11.67	2.16	2.36	2.17
Extended VAR – *rer* channel open	Point estimate	2.00	7.07	11.76	2.00	2.25	2.19
1985-2004	Point estimate	1.54	5.96	10.31	1.55	1.84	1.83
Large EU-5	Point estimate	2.65	6.08	6.58	2.66	2.06	1.39
Small EU-9	Point estimate	1.63	6.26	11.10	1.63	2.09	2.22
EU5-core	Point estimate	1.97	3.65	4.60	1.98	1.23	0.98

Panel B: Net tax cut (1% of GDP)

		Cumulative			***Normalised***		
		Impact Effect	After 2 years	After 5 years	Impact Effect	After 2 years	After 5 years
Baseline	Point estimate	0.59	1.92	2.30	0.59	0.81	0.79
	Lower (5%)	0.40	1.18	0.61	0.40	0.49	0.21
	Upper (95%)	0.80	2.73	4.14	0.80	1.16	1.42
Extended VAR – *rer* channel shut	Point estimate	0.47	1.47	1.61	0.47	0.63	0.57
Extended VAR – *rer* channel open	Point estimate	0.31	0.72	0.36	0.31	0.31	0.13
1985-2004	Point estimate	0.33	1.52	2.18	0.33	0.59	0.68
Large EU-5	Point estimate	0.59	2.56	3.29	0.59	1.04	0.95
Small EU-9	Point estimate	0.51	1.55	2.30	0.51	0.70	0.95
EU5-core	Point estimate	0.42	1.46	1.86	0.42	0.65	0.61

Notes: Numbers are in percent of annual trade. They are based on 1000 Monte Carlo simulations. "Point estimate" is the mean value of the simulations, "Lower" and "Upper" are respectively the 5th and the 95th percentiles. The fiscal shock that takes place at time 0 equals 1% of GDP. The "cumulative" effect is the sum of the percentage deviations of exports from their original value. Specifically, after *t* years, it is given as $100 * \sum_{\tau=0}^{t} \left(X_\tau - \overline{X} \right) / \overline{X}$, where $\overline{X}$ is the original value of the exports. The "normalised" effect takes into account that the fiscal variable, after the initial shock, continues to deviate from its original value. Specifically, taking the case of a spending increase as an example, the normalised effect after *t* years is given by $\left[\sum_{\tau=0}^{t} \left(X_\tau - \overline{X} \right) / \overline{X} \right] / \left[\sum_{\tau=0}^{t} \left(G_\tau - \overline{G} \right) / \overline{Y} \right]$, where $\overline{G}$ and $\overline{Y}$ are the original values of government spending and GDP. Hence, this number can be interpreted as the average over the period 0 to *t* of additional exports per percent of output increase in public spending. For the computation we use average spending and net tax ratios of GDP over the whole sample. "Baseline" combines the baselines of the panel VAR and the panel trade model. The other cases are variants described in the text.

Table 5: "Normalised" foreign output multipliers of fiscal shocks in Germany and France

Panel A: Spending increase (1% of GDP)

	Origin of spending increase:					
	Germany			***France***		
	Impact Effect	After 2 years	After 5 years	Impact Effect	After 2 years	After 5 years
Austria	0.353	0.401	0.377	0.038	0.043	0.040
Belgium-Lux	0.394	0.448	0.420	0.265	0.301	0.283
Denmark	0.196	0.223	0.209	0.041	0.047	0.044
Finland	0.113	0.129	0.121	0.027	0.031	0.029
France	0.094	0.107	0.100	-	-	-
Germany	-	-	-	0.067	0.076	0.072
Greece	0.050	0.057	0.054	0.013	0.015	0.014
Ireland	0.191	0.217	0.204	0.108	0.123	0.116
Italy	0.085	0.096	0.090	0.058	0.065	0.061
Netherlands	0.374	0.425	0.399	0.118	0.135	0.126
Portugal	0.126	0.144	0.135	0.081	0.092	0.086
Spain	0.081	0.092	0.087	0.100	0.114	0.107
Sweden	0.103	0.118	0.110	0.039	0.044	0.041
UK	0.063	0.072	0.067	0.044	0.050	0.047
W-Average	0.128	0.145	0.137	0.071	0.081	0.076

Panel B: Net tax cut (1% of GDP)

	Origin of net tax cut:					
	Germany			***France***		
	Impact Effect	After 2 years	After 5 years	Impact Effect	After 2 years	After 5 years
Austria	0.100	0.137	0.133	0.013	0.018	0.017
Belgium-Lux	0.112	0.153	0.149	0.090	0.123	0.120
Denmark	0.056	0.076	0.074	0.014	0.019	0.018
Finland	0.032	0.044	0.043	0.009	0.013	0.012
France	0.027	0.037	0.036	-	-	-
Germany	-	-	-	0.023	0.031	0.030
Greece	0.014	0.020	0.019	0.005	0.006	0.006
Ireland	0.054	0.074	0.072	0.037	0.050	0.049
Italy	0.024	0.033	0.032	0.019	0.027	0.026
Netherlands	0.106	0.146	0.141	0.040	0.055	0.053
Portugal	0.036	0.049	0.048	0.027	0.037	0.036
Spain	0.023	0.032	0.031	0.034	0.046	0.045
Sweden	0.029	0.040	0.039	0.013	0.018	0.017
UK	0.018	0.025	0.024	0.015	0.020	0.020
W-Average	0.036	0.050	0.048	0.024	0.033	0.032

Notes: Numbers are in percent of annual GDP. Panel A (Panel B) shows the normalised multipliers of foreign output as a result of an initial government spending increase (net tax cut) equal to 1% of GDP. In our computations we use the 2003 bilateral exports over GDP ratio for each country and the 2004 cyclically adjusted net tax and government spending over GDP ratios for each country. These are the latest-available ratios. Further "W-Average" stands for "weighted average", with weights based on GDP. For details on the computations, see Notes to Table 4.

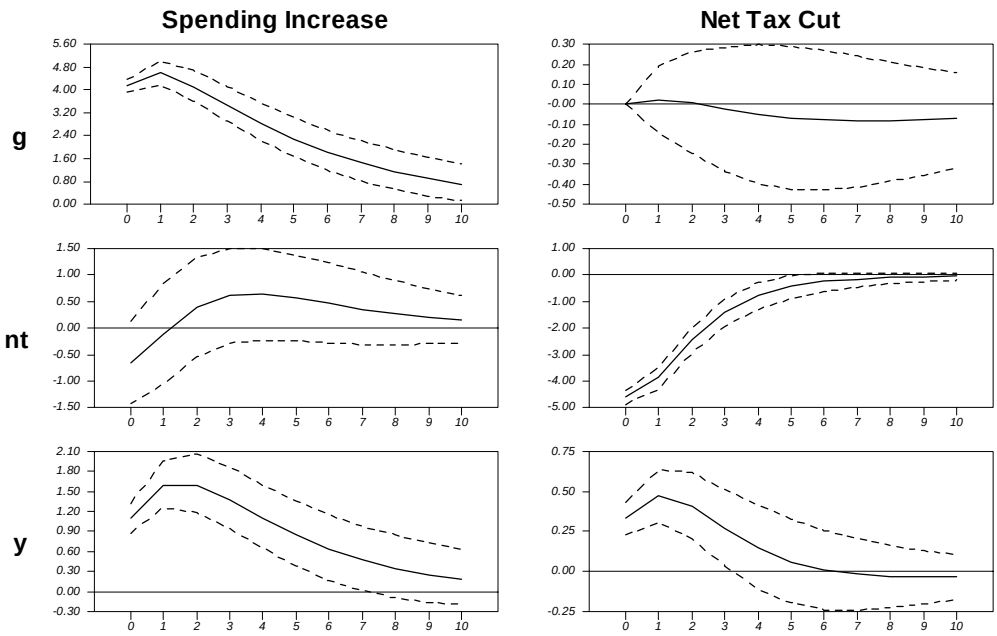
Table 6: Instrument settings when accounting for or neglecting spill-overs

	Scenario I				Scenario II			
Instr./Spill.	e_F^g	e_G^g	dy_F	dy_G	e_F^g	e_G^g	dy_F	dy_G
G/yes	3.51	3.53	1	1	4.07	-4.05	1	1
G/no	3.79	3.79	1.08	1.07	3.79	-3.79	0.93	-0.94
NT^{CA}/yes	-12.87	-12.93	1	1	-14.91	14.84	1	1
NT^{CA}/no	-13.89	-13.89	1.08	1.07	-13.89	13.89	0.93	-0.94

Notes: “Instr.” stands for “instrument” and “Spill.” stands for “spill-over”. When “Spill.” is “no”, then this means that the spill-over is ignored, while when it is “yes”, the spill-over is taken into account. “Scenario I” refers to both France and Germany implementing a fiscal expansion aimed at expanding national GDP by 1%. In “Scenario II” France aims at expanding GDP by 1%, whereas Germany aims at contracting GDP by 1%.

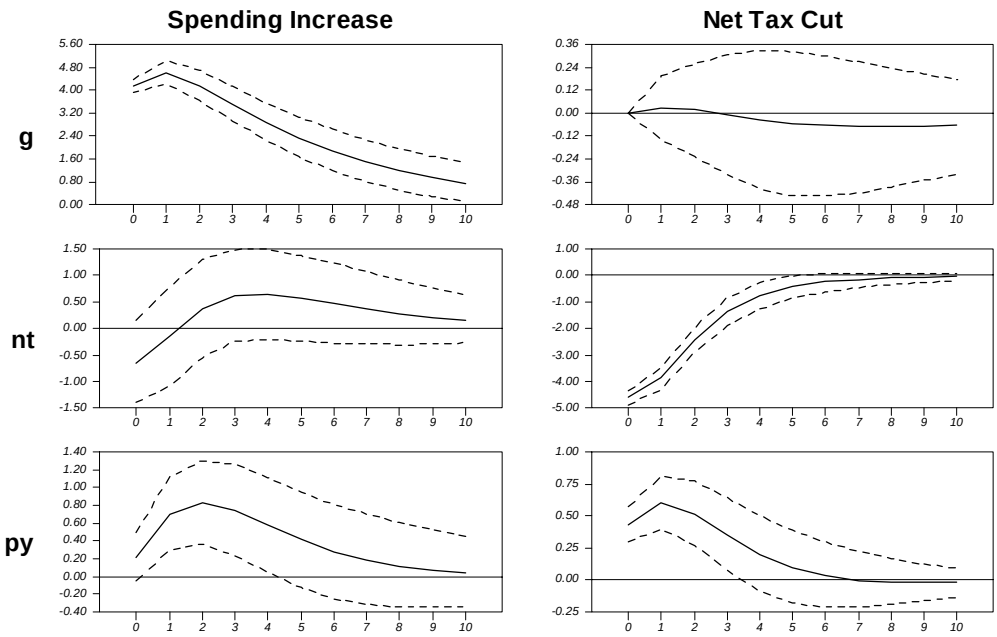
FIGURES

Figure 1: Impulse responses for the fiscal block (baseline panel VAR)



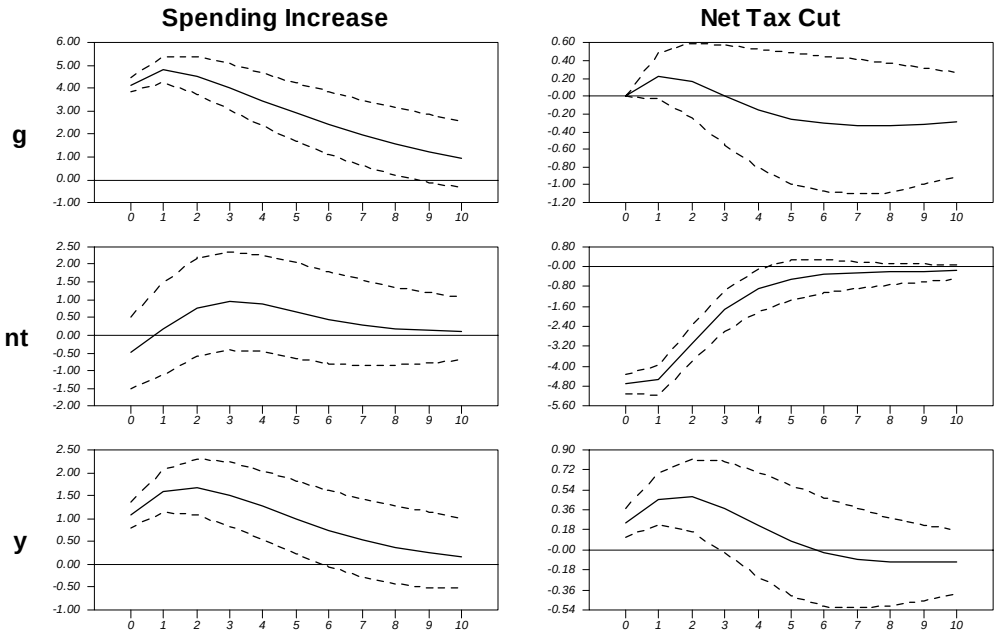
Notes: Confidence bands are the 5th and the 95th percentiles from Monte Carlo simulations based on 1,000 replications.

Figure 2: Impulse responses for the fiscal block (panel VAR with private output)



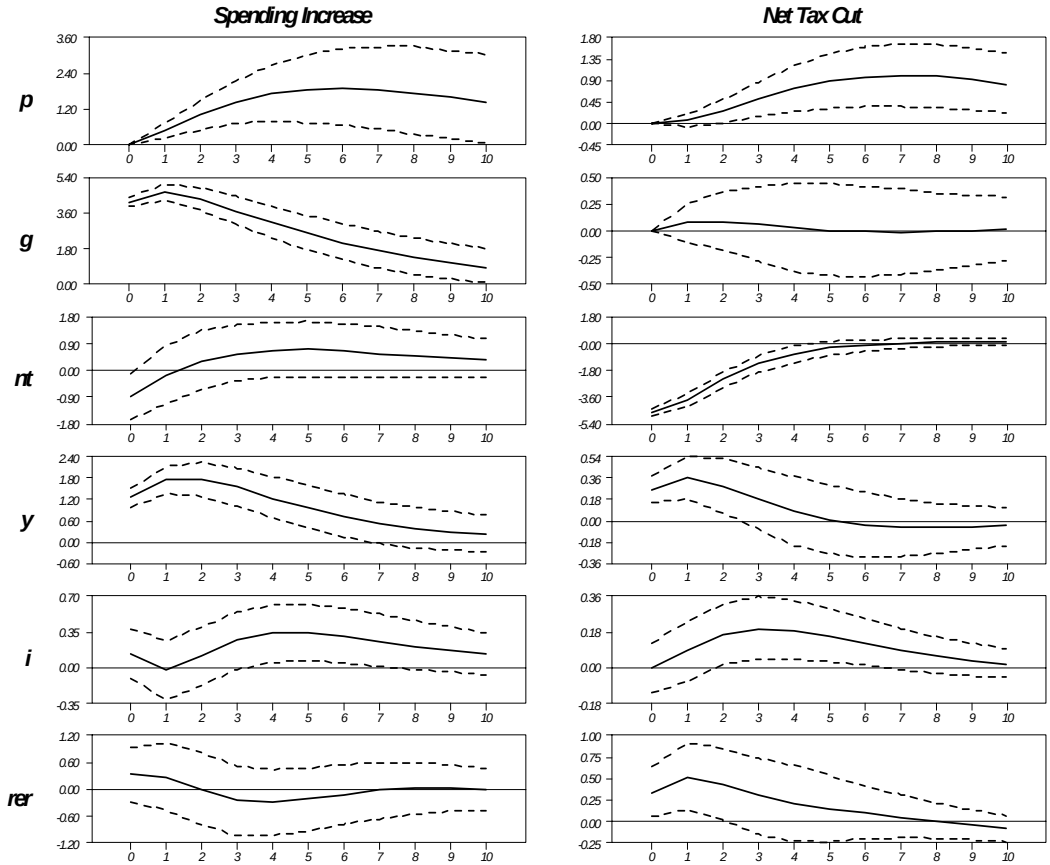
Notes: See Notes to Figure 1.

Figure 3: Impulse responses for the fiscal block (sample is 1985-2004)



Notes: See Notes to Figure 1.

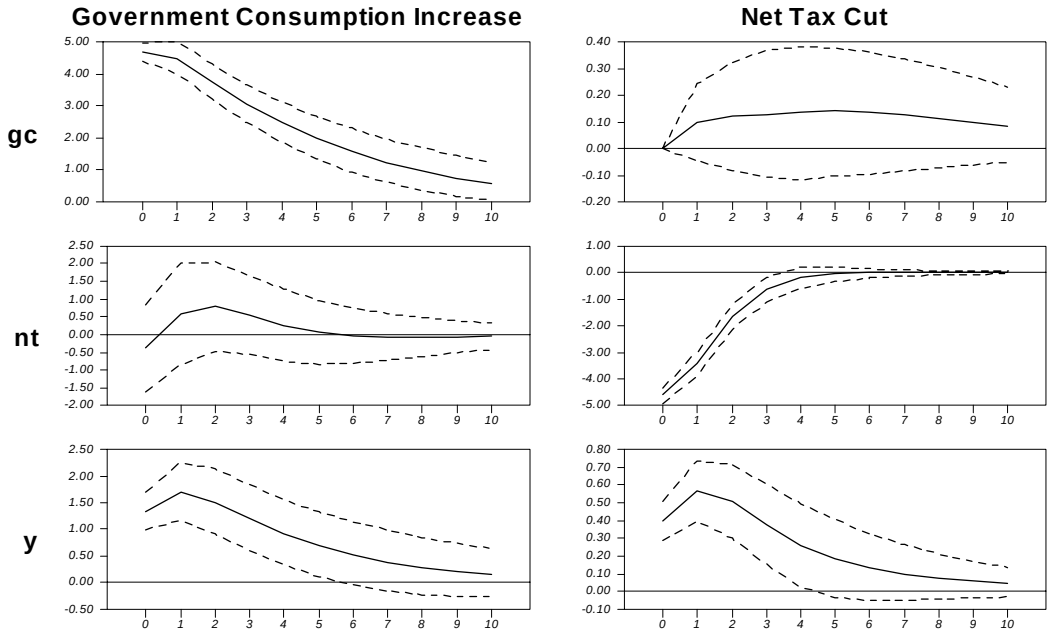
Figure 4: Impulse responses for the fiscal block (extended panel VAR)



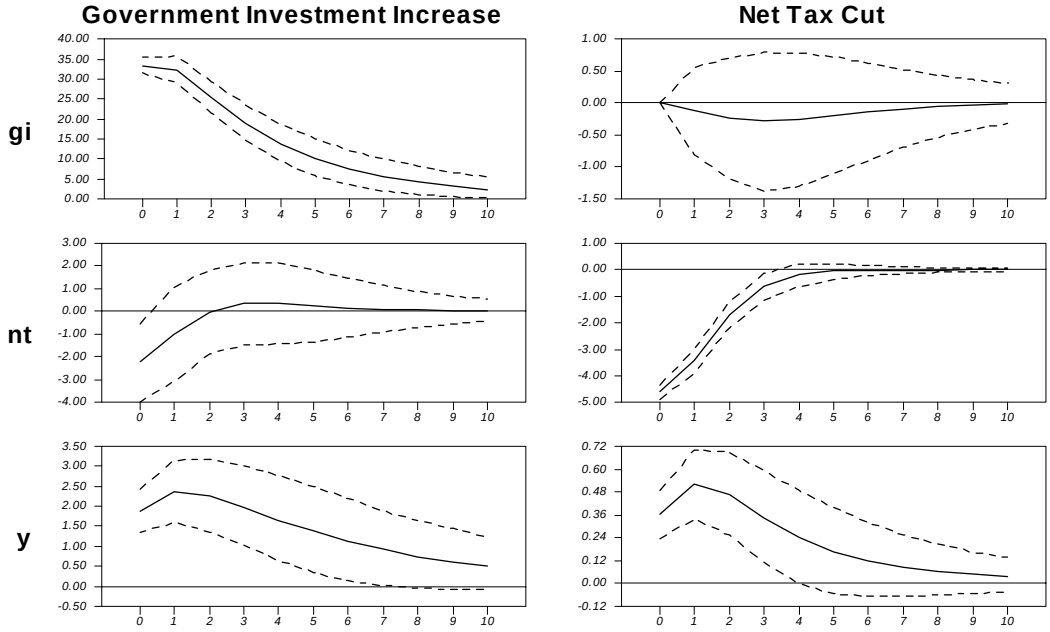
Notes: in addition to the definitions already included in the main text, we have *p* = log of price level, *i* = short-run interest rate and *rer* = log of real multilateral exchange rate. In the figure, a rise in *rer* corresponds to a real *de*preciation. Further, see Notes to Figure 1.

Figure 5: Decomposition of government spending

(a) Impulse responses for fiscal block (with government consumption)



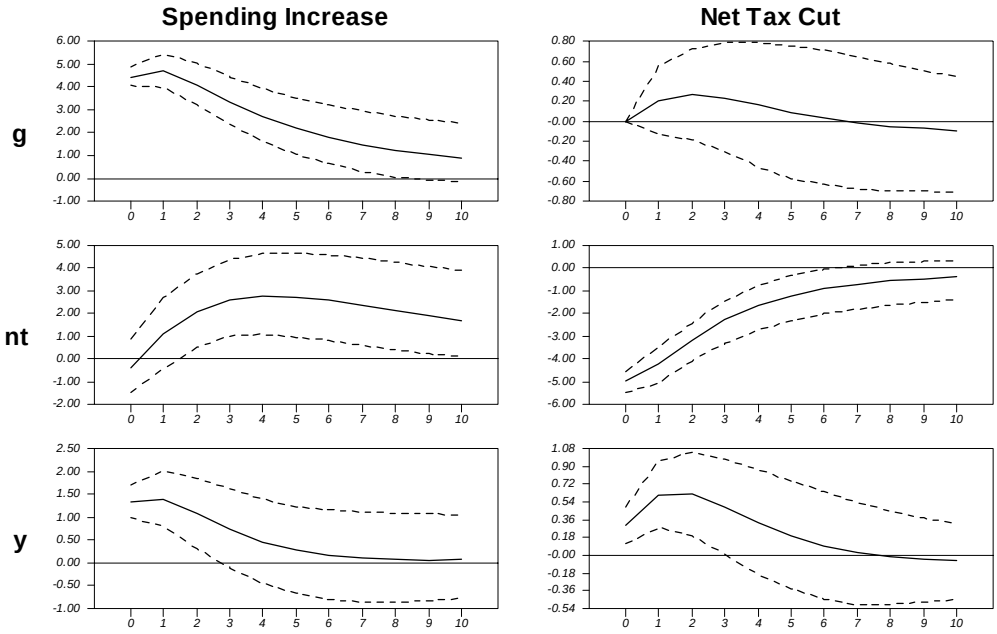
(b) Impulse responses for fiscal block (with government investment)



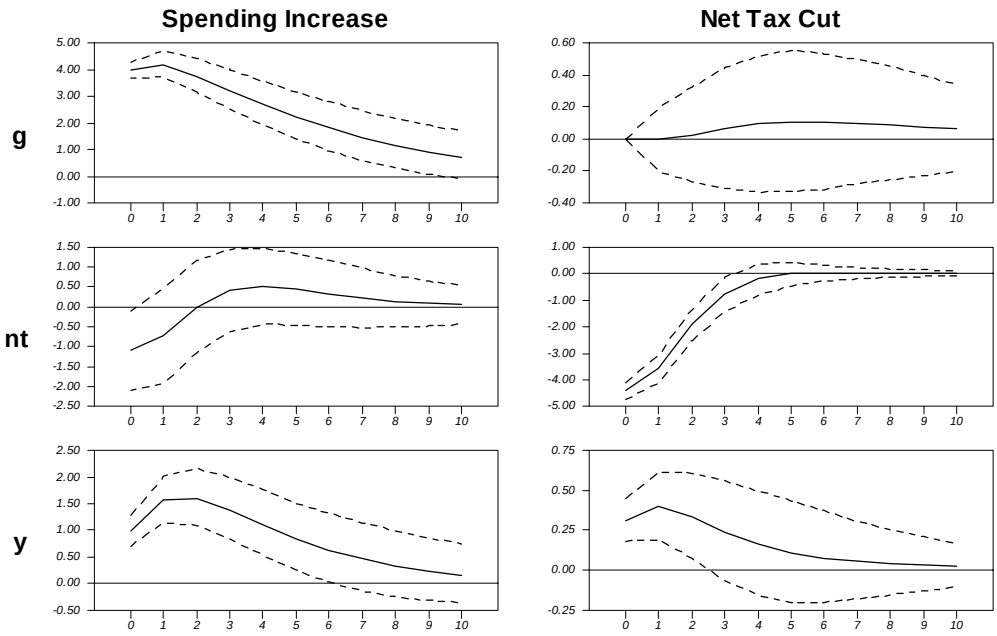
Notes: see Notes to Figure 1.

Figure 6: Split into large and small countries

(a) Impulse responses for group of large countries

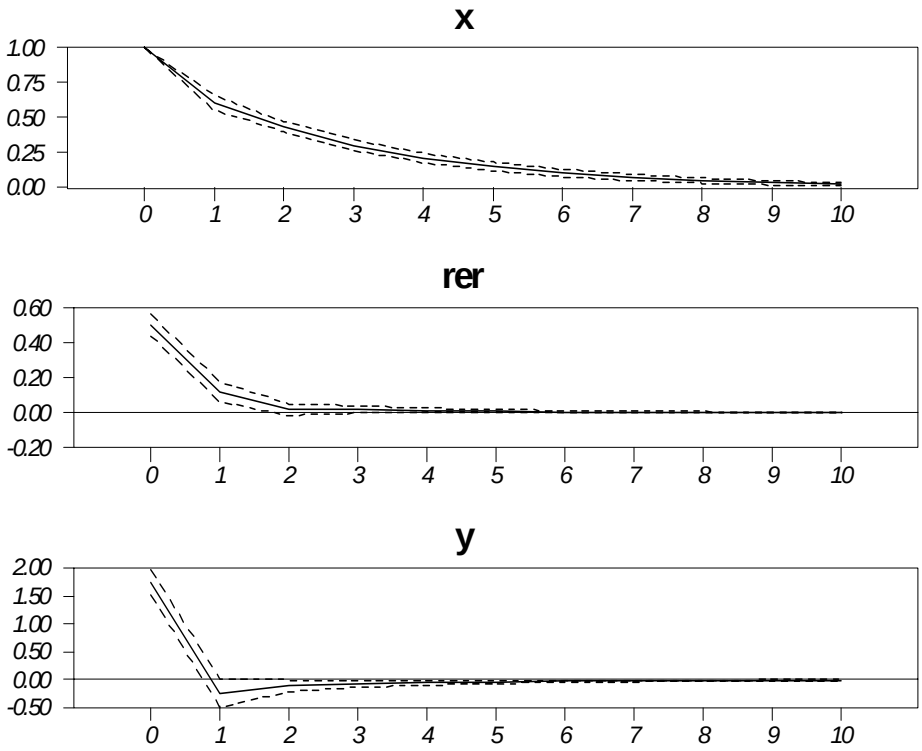


(b) Impulse responses for group of small countries



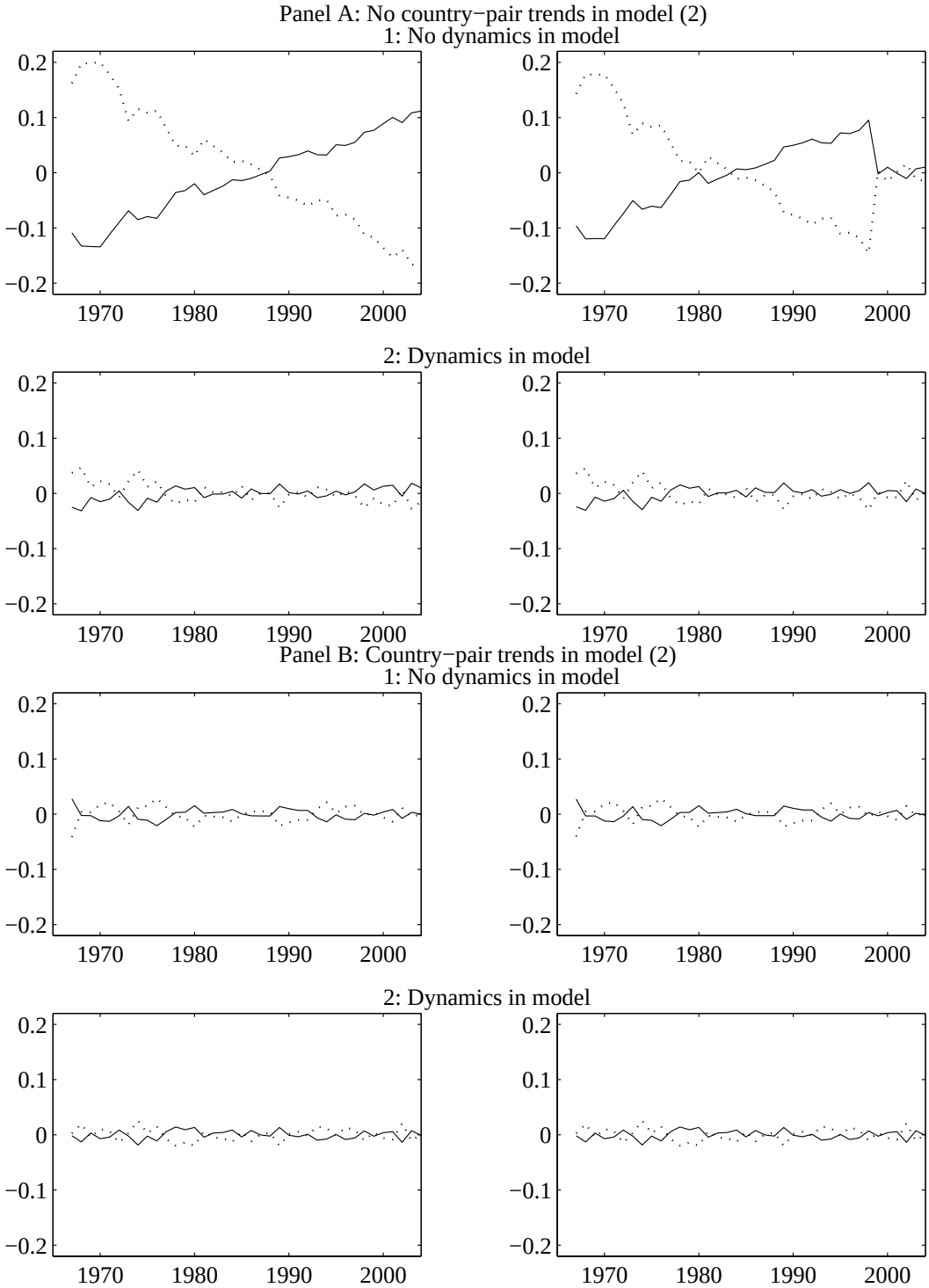
Notes: The group of large countries consists of Germany, France, UK, Italy and Spain. The remaining countries in the sample together form the group of small countries. Further, see Notes to Figure 1.

Figure 7: Impulse responses of bilateral foreign exports (baseline model of trade block)



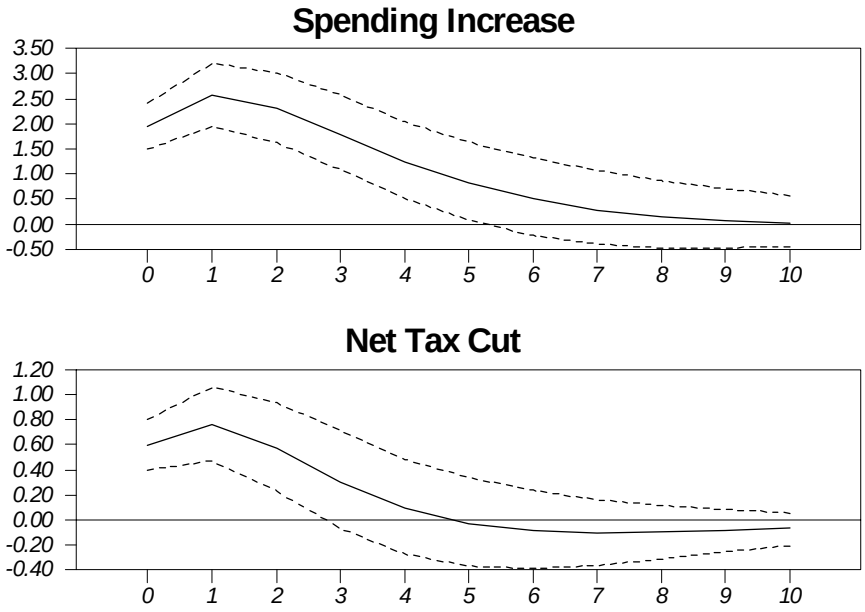
Notes: this figure shows the impulse responses of bilateral exports from the foreign country to the domestic country, after, respectively, a positive shock (of size 1) to exports (*x*), a depreciation of the foreign real exchange rate (*rer*) and a shock to domestic GDP (*y*). Further, see Notes to Figure 1.

Figure 8: Residuals from trade block averaged over Euro country pairs versus other pairs



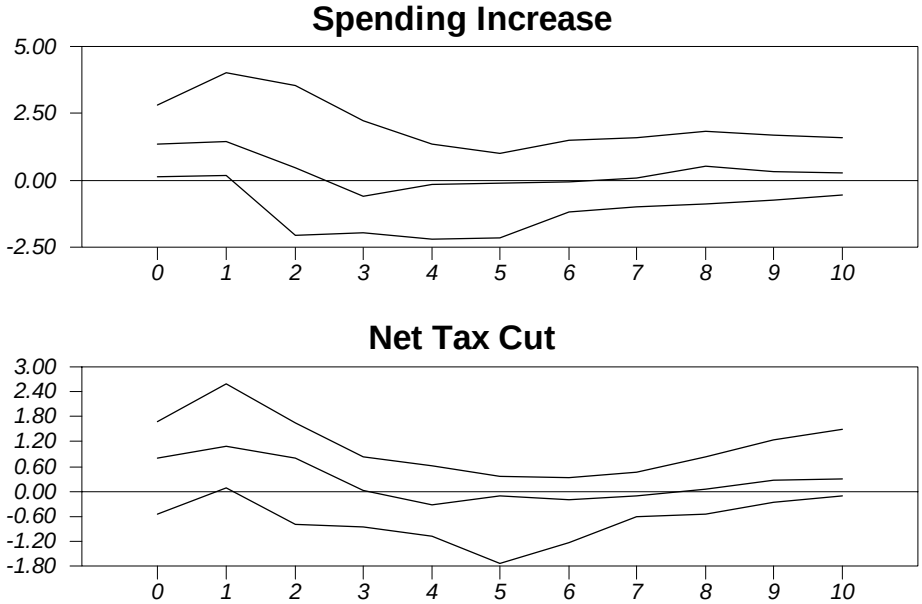
Notes: for each year, the solid line gives the average of the residuals over the 110 Euro-country pairs (i.e., the pairs of countries that are now part of the Euro-area), while the dotted line is the average residual over all remaining country-pairs. The left figures are based on (2), that is, without a Euro dummy. The right figures include a Euro dummy as regressor.

Figure 9: Impulse response of bilateral foreign exports to a domestic fiscal shock after combining the fiscal and trade blocks



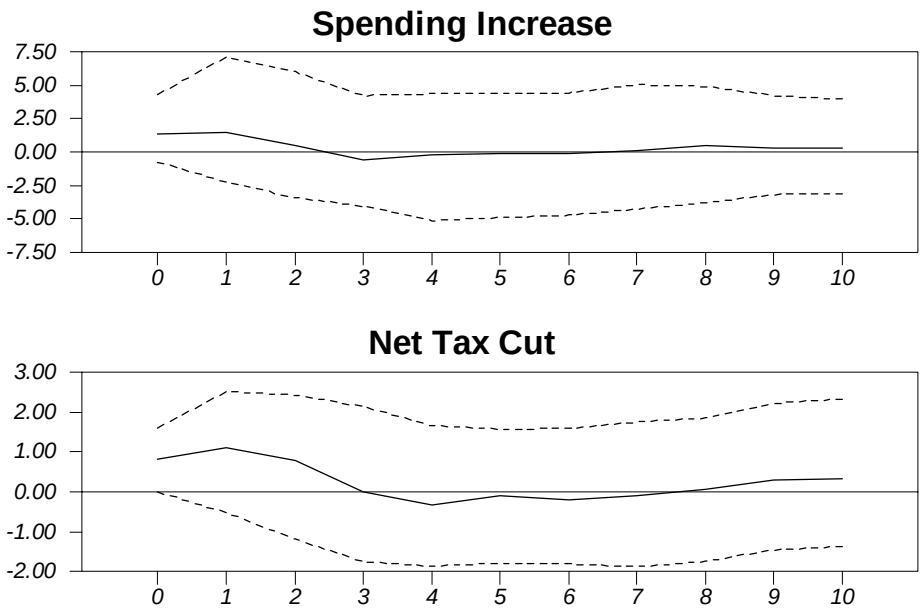
Notes: In each case the size of the fiscal shock equals one per cent of GDP. The estimates and the 90% confidence bands are based on Monte Carlo simulations, where we multiply 1000 draws for the impulse-response function from the fiscal model by 1000 draws for the distributed-lag function from the trade model.

Figure 10: Impulse response of bilateral foreign exports to a domestic fiscal shock after combining the fiscal and trade blocks estimated for individual countries – boundaries of ten responses closest to median



Notes: In each case the size of the fiscal shock equals one per cent of GDP. The figure is based on the combined responses for each individual country and takes the 10 responses closest to the median (the solid line, which is the average of the 7th and 8th response). The lower dashed line thus corresponds to the 3rd response, while the upper dashed line corresponds to the 12th response.

Figure 11: Impulse response of bilateral foreign exports to a domestic fiscal shock after combining the fiscal and trade blocks estimated for individual countries – medians of confidence intervals and central line



Notes: In each case the size of the fiscal shock equals one per cent of GDP. The figure is based on the responses for each individual country and shows the median over the lower- and upper-bounds of the confidence intervals for the 14 countries, as well as the median over the individual mean responses.