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Beetsma, R.; Giuliadori, M.; Klaassen, F.

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THE EFFECTS OF PUBLIC SPENDING SHOCKS ON TRADE BALANCES AND BUDGET DEFICITS IN THE EUROPEAN UNION

Roel Beetsma
University of Amsterdam

Franck Klaassen
University of Amsterdam

Massimo Giuliadori
University of Amsterdam

Abstract

We investigate the consequences of an increase in public spending for trade balances and budget deficits in the European Union, using a panel VAR approach. Whereas the literature tends to treat the trade balance/GDP ratio as a single variable, we include exports and imports as separate variables. This allows us to track in more detail the sources of trade balance movements. Further, we use annual rather than quarterly data. This facilitates the interpretation of the shocks and reduces potential anticipation effects of fiscal policy changes. However, the identification assumptions become stronger, and we extensively check their validity. According to our baseline estimate, a 1% GDP increase in public spending produces a 1.2% on impact, with a 1.6% peak rise in GDP. Rising imports and falling exports are responsible for a fall of the trade balance by 0.5% of GDP on impact and a peak fall of 0.8%. In addition, the spending increase produces a 0.7% impact (and peak) budget deficit, thereby pointing to the potential relevance of the twin deficits hypothesis for the European Union. (JEL: E62, H60)

1. Introduction

Recently a number of papers have started to investigate the consequences of fiscal policy shocks for international trade. In this context, researchers (e.g., Kim and Roubini 2003; Corsetti and Müller 2006) have paid specific attention to the “twin deficit hypothesis,” under which a public spending or tax shock produces a positive co-movement of the deficits on the public budget and the trade balance.

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E-mail addresses: Beetsma: R.M.W.J.Beetsma@uva.nl; Giuliadori: M.Giuliadori@uva.nl; Klaassen: F.J.G.M.Klaassen@uva.nl

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In this paper we follow up on this line of empirical work and extend it into a variety of directions. First, although the literature mostly focuses on the U.S., we investigate this hypothesis specifically for the European Union (EU) countries. Secondly, in contrast to the usual approach, which includes the trade balance share of GDP as a single variable in a VAR, we split it into its components (exports, imports, and GDP) and include each of these as elements in a VAR. This provides us with information on the sources of trade balance movements. Third, we use annual rather than quarterly observations. Generally, there is no quarterly calendar for fiscal policy revisions. Hence, by using annual data the interpretation of shocks may be facilitated. Also, potential anticipation effects of fiscal policy changes play a smaller role with annual data. However, the identifying assumption that public spending does not within the observation period react to output movements becomes stronger. Nevertheless, examining this assumption in a number of ways suggests that it is justified. As a final extension, we use a panel approach to increase estimation accuracy.

For our baseline estimation, we find that a 1% GDP public spending impulse produces a 1.2% output rise on impact and a 1.6% peak response of output. Rising imports and falling exports together produce an impact fall of the trade balance of 0.5% of GDP and a peak fall of 0.8% of GDP. The public budget moves into a deficit of 0.7% of GDP on impact. Together, these results provide support for the twin deficits hypothesis. A large number of robustness tests leave these results essentially unchanged.

The sequel of this paper is structured as follows. Section 2 provides the baseline estimates. Section 3 discusses robustness. Finally, Section 4 concludes the paper.

2. The Baseline Empirical Model

We focus only on public spending shocks rather than tax shocks, because the effects of the former have been investigated in more detail both in theory and in empirical work. Further, for several reasons we use trade balance rather than current account data: The latter tend to be of poorer quality (owing to its net factor payments component) and its time series tend to be shorter. Moreover, a split into its components is not directly possible.

Our baseline is a vector auto-regression (VAR) specification based on the vector of endogenous variables $[g, nt, x, y, m, reer]'$, where g is public spending (i.e., government consumption plus government investment), nt is cyclically adjusted net taxes (with country-specific cyclical adjustment—see Van den Noord 2000 and OECD 2005) defined as taxes minus transfers, x is exports, y is output, m is imports (all real and in natural logarithms with y and nt deflated by the GDP deflator and g , x , and m deflated by their own deflators) and

reer is the natural log of the real effective exchange rate (based on relative CPIs; an increase in *reer* means a real domestic depreciation).¹ Our identifying scheme is based on a lower-triangular Cholesky decomposition with the indicated ordering. Hence, each variable in the vector $[g, nt, x, y, m, reer]'$ is allowed to react contemporaneously to all variables above it, but not to any of the variables below it. In contrast to much of the related literature (e.g., Corsetti and Müller 2006; Monacelli and Perotti 2006) we include the components *x* and *m* of the trade balance share of GDP (*tby*) as separate variables in the model. This helps us in tracing the sources of trade balance movements. However, the impulse responses of the other variables, as well as the constructed response of *tby* (see subsequent discussion), will be hardly affected by this choice.

Fn 1

Our sample consists of 14 EU countries (Austria, Belgium, Denmark, Finland, France, Ireland, Italy, Germany, Greece, the Netherlands, Portugal, Spain, Sweden, and the U.K.) over the period 1970–2004. Our data are from the OECD Economic Outlook and Main Economic Indicators. Importantly, we use annual data instead of quarterly data, as is common in much of the literature. An advantage of using annual data is that shocks identified with annual data may be closer to the actual shocks, as (substantial) fiscal revisions do not usually take place at the quarterly frequency. Moreover, potential anticipation effects of policy changes play a smaller role, as the identified shocks are more likely to be truly unanticipated (as is also forcefully argued by Ramey 2006). Finally, potential seasonal effects are absent from the data and there is less need to be concerned with the details of the institutional setting.

The main drawback of annual data is that our identifying assumptions become stronger. In particular, our identification scheme excludes within-year responses of *g* to *y* and *nt*. This assumption is motivated by the fact that spending plans are usually determined before the new fiscal year starts. In Section 3, we put our identifying assumptions to further scrutiny. Another potential drawback is that we have fewer observations, although for large parts of our sample non-interpolated quarterly fiscal data are unavailable in any case. To increase the precision of our estimates, we estimate the VAR model in panel format, including all countries in the sample. To deal with possible heterogeneity, we include country-fixed effects and country-specific time trends. To reduce cross-country contemporaneous residual correlation we also include time-fixed effects. Still, we need to impose some homogeneity restrictions. However, the selected country sample should be conducive in this regard, as our EU-14 countries share many similarities. Extensive testing in Beetsma, Giuliodori, and Klaassen (2006) suggests that our homogeneity assumptions are not unreasonable. Subsequently, we shall also

1. We include two lags of each variable in the VAR, which is enough to eliminate any autocorrelation in the residuals. In fact, the results are insensitive to the precise lag length.

FIGURE 1. Baseline impulse responses.

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

explore a variant in which we split the country sample into relatively closed and relatively open economies.

Figure 1
Fn 2

Figure 1 shows the impulse responses for an increase in public spending by 1% of GDP.² On impact, GDP increases by 1.2%, and it reaches a peak response of 1.6% after one year, after which it dies out only slowly. These effects are substantial, but (in contrast to most related literature and apart from other differences such as data frequency) they are found for a sample that is dominated by limited exchange rate flexibility against the main trading partners. Somewhat surprisingly, cyclically adjusted net taxes fall upon impact. Here, we can only speculate about the source of this finding. Rather than being caused by some fundamental economic mechanism, it could, for example, also be the result of an overestimation of the elasticities of taxes with respect to output. In fact, unadjusted net taxes (not reported here) increase on impact. Exports react by becoming negative. This would be consistent with the increase in public spending mainly falling on government wage consumption, thereby driving up economy-wide wages and prices (see Lane and Perotti 1998). This effect would also

2. In the following discussion, statistical “significance” will always be based on the 10% confidence level.

occur for other types of public spending if the economy is capacity-constrained. Imports rise substantially, which is consistent with the increase in income. Finally, the real exchange rate appreciates, though with some delay. A possible channel is that higher spending leads to wage rises that push up domestic product prices.³

Fn 3

Figure 1 also shows constructed impulse responses for the primary budget (as a share of GDP) and *tby*. The former is constructed as the percentage-point change in $(NT_t^{NA} - G_t)/Y_t$, which in turn is computed as $(NT_t^{NA}/Y)[(\hat{NT}_t + \xi \hat{Y}_t) - \hat{Y}_t] - (G_t/Y)[\hat{G}_t - \hat{Y}_t]$ (see Beetsma, Giuliodori, and Klaassen 2006). Here, NT_t^{NA} , G , and Y are real government purchases, unadjusted net taxes, and GDP in levels, and a hat denotes the percent deviation from the initial value (the impulse response). Finally, ξ is the elasticity of net taxes with respect to real output. The approximation is evaluated at the overall sample mean shares of G and NT_t^{NA} over Y . The impulse response for *tby* is constructed as $(X/Y)(\hat{X}_t - \hat{Y}_t) - (M/Y)(\hat{M}_t - \hat{Y}_t)$ and evaluated at the overall sample mean share of X and M over Y .

The responses of exports and imports reduce the trade balance, which falls by 0.5% of GDP on impact and peaks at -0.8% of GDP after two years. The public budget deteriorates by 0.7% on impact. In contrast to the budget balance, which features a U-shaped time pattern, the trade balance returns monotonically (and faster) to its steady state. Overall, our findings are consistent with the twin deficit hypothesis in response to a public spending shock.⁴

Fn 4

It may be useful to compare our findings to some of those in the literature. We can compare our “domestic effects” with Perotti (2007). He estimates “closed-economy” VAR models in government spending, (net) taxes, GDP, and other variables for the U.S. Australia, Canada, and the U.K. using an identification scheme similar to ours. Output tends to react positively to a public spending shock, although only for the U.S. the size of the response becomes comparable to what we find for the EU. The main differences with our approach are that he uses quarterly, instead of annual, observations and that his country sample has only limited overlap with our sample. Monacelli and Perotti (2006) estimate individual-country VARs and Ravn, Schmitte-Grohé, and Uribe (2007) estimate panel VARs similar to ours on Perotti’s (2007) country sample. There is a tendency

3. In an attempt to track the sources of the real exchange rate response, we also replaced *reer* with the nominal effective exchange rate, including in addition the log of the GDP deflator (ordered first, as it is expected to be sticky). A shock to the system would move the real exchange rate by moving the domestic price level and/or the nominal exchange rate. The price increase gets close to significance, whereas the nominal effective exchange rate does not change, suggesting that the effect of the fiscal shock on domestic inflation is driving the real exchange rate.

4. We have also estimated the model with the price deflators of exports, imports, and GDP included as additional endogenous variables (all positioned before public spending). The constructed impulse response for *tby* was modified to take account of the relative changes in the three deflators. Quantitatively, it remained virtually unchanged.

for GDP to increase immediately or with a lag, for the trade balance to deteriorate during the first couple of years and for the real exchange rate to depreciate. Again, these analyses differ from ours along the same dimensions as Perotti's analysis.

3. Robustness Tests

3.1. Identifying Restrictions

In this section we explore the robustness of our results. Our main assumption is that public spending within the year does not react to unexpected output shocks. The assumption is in line with Beetsma, Giuliodori, and Klaassen (2006), where we estimate a panel VAR model in $[g, y]$ for seven EU countries for which we have non-interpolated quarterly public spending data, assuming that g does not react to y within the quarter. From the quarterly data estimates we construct an estimate of the response of public spending to output, α_{gy} , at annual data frequency and find that it is not significantly different from zero.

However, to further explore the sensitivity of our results to the assumption $\alpha_{gy} = 0$, we relax it by restricting α_{gy} to different values than zero. Given that public spending in principle does not automatically react to the business cycle, a positive (negative) value of α_{gy} indicates a pro-cyclical (counter-cyclical) discretionary spending response to unexpected output shocks. Panels A1 and A2 of Table 1 report the responses of output, imports, exports, the trade balance, and the budget surplus on impact and at a number of lags, when we vary α_{gy} over the range from -0.25 to 0.25 .⁵ The impulse responses are qualitatively similar to those under the baseline, although there are some quantitative differences. When varying α_{gy} from negative to positive, the effects on output and, hence, on imports and the trade balance as a share of GDP become weaker. Even so, the peak estimate of the spending to output multiplier still exceeds unity under the pro-cyclical response.

We also explore an indirect way of assessing the assumption that public spending does not react to output within the year. First, we subtract public spending from output to obtain what we refer to as "private output." In our baseline regression we replace output with private output. Hence, for this regression public spending precedes private output in our identification ordering. Private output reacts significantly to a public spending shock with a maximum response of 0.69 after one year. We also estimate a variant in which we place private output just

Table 1

Fn 5

5. This range comfortably covers the 90% confidence band on α_{gy} that Beetsma, Giuliodori, and Klaassen (2006) obtain from their quarterly panel VAR and the aggregation to annual-level parameter estimates.

TABLE 1. Responses to a public spending increase (1% of GDP).

	Impact Effect	After 1 Year	After 3 Years	After 5 Years	Impact Effect	After 1 Year	After 3 Years	After 5 Years
Case	A1: Contemporaneous Response of g to y Set at -0.25				A2: Contemporaneous Response of g to y Set at 0.25			
Exports	-0.27	-0.77*	-0.74	-0.35	-0.71*	-1.05*	-0.62	-0.19
Output	1.57*	2.00*	1.37*	0.65*	0.80*	1.13*	0.81*	0.39
Imports	1.55*	2.09*	1.63*	0.89*	0.64	1.07*	1.21*	0.87*
Surplus/GDP	-0.65*	-0.38*	-0.18	-0.24*	-0.80*	-0.57*	-0.28*	-0.21*
Trade balance/GDP	-0.60*	-0.94*	-0.78*	-0.41*	-0.44*	-0.70*	-0.60*	-0.35*
Case	B1: Only Goods Trade (with all countries)				B2: Only Goods Trade (with EU countries only)			
Exports	0.22	-0.56	-0.58	-0.47	-0.16	-0.61	-0.47	-0.25
Output	1.20*	1.57*	1.21*	0.68*	1.19*	1.56*	1.24*	0.74*
Imports	1.40*	1.96*	1.51*	0.64	2.32*	3.02*	2.77*	1.78*
Surplus/GDP	-0.72*	-0.46*	-0.18	-0.19*	-0.73*	-0.45*	-0.14	-0.12
Trade balance/GDP	-0.32*	-0.69*	-0.57*	-0.30*	-0.46*	-0.67*	-0.60*	-0.37*
Case	C1: "Closed" Economies				C2: "Open" Economies			
Exports	-0.58	-1.02	-0.51	-0.33	-0.83*	-0.86	-0.97	-0.95
Output	1.43*	1.75*	1.15*	0.53	0.83*	0.94*	0.55	0.01
Imports	1.03*	1.00	1.52*	1.10*	0.41	1.28*	0.42	-0.66
Surplus/GDP	-0.58*	-0.00	-0.04	-0.11	-0.85*	-1.12*	-0.61*	-0.30
Trade balance/GDP	-0.38*	-0.48*	-0.49*	-0.34*	-0.55*	-0.95*	-0.62*	-0.13

Note: *Statistically significant at the 10% level.

before public spending,⁶ keeping the ordering in the VAR otherwise as before. **Fn 6** The estimated responses to the public spending shock are close to each other for the two orderings and confidence bands practically overlap, suggesting that our identifying assumption is justified.

Next, we re-estimated the model ordering nt before g , but leaving the specification unaltered otherwise. The results are roughly unchanged. We also made the current real effective exchange rate exogenous by ordering it first in the panel VAR system, but leaving the ordering the same otherwise. We thus allow for a within-year reaction of public spending to this variable. The results, and in particular the effects on the trade balance share of GDP, are basically unchanged. Another variant includes the real effective exchange rate (up to two lags) as an exogenous variable. Again, our results are essentially unchanged. These findings suggest that the main source of movement of the trade balance is an increase in output following the increase in public spending.

6. We cannot reasonably do this regression with total output ordered before spending, because the latter almost surely affects the former within the year, as the national income identity suggests.

3.2. *International Trade in Goods Only*

The trade data used so far include both trade in goods and services. However, data on services trade would generally be considered less reliable than data on goods trade. Here, we re-estimate our baseline model replacing the “total” imports and exports figures with those for goods only. The bulk of trade is in goods. Exports and imports of goods are on average in our sample roughly 28% of GDP, whereas exports and imports of services are roughly 5% on average. Because we have bilateral data on goods trade (from the IMF Direction of Trade Statistics), focusing on goods only also allows for more flexibility as we can vary the sample of countries with which our EU countries trade.

Repeating our baseline regression replacing total trade with only goods trade (of our sample countries with all other countries in the world) produces impulse responses (not shown here) similar to those in Figure 1. The only difference is that exports no longer decrease significantly—see panel B1 of Table 1. Although differences in the source and quality of the trade data could be part of the explanation, there may also be more fundamental reasons for this finding. For example, the wage component of public spending could be biased towards labor that is more closely substitutable with the labor employed in the services sector than that in the goods sector (thus creating a relatively large international competitive disadvantage in the former). Panel B2 of Table 1 reports impulse responses for the case in which goods trade is only with other countries in the EU. Interestingly, in spite of the output responses being virtually identical in the two cases, the estimated imports response is much stronger when goods trade is only with the EU countries, indicating that the sensitivity to income of imports from other EU countries is higher than that of imports from countries outside the EU.⁷

Fn 7

3.3. *Sample Split: Open and Closed Economies*

As Corsetti and Müller (2006) argue, the effect of a public spending increase on the trade balance may depend on the degree of trade openness of a country. To explore the relevance of openness for our sample, and also as a check for an obvious potential source of heterogeneity, we split our sample into groups of “open” and “closed” economies and re-estimate the baseline model for each of the two groups. Open (closed) economies are those for which the ratio of exports plus imports over GDP has on average over time been in the upper (lower) half of the sample. The open economies are Austria, Belgium, Denmark, Ireland,

7. Even so, the response of *tby* does not increase much in magnitude, because it is expressed in percentage points of GDP and we are now aggregating exports and imports over fewer trading partners.

The Netherlands, Portugal and Sweden. Table 1, panels C1 and C2, reports the responses of our variables of interest for the two groups of countries. The closed economies exhibit a stronger output response to the spending shock (on impact 1.43 versus 0.83 for the open group), which is consistent with the hypothesis that for this group less of the fiscal stimulus leaks abroad. In fact, for the open economies, the spending multiplier never exceeds unity. A comparison of the responses of imports and exports is complicated by the difference in the output responses. Dividing the impact effects on imports by that on output, we obtain “normalized” responses. Somewhat surprisingly, the normalized import response of the closed economies exceeds that of the open economies (0.72 versus 0.49). The normalized export responses, however, are (in absolute value) much smaller for the closed than for the open economies (-0.41 versus -0.99). Indeed, the deterioration of the trade balance over the first three years is larger for the open than for the closed economies. The budget deficit is smaller for the closed economies on impact and vanishes within a year, whereas for the open economies it rises even further after one year and it takes a couple of years to vanish.

3.4. Further Robustness Testing

We explore the robustness of our baseline results further for a variant of our model without country-specific time trends and one with linear-quadratic country-specific time trends. The results remain largely the same. Only the real exchange rate switches from insignificance in the absence of trends to a significant fall after a year under linear-quadratic trends.

We also explore the robustness of our findings for changes in the sample period. To retain a sufficiently long sample in the time dimension, we have only limited flexibility in this regard. In one variant, we leave out the EMU period, implying the sample period 1970–1998. The other variant leaves out the 1970s and, hence, the turbulent period of the major oil shocks. The sample then covers the period 1980–2004. Both variants leave the results qualitatively unchanged, although the size of the output and imports responses is somewhat larger (smaller) than the baseline under the 1970–1998 (1980–2004) sample.

As another variation on our baseline we include the short-term ex post real interest rate (positioned after public spending in the VAR). The impulse responses of the baseline variables are unaffected, although there is some widening of the error bands. The interest rate response rises to the border of significance. Including the long interest rate, the latter becomes significantly positive after 3 years, whereas the other responses remain unchanged.

Adding the first two lags of the government debt—GDP ratio as exogenous variables to the regression, as suggested by Favero and Giavazzi (2007), has no

consequences either, although the real exchange rate after two years shows a rather strong appreciation. Our finding that the results are otherwise unchanged is potentially due to the country-specific trends picking up the effects of movements in the debt–GDP ratios.

4. Concluding Remarks

In this paper, we have explored the effects of public spending shocks for trade balances and budget balances in the EU. To this end, we employed an annual panel VAR with exports and imports as separate variables, and constructed the responses of trade and budget balances from their composing variables. We could thus track in detail the sources of movements in the trade balance. Our results suggest a substantial output multiplier of public spending. They are also consistent with the twin deficits hypothesis. A split of our sample into relatively closed and open economies shows that a public spending shock has a larger effect on output of the former group, while for the latter group the trade balance exhibits a stronger deterioration.

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