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Cross-country heterogeneity and the trade-income relationship

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Abstract

This paper makes two contributions to the literature on the impact of trade on income. First, we use heterogeneous panel cointegration techniques that are robust to omitted variables and endogenous regressors to estimate the effect of trade on income for 81 developed and developing countries, both for the sample as a whole and for each individual country. Second, we use a general-to-specific variable selection approach to identify important determinants of the effect of trade on income. Our main findings are: (i) A one percent increase in the trade share of GDP yields, on average, a statistically significant increase in income per worker of about 0.16 percent. This result is in contrast to previous studies, which tend to produce either unreasonably large or statistically insignificant estimates of the impact of trade on income. (ii) There are large cross-country differences in the income effects of trade, in particular between developed and developing countries. For developed countries the income effect of trade is positive, whereas trade has, on average, a negative impact on income in developing countries. (iii) The cross-country heterogeneity in the impact of trade on income can be explained mainly by cross-country differences in primary export dependence, labour market regulation, and property rights protection. The level of property rights protection is positively related, while the level of primary export dependence and labour market regulation is negatively related to the income effect of trade.

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1. Introduction

Is the effect of international trade on real income the same for all countries? This may seem a strange question, since we know from theory that whether and to what extent countries gain from trade depends on several country-specific factors, including the degree of factor mobility between sectors, the type of specialization, and the ability of a country to invest in physical or human capital or adopt foreign technology. Thus, the answer to the question is a clear "no"—that is, the effect of trade on income must be heterogeneous across countries. Nevertheless, existing studies on trade and income use cross-country regressions or homogeneous panel data models, which, by definition, are not able to capture the heterogeneity in the relationship between trade and income across countries. Moreover, and perhaps more importantly, the estimates in these studies may be seriously biased in the presence of such heterogeneity. The reason is the following. Cross-country differences in the impact of trade on income are due to several country-specific factors that generally cannot be fully controlled in cross-country regressions, and this gives rise to omitted variable bias. Panel data regressions, on the other hand, allow control for omitted variables. However, traditional homogeneous panel estimators, such as the ones used in the existing trade-income literature, produce inconsistent and potentially misleading estimates of the average values of the parameters in dynamic panel data models when the slope coefficients are heterogeneous (see, e.g., Pesaran and Smith, 1995).

This issue of cross-country heterogeneity in the relationship between trade and income is the subject of the present paper. Specifically, we make the following contributions:

(1) We employ heterogeneous panel cointegration techniques that are robust to omitted variables and endogenous regressors to estimate the effect of trade on income for 81 developed and developing countries both individually and as a whole. To preview the main results: We find that a one percent increase in the trade share of GDP yields, on average, a statistically significant increase in income per worker of about 0.16 percent. This result is in

contrast to previous studies, which tend to produce either unreasonably large or statistically insignificant estimates of the impact of trade on income. Furthermore, our results show that there are large cross-country differences in the income effect of trade, in particular between developed and developing countries. For developed countries the income effect of trade is positive, whereas trade has, on average, a negative impact on income in developing countries.

(2) We adopt a variable-selection approach which is based on a general-to-specific methodology to systematically search for country-specific conditions that are important factors in explaining the cross-country differences in the effect of trade on income. Our main result is that cross-country differences in the income effect of trade can be explained mainly by cross-country differences in primary export dependence, labour market regulation, and property rights protection. To be more precise, the effect of trade on income is positively related to the level of property rights protection, and negatively related to the degree of primary export dependence and the level of labour market regulation.

Finally, as the above remarks imply, a methodological contribution of this paper is the application of a two-step estimation procedure that combines panel and cross-sectional methods: The first step is to estimate the effect of trade on income for each country using heterogeneous panel estimators; the second step involves using cross-sectional regressions with the estimated income effect as the dependent variable to examine the determinants of the income effect of trade.

The plan of the paper is as follows. Section 2 summarises the existing literature on the impact of trade on income. Section 3 re-examines the impact of trade on income. Section 4 analyzes the determinants of the income effect of trade. Section 5 concludes.

2. Review of the empirical literature

In this section, we review the empirical literature on the impact of trade on income, which can be roughly divided into two categories: cross-country studies and panel studies. First, we describe the general empirical approach in cross-country studies. Then, we summarise the main arguments and results of these studies. Finally, we discuss the results of recent panel studies in this literature.

2.1. General approach in cross-country studies

Cross-country studies of the relationship between trade and income generally follow the methodology of Frankel and Romer (1999), who propose the following regression model:

$$\ln(Y_i) = a + \beta T_i + c S_i + \varepsilon_i \,, \tag{1}$$

where $\ln(Y_i)$ is the natural logarithm of income per person or income per worker in country *i*, T_i is the trade share of GDP (measured in logarithms or levels), and S_i is country size, usually proxied by the logarithm of population and the logarithm of area. Country size is included in the regression model for two reasons: first, it serves as a crude proxy for the amount of trade within a country. Accordingly, the estimate of c can be used to assess whether countries also benefit from within-country trade. Second, because larger (smaller) countries tend to have more (less) opportunities for trade within their borders and therefore lower (higher) trade shares, it is necessary to control for country size in estimating the impact of international trade on income. Otherwise, S_i would enter the error term, thereby inducing a negative correlation between ε_i and T_i and thus a downward bias in the estimate of β.

As the literature on the trade-income relationship recognises, equation (1) cannot be estimated by OLS, first, because of the likely endogeneity of trade, and second, because of omitted variables due to unobserved country-specific effects. The endogeneity problem can be illustrated as follows. It is reasonable to assume, for example, that countries with higher income levels have better infrastructure and transportation systems which allow them to trade more. Moreover, high-income countries generally have institutions and resources needed to tax domestic economic activity, and thus do not have to rely on tariffs to finance government spending. In addition, high-income countries tend to demand a greater variety of products that are traded internationally. And finally, high-income countries typically offer more opportunities for firms to acquire the knowledge and resources necessary to enter export markets. Thus, it can be assumed that the volume of trade tends to increase with the level of income. Now imagine a situation in which increased trade leads to increased income, which, in turn, feeds back into increased trade (the problem of reverse causality). In this case, however, the estimated OLS regression coefficient will tend to conflate these two effects and hence will be an inconsistent estimate of the causal effect of trade on income.

A second, closely related, problem is that of unobserved country-specific effects that are correlated with both income and trade. Given that these effects are unobserved, they are omitted from the estimation and thus enter the error term, in turn implying that the assumption of independence of the errors and regressors is violated. To give an example, suppose that countries that eliminate tariff and non-tariff barriers also adopt policies to correct domestic market distortions and to improve institutional quality. Since such factors are likely to affect both trade and income, their omission will cause an upward bias in the OLS estimate of the impact of trade on income.

To overcome these problems, Frankel and Romer (1999) suggest an instrumental variable (IV) approach. A valid instrument is correlated with the endogenous variable, but uncorrelated with the error term and thus not associated with the dependent variable through any channel other than the endogenous variable. To construct such an instrument, Frankel and Romer propose the following two-step procedure. The first step is to estimate a gravity equation for bilateral trade shares using distance between trading partners and country size as explanatory variables (components of trade, which are assumed to be independent of income). The second step involves calculating a predicted aggregate trade share for each country on the basis of the estimated coefficients of the gravity equation. This predicted trade share is then used as a geography-based instrument for trade in regression (1).

2.2. Results of cross-country studies

Using the geographically-constructed trade share, Frankel and Romer (1999) find a large and statistically significant positive effect of trade on income. Specifically, it is estimated that a one-percentage-point increase in the trade share would cause an increase in GDP per worker of 1.97 to 2.96 percent.[1]

Rodríguez and Rodrik (2001), however, question this finding, arguing that Frankel and Romer's trade instrument is invalid. More specifically, they argue that the Frankel and Romer findings simply reflect the impact of geography on income rather than the impact of trade on income, since the geography-based instrument is correlated with other geographic variables that affect income through non-trade channels, such as morbidity, agricultural productivity, and institutions. To support their claim, Rodríguez and Rodrik re-estimate the Frankel-Romer regression adding additional controls for geography (such as distance from the equator, the percentage of a country's land area that lies in the tropics, and regional dummies), and find that the IV coefficient estimates on trade become statistically insignificant once additional geography variables are included. This result is consistent with the results of Irwin and Tervio (2002) and Felbermayr (2005), who also obtain insignificant trade coefficients using geographical controls.

Several other studies also include institutional variables in the IV regression. These are intended to explicitly control for potential income effects of the geography-based trade instrument that might be associated with the effects of geography on income through institutions. Frankel and Rose (2002) as well as Noguer and Siscard (2005), for example, estimate equation (1) with and without additional controls for both geography and institutions. They detect a large and statistically

[1] Frankel and Romer (1999) interpret their results in terms of effects of trade on income per capita. In fact, they use income per worker as the dependent variable (see Frankel and Romer, 1999, Appendix Table A1).

significant effect of trade on income that is robust to the inclusion of additional control variables. A similar result is obtained by Hall and Jones (1999), who find a significant positive coefficient on the Frankel-Romer predicted trade share using regression (1) without country size but with proxies for geography and institutions.

A common feature of these studies is that they construct the trade instrument based on the ratio of imports plus exports in current prices to GDP in current prices. Alcalá and Ciccone (2004), however, argue that this conventional openness measure yields downwardly biased estimates. The reason is as follows. Suppose that trade increases productivity, but that the productivity gains are greater in the tradable than in the nontradable sector (a plausible assumption). This will lead to a rise in the relative price of nontradables, and a decrease in the trade/nominal GDP ratio under the assumption that the demand for nontradables is relatively inelastic, as it may raise the denominator more than the numerator. Consequently, trade-induced productivity gains may go hand in hand with a decline in the trade/nominal GDP ratio. To remedy this problem, Alcalá and Ciccone propose the use of nominal trade divided by GDP at PPP, which they call "real openness" (whose denominator now corrects for international differences in the price of nontradable goods). They find, controlling for geography and institutions, that the causal effect of trade on income is statistically and economically significant when real openness is used, but insignificant (at the five-percent level) when the conventional openness measure is used.

However, this result is in contrast to the results of Dollar and Kraay (2003). They construct the Frankel-Romer trade instrument using PPP-adjusted bilateral trade shares, as in Alcalá and Ciccone (2004), and find that the coefficient on the instrument for real openness turns out to be insignificant after including geographical and institutional proxies. Similarly, Rodrik et al. (2004) control for geography and institutions, and find no significant effects of trade on income, regardless of whether real openness or the conventional openness measure is used.

The coefficient estimates of all these studies are summarized in Table 1. The table shows the lowest and highest estimates for the impact of trade on income obtained; significant coefficients are indicated by bold values. As can be seen, while several coefficients are considerably high and statistically significant, others are insignificant and sometimes negative. In particular, it appears that the studies summarised tend to produce either unreasonably large and statistically significant estimates of the impact of trade on income or insignificant estimates. The former can be explained by unresolved endogeneity and omitted variable problems. In fact, there are so many factors affecting both income and trade through various channels that it is very likely that even the coefficient on the geography-based trade instrument is picking up a correlation with these omitted country-specific variables. A possible explanation for the insignificant coefficients is the correlation between several geographical controls, institutional proxies, and the Frankel-Romer trade instrument. Dollar and Kraay (2003), for example, find that in instrumented regressions of income on trade and institutions, there is a severe multicollinearity problem, which makes it impossible to identify the partial effects of either variable on income.

[Table 1 around here]

2.3. Results of panel studies

Given the problems inherent to cross-country regressions, several studies use panel data techniques. Panel estimation makes it possible to account for unobserved country-specific effects, thus eliminating the omitted-variable bias. Moreover, by including lagged explanatory variables, panel procedures allow control for potential endogeneity problems. Dollar and Kraay (2003, 2004), for example, apply a GMM estimation strategy, which involves (i) rewriting equation (1) as a dynamic panel data model with fixed effects, (ii) removing the fixed effects by first-differencing, and then (iii) instrumenting the differenced right-hand-side variables using lagged values of the original regressors. Specifically, their regression model relates changes in per capita growth to instrumented changes in the explanatory variables, such as trade (measured by real openness) and institutions. They find that the effect of changes in trade volumes on changes in growth is significantly positive and quite robust.

An important difference between the panel study by Dollar and Kraay and the cross-country studies just discussed is the change in model specification from a relationship between trade and income in levels to a relationship between the variables in changes, thereby limiting the comparability of the results. Dollar and Kraay justify this modification by arguing that the correlation between the changes in the explanatory variables is lower than the correlation between their levels, so that potential multicollinearity problems between trade and institutions are minimised. Given, however, the fact that they use lagged levels of trade volumes and institutions as instruments, the potential collinearity problem is hardly solved. Moreover, it is well known that lagged levels are weak instruments for the regression equation in differences if the variables are persistent over time.

To reduce the potential biases associated with the difference estimator, Felbermayr (2005) uses a system GMM estimator that combines the difference regression with the level regression where the instruments are lagged values of the differenced regressors. Consistent with many studies cited above, he finds a large and statistically significant positive effect of trade on income (using both the real openness and the nominal openness measure). According to his estimates, an increase in the trade/GDP ratio by one percentage point would increase per capita income by around 1.5 percent.

Thus, the overall picture that emerges from these studies is that trade tends to have a large positive impact on income. Yet, all these studies are limited by one important factor: they do not capture the potential heterogeneity in the relationship between trade and income across countries. Rather, they implicitly assume that the effect of trade on income is the same for all countries, which is an implausible assumption as there is nothing in the theoretical literature to suggest such homogeneity. Furthermore, recent advances in the heterogeneous dynamic panel literature suggest that estimation and inference in standard dynamic panel models, such as the ones used by Dollar and Kraay (2003, 2004) and Felbermayr (2005), can be misleading when the slope coefficients differ across cross-section units.[2] Similarly, parameter heterogeneity due to omitted variables may seriously bias the results of cross-country regressions. In the following analysis, we will carefully examine this heterogeneity in the trade-income relationship.

3. The impact of trade on income

This section examines the impact of trade on income. Specifically, we use panel data techniques that allow us (i) to control for omitted variable and endogeneity bias and (ii) to detect possible cross-country differences in the income effects of trade. The section proceeds as follows: first, we describe the empirical model and the data used in the empirical analysis. Then, we examine the basic time-series properties of the data. Thereafter, we test for the existence of a long-run relationship between trade and income, and then provide estimates of this relationship. Finally, we test the direction of causality between the two variables.

3.1. Model and data

In order to estimate the effect of international trade on income we consider a bivariate long-run relationship of the form:

$$\ln(Y_{it}) = a_i + \delta_i t + \beta \ln(T_{it}) + \varepsilon_{it}, \tag{2}$$

where Y_{it} represents income per worker over time periods $t = 1, 2, ..., T$ and countries $i = 1, 2, ..., N$, and T_{it} stands for the trade share of GDP over the same time periods and countries. The symbol ln indicates that both variables are log-transformed, as in Alcalá and Ciccone (2004), and the coefficient β denotes the cross-country average of the effects of trade on income, β_i, which are

[2] Pesaran and Smith (1995) show that slope heterogeneity generates a correlation between the regressors and the error

allowed to be country specific and thus to vary across countries. The a_i and $\delta_i t$ are, respectively, country-specific fixed effects and country-specific deterministic time trends, capturing any country-specific omitted factors that are relatively stable over time or evolve smoothly over time. Accordingly, in contrast to the studies reviewed above, we do not need to control for omitted variable bias by including direct proxies for country size, geography, and institutions, since it can be assumed that all these factors are absorbed into the fixed effects and/or country-specific trend terms.

Equation (2) assumes that, in the long-run, permanent changes in the log-level of the trade share are associated with permanent changes in the log-level of income per worker. Empirically, this implies that both the individual time series for income per worker and the individual series for the trade/GDP-ratio must exhibit unit-root behaviour and that $\ln(Y_{it})$ must be cointegrated with $\ln(T_{it})$. Thus, we select from the Heston, Summers, and Aten (2006) Penn World Table a panel of counties for which both real (PPP) GDP per worker and trade relative to GDP at PPP (the real openness measure suggested by Alcalá and Ciccone (2004)) have unit roots. In practice, this means that we eliminate from 97 countries for which data on real GDP per worker and real openness are available over the period 1960 to 2003 those countries for which the individual time series do not pass a simple screening for a unit root via the ADF and the KPSS tests.[3] In addition, we exclude countries whose average populations between 1960 and 2003 were less than one million and whose data receive a grade of "D" (lowest quality) from Heston, Summers, and Aten. Many small economies, where international trade is important, have implausibly high historical levels of income, which is typically due to questionable national accounts deflators, especially for the foreign sector. Hence, we omit small economies if their data are assigned a grade of "D". This sample selection procedure yields a sample of 81 countries.

term as well as a serial correlation in the disturbances, and thus introduces a bias in traditional panel data estimators.

[3] The unit root-tests indicate that both $\ln(T)$ and $\ln(Y)$ are nonstationary for 95 countries. Only for two countries (Algeria and Syria) we find evidence that $\ln(T)$ is stationary.

3.2. Panel unit-root tests

To ensure that the failure to reject the null hypothesis of a unit root is not simply due to the low power inherent in the individual country unit root tests, we compute the panel unit root test developed by Im, Pesaran, and Shin (2003) (IPS). This allows us to test the null hypothesis that all of the individuals of the panel have a unit root against the alternative that some fractions are (trend) stationary. The IPS test is based on the ADF regression:

$$\Delta x_{it} = z_{it}'\gamma + \rho_i x_{it-1} + \sum_{j=1}^{p_i} \varphi_{ij} \Delta x_{it-j} + \varepsilon_{it}, \tag{3}$$

where p_i is the lag order and z_{it} represents deterministic terms, such as fixed effects or fixed effects combined with individual time trends. In model (3), the unit root null hypothesis, $H_0 : \rho_i = 0$, $\forall i = 1, 2, \ldots, N$, is tested against the alternative of (trend) stationary, $H_1 : \rho_i < 0$, $i = 1, 2, \ldots, N_1$; $\rho_i = 0$, $i = N_1 + 1, N_1 + 2, \ldots, N$, using the standardized t-bar statistic:

$$\Gamma_{\bar{t}} = \frac{\sqrt{N}\left[\bar{t}_{NT} - \mu\right]}{\sqrt{v}}, \tag{4}$$

where $\bar{t}_{NT}$ is the average of the N (=81) cross-sectional ADF t-statistics, μ and v are, respectively, the mean and variance of the average of the individual t-statistics, tabulated by Im, Pesaran, and Shin (2003).

However, the standard IPS test can lead to spurious inferences if the errors, ε_{it}, are not independent across i. Therefore, we also employ the cross-sectionally augmented IPS test proposed by Pesaran (2007), which is designed to filter out the cross-section dependency by augmenting the ADF regression with the cross-section averages of lagged levels and first-differences of the individual series. Accordingly, the cross-sectionally augmented ADF (CADF) regression is given by:

$$\Delta x_{it} = z_{it}'\gamma + \rho_i x_{it-1} + \sum_{j=1}^{p_i} \varphi_{ij} \Delta x_{it-j} + \alpha_i \bar{x}_{t-1} + \sum_{j=0}^{p_i} \eta_{ij} \Delta \bar{x}_{t-j} + v_{it}, \tag{5}$$

where $\bar{x}_t$ is the cross-section mean of x_{it}, $\bar{x}_t = N^{-1}\sum_{i=1}^{N} x_{it}$. The cross-sectionally augmented IPS statistic is the simple average of the individual CADF statistics:

$$CIPS = t\text{-bar} = N^{-1}\sum_{i=1}^{N_i} t_i \,, \tag{6}$$

where t_i is the OLS t-ratio of ρ_i in (5). Critical values are tabulated by Pesaran (2007).

The test results for the variables in levels and in first differences are presented in Table 2. As can be seen, both the IPS and the CIPS test statistics are unable to reject the hypothesis that all countries have a unit root in levels. Since the unit root hypothesis can be rejected for the first differences, we conclude that $\ln(Y_{it})$ and $\ln(T_{it})$ are integrated of order one, $I(1)$. Thus, the next step in our analysis is an investigation of the cointegration properties of the variables.

[Table 2 around here]

3.3. Cointegration tests

We first test for cointegration using the Larsson et al. (2001) approach, which is based on Johansen's (1988) full-information maximum likelihood (FIML) estimation technique. Like the Johansen time-series cointegration test, the Larsson et al. panel test treats all variables as potentially endogenous, thus avoiding the normalization problems inherent to residual-based cointegration tests. It involves estimating the Johansen vector error correction model for each country and then computing the individual trace statistics $LR_{iT}\{H(r)|H(p)\}$, which allows us to account for heterogeneous cointegrating vectors across countries. The null hypothesis is that all countries have the same number of cointegrating vectors r_i among the p variables $H_0 : rank(\Pi_i) = r_i \leq r$, and the alternative hypothesis is $H_1 : rank(\Pi_i) = p$, for all $i = 1, \ldots, N$, where Π_i is the long-run matrix of order $p \times p$. To test H_0 against H_1, a panel cointegration rank trace test is constructed by calculating the average of the N individual trace statistics,

$$\overline{LR}_{NT}\{H(r)|H(p)\} = \frac{1}{N}\sum_{i=1}^{N} LR_{iT}\{H(r)|H(p)\}, \tag{7}$$

and then standardizing it as follows:

$$\Psi_{\overline{LR}}\{H(r)|H(p)\} = \frac{\sqrt{N}\left(\overline{LR}_{NT}\{H(r)|H(p)\} - E(Z_k)\right)}{\sqrt{Var(Z_k)}} \Rightarrow N(0,1), \tag{8}$$

where the mean $E(Z_k)$ and variance $Var(Z_k)$ of the asymptotic trace statistic are tabulated by Breitung (2005) for the model we use (the model with a constant and a trend in the cointegrating relationship). As shown by Larsson et al. (2001), the standardized panel trace statistic has an asymptotic standard normal distribution as N and $T \rightarrow \infty$.

In addition we compute the Fisher statistic proposed by Madalla and Wu (1999), which is defined as:

$$\lambda = -2\sum_{i}^{N} \log(p_i), \tag{9}$$

where p_i is the p-value of the trace statistic for country i, calculated from the response surface estimates in MacKinnon et al. (1999). The Fisher statistic is distributed as χ^2 with $2 \times N$ degrees of freedom.

However, these test procedures do not take account of potential error cross-sectional dependence, which could bias the results. To test for cointegration in the presence of possible cross-sectional dependence we follow Holly et al. (forthcoming) and adopt a residual-based two-step approach in the style of Pedroni (1999, 2004). But unlike Pedroni, we use the common correlated effects (CCE) estimation procedure developed by Pesaran (2006) in the first-step regression. This procedure allows for cross-sectional dependencies that potentially arise from multiple unobserved common factors by including the cross-sectional averages of the dependent variable and the observed regressors as proxies for the unobserved factors. Accordingly, the cross-sectionally augmented cointegrating regression we estimate for each country is given by:

$$\ln(Y_{it}) = a_i + \delta_i t + \beta_{it}\ln(T_{it}) + g_{i0}\overline{\ln(T_t)} + g_{i1}\overline{\ln(Y_t)} + e_{it}, \tag{10}$$

where $\overline{\ln(T_t)}$ and $\overline{\ln(Y_t)}$ are the cross-sectional averages of $\ln(T_{it})$ and $\ln(Y_{it})$ in year *t*. In the second step, we compute the cross-sectionally augmented IPS statistic for the residuals from the individual CCE long-run relations, $\hat{\mu}_{it} = \ln(Y_{it}) - \hat{\delta}_i t - \hat{\beta}_{it} \ln(T_{it})$, including an intercept. This allows us to account for unobserved common factors that could be correlated with the observed regressors in both steps. If the presence of a unit root in $\hat{\mu}_{it}$ can be rejected, we can conclude that there is a cointegrating relationship between trade and income.

The results of these tests are presented in Table 3. For completeness, we also report the standard panel and group ADF test statistics suggested by Pedroni (1999, 2004). As can be seen, all tests strongly suggest that $\ln(Y_{it})$ and $\ln(T_{it})$ are cointegrated. The standardised trace statistics and the Fisher χ^2 statistics clearly support the presence of one cointegrating vector. Also, the CIPS, the panel ADF and the group ADF statistics reject the null hypothesis of no cointegration at the one-percent level, implying that there exists a long-run relationship between trade and income.

[Table 3 around here]

3.4. The long-run relationship between trade and income

Having found that trade and income are cointegrated, the next step in our analysis is to determine the magnitude of the long-run impact of international trade on income. To this end, we estimate the coefficient β in equation 2 using the between-dimension, group-mean panel DOLS estimator suggested by Pedroni (2001). Pedroni emphasises several advantages of using between-dimension group-mean-based estimators over the within-dimension approach. For example, it is argued that the between-dimension estimator allows for greater flexibility in the presence of heterogeneous cointegrating vectors, whereas under the within-dimension approach, the cointegrating vectors are constrained to be the same for each country. Clearly, this is an important advantage for applications such as the present one, because there is no reason to assume that the effect of trade on income is the same across countries. Another advantage of the between-dimension estimators is that the point estimates provide a more useful interpretation in the case of heterogeneous cointegrating vectors, since they can be interpreted as the mean value of the cointegrating vectors, which does not apply to the within estimators. And finally, the between-dimension estimators suffer from much lower small-sample size distortions than is the case with the within-dimension estimators.

The DOLS regression in our case is given by:

$$\ln(Y_{it}) = a_i + \delta_i t + \beta_{it}\ln(T_{it}) + \sum_{j=-p_i}^{p_i} \Phi_{ij}\Delta(T_{it-j}) + \varepsilon_{it}\,, \tag{11}$$

where Φ_{ij} are coefficients of lead and lag differences, which account for possible serial correlation and endogeneity of the regressor(s), thus yielding unbiased estimates. Consequently, in contrast to conventional cross-country approaches, the approach we use does not require unrealistic exogeneity assumptions nor does it require the use of notoriously unreliable instruments.

From regression (11), the group-mean DOLS estimator for β is constructed as:

$$\hat{\beta} = \left[N^{-1}\sum_{i=1}^{N}\left(\sum_{t=1}^{T} z_{it}z'_{it}\right)^{-1}\left(\sum_{t=1}^{T} z_{it}\tilde{s}'_{it}\right)\right]_1, \tag{12}$$

where z_{it} is the $2(K+1)\times 1$ vector of regressors $z_{it} = ((\ln(T_{it}) - \overline{\ln(T_i)}, \Delta\ln(T_{it-K}), \ldots, \Delta\ln(T_{it+K}))$, $\tilde{s}_{it} = s_{it} - \bar{s}_i$, and the subscript 1 outside the brackets indicates that only the first element of the vector is taken to obtain the pooled slope coefficient. Because the expression following the summation over the i is identical to the conventional time-series DOLS estimator, the between-dimension estimator for β can be calculated as:

$$\hat{\beta} = N^{-1}\sum_{i=1}^{N}\hat{\beta}_i\,, \tag{13}$$

where $t_{\hat{\beta}} = N^{-1/2}\sum_{i=1}^{N} t_{\hat{\beta}_i}$ is the associated t-statistic and $\hat{\beta}_i$ is the conventional DOLS estimator applied to the ith country of the panel. As found by Stock and Watson (1993), this estimator performs well in short time series compared to other cointegration estimators, such the FIML estimator of Johansen (1988) or the fully modified ordinary least squares (FMOLS) estimator of Phillips and Hansen (1990).

We present the DOLS group-mean point estimate of the impact of international trade on income in the second column of Table 4. As expected, the regression shows a statistically significant relationship between trade and income. The *t*-statistic on $\ln(T_{it})$ is 6.4 and the point estimate implies that an increase in the trade/GDP ratio by one percent increases GDP per worker by 0.165 percent, on average. An important aspect of this result is that the point estimate is much smaller than most cross-country estimates, which tend to yield unreasonably large values for the impact of trade on income (as discussed in Section 2.2). Accordingly, we obtain a more reliable estimate of the impact of trade on income despite the fact that our panel regression does not include direct proxies for geographical and institutional characteristics and despite the endogeneity of trade. This is due to the fact that in our panel model any effects of unobserved or omitted variables are captured by the deterministic fixed effects and heterogeneous time trends, and the fact that the group-mean DOLS estimator is robust to both the presence of endogenous regressors and the presence of heterogeneity in the effects of trade on income across countries.

[Table 4 around here]

Given, however, that the group-mean DOLS estimator is very sensitive to outliers, we re-estimate the DOLS regression, excluding one country at a time from the sample. The estimated group-mean coefficients and their *t*-statistics are presented in Figure 1. Since they are fairly stable around 0.165 and always statistically significant, we conclude that the (average) effect of trade on income is not seriously affected by possible outliers.

Nevertheless, the estimated impact of trade may be biased by the presence of cross-sectional dependencies.[4] To evaluate this issue, the third column of Table 4 reports the result of the common correlated effects mean group estimator (CCEMG) suggested by Pesaran (2006). This estimator is the simple average of the individual common correlated effects (CCE) estimators given by equation (10). As can be seen, the CCEMG estimator and the group-mean DOLS estimator produce similar results, suggesting that cross-sectional dependence is not a serious problem. Admittedly, the CCEMG estimate is somewhat lower than the DOLS estimate. However, the CCEMG estimation procedure implicitly assumes that trade is exogenous and that the cointegration between trade and income is driven by a stochastic trend that is common to all countries of the panel. Since both assumptions are likely to be violated, the CCEMG result has to be interpreted with caution. We therefore continue to rely on the DOLS estimates.

The individual country DOLS point estimates and their *t*-statistics are presented in Table 5. The most striking feature of these estimates is the heterogeneity in the slope coefficients, ranging from -1.0723 (Ecuador) to 2.1883 (Denmark). Accordingly, there are large cross-country differences in the impact of international trade on income that are not captured in standard cross-country and panel regressions. Moreover, while most studies obtain a positive coefficient on trade openness, we find for 32 out of 81 countries that an increase in trade is associated with a decrease in income per worker. Thus, a substantial fraction of countries do not gain from trade. Interestingly, all these countries are developing countries, whereas for developed countries the estimated trade coefficient is unanimously positive. To make the differences between developed and developing countries more obvious, we report the DOLS group-mean estimates for these two country groupings in the bottom row of Table 5. The estimated effect of trade is statistically significant positive for developed and significant negative for developing countries, reflecting the heterogeneity between these groups. But even within the group of developing countries, the individual country estimates show considerable heterogeneity. For example, the point estimates suggest that Uruguay, Chile, and Indonesia benefit significantly from trade. In contrast, for other countries, such as Nigeria and

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[4] The cross-section dependence test suggested by Pesaran (2004) rejects the null hypothesis of no cross-section dependence at the one percent level.

Burkina Faso, the positive trade effects are marginal, whereas for many countries, such as Ecuador, Panama, Paraguay, and Chad, trade has a strong negative effect on income.

[Table 5 around here]

Given that the impact of trade on income is not constant across countries, we now ask whether it is constant over time. To answer this question, we compute for each country-DOLS regression the *MeanF* test developed by Hansen (1992). This test is a Chow-type test for parameter constancy in cointegrating regressions with unknown change points and is designed to detect any gradual changes in the regression coefficients.[5] The results of this test are reported in the columns 4 and 8 of Table 5. They show that the null hypothesis of parameter stability is rejected at least at the five percent level in about 35 percent of cases, suggesting that in several countries the impact of trade on income has changed over time. Interestingly, most of them (about 85 percent) are developing countries, which is also is also reflected in the average *MeanF* statistics presented in the bottom row of Table 5. For developed countries as a whole, the average *MeanF* statistic implies a fairly stable relationship between trade and income. In contrast, the average *MeanF* statistic suggests that in developing countries, the trade-income relationship tends to be rather unstable. A possible explanation for this finding is that the impact of trade on income depends on several political and institutional factors that are often not constant, especially in developing countries. If policies and institutions affecting the trade-income relationship change over time, then also the effect of trade on income changes over time. The hypothesis that the income effect of trade depends on several country-specific factors is examined in detail in Section 4. Before examining this issue, it is necessary to test the direction of causality.

3.5. Long-run causality

The above interpretation of the estimation results is based on the assumption that long-run causality runs from $\ln(T_{it})$ to $\ln(Y_{it})$. However, a statistically significant coefficient on $\ln(T_{it})$ does not necessarily need to be the result of an impact of trade on income. Given that the volume of trade generally tends to increase with the level of income (as discussed in Section 2.1.), a positive correlation can also be compatible with causality running from income to trade. To test the direction of causality, we enter the residuals from the individual DOLS long-run relations,

$$ec_{it} = \ln(Y_{it}) - [\hat{a}_i + \hat{\delta}_i t + \hat{\beta}_i \ln(T_{it})], \tag{14}$$

as error correction terms into a simple panel vector error correction model (VECM):

$$\begin{bmatrix} \Delta\ln(Y_{it}) \\ \Delta\ln(T_{it}) \end{bmatrix} = \begin{bmatrix} c_{1i} \\ c_{2i} \end{bmatrix} + \sum_{j=1}^{k} \Gamma_j \begin{bmatrix} \Delta\ln(Y_{it-j}) \\ \Delta\ln(T_{it-j}) \end{bmatrix} + \begin{bmatrix} a_1 \\ a_2 \end{bmatrix} ec_{it-1} + \begin{bmatrix} \varepsilon_{1it} \\ \varepsilon_{2it} \end{bmatrix}, \tag{15}$$

where the c_is are fixed effects. A significant error correction term implies long-run Granger causality from the explanatory to the dependent variables, where long-run Granger non-causality and weak exogeneity can be regarded as equivalent (see, e.g., Hall and Milne, 1994). Following Herzer (2008), we test for weak exogeneity by first imposing zero restrictions on the statistically insignificant short-run parameters (Γ_j) and then using a conventional likelihood ratio test of the null hypothesis $a_{1,2} = 0$.

Model (15) allows for heterogeneous long-run relationships, but assumes homogeneous short-run dynamics and homogeneous adjustment coefficients. Because, however, the homogeneity assumption may be empirically incorrect, we also allow for complete heterogeneity by estimating the VECM separately for each country. More precisely, we eliminate the insignificant short-run parameters from the VECM and compute the *p*-values for testing the null hypothesis of weak

[5] Hansen (1992) develops the stability tests using the FMOLS estimator. Because the DOLS estimator is asymptotically equivalent to the FMOLS estimator, the test statistics have the same distributions and are thus applicable to both estimators.

exogeneity for each country individually. The panel weak exogeneity test is then conducted using the Fisher statistic given by equation (9).

Table 6 presents the results. As can be seen, both the standard Wald statistic and the Fisher statistic reject the null hypothesis of weak exogeneity for both $\ln(Y_{it})$ and $\ln(T_{it})$ at the one percent significance level. Accordingly, income per worker is endogenous in the long run, implying that the above interpretation of the DOLS results as indicating a causal impact of trade on income is valid. Nonetheless, the long-run causality is bidirectional, as expected. Thus, increased trade is both a consequence and a cause of increased income.

[Table 6 around here]

4. The determinants of the impact of trade on income

In the previous section, we found considerable differences in the impact of trade on income across countries. This section systematically searches for country-specific conditions that are important factors in explaining these differences; that is, we try to identify important determinants of the income effect of trade. These determinants have hardly been investigated so far. Two exceptions are the studies by Bormann et al. (2006) and Freund and Bolaky (2008), who find that the effect of trade on income is negatively related to the level of regulation, whereas there is no robust association between the income effect of trade and institutional quality in terms of good governance.[6] Both studies use cross-country income regressions that include interaction terms between trade and a small number of potential determinants of the income effect of trade.[7] In this section, we follow a different approach: We use a regression model with the estimated income effect as dependent variable to consider a large number of possible determinants of the trade-income relationship. Because we use the income *effect* of trade rather than *income* as the dependent

[6] Bormann et al. (2006) define institutional quality in terms of good governance (as usual) and government regulations, and find insignificant effects of the former and significant effects of the latter.

variable, and because we include as many variables as possible relevant to the income effect of trade, our approach is less subject to endogeneity and omitted variable bias than the conventional interaction-term approach used by Bormann et al. and Freund and Bolaky.

We proceed as follows: we first describe the variables that we consider to be potentially relevant to the trade-income relationship and that we use in the empirical analysis. Then, we present the empirical analysis and discuss the results.

4.1. Variables and data

The first three variables that we consider are: the general level of development, human capital, and the level of development of local financial markets. The reason why these variables might be important for explaining cross-country differences in the income effect of trade can be intuitively explained as follows: an important source of gains from trade is the existence of cross-border knowledge spillovers. The ability to absorb foreign knowledge and technology depends, however, on absorptive capacity, which, in turn, is linked to the general level of development. Accordingly, low developed countries using very backward production technology may be unable to make effective use of technology spillovers. In a similar way, it can be argued that a certain level of human capital may be necessary to adopt foreign technology. And finally, knowledge spillovers are typically realised only if importers, exporters, and domestic producers have the ability to invest in absorbing foreign knowledge, which may be restricted by underdeveloped local financial markets. Thus, it can be hypothesised that the income effect of trade depends on the general level of development, the level of human capital, and the level of financial market development. In our analysis, the general level of development is represented by real per capita GDP; the secondary school enrolment rate is used as a proxy for human capital; and the ratio of domestic credit to the

[7] Bormann et al. (2006) and Freund and Bolaky (2008) find that trade per se does not exert a robust effect on income, but that trade has positive effects on income only if the level of business and labour regulation is below a certain threshold.

private sector to GDP is our measure of financial development. All these measures are taken from the World Bank's World Development Indicators (WDI).

Furthermore, we consider primary export dependence to be a possible factor explaining the cross-country differences in the income effect of trade. Several authors hypothesise that primary exports may be an obstacle to higher standard of living. The main arguments advanced in support of this hypothesis are (see, e.g., Sachs and Warner, 1995): (i) Increased primary exports can lead economies to shift away from competitive manufacturing sectors in which many externalities necessary for growth are generated, whereas the primary sector possesses no sustainable potential for generating knowledge spillovers. (ii) Revenues from primary product exports often accrue to a few wealthy individuals and thus tend to be wasted through profligate or inappropriate consumption rather than invested in productive activities. (iii) Primary exports are subject to large price and volume fluctuations. Increased primary exports may therefore lead to increased GDP variability and macroeconomic uncertainty. High instability and uncertainty may, in turn, hamper efforts at economic planning and reduce the quantity as well as efficiency of investment. Accordingly, a possible factor to explain the cross-country variations in the income effect of trade is primary export dependence. We use the ratio of primary exports to GDP from the WDI as measure of primary export dependence.

Next, we consider the possibility that the income effect of trade depends on the level of regulation, as suggested by Bormann et al. (2006) and Freund and Bolaky (2008). The logic behind this is simple: in standard theory, gains from trade arise from a reallocation of resources from import-competing sectors to export sectors in which a country has comparative advantage, implying a contraction in the activity of the former and an expansion of the latter. Government regulations, however, may impede the reallocation of resources to comparative advantage sectors, thereby reducing the gains from trade. We examine three forms of regulation: labour regulation, business regulation, and price regulation.

- Labour regulation is measured by the flexibility of firing index from the World Bank's "Doing Business" database (World Bank 2004). The higher the index, the more a country regulates the process of firing employed labour and thus the movement of labour across sectors.
- Business regulation is represented by the business freedom index published by the Heritage Foundation.[8] The business freedom index assesses the ability to create, operate, and close an enterprise quickly and easily. The higher the index, the lower the level of business regulation, and thus the higher the potential to reallocate factors of production between sectors.
- Price regulation is measured by the Heritage Foundation's monetary freedom index, which combines an assessment price controls with a measure of price stability. We use this combined index because both price controls and inflation may hinder the efficient allocation of resources according to comparative advantage. The higher the index, the lower the levels of price controls and inflation.

We also include two infrastructure variables from the WDI in the analysis: total length of railway lines per square kilometre of land area and telephone mainlines per 1000 people. The idea behind this is that gains from trade depend on the potential of the trade sector to generate linkages with the rest of the economy, which in turn may depend on the level of infrastructure development.

And finally, we hypothesise that the income effect of trade depends on the quality of institutions. Institutions such as property rights lower transaction costs by reducing uncertainty and establishing a stable structure to facilitate interactions, thus helping to allocate resources to their most efficient uses. Without institutions, individuals do not have incentives to invest in physical or human capital or adopt more efficient technologies, implying that resources are misallocated and potential gains from trade go unexploited. For the empirical analysis, we use nine measures of institutional (or governance) quality. Our first measure is the property rights index published by the Heritage Foundation. This index assesses the ability of individuals to accumulate private property,

[8] See http://www.heritage.org/research/features/index/downloads.cfm.

secured by clear laws that are fully enforced by the state. The remaining eight measures are compiled from the International Country Risk Guide (ICRG), published by the Political Risk Services (PRS) Group.[9] They are defined as follows:

- Corruption—this index assesses the level of corruption within the political system.
- Government stability—this factor measures the government's ability to carry out its declared program(s) and its ability to stay in office.
- Bureaucratic quality—this is an assessment of the institutional strength and quality of the bureaucracy in terms of acting as a shock absorber to minimise revisions of policy when governments change.
- Investment profile—this measure assesses the factors affecting the risk to investment that are not covered by other political, economic or financial risk components, such as contract viability or payment delays.
- Socioeconomic conditions—this index quantifies socioeconomic pressures at work in society that could constrain government action or fuel social dissatisfaction and thus destabilise the political regime.
- Democratic accountability—this is an assessment of the responsiveness of the government to its citizens.
- Internal conflict—the internal conflict measure is an assessment of political violence within a country (such as civil war, terrorism, or civil disorder) and its actual or potential impact on governance.
- External conflict—the external conflict measure assesses the risk to the incumbent government from foreign action, ranging from non-violent external pressure to violent external pressure.

It is important to note that the indicators for corruption, external and internal conflict are rescaled so that higher values always reflect higher institutional quality.

[9] See https://www.prsgroup.com/prsgroup_shoppingcart/pc-75-7-icrg-historical-data.aspx

The variables, their definitions, and sources are listed in Table 7. All variables are used in logarithmic form except for the dependent variable. The dependent variable is the estimated effect of trade on income from Table 5, $\hat{\beta}_i$. As discussed in Section 3.4, this effect can be assumed to be time-constant in 65 percent of the countries in our sample and thus be treated as average impact per year. For the remaining 35 percent, we found that the estimated income effect of trade is indeed not constant; nevertheless, it can be interpreted as a time average over the period 1960-2003. Consequently, we also use time averages for the independent variables in that period. An exception is the flexibility of firing index for which data before 2003 are not available, so that we are constrained to use values for that single year. Moreover, we do not have complete data on all variables for all countries, forcing us to limit our sample to 64 countries. The country composition of the sample is given in the Appendix.

[Table 7 around here]

4.2. Empirical analysis

We start with some simple bivariate regressions of the estimated income effect of trade on the above variables. The results of these regressions are reported in Table 8. They show that, without exception, all coefficients are statistically significant and have the expected signs. From this it follows that (as expected) each variable could act as an important determinant of the trade-income relationship. By definition, bivariate cross-sectional regressions are, however, unable to identify which of the variables are important—that is, robust to the omission of other relevant factors.[10]

[Table 8 around here]

To determine which of the variables are important (or robust) in explaining the cross-country variations in the effect of trade on income, we use the general-to-specific model selection approach suggested by Hoover and Perez (2004). Hoover and Perez show by means of Monte Carlo simulations that this approach is very effective in identifying the true parameters of the data generating process, thus outperforming other variable selection procedures, such as the extreme bounds approaches of Levine and Renelt (1992) and Sala-i-Martin (1997).

Following the Hoover and Perez (2004) approach, we start by estimating a general specification, in which all variables are included, and subject the estimated model to a series of specification tests. The test battery includes a Jarque-Bera test (*JB*) for normality of the residuals, a Ramsey RESET test for general nonlinearity and functional form misspecification (*RESET*), a Breusch-Pagan-Godfrey test for heteroscedasticity (*HET*), and a sub-sample stability test (*STABILITY*) using an *F*-test for the equality of the variances of the first three-fourths versus the last one-fourth of the sample. The results of these tests are presented in the top part of Table 9. They show clear evidence of non-normality and misspecification.

[Table 9 around here]

However, we find that Denmark, Greece, and the Netherlands produce large outliers in the residuals. Therefore, we introduce dummy variables for these countries to obtain a well-specified equation. The diagnostic test statistics are presented at the bottom of Table 9. They suggest that the model is now well specified. The assumption of normally distributed residuals cannot be rejected, and the *RESET* test does not suggest nonlinearity or misspecification. The model also passes the Breusch-Pagan-Godfrey test for heteroscedasticity and the *F*-test for parameter stability.

Next, we use the general model with country dummies and simplify it by removing insignificant variables. To this end, the variables are first ranked according to their *t*-statistics. We then employ five simplification paths in which each of the five variables with the lowest *t*-statistics is the first to be removed. Accordingly, we have five equations. From these equations, variables with insignificant coefficients are then eliminated sequentially according to the lowest *t*-values until the remaining variables are significant at the five-percent level. After removal of each variable, the

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[10] This does not necessarily apply to (panel) cointegration estimators (such as the one used in Section 3.4), which are robust to the omission of variables that do not form part of the cointegrating relationship.

above tests of model adequacy are performed. Furthermore, an F-test of the hypothesis that the current specification is a valid restriction of the general specification is used after each step. The result is that all of these tests are passed, implying five well-specified parsimonious equations, which are all valid restrictions of the general model. Finally, we construct the non-redundant joint model from each of these equations by taking all specifications and performing the F-test for encompassing the other specifications. This procedure yields the final specification in Table 10. As can be seen, the final model passes all the diagnostic tests. Moreover, in Figures 2, CUSUM and CUSUM of square tests are presented, which unanimously support a stable model for the countries involved. Thus, statistically valid inferences can be drawn from the regression results in Table 10.

[Table 10 around here]

[Figure 2 around here]

The results imply that the cross-country variations in the income effect of trade can be explained mainly by cross-country differences in the level of primary export dependence (measured by the share of primary exports in GDP), the level of labour market regulation (measured by flexibility of firing index), and property rights. According to the estimated coefficients, a one percent increase in the share of primary exports in GDP is associated with a 0.16 percentage point decrease in the income effect of trade, and each extra percent of labour regulation is estimated to reduce the impact of trade on income by 0.29 percentage points, whereas an increase in the property rights index by one percent raises the effect of trade on income by 0.485 percentage points per year.

Moreover, the coefficients on the country dummies for Denmark, Greece, and the Netherlands are positive and large in magnitude, indicating that trade has strong positive effects on income in these countries (see also Table 5). Given, however, that the dummy variables reflect country-specific characteristics that are not captured by any of the variables involved, we admit that the estimated models do not provide a complete picture of the potential determinants of the cross-country differences in the income effect of trade.

Another important finding is that the impact of trade on income appears not to depend (directly) on the level of per capita income. The per capita income variable turned out to be insignificant and hence was removed from the general model. From this we conclude that the negative income effect of trade we found for the average of developing countries (in Section 3.4) cannot be explained by low levels of per capita income per se. Rather, low levels of per capita income might be associated with low levels of property rights protection, high levels of labour market regulation, and high levels of primary export dependence, thereby explaining the negative impact of trade in developing countries.

This issue is examined further in Table 11, which provides some information about the performance of the variables that were omitted from the final specification. The second column reports the *t*-statistic of each omitted variable when added individually to the regression in Table 10, while the last three columns give an indication of the extent to which the omitted variables are collinear with the regressors of the final model, showing the pair-wise correlation coefficients and their *t*-statistics.

[Table 11 around here]

When added individually to the final model, several variables, such as per capita GDP, secondary schooling, business freedom, and monetary freedom, have the wrong sign. This is in contrast to the bivariate regression results in Table 8, in which all variables are correctly signed, and suggests a high degree of collinearity. Consequently, the omitted variables are correlated with the variables in the final model, implying that the excluded variables might play an indirect role in the trade-income relationship by affecting the included variables or being affected by them. In fact, the pair-wise correlation coefficients reveal that property rights are significantly positively correlated with all the excluded variables, and that regulations on firing workers are significantly negatively correlated with all the omitted variables, while the share of primary exports in GDP has a strong negative correlation only with GDP per capita. Thus, the level of per capita income is significantly correlated with the level of property rights protection, the level of labour market regulation, and the level of primary export dependence, which might (at least partly) explain why the impact of trade on income is negative in developing countries on average.

Taken together, the findings suggest that cross-country variations in the income effect of trade can be explained mainly by cross-country differences in primary export dependence, labour market regulation, and property rights. This, however, does not imply that all other variables are irrelevant for exploiting the potential of trade to increase the standard of living. There are several factors—such as per capita income, business freedom, bureaucratic quality, socioeconomic conditions, and democratic accountability—that are related to the level of property rights protection, the level of labour regulation, and/or the level of primary export dependence through various mechanisms and thus are likely to play an important indirect role in the relationship between trade and income.

5. Conclusions

We first examined the nature of the income effect of trade using panel cointegration techniques that are specifically designed to deal with the key problem plaguing previous studies of the trade-income relationship: namely, the inability to capture the heterogeneity in the relationship between trade and income across countries. Employing data for 81 developed and developing countries over the period 1960 to 2003, we found that a one percent increase in the trade share of GDP yields, on average, a statistically significant increase in income per worker of about 0.16 percent. This estimate is smaller than the findings reported by most other studies, and suggests that failure to account for cross-country heterogeneity can lead to misleading inferences about the average effect of trade on income. In fact, our results indicate that there are large cross-country differences in the income effect of trade, in particular between developed and developing countries:

in developed countries the income effect of trade is positive, while in developing countries, it is, on average, negative.

Next, we used a general-to-specific model selection approach to identify important country-specific factors explaining the cross-country differences in the income effect of trade. Our results suggest that these differences can be explained mainly by cross-country differences in primary export dependence, labour market regulation, and property rights protection. However, several factors, such as per capita income, business freedom, bureaucratic quality, socioeconomic conditions, and democratic accountability, are correlated with the level of property rights protection, the level of labour regulation, and the level of primary export dependence, suggesting that these factors play an important indirect role in the trade-income relationship.

Thus, it can be concluded the negative effect of trade we found for many developing countries will not necessarily remain negative forever. Reforms aimed at

(i) improving institutional quality,

(ii) increasing labour market flexibility, and

(iii) removing primary export dependence by diversifying the economy

can not only protect countries from the potential negative consequences of trade but also help to exploit the gains form trade in the long run.

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APPENDIX

Sample of countries used in the analysis of the determinants of the impact of trade on income

Argentina	France	Malaysia	Senegal
Australia	Greece	Mexico	South Africa
Austria	Guatemala	Morocco	Spain
Belgium	Guinea	Mozambique	Sri Lanka
Brazil	Honduras	Netherlands	Sweden
Burkina Faso	India	New Zealand	Switzerland
Cameroon	Indonesia	Nicaragua	Tanzania
Canada	Ireland	Nigeria	Thailand
Chile	Israel	Norway	Togo
China	Italy	Pakistan	Uganda
Colombia	Jamaica	Panama	United Kingdom
Denmark	Japan	Paraguay	United States
Dominican Republic	Jordan	Peru	Uruguay
Ecuador	Korea, Republic of	Philippines	Venezuela
El Salvador	Madagascar	Portugal	Zambia
Finland	Malawi	Romania	Zimbabwe

Table 1

Estimated effects of trade on income in selected cross-country regressions (IV estimates)

Study	Dependent variable	Independent variable			Geographical controls	Institutional controls
		Trade/GDP nominal	ln(Trade/GDP) nominal	ln(Trade/GDP) real		
Frankel and Romer (1999)	ln(GDP per worker)	**1.97 / 2.96**			No	No
Hall and Jones (1999)	ln(GDP per worker)		**0.185**		Yes	Yes
Rodríguez and Rodrik (2001)	ln(GDP per capita)	**1.97**			No	No
	ln(GDP per capita)	0.21 / 0.34			Yes	No
Frankel and Rose (2002)	ln(GDP per capita)	**1.59 / 1.96**			No	No
	ln(GDP per capita)	**1.13 / 1.28**			Yes	No
	ln(GDP per capita)	**0.68**			Yes	Yes
Irwin and Tervio (2002)	ln(GDP per capita)	0.65 / **4.91**			No	No
	ln(GDP per capita)	-7.19 / 1.30			Yes	No
Dollar and Kraay (2003)	ln(GDP per capita)			**1.67**	No	No
	ln(GDP per capita)			-3.40 / 0.18	No	Yes
	ln(GDP per capita)			-1.67 / 0.79	Yes	Yes
Alcalá and Ciccone (2004)	ln(GDP per worker)	0.394 / 1.013			Yes	Yes
	ln(GDP per worker)			**1.002 / 1.482**	Yes	Yes
Rodrik et al. (2004)	ln(GDP per capita)		-0.87 / 0.02		Yes	Yes
	ln(GDP per worker)		-0.42 / -0.30		Yes	Yes
	ln(GDP per capita)			-0.94 / -0.77	Yes	Yes
Noguer and Siscard (2005)	ln(GDP per capita)	**2.59 / 2.96**			No	No
	ln(GDP per capita)	**0.89 / 1.22**			Yes	No
	ln(GDP per capita)	**0.82 / 1.23**			Yes	Yes
Felbermayr (2005)	ln(GDP per capita)	-0.344			Yes	No

Notes: Bold indicates that the estimated coefficients were found to be significant at least at the five-percent level. Only the lowest and highest coefficient estimates are reported.

Table 2

Panel unit root tests

Variable	Deterministic terms	IPS statistics	CIPS statistics
Levels			
$\ln(Y)$	c, t	-0.06	-2.21
$\ln(T)$	c, t	-0.69	-2.16
First differences			
$\Delta\ln(Y)$	C	-10.03**	-2.42**
$\Delta\ln(T)$	C	-11.50**	-2.52**

Notes: c (t) indicates that we allow for different intercepts (and time trends) for each country. Four lags were selected to adjust for autocorrelation. The IPS statistic is distributed as $N(0, 1)$. The relevant five (one) percent critical value for the CIPS statistics is -2.58 (-2.68) with an intercept and a linear trend, and -2.10 (-2.20) with an intercept. ** denote significance at the one percent level.

Table 3

Panel cointegration tests

	Cointegration rank	
	$r = 0$	$r = 1$
Standardized panel trace statistics; $\Psi_{\overline{LR}}\{H(r)\|H(2)\}$	6.58**	-0.75
Fisher statistics	280.80**	141.84
CIPS statistic	-2.22**	
Panel ADF statistic	-3.62**	
Group ADF statistic	-2.91**	

Notes: The panel trace statistic, the panel ADF statistic and the group ADF statistic are distributed as $N(0, 1)$**.** The Fisher statistic is distributed as χ^2 with $2 \times N$ degrees of freedom. It has a critical value of 206.8 (192.7) at the one (five) percent level. The relevant five (one) percent critical value for the CIPS statistic is -2.10 (-2.20). The number of lags was determined by the Schwarz criterion with a maximum of five lags. ** indicate a rejection of the null hypothesis of no cointegration at the one percent level.

Table 4

Estimates of the long-run impact of trade on income

Independent variable	Group-mean DOLS estimator (Pedroni, 2001)	Common correlated effects mean group estimator (Pesaran, 2006)
$\ln(T)$	0.165** (6.40)	0.149** (3.11)

Notes: The dependent variable is Ln(Y). t-statistics in parenthesis. ** indicate significance at the one percent level. The number of leads and lags in the individual DOLS regressions was determined by the Schwarz criterion with a maximum of five lags.

Table 5

DOLS country estimates and stability tests

Country	ln(*T*)	*t*-stat	*MeanF*	Country	ln(*T*)	*t*-stat	*MeanF*
Argentina	-0.1208	-1.11	10.03**	Lesotho	0.1496**	2.93	2.23
Australia	0.0973	1.10	4.19	Luxembourg	0.5847*	2.31	16.65**
Austria	1.0333**	5.36	3.24	Madagascar	0.1402	2.07	2.43
Barbados	0.8159*	2.51	3.37	Malawi	0.5660**	4.33	2.08
Belgium	0.9081**	5.12	15.80**	Malaysia	-0.3663**	-4.90	7.25*
Benin	0.1730**	4.52	3.66	Mauritius	0.3932*	2.27	2.93
Brazil	-0.4599*	-2.16	8.08*	Mexico	-0.4567**	-6.64	21.49
Burkina Faso	0.0153	0.50	1.81	Morocco	-0.8603**	-8.70	3.92
Burundi	-0.4250**	-5.81	17.38	Mozambique	0.5616**	6.53	1.89
Cameroon	-0.3859*	-2.21	8.03*	Nepal	0.0733	1.65	22.08**
Canada	0.2260**	3.28	2.81	Netherlands	1.7712**	9.60	2.81
Chad	-0.9048*	-2.62	12.35**	New Zealand	0.1406	0.69	4.48
Chile	1.0147**	3.84	19.16**	Nicaragua	-0.0869	-0.81	23.01**
China	-0.2223**	-3.34	2.28	Niger	0.3121*	2.69	8.64***
Colombia	-0.4991**	-5.46	12.53**	Nigeria	0.0194	0.41	5.77
Costa Rica	0.4908**	4.63	3.36	Norway	0.3065**	3.29	4.12
Cote d`Ivoire	0.2625*	2.27	18.44**	Pakistan	0.3843	1.42	13.86**
Denmark	2.1883**	6.28	4.31	Panama	-1.0165**	-5.43	3.14
Dominican Republic	0.1727	1.63	2.68	Paraguay	-0.9748**	-5.79	7.06*
Ecuador	-1.0723	-2.02	15.87**	Peru	-0.3757**	-3.33	14.83**
Egypt	-0.1405*	-2.72	5.13	Philippines	-0.9033	-8.46	11.68**
El Salvador	0.1573	1.06	8.04*	Portugal	-0.16482	-1.87	2.48
Ethiopia	-0.0756	-0.64	3.21	Romania	-0.0675	-0.22	33.34**
Finland	0.0865	0.60	5.53	Senegal	0.5676*	2.38	4.40
France	0.4742**	2.94	3.74	Singapore	0.31041	1.28	6.00
Gambia	-0.2517*	-2.21	6.48*	South Africa	-0.2050*	-2.15	1.23
Greece	1.7661	1.44	2.46	Spain	1.1401**	5.16	2.92
Guatemala	0.1593	1.60	7.22*	Sri Lanka	-0.1101**	-3.50	3.32
Guinea	-0.3745**	-2.76	4.05	Sweden	0.2816**	3.08	4.29
Honduras	-0.6165**	-4.50	5.39	Switzerland	0.5786*	2.18	5.41
Hong Kong	-0.2436*	-2.36	1.40	Tanzania	-0.63867**	-3.67	14.52**
India	0.2059**	9.80	0.83	Thailand	0.0537	0.60	1.04
Indonesia	0.9489**	8.38	2.99	Togo	-0.6612**	-3.96	5.46
Ireland	0.6044**	4.63	10.44*	Trinidad &Tobago	-0.0887	-0.17	23.90**
Israel	1.5980**	7.94	3.51	Uganda	0.0949	0.84	37.73**
Italy	0.3654	1.74	24.03**	United Kingdom	0.4604**	4.21	2.70
Jamaica	0.5903**	3.99	5.10	United States	0.2224*	2.70	2.45
Japan	1.6716**	3.55	4.72	Uruguay	1.0172**	5.78	4.29
Jordan	0.7750**	10.65	4.478	Venezuela	-0.1718	-1.50	12.13**
Korea, Republic of	0.0294	0.72	3.24	Zambia	-0.2919**	-3.21	2.77
				Zimbabwe	-0.3792**	-6.65	3.87
Developed countries	0.7677**	16.76	6.05 (average)	Developing countries	-0.0507*	-2.31	8.13* (average)

Notes: The dependent variable is Ln(*Y*). ** (*) indicate significance at the one (five) percent level. The number of leads and lags was determined by the Schwarz criterion with a maximum of five lags. The *MeanF* test is a Chow-type test for parameter constancy in cointegrating regressions. The five (one) percent critical value for the stability test (*MeanF*) is 6.22 (8.61) (Hansen, 1992).

Table 6

Weak exogeneity tests / long-run causality tests

Variable (Coefficient)	ln(Y) (α_1)	ln(T) (α_2)
$\chi^2(1)$	154.51	15.91
(p-values)	(0.000)	(0.000)
Fisher statistics	583.01	208.11
(p-values)	(0.000)	(0.009)

Notes: The number of degrees of freedom υ in the standard χ^2(υ) tests correspond to the number of zero restrictions. The Fisher statistic is distributed as χ^2 with $2 \times N$ degrees of freedom. It has a critical value of 206.8 at the one percent level. The models were estimated with up to three lags.

Table 7

Variables and sources

Variables	Definition	Source
ln(gdp)	Log of real per capita GDP (in constant 2000 US dollars at PPP). Data averaged over the period 1975 to 2003.	WDI 2008
ln(schooling)	Log of the secondary school enrolment rate. Data averaged over the period 1991 to 2003.	WDI 2008
ln(credit)	Log of the private sector bank loans/GDP ratio. Data averaged over the period 1960 to 2003.	WDI 2008
ln(primaryexports)	Log of the primary exports/GDP ratio. (Agricultural raw materials exports + food exports + fuel exports + ores and metals exports divided by GDP). Data averaged over the period 1962 to 2003.	WDI 2008
ln(firing)	Log of flexibility of firing. Data are from 2003.	Doing Business, World Bank (2004)
ln(businessfreedom)	Log of business freedom. Data averaged over the period 1995 to 2003.	Heritage Foundation
ln(monfreedom)	Log of monetary freedom. Data averaged over the period 1995 to 2003.	Heritage Foundation
ln(railway)	Log of kilometers of railways per square kilometre of land area. Data averaged over the period 1975 to 2003.	WDI 2008
ln(telephone)	Log of telephone mainlines per 1000 people. Data averaged over the period 1975 to 2003.	WDI 2008
ln(propertyrights)	Log of property rights. Data averaged over the period 1995 to 2003.	Heritage Foundation
ln(corruption)	Log of corruption. Data averaged over the period 1984 to 2003.	PRS Group
ln(govstab)	Log of government stability. Data averaged over the period 1984 to 2003.	PRS Group
ln(bureaucratic)	Log of bureaucratic quality. Data averaged over the period 1984 to 2003.	PRS Group
ln(invest)	Log of investment profile. Data averaged over the period 1984 to 2003.	PRS Group
ln(socio)	Log of socioeconomic conditions. Data averaged over the period 1984 to 2003.	PRS Group
ln(democratic)	Log of democratic accountability. Data averaged over the period 1984 to 2003.	PRS Group
ln(intconflict)	Log of internal conflict. Data averaged over the period 1984 to 2003.	PRS Group
ln(extconflict)	Log of external conflict. Data averaged over the period 1984 to 2003.	PRS Group
Dependent variable: $\hat{\beta}_i$	Impact of trade on income, individual DOLS estimates of the coefficient on ln(T) over the period 1960 to 2003.	Table 5

Table 8

Bivariate regressions of the estimated income effect of trade on several variables

Variables	Estimated coefficients (t-statistics)								
ln(gdp)	0.25** (3.56)								
ln(schooling)		0.29* (2.58)							
ln(credit)			0.18* (2.08)						
ln(primaryexports)				-0.21* (-2.45)					
ln(firing)					-0.49** (-3.97)				
ln(businessfreedom)						0.75** (3.34)			
ln(monfreedom)							1.10* (2.45)		
ln(railway)								0.25** (4.78)	
ln(telephone)									0.17** (3.71)
Adj. R^2	0.16	0.08	0.05	0.07	0.19	0.14	0.07	0.26	0.17
ln(propertyrights)	1.02** (4.79)								
ln(corruption)		0.97** (4.40)							
ln(govstab)			2.37** (3.36)						
ln(bureaucratic)				0.58** (4.08)					
ln(invest)					1.92** (4.06)				
ln(socio)						1.23** (3.80)			
ln(democractic)							0.94** (4.08)		
ln(intconflict)								0.65* (2.09)	
ln(extconflict)									1.25* (2.30)
Adj. R^2	0.26	0.23	0.14	0.20	0.20	0.18	0.20	0.05	0.06

Notes: The dependent variable is $\hat{\beta}_i$. *t*-statistics in parenthesis. ** (*) indicate significance at the one (five) percent level. The higher the flexibility of firing index, ln(firing), the more a country regulates the process of firing employed labour. Similarly, the indicators for corruption, external and internal conflict are rescaled so that higher values always reflect higher institutional quality.

Table 9

Diagnostic tests: general specification

Without country dummies	
JB ($\chi^2_{(2)}$)	5.66 [0.059]
RESET ($\chi^2_{(1)}$)	2.96 [0.085]
HET	$F(18, 45) = 0.35$ [0.990]
STABILITY	$F(15, 47) = 1.65$ [0.193]
With country dummies	
JB ($\chi^2_{(2)}$)	1.04 [0.595]
RESET ($\chi^2_{(1)}$)	0.48 [0.487]
HET	$F(21, 42) = 0.88$ [0.608]
STABILITY	$F(15, 47) = 1.10$ [0.756]
Number of observations	64

Notes: *JB* is the Jarque-Bera test for normality, *RESET* is the usual test for general nonlinearity and misspecification, *HET* is the Breusch-Pagan-Godfrey test for heteroscedasticity, and *STABILITY* is an *F*-test for the equality of the variances of the first three-fourths versus the last one-fourth of the sample. Numbers in brackets behind the values of the diagnostic test statistics are the corresponding *p*-values.

Table 10

General-to-specific approach: final specification

Independent variable	Dependent variable: $\hat{\beta}_i$
ln(primaryexports)	-0.160* (-2.513)
ln(firing)	-0.290* (-2.583)
ln(propertyrights)	0.485* (2.391)
Denmark dummy	1.591** (3.190)
Greece dummy	1.614** (3.267)
Netherlands dummy	1.566** (3.106)
Diagnostic tests	
Adj. R^2	0.53
JB ($\chi^2_{(2)}$)	2.06 [0.357]
RESET ($\chi^2_{(1)}$)	0.11 [0.744]
HET	$F(6, 57) = 0.32$ [0.923]
STABILITY	$F(15, 47) = 1.10$ [0.756]
REST	$F(15, 42) = 0.60$ [0.859]
Number of observations	64

Notes: *t*-statistics in parentheses. ** (*) indicate significance at the one (five) percent level. *JB* is the Jarque-Bera test for normality, *RESET* is the usual test for general nonlinearity and misspecification, *HET* is the Breusch-Pagan-Godfrey test for heteroscedasticity, *STABILITY* is an *F*-test for the equality of the variances of the first three-fourths versus the last one-fourth of the sample, and *REST* is an *F*-test of the hypothesis that the model is a valid restriction of the general model. Numbers in brackets behind the values of the diagnostic test statistics are the corresponding *p*-values.

Table 11

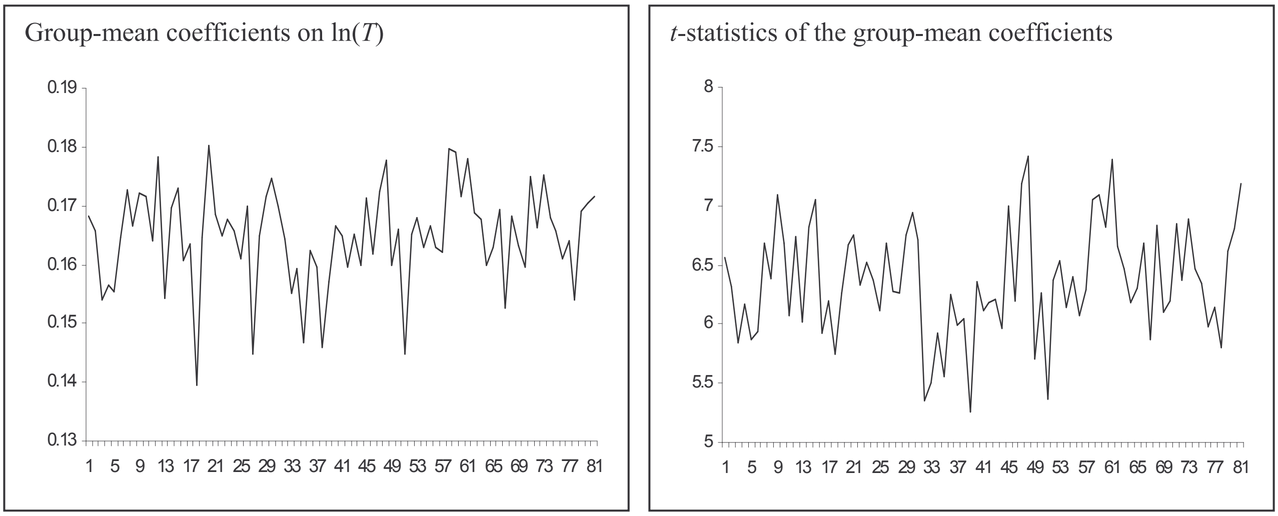
Effects of adding further regressors individually to the Table 10 regression and correlation coefficients

Regressor	*t*-statistic of added variable	Correlation coefficients		
		ln(primaryexports)	ln(propertyrights)	ln(firing)
ln(gdp)	-0.82	-0.42** (-3.67)	0.73** (8.47)	-0.45** (-3.93)
ln(schooling)	-0.65	-0.10 (-0.78)	0.61** (6.13)	-0.33** (-2.79)
ln(credit)	0.63	-0.12 (-0.96)	0.27* (2.20)	-0.28* (-2.34)
ln(businessfreedom)	-0.82	-0.20 (-1.60)	0.78** (9.68)	-0.42** (-3.65)
ln(monfreedom)	-0.39	-0.15 (-1.20)	0.60** (5.91)	-0.29* (-2.38)
ln(railway)	1.08	-0.18 (-1.43)	0.52** (4.85)	-0.42** (-3.68)
ln(telephone)	-0.68	-0.19 (-1.50)	0.74** (8.56)	-0.45** (-3.95)
ln(corruption)	0.51	-0.22 (-1.77)	0.64** (6.62)	-0.39** (3.32)
ln(govstab)	0.45	-0.09 (-0.74)	0.58** (5.59)	-0.44** (-3.85)
ln(bureaucratic)	0.23	-0.22 (-1.79)	0.73** (8.36)	-0.45** (-3.94)
ln(invest)	-0.13	-0.20 (-1.60)	0.81** (10.91)	-0.51** (-4.64)
ln(socio)	-1.07	-0.19 (-1.50)	0.82** (11.08)	-0.53** (4.95)
ln(democractic)	0.05	-0.22 (-1.77)	0.74** (8.57)	-0.44** (-3.90)
ln(intconflict)	-1.24	-0.06 (-0.47)	0.54** (5.02)	-0.37** (-3.11)
ln(extconflict)	-0.02	-0.01 (-0.09)	0.51** (4.67)	-0.26* (-2.08)

Notes: t-statistics in parentheses. ** (*) indicate significance at the one (five) percent level.

Figure 1

Group-mean estimation with single country excluded from the sample

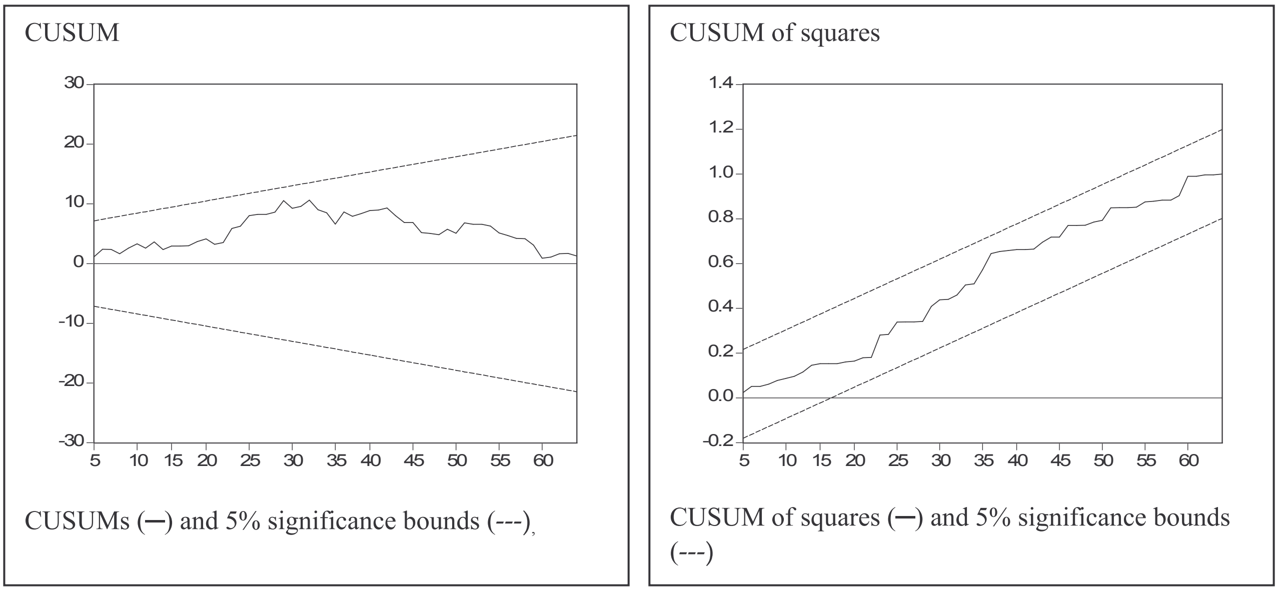


Group-mean coefficients on ln(T)

t-statistics of the group-mean coefficients

Figure 2

Stability Tests



CUSUM

CUSUMs (—) and 5% significance bounds (---)

CUSUM of squares

CUSUM of squares (—) and 5% significance bounds (---)

Notes: Outliers (Denmark, Greece, Netherlands) were excluded to compute the recursive residuals and the CUSUM and CUSUM of squares statistics.