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## Cross-country heterogeneity and the trade-income relationship

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# Cross-country heterogeneity and the trade-income relationship

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ABSTRACT

This paper makes the following 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 75 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 results, on average, in a statistically significant increase in income per worker of about 0.18 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 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. (iii) The cross-country heterogeneity in the impact of trade on income can be explained mainly by cross-country differences in primary export dependence, labor market regulation, and property rights protection. The level of property rights protection is positively related, while the levels of primary export dependence and labor market regulation are negatively related to the income effect of trade.

*Keywords:*Trade; Income; Cross-country heterogeneity; Panel cointegration; General-to-specific approach

*JEL-Classification:* F43; F14; C23; C52

## 1. Introduction

Is the effect of international trade on real income the same for all countries? Of course, this may seem a strange question, since we know from theory that whether or not and to what extent countries might 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 highly 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

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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 for in cross-country regressions, and this fact 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 three 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 75 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 0.18 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, labor 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 labor market regulation.

(3) 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 involves estimating 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 from the first stage as the dependent variable. The aim is to identify which country-specific factors are empirically important determinants of the income effect of trade.

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The plan of the paper is as follows. Section 2 summarizes 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, and Section 5 concludes.

## 2. Review of the empirical literature

In this section, we review the empirical literature addressing the impact of trade on income. This literature can be roughly divided into two categories: cross-country studies and panel studies. To begin with, we describe the general empirical approach in cross-country studies. Then, we summarize 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:

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

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 $\varepsilon_i$ and $T_i$ and thus a downward bias in the estimate of $\beta$ .

As the literature on the trade-income relationship recognizes, Eq. (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 in the following way. 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 need not 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 concurrently with increases in 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 to trade 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 can be associated with the effects of geography on income through institutions. Frankel and Rose (2002), as well as Noguer and Siscart (2005), for example, estimate equation (1) with and without additional controls for both

[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).

geography and institutions. They detect a large and statistically 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.

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.

**[Table 1 about here]**

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 summarized tend to produce either unreasonably large and statistically significant estimates of the impact of trade on income or insignificant estimates.[2] 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

[2] The augmented Solow model of Mankiw et al. (1992), for example, predicts that the estimated coefficient on the log of the investment rate in the steady state should be about 1 across countries; that is, an increase in the investment rate by 1% is predicted to lead to a long-run increase in GDP per worker by about 1% across countries. Given that several theoretical models suggest that increased trade can lead to income losses, it is theoretically implausible that the cross-country effect of trade on income is greater than the cross-country effect of investment on income. Cross-country income regressions with coefficients on (log) trade exceeding 1 are thus hard to justify theoretically.

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.

*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 fixed effects, thus eliminating a possible source of 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 Eq. (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 (2003, 2004) 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 minimized. However, given 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 most of the above-cited 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 about 1.5 percent.

A different approach is used by Feyrer (2009a, 2009b). He addresses the problems of endogeneity and omitted variables by constructing time-varying trade instruments based on (i) changes in the effects of air distance and sea distance on trade over time, due to changes in transportation technology, and (ii) the temporary change in sea distance caused by the closure of the Suez Canal between 1967 and 1975, as a result of wars in the Middle East (the Six-Day War and the Yom Kippur War). The time variation makes possible the inclusion of country fixed effects (which control for all time-invariant correlates with income, such as distance from the equator and the share of a country's area that is in the tropics). Using contemporaneous OLS fixed-effects panel regressions, Feyrer finds that trade increases income with elasticities between 0.157 and 0.578. Unfortunately, however, these estimates are not directly comparable to those reported in Table 1, given that Feyrer uses the volume of trade as an explanatory variable, rather than trade as a percentage of GDP. Nevertheless, Feyrer's values seem somewhat more reasonable, although overall, they appear to be still relatively large compared to, for example, the capital-elasticity of output in the constant-return-to-scale production process.[3]

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 panel literature suggest that estimation and inference in standard panel models can be misleading when the slope coefficients differ across cross-sectional units.[4] Similarly, parameter heterogeneity due to omitted variables may substantially 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. We begin this section by first describing 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] The results by Feyrer (2009a, 2009b) should be viewed with caution, since the possibility that the instruments are acting through channels other than trade cannot be fully excluded. Feyrer (2009a) admits, for example, that changes in transportation technology might not only affect trade but also foreign direct investment and cross-border movements of people. Thus, the coefficient on the instrument based on the change in the effect of distance on trade may, at least in part, reflect the income effect of these omitted variables. Similarly, one should keep in mind that the 1967 Arab oil embargo was a reaction to the Six-Day War between Israel and Egypt, Jordan, Lebanon and Syria, while the Yom Kippur War between Israel on one side, and Egypt and Syria on the other, was associated with the Arab oil embargo in 1973-74 and the worldwide energy crisis of 1973-74 (see, e.g., Salameh, 2004). Thus, it could well be that the increase in sea distance due to the closure of the Suez Canal at that time is related to income through the worldwide consequences of two oil embargos and the oil crisis of 1973-74.

[4] Pesaran and Smith (1995), for example, show that slope heterogeneity generates a correlation between the regressors and the error term, as well as a serial correlation in the disturbances, and thus introduces a bias in traditional panel data estimators.

*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

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

where $Y_{it}$ represents income per worker over time periods $t = 1, 2, ..., T$ and countries $i = 1, 2, ..., N$. $T_{it}$ 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 $\beta$ denotes the cross-country average of the effects of trade on income, $\beta_i$, which are 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.[5]

Eq. (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 behavior and that $\ln(Y_{it})$ must be cointegrated with $\ln(T_{it})$. A regression consisting of two cointegrated variables has a stationary error term, in turn implying that no relevant integrated variables are omitted; any omitted nonstationary variable that is part of the cointegrating relationship would enter the error term, thereby producing nonstationary residuals and thus leading to a failure to detect cointegration. If, on the other hand, cointegration between a set of variables is detected, this same stationary relationship will also be found in an enlarged variable set. Thus, an important implication of finding cointegration is that no relevant integrated variables in the cointegrating vector are omitted. Cointegration estimators are therefore robust (under cointegration) to the omission of variables that do not form part of the cointegrating relationship (see, e.g., Johansen, 2000). This justifies a reduced form model such as Eq. 2 (if cointegrated).

Thus, we select from the Heston, Summers, and Aten (2006) Penn World Table (Version 6.2) 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 from 97 countries for which data on real GDP per worker and real openness are available over the period from 1960 to 2003, we eliminate those countries for which the individual time series do not pass a simple screening for a unit root via the ADF, the PP and the KPSS tests. In addition, we exclude countries having average populations between 1960 and 2003 of less than one million, as well as countries for which the data received

[5] Admittedly, changes in institutions can be abrupt, causing structural breaks in the intercept and/or trend. We therefore tested whether the estimated $\beta$ coefficient is biased due to potential unmodeled structural breaks. Specifically, we included dummy variables for each possible structural break detected by a sequential Wald test, and found almost identical $\beta$ coefficients.

a grade of "D" (lowest quality) from Heston, Summers, and Aten.[6] Many small economies, for which international trade is important, have implausibly high historical levels of income, which is typically due to questionable national accounts deflators, particularly for the foreign sector. Therefore, we also omit small economies. This sample-selection procedure yields a sample of 75 countries.

*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, …, $N$, is tested against the alternative of (trend) stationary, $H_1 : \rho_i < 0$, $i$ = 1, 2, …, $N_1$; $\rho_i = 0$, $i = N_1 + 1$, $N_1 + 2$, …, $N$, 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 $N$ (=75) cross-sectional ADF $t$-statistics, and $\mu$ 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, $\varepsilon_{it}$, 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-sectional dependency by augmenting the ADF regression with the cross-sectional 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-sectional 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:

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

[6] Our sample excludes 13 countries, the data for which receive a grade of "D" (and/or which had populations in the period 1960-2003 of less than one million) and 9 countries for which the time series did not pass the unit-root tests. The

where $t_i$ is the OLS $t$-ratio of $\rho_i$ in Eq. (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 1, $I(1)$. Thus, the next step in our analysis is an investigation of the cointegration properties of the variables.

**[Table 2 about 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 separately, 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 $\Pi_i$ 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 $\chi^2$ with $2 \times N$ degrees of freedom.

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finding that in most, but not all, countries, both GDP per worker and trade openness exhibit a unit root is in line with previous studies (see, e.g., McCoskey, 2002).

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. (2010) 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:

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

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 standardized trace statistics and the Fisher $\chi^2$ 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 about 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 $\beta$ in Eq. (2) using the between-dimension, group-mean panel DOLS estimator suggested by Pedroni (2001). Pedroni emphasizes 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

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

where $\Phi_{ij}$ are coefficients of lead and lag differences, which account for possible serial correlation and endogeneity of the regressor(s), thus yielding unbiased estimates. Therefore, an important feature of the DOLS procedure is that it generates unbiased estimates for variables that cointegrate, even with endogenous regressors. Consequently, in contrast to conventional cross-country approaches, the approach does not require exogeneity assumptions nor does it require the use of instruments. In addition, the DOLS estimator is superconsistent under cointegration, and it is also robust to the omission of variables that do not form part of the cointegrating relationship.

From regression (11), the group-mean DOLS estimator for $\beta$ 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 $z_{it}$ = $((\ln(T_{it})$ – $\overline{\ln(T_i)}$, $\Delta\ln(T_{it-K})$, …, $\Delta\ln(T_{it+K}))$, $\tilde{s}_{it}$ = $s_{it} - \bar{s}_i$, 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 $\beta$ 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 $i$th 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 6.38 and the point estimate implies that an increase in the trade/GDP ratio by one percent increases GDP per worker by 0.181 percent, on average. An important aspect of this result is that the point estimate is much smaller than most cross-country regression estimates, which tend to yield unreasonably large values for the impact of trade on income (as discussed in Section 2.2). We thus 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, as well as 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 about here]**

Nevertheless, we have to admit that the estimated impact of trade may be biased by the presence of potential cross-sectional dependencies.[7] 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 CCE estimators given by Eq. (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 its DOLS counterpart. However, the CCEMG estimation procedure implicitly assumes that the cointegration between trade and income is driven by a stochastic trend that is common to all countries of the panel, which may be incorrect. Moreover, and perhaps more importantly, the CCEMG estimator is intended for the case in which the regressors are exogenous, so that we lose the ability to account for the likely endogeneity of trade. Therefore, we prefer the DOLS estimate in column two.

Since a main contribution of this paper is the use of estimation techniques that are robust (under cointegration) to a variety of estimation problems that often plague empirical work, including omitted variables, endogeneity, and heterogeneity, we need to ensure that the differences in the estimates between this and previous studies are due exclusively to the estimation method, rather than to other factors, such as outliers, sample selection, and different data sets.

To examine whether outliers are responsible for the smaller estimated effect of trade on income, we re-estimate the DOLS regression, excluding one country at a time from the sample. The sequentially estimated group-mean coefficients and their *t*-statistics are presented in Fig. 1. As they are relatively stable between 0.15 and 0.20 and always significant at the one-percent level, we conclude that the relatively small cross-country effect is not the result of potential outliers.

**[Figure 1 about here]**

Next, we examine whether the relatively small estimate of $\beta$ is due to sample-selection bias. Sample-selection bias occurs when the selected sample is not random and thus not representative. A potential problem with our sample could be that we excluded 13 countries with data quality of grade "D" (and also with populations of less than one million) and nine countries having time series which did not pass the ADF test, the PP test, or the KPSS test. However, given that these tests may suffer from severe size distortions (implying that there could be a significant unit-root component that has not been detected by these tests), and that these 22 excluded countries could have a significant effect on the results, we re-estimate the DOLS regression for the whole sample of 97 countries. The resulting group-mean coefficient is given in the second

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

column of Table 5. As can be seen, this coefficient is even somewhat smaller than that of the original sample and still statistically significant at the one-percent level, suggesting that our relatively small estimate of the cross-country effect of trade on income is not the result of sample-selection bias.

Finally, we investigate whether the discrepancies in the results between the present and previous studies are due to the use of different data sets. Most previous studies, including Frankel and Romer (1999), Dollar and Kraay (2003), and Alcalá and Ciccone (2004), are based on data from the Penn World Table (PWT), Version 5.6, whereas we use the PWT Version 6.2. However, it has recently been shown that some country data differ significantly between different versions of the PWT and that, therefore, conclusions based on one version of the PWT do not necessarily hold under another version (see, e.g., Johnson et al., 2009; Ponomareva and Katayama, 2010). In light of this finding, we re-run the DOLS regression with data from the PWT 5.6. The sample, in this case, consists of 68 countries over the period from 1955 to 1990. As the result in column three of Table 5 shows, the trade coefficient is slightly greater than its counterpart in Table 4, but still much smaller than the coefficients reported in previous studies. Thus, the differences in the results appear to not be due to the use of different versions of the PWT.

**[Table 5 about here]**

The individual country DOLS point estimates (for the original sample and data source) and their *t*-statistics are presented in Table 6. 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 that for 29 out of 75 countries, an increase in trade is associated with a decrease in income per worker. Thus, a substantial portion of countries do not gain from trade. Interestingly, all of 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 6. The estimated effect of trade is statistically significant and positive for developed and significant and negative for developing countries, reflecting the heterogeneity between these groups.[8] 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 Burkina Faso, the positive trade effects are marginal, whereas in many countries, such as Ecuador, Panama, and Paraguay, trade has a strong negative effect on income.

**[Table 6 about here]**

Given that the impact of trade on income is not constant across countries, we 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

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[8] Similarly, DeJong and Ripoll (2006) find that the effects of tariffs on growth are negative for rich countries, but positive for poor countries.

regressions with unknown change points and is designed to detect any gradual changes in the regression coefficients.[9] The results of this test are reported in the columns 4 and 8 of Table 6. 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 fact is also reflected in the average *MeanF* statistics presented in the bottom row of Table 6. 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. For example, many developing countries underwent significant changes in institutions and regulation between 1960 and 2003, going from dictatorships to democracies and from extremely market-unfriendly to market-friendly policies. If policies and institutions affecting the trade-income relationship change over time, then the effect of trade on income changes over time, as well. 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, we finally test the direction of causality.

### *3.5. Long-run causality*

Even though estimation by DOLS does not require the regressor(s) to be exogenous (and even though cointegration implies long-run Granger causality in at least one direction), we are interested in detecting the direction of long-run causality. Specifically, given that the volume of trade generally tends to increase with the level of income (as discussed in Section 2.1.), it is likely that causality runs in both directions, that is, not only from trade to income but also from income to trade. To test the direction of long-run 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) of the form

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

where the $c_i$s are fixed effects, the error-correction term, $ec_{it-1}$, represents the error in, or deviation from, the equilibrium, and the adjustment coefficients $a_1$ and $a_2$ capture how $\ln(Y_{it})$ and $\ln(T_{it})$ respond to deviations from the equilibrium relationship. From the Granger representation theorem, we know that at least one of the adjustment coefficients must be non-zero if a long-run relationship between the variables is to hold. A significant error-correction term also suggests long-run Granger causality and thus long-run endogeneity

[9] 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.

(see, e.g., Hall and Milne, 1994), whereas a non-significant adjustment coefficient implies long-run Granger non-causality from the independent to the dependent variable(s), as well as weak exogeneity. Following Herzer (2008), we test for weak exogeneity by first imposing zero restrictions on the statistically insignificant short-run parameters ($\Gamma_j$) 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, this homogeneity assumption may be empirically incorrect, we also allow for complete heterogeneity by estimating the VECM separately for each country. More specifically, we eliminate the insignificant short-run parameters from the VECM and compute the $p$-values for testing the null hypothesis of weak exogeneity for each country, individually. The panel weak exogeneity test is then conducted using the Fisher statistic given by Eq. (9).

**[Table 7 about here]**

Table 7 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. From this it can be concluded that the statistical long-run causality is bidirectional, suggesting that increased trade is both a consequence and a cause of increased income, as expected.

## 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 to date. However, two exceptions are the studies by Bormann et al. (2006), and Freund and Bolaky (2008), which 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.[10] 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.[11] 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 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. Finally, it should be noted that, in contrast to our approach, the conventional interaction-term approach is unable to identify which variable determines the

[10] 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.

[11] 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 labor regulation is below a certain threshold.

effect of the other variable. For example, a statistically significant interaction term between trade and regulation does not necessarily imply that the income effect of trade depends on the level of regulation; it can instead be compatible with the possibility that the effect of regulation on income is determined by the level of trade openness.

We proceed in our analysis by first describing the variables that we consider to be potentially relevant to the trade-income relationship and which we use in the empirical analysis, and then presenting the empirical analysis, and discussing 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 for the adoption of foreign technology. And finally, knowledge spillovers are typically realized 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 hypothesized that the income effect of trade depends upon 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 enrollment rate is used as a proxy for human capital, and the ratio of domestic credit to the 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 hypothesize that primary exports may be an obstacle to attaining a higher standard of living. The main arguments advanced in support of this hypothesis are threefold (see, e.g., Sachs and Warner, 1995; Herzer, 2007): (i) increased primary exports can lead economies to shift away from the competitive manufacturing sectors in which many externalities necessary for growth are generated, while the primary export sector itself does not (by its nature) have many linkages with, and spillovers into, the economy (Helpman and Krugman (1985), for example, show that if opening up to trade induces an expansion of sectors that do not exhibit positive externalities, while other sectors with positive externalities shrink, trade can lead to welfare losses); (ii) revenues from primary product exports often only accrue to a few wealthy individuals and thus tend to be wasted through profligate or inappropriate consumption, rather than invested in productive activities; and (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 explaining 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 specific export sectors in which a country has a 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. In fact, several theoretical models suggest that in a scenario of severe factor-market imperfections that limit both the mobility of factors between sectors and the flexibility of factor prices, increased trade may be associated with unemployment or underemployment and, as a consequence, with income losses (see, e.g., Haberler, 1950; Edwards, 1988; Krishna and Yavas, 2005; Chang et al., 2009). We examine three forms of regulation: labor regulation, business regulation, and price regulation.

- Labor 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 labor and thus the movement of labor across sectors.
- Business regulation is represented by the business freedom index published by The Heritage Foundation.[12] 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: the total length of railway lines per square kilometer of land area and telephone mainlines per 1,000 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 hypothesize 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 to adopt more

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

efficient technologies, implying that resources are misallocated and potential gains from trade go unexploited. In addition, recent studies argue that institutions are a source of comparative advantage. Desroches and Francis (2006), for example, develop a theoretical model in which countries that have good institutions will tend to export relatively more capital-intensive (or sophisticated) goods compared to countries that have poor institutions. In their model, trade magnifies the impact of weak institutions on income, leading to greater income divergence than if countries remained in autarky. Similarly, Levchenko (2007) shows that when institutions are the source of comparative advantage, countries with good institutions gain the most from trade, while countries with bad institutions may lose as a result of trade. 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, secured by clear laws that are fully enforced by the government. The remaining eight measures are compiled from the International Country Risk Guide (ICRG), published by the Political Risk Services (PRS) Group.[13] 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 minimize 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 destabilize 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 and external and internal conflict are rescaled so that higher values always reflect higher institutional quality.

The variables, their definitions, and sources are listed in Table 8. 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 6, $\hat{\beta}_i$. 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 can thus be treated as the average impact per year. For the remaining 35 percent of the countries, we found that the estimated income effect of trade is indeed not

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

constant; nevertheless, it can be roughly interpreted as a time average over the period of 1960 to 2003. Consequently, we also use time averages for the independent variables for 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 62 countries. The country composition of the sample is given in the Appendix.

*4.2. Empirical analysis*

We start with bivariate regressions of the estimated income effect of trade on the above variables. The results of these regressions are reported in Table 9. 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. Moreover, the fact that all coefficients are significant with the correct sign implies that the individual country estimates of the effect of international trade on income (reported in Table 6) are fairly accurate. By definition, bivariate cross-sectional regressions are, however, unable to identify which of the variables are really important—that is, robust to the inclusion of other potentially relevant factors.[14] To determine which of the variables are important (or most important) 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). The general-to-specific approach is adopted here because a comprehensive theory to explain the cross-country variations in the income effect of trade does not exist. Admittedly, a criticism of the uses of a general-to-specific modeling approach is that sequential test procedures using conventional critical values may understate the true size of the joint test implicit in the search procedure. Hoover and Perez, however, argue that this applies only to undisciplined or wrongly disciplined data mining but not to a disciplined search procedure. More specifically, they show that their particular general-to-specific procedure has both a near-nominal size and high power and is therefore very effective in identifying the true parameters of the data-generating process. In addition, they demonstrate that their approach outperforms other variable-selection procedures, such as the extreme-bounds approaches of Levine and Renelt (1992) and Sala-i-Martin (1997).

**[Table 9 about here]**

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*),[15] and a sub-sample stability test (*STABILITY*) using an *F*-test for the equality of the

[14] 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.

[15] Since an estimated dependent variable may introduce heteroscedasticity into the regressions (see, e.g., Saxonhouse, 1976), it is particularly important to test for heteroscedasticity. An alternative is to use White's heteroscedasticity-consistent standard errors. Because our models are free from heteroscedasticity, the use of White's standard errors does not change the significance levels. Results are available on request.

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 10. They show clear evidence of non-normality and misspecification.

**[Table 10 about 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 10. 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, yielding 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 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. In our case, all of these tests are passed, implying five well-specified parsimonious equations, all of which are 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 11. As can be seen, the final model passes all of the diagnostic tests. Moreover, in Fig. 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 11.

**[Table 11 about here]**

**[Figure 2 about here]**

The results imply that the cross-country variations in the income effect of trade can be explained mainly, or most directly, by cross-country differences in the level of primary export dependence (measured by the share of primary exports in GDP), the level of labor market regulation (measured by the 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 0.156 percentage-point decrease in the income effect of trade, and each extra percent of labor regulation is estimated to reduce the impact of trade on income by 0.295 percentage points, whereas an increase in the property rights index by one percent raises the effect of trade on income by 0.464 percentage points.

Note that this finding can also explain why the income effect of trade is, perhaps surprisingly, negative in many countries, such as Malaysia, Mexico, and China (see Table 6): According to our data, Malaysia is heavily dependent upon primary commodity exports, Mexico is subject to excessive labor regulations (measured by the flexibility-of-firing index), and the level of property rights protection is extremely low in China.

On the other hand, 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 6). 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.

Table 12 provides some information about the performance of the variables that are omitted from the final specification. The second column reports the *t*-statistic of each omitted variable when added individually to the regression in Table 11, 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. When added individually to the final model, all omitted variables are insignificant, and several, such as per-capita GDP, secondary schooling, and business freedom, also have the wrong sign. This is in contrast to the bivariate regression results in Table 9 (where all variables are correctly signed) and suggests a high degree of collinearity. Thus, it can be assumed that several of the omitted variables are highly correlated with the variables in the final model, in turn implying that some of the excluded variables might play an important indirect role in the trade-income relationship by affecting the included variables or being affected by them. In fact, the pair-wise correlation coefficients show that regulations on firing workers are highly significantly (at the one-percent level) correlated with many of the omitted variables: GDP per capita, schooling, business freedom, rail lines, telephone mainlines, corruption, bureaucratic quality, investment profile, socioeconomic conditions, the level of democracy, and internal conflict. Similarly, property rights have highly significant correlations with all of the excluded variables except external conflict, while the share of primary exports in GDP is significantly correlated only with GDP per capita.

**[Table 12 about here]**

In Table 13, we present regressions of the income effect of trade on the most significant correlates of the variables of the final model (those which are significant at the one-percent level for at least two of the three variables). To avoid collinearity problems, we included only the primary export share as a control variable, since ln(primaryexports) is the only variable of the final specification that is not significantly correlated with most of the omitted variables. In contrast to Table 12, all variables are again significant at least at the five-percent level, with the exception of schooling and internal conflict, suggesting that these significant variables, namely GDP per capita, business freedom, physical infrastructure, corruption, bureaucratic quality, investment profile, socioeconomic conditions, democracy, and internal conflict, do in fact play an important indirect role in the long-run relationship between trade and income.

**[Table 13 about here]**

Overall, these results are consistent with the finding in Section 3.4 that trade has, on average, a negative long-run effect on income in developing countries. The GDP per-capita variable was positive and statistically significant in several specifications, indicating that the income effect of trade tends to increase as the level of development increases. Of course, this does not necessarily explain the average negative effect for developing countries, but it at least shows that there are significant differences in the income effects of trade between developed and developing economies. To explain specifically why the long-run effect of trade on income is negative for most developing countries, and why the results of this section support this finding, it is useful to recall that our final specification (in Table 11) does not include per-capita income. As discussed above, this suggests that the impact of trade on income is not directly related to the level of development. Rather, the level of development appears to play an indirect role in the trade-income relationship by interacting with the included variables and their correlates.

From this it follows that one key factor in the relationship between the level of development and the income effect of trade is labor regulation and its association with related variables, such as business regulation and the investment profile. Since many developing countries are subject to high investment risks and excessive labor and business regulations (see, e.g., World Bank, 2009), and since such factor-market imperfections may severely limit both the mobility of factors between sectors and the flexibility of factor prices, trade can lead to welfare losses in these countries, as theory suggests.

Another, and perhaps the most important, factor is the protection of property rights. This variable not only has the largest coefficient but is also highly correlated with almost all omitted variables, of which several appear to have important indirect effects on the income effect of trade. Thus, the protection of property rights captures a wide range of institutional factors that impact the income effect of trade, including investment risks, socioeconomic conditions, business freedom, democratic accountability, bureaucratic quality, and the level of corruption within the political system. Given that in many developing countries institutions are weak or non-existent, our findings are consistent with models suggesting that countries with weak institutions may lose from trade.

Finally, many developing countries are still heavily dependent on primary commodity exports. Several of them, such as Mexico, Venezuela, Zambia, and Zimbabwe, experienced long periods of stagnation, or even decline. Our results suggest that the income effect of trade is negatively associated with primary export dependence (for the reasons discussed above), which may, at least in part, explain why some developing countries experience losses from trade. However, a word of caution is needed. It does not follow from this conclusion that there is a negative relationship between the income effect of trade and natural resource abundance. Many resource-abundant countries, such as Chile, India, Indonesia, Australia, Ireland, and Norway (all of which have positive coefficients on the trade variable), have diversified their exports in the past decades in order to reduce their dependence on primary product exports. Therefore, it seems unlikely that natural resource abundance per se is negatively related to the income effect of trade, although there might be a certain correlation between natural resource abundance and primary export dependence.

## 5. Conclusion

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 75 developed and developing countries over the period or 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 0.18 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 the income effect is negative, on average.

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, or most directly, by cross-country differences in primary export dependence, labor market regulation, and property rights protection. However, it must be emphasized that there are several factors, such as GDP per capita, business regulations, infrastructure, corruption, bureaucratic quality, investment risk, socioeconomic conditions, democracy, and internal conflict, that are, on the one hand, highly correlated with the level of property rights protection, the level of labor regulation, and the degree of primary export dependence, and, on the other, also significantly associated with the income effect of trade in many specifications, suggesting that these factors play an important indirect role in the long-run relationship between trade and income.

A final conclusion is that the negative effect of trade found for many developing countries need not remain negative; it can become positive over time when certain country-specific factors determining the effect of trade change. Specifically, reforms aimed at

(i) improving institutional quality,
(ii) increasing labor market flexibility,
(iii) minimizing the regulatory burden on business, and
(iv) 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 from trade in the long run.

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

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

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**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 | | | |
| $\ln(Y)$ | $c, t$ | 0.023 | -2.23 |
| $\ln(T)$ | $c, t$ | -0.788 | -2.20 |
| | | | |
| First differences | | | |
| $\Delta\ln(Y)$ | $c$ | -9.65** | -2.46** |
| $\Delta\ln(T)$ | $c$ | -11.08** | -2.56** |

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; $\Psi_{\overline{LR}}\{H(r)\vert H(2)\}$ | 4.83** | -1.11 |
| Fisher statistics | 219.0** | 122.2 |
| CIPS statistic | -2.28** | |
| 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 $\chi^2$ with $2\times N$ degrees of freedom. It has a critical value of 193.2 (179.6) at the one (five) percent level. The relevant five (one) percent critical value for the CIPS statistic is -2.10 (-2.20). One lag was used to form the panel trace statistic. For the panel ADF statistic, the group ADF statistic, and CIPS statistic, 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*) | 0.181** | 0.159** |
| | (6.38) | (3.98) |

Notes: The dependent variable is Ln(*Y*). *t*-statistics are 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**

Estimates of the long-run impact of trade on income using different samples and data sets.

| Independent variable | Sample with 97 countries | PWT 5.6 data |
|---|---|---|
| ln(*T*) | 0.112** | 0.209** |
| | (3.71) | (20.00) |

Notes: The dependent variable is Ln(*Y*). *t*-statistics are in parenthesis. ** indicate significance at the one-percent level. There are 68 countries in the PWT 5.6 that have complete data on GDP per worker, nominal openness and the price level (GDP in exchange rate US$ relative to GDP in PPP US$) over the period 1955-1990. Following Alcalá and Ciccone (2004), we calculated real openness by multiplying (nominal) openness by the price level. The number of leads and lags in the individual DOLS regressions was determined by the Schwarz criterion with a maximum of three (five) lags for the PWT 5.6 (PWT. 6.2) sample.

**Table 6**

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** | Luxembourg | 0.5847* | 2.31 | 16.65** |
| Australia | 0.0973 | 1.10 | 4.19 | Madagascar | 0.1402* | 2.07 | 2.43 |
| Austria | 1.0333** | 5.36 | 3.24 | 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 | 2.94 |
| 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** | -8.67 | 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 |
| Chile | 1.0147** | 3.84 | 19.16** | New Zealand | 0.1406 | 0.69 | 4.48 |
| China | -0.2223** | -3.34 | 2.28 | Nicaragua | -0.0869 | -0.81 | 23.01** |
| 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 | 1.60 | 8.03* | Portugal | 0.1648 | 1.87 | 2.48 |
| Ethiopia | -0.0756 | -0.64 | 3.21 | Romania | -0.0675 | -0.22 | 8.41** |
| Finland | 0.0865 | 0.60 | 5.53 | Senegal | 0.5676** | 2.38 | 4.40 |
| France | 0.4742** | 2.94 | 3.74 | Singapore | 0.3104 | 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 | 0.1592 | 1.60 | 7.22* | Sri Lanka | -0.1101** | -3.50 | 3.31 |
| Guinea | -0.3744** | -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 | -0.6387** | -3.67 | 14.52** |
| India | 0.2060** | 9.80 | 0.83 | Thailand | 0.0537 | 0.60 | 1.04 |
| Indonesia | 0.9490** | 8.38 | 2.99 | Trinidad & Tobago | -0.0887 | -0.17 | 23.90** |
| Ireland | 0.6044** | 4.63 | 10.44** | United Kingdom | 0.4604** | 4.21 | 2.70 |
| Israel | 1.5980** | 7.94 | 3.52 | United States | 0.2224* | 2.70 | 2.45 |
| Italy | 0.3654 | 1.74 | 24.03** | Uruguay | 1.0172** | 5.78 | 4.29 |
| Jamaica | 0.5904** | 3.99 | 5.10 | Venezuela | -0.1717 | -1.50 | 12.13** |
| Japan | 1.6716** | 3.55 | 4.72 | Zambia | -0.2919** | -3.21 | 2.77 |
| Jordan | 0.7750** | 10.65 | 4.47 | Zimbabwe | -0.3792** | -6.65 | 3.87 |
| Korea, Republic of | 0.0294 | 0.72 | 3.24 | | | | |
| Developed countries | 0.7427** | 16.06 | 6.05 (average) | Developing countries | -0.0528* | -2.76 | 8.37* (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 7**

Weak exogeneity tests / long-run causality tests.

| Variable (Coefficient) | ln(*Y*) ($\alpha_1$) | ln(*T*) ($\alpha_2$) |
|---|---|---|
| $\chi^2(1)$ | 142.22 | 25.47 |
| (*p*-values) | (0.000) | (0.000) |
| Fisher statistics | 540.69 | 204.10 |
| (*p*-values) | (0.000) | (0.002) |

Notes: The number of degrees of freedom υ in the standard $\chi^2(\upsilon)$ tests corresponds to the number of zero restrictions. The Fisher statistic is distributed as $\chi^2$ with $2\times N$ degrees of freedom. It has a critical value of 193.2 at the one-percent level. The models were estimated with up to three lags.

**Table 8**

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 enrollment 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 kilometer 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 6 |

**Table 9**

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

| Variables | Estimated coefficients (t-statistics) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| ln(gdp) | 0.25** (3.42) | | | | | | | | |
| ln(schooling) | | 0.29* (2.47) | | | | | | | |
| ln(credit) | | | 0.21* (2.35) | | | | | | |
| ln(primaryexports) | | | | -0.20* (-2.25) | | | | | |
| ln(firing) | | | | | -0.49** (-3.93) | | | | |
| ln(businessfreedom) | | | | | | 0.81** (3.08) | | | |
| ln(monfreedom) | | | | | | | 1.12* (2.51) | | |
| ln(railway) | | | | | | | | 0.25** (4.80) | |
| ln(telephone) | | | | | | | | | 0.17** (3.57) |
| Adj. $R^2$ | 0.15 | 0.08 | 0.07 | 0.06 | 0.19 | 0.12 | 0.08 | 0.27 | 0.16 |
| ln(propertyrights) | 1.00** (4.35) | | | | | | | | |
| ln(corruption) | | 0.96** (4.18) | | | | | | | |
| ln(govstab) | | | 2.29** (3.18) | | | | | | |
| ln(bureaucratic) | | | | 0.58** (3.86) | | | | | |
| ln(invest) | | | | | 1.89** (3.93) | | | | |
| ln(socio) | | | | | | 1.21** (3.70) | | | |
| ln(democractic) | | | | | | | 0.100** (3.93) | | |
| ln(intconflict) | | | | | | | | 0.78** (2.93) | |
| ln(extconflict) | | | | | | | | | 1.30* (2.22) |
| Adj. $R^2$ | 0.25 | 0.22 | 0.14 | 0.19 | 0.19 | 0.17 | 0.19 | 0.11 | 0.06 |

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

**Table 10**

Diagnostic tests: general specification.

| | |
|---|---|
| Without country dummies | |
| *JB* ($\chi^2_{(2)}$) | 18.13 [0.000] |
| *RESET* ($\chi^2_{(1)}$) | 4.27 [0.039] |
| *HET* | $F(18, 43) = 0.60$ [0.881] |
| *STABILITY* | $F(16, 44) = 1.35$ [0.418] |
| With country dummies | |
| *JB* ($\chi^2_{(2)}$) | 1.24 [0.538] |
| *RESET* ($\chi^2_{(1)}$) | 0.55 [0.458] |
| *HET* | $F(21, 40) = 1.12$ [0.369] |
| *STABILITY* | $F(44, 16) = 1.17$ [0.754] |
| Number of observations | 62 |

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 11**

General-to-specific approach: final specification.

| Independent variable | Dependent variable: $\hat{\beta}_i$ |
|---|---|
| ln(primaryexports) | -0.156* (-2.349) |
| ln(firing) | -0.295* (-2.549) |
| ln(propertyrights) | 0.464* (2.182) |
| Denmark dummy | 1.595** (3.145) |
| Greece dummy | 1.616** (3.216) |
| Netherlands dummy | 1.568** (3.060) |
| Diagnostic tests | |
| Adj. $R^2$ | 0.52 |
| *JB* ($\chi^2_{(2)}$) | 1.36 [0.442] |
| *RESET* ($\chi^2_{(1)}$) | 0.10 [0.756] |
| *HET* | $F(6, 55) = 0.32$ [0.923] |
| *STABILITY* | $F(16, 44) = 1.12$ [0.675] |
| *REST* | $F(15, 40) = 0.66$ [0.848] |
| Number of observations | 62 |

Notes: *t*-statistics are 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 12**

Effects of adding further regressors individually to the Table 11 regression and correlation coefficients.

| Regressor | *t*-statistic of added variable | Correlation coefficients | | |
|---|---|---|---|---|
| | | ln(primaryexports) | ln(propertyrights) | ln(firing) |
| ln(gdp) | -0.85 | -0.44** (-3.72) | 0.74** (8.49) | -0.47** (-4.05) |
| ln(schooling) | -0.68 | -0.10 (-0.76) | 0.63** (6.18) | -0.35** (-2.83) |
| ln(credit) | 0.63 | -0.13 (-1.04) | 0.28* (2.24) | -0.30* (-2.44) |
| ln(businessfreedom) | -0.82 | -0.11 (-0.87) | 0.79** (9.48) | -0.50** (-4.42) |
| ln(monfreedom) | -0.39 | -0.15 (-1.16) | 0.62** (6.08) | -0.31* (-2.54) |
| ln(railway) | 1.19 | -0.22 (-1.77) | 0.54** (4.91) | -0.45** (-3.85) |
| ln(telephone) | -0.75 | -0.18 (-1.43) | 0.75** (8.72) | -0.47** (-4.12) |
| ln(corruption) | 0.51 | -0.20 (-1.60) | 0.63** (6.30) | -0.40** (3.32) |
| ln(govstab) | 0.43 | -0.07 (-0.56) | 0.57** (5.40) | -0.44** (-3.77) |
| ln(bureaucratic) | 0.15 | -0.21 (-1.61) | 0.73** (8.17) | -0.46** (-4.02) |
| ln(invest) | -0.06 | -0.19 (-1.49) | 0.82** (10.92) | -0.52** (-4.64) |
| ln(socio) | -1.00 | -0.19 (-1.49) | 0.83** (11.55) | -0.53** (4.78) |
| ln(democractic) | -0.07 | -0.21 (-1.62) | 0.76** (8.86) | -0.47** (-4.04) |
| ln(intconflict) | -1.27 | -0.07 (-0.54) | 0.53** (4.80) | -0.38** (-3.12) |
| ln(extconflict) | -0.08 | -0.02 (-0.13) | 0.53** (4.81) | -0.26* (-2.05) |

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

**Table 13**

Regressions of the estimated income effect of trade on the most significant correlates of the variables of the final specification.

| Variables | Estimated coefficients (*t*-statistics) | | | | | |
|---|---|---|---|---|---|---|
| ln(primaryexports) | -0.18*<br>(-2.35) | -0.19*<br>(-2.57) | -0.19*<br>(-2.55) | -0.24**<br>(-3.09) | -0.17*<br>(-2.32) | -0.16*<br>(-2.19) |
| ln(gdp) | 0.15*<br>(2.24) | | | | | |
| ln(schooling) | | 0.17<br>(1.67) | | | | |
| ln(businessfreedom) | | | 0.52*<br>(2.30) | | | |
| ln(railway) | | | | 0.14**<br>(2.80) | | |
| ln(telephone) | | | | | 0.10*<br>(2.36) | |
| ln(corruption) | | | | | | 0.58**<br>(2.71) |
| Adj. $R^2$ | 0.38 | 0.36 | 0.39 | 0.50 | 0.39 | 0.40 |
| ln(primaryexports) | -0.19*<br>(2.64) | -0.16*<br>(-2.20) | -0.16*<br>(-2.20) | -0.17*<br>(-2.26) | -0.16*<br>(-2.20) | -0.20*<br>(-2.66) |
| ln(govstab) | 1.68**<br>(2.75) | | | | | |
| ln(bureaucratic) | | 0.39**<br>(2.90) | | | | |
| ln(invest) | | | 1.36**<br>(3.21) | | | |
| ln(socio) | | | | 0.80**<br>(2.73) | | |
| ln(democractic) | | | | | 0.65**<br>(2.83) | |
| ln(intconflict) | | | | | | 0.30<br>(1.09) |
| Adj. $R^2$ | 0.40 | 0.41 | 0.43 | 0.41 | 0.41 | 0.34 |

Notes: The dependent variable is $\hat{\beta}_i$. *t*-statistics are in parenthesis. ** (*) indicate significance at the one (five) percent level. Each regression includes dummy variables for Denmark, Greece, and the Netherlands. The indicators for corruption and internal conflict are rescaled so that higher values reflect higher institutional quality.

**Fig. 1.** Group-mean estimation with single country excluded from the sample.

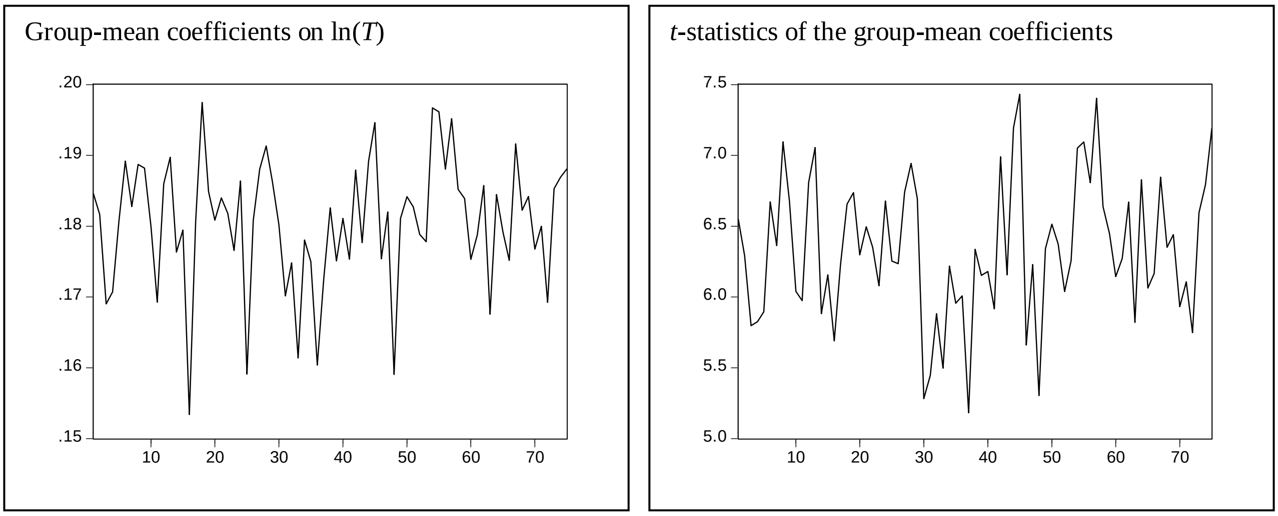


**Fig. 2.** Stability Tests. Outliers (Denmark, Greece, Netherlands) were excluded to compute the recursive residuals and the CUSUM and CUSUM of squares statistics.