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Does Trade Cause Divergence? Dynamic Panel Data Evidence

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Abstract

This paper argues that the empirical trade–growth relationship should be modelled using a dynamic panel data approach and that it is best estimated with Blundell and Bond's (1999) system-GMM estimator. This procedure remedies some econometric problems such as regressor endogeneity, measurement error and weak instruments, and allows to control for time-invariant country-specific effects such as institutions or geography. The findings are largely plausible and satisfy intuition better than previous results. They confirm the existence of a strong causal effect of trade on growth but fail to find evidence for trade as an independent factor of divergence. Hence, one cannot blame trade as such for the disappointing performance of initially poor countries.

Keywords: growth empirics, trade, convergence, generalized methods of moments.

JEL - codes: .F43, O40

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1 Introduction

This paper has two objectives. First, it argues that the empirical trade-growth relationship should be written as a dynamic panel data model and estimated using the system-GMM procedure proposed by Blundell and Bond (1998). Second, it applies that methodology to test a hypothesis frequently found in the earlier endogenous growth models or in the current popular debate on the effects of globalization, namely, that international trade is less beneficial for initially poor countries than it is for more advanced ones. Is the disappointing growth performance of initially poor countries causally related to trade liberalization or is it due to other determinants such as institutional flaws or bad governance?

Most economists agree that openness to trade delivers substantial economic benefits: according to the classical gains from trade theorem, in an undistorted static environment, international trade increases the income of a country and helps the representative consumer to attain a higher level of utility. While it is possible that the gains from trade are unevenly distributed between trading partners, there is nothing in the theory that attributes a decisive role to the initial level of per capita income. The classical theory is essentially static and requires some rather strong assumptions such as perfectly competitive markets and the absence of increasing returns to scale.[1]

A large number of *dynamic* theoretical papers studies the role of trade openness in multi-country models of endogenous growth. The older literature emphasizes the possibility that static comparative advantage may lock poor countries into specialization patterns that are not conducive to future productivity growth (Lucas, 1988; Stokey, 1991; Young, 1991). Depending on the initial value of some state variable (per capita capital stock, per capita income or the stock of human capital), countries are sorted into *low- and high-productivity-growth* groups, while under autarky, *all* countries would grow at the same rate. These papers are rather pessimistic because trade

[1] Feenstra (2004) provides an up-to-date review on the theory and empirics of international trade.

causes divergence not because of some policy failure or institutional flaws, but because initial conditions place unlucky countries onto the low-growth path. Trade leads to specialization, which then *necessarily* implies divergence.

More recently, theorists have provided more detailed arguments why trade may lead countries to specialize on low productivity growth sectors and/or why trade may induce a bias in the speed of technical change against some countries (see Gancia, 2003, and the references therein). These papers study the effect of trade liberalization when countries are affected by other distortions in the form of institutional flaws, such as poor enforcement of property rights. The theory of the second best implies that in this case trade liberalization can either aggravate or mitigate the adverse effects of the remaining distortions, so that it *may* cause some countries to grow more slowly than others. This stream of research is more optimistic, because it links detrimental effects of trade to the existence of some distortions which can – in principle – be removed by institutional reforms.[2] Since initially poor countries tend to be those with poor institutions, the empirical question arises: how can the causal effects of initial conditions and institutional quality be appropriately separated? The present paper looks at the interaction between initial income and trade openness and concludes that trade has not been an engine for divergence in the last-half decade.

In order to estimate the effect of trade on income, empirical researchers typically run cross-country regressions which are meant to explain income by some measure of trade openness. This empirical strategy is required, because the direct growth promoting effects of trade – essentially through spillovers are hard to observe empirically.

[2] Of course, in endogenous growth models trade is not necessarily an engine of divergence. The Schumpeterian research tradition typically concludes that trade leads to convergence. In contrast to Lucas (1988) type models, where there are no international technology spillovers, Grossman and Helpman (1991) and the subsequent R&D based endogenous growth literature, allow for some kind of technology diffusion: either through the public good character of knowledge (chapter 9), or through the possibility of poor countries to imitate leaders (chapter 11). In all of these models, trade leads to income convergence. There are also different approaches such as Acemoglu and Ventura (2002) or Felbermayr (2004) where trade does not play any role in divergence or convergence.

However, in order to identify the *causal* effect of trade, the researcher has to make sure that the measure of trade used is orthogonal to the dependent variable. The first paper that uses a convincing instrument to achieve this aim is the one by Frankel and Romer (1999), who apply a gravity approach to bilateral trade data and construct trade shares that are by construction orthogonal to income but still strongly correlated to the actual trade shares. Acemoglu et al. (2001) instrument the quality of institutions with the colonial past of a country. Since then, many other empirical papers have studied the interaction between per capita income, trade integration and institutions. However, the vast majority of these models stick with static cross-section regressions, which assume that all countries are on their respective balanced growth path.[3] This assumption is likely to be violated, at the least because the international trading system has undergone major changes in the last few decades and the speed of convergence, typically estimated in the empirical literature, is rather small. I reconsider Frankel and Romer's (1999) model (henceforth F&R), and, using their instrument for trade, find that it actually performs better in a standard Barro-type cross sectional growth regression, where conditional convergence is allowed for by including initial income in the list of covariates.

The present paper uses a rather novel approach to modelling dynamic panel data, namely, the system-GMM estimator developed by Blundell and Bond (2000). In this setting, country-specific fixed effects such as geographical or time-invariant institutional characteristics are controlled for by first-differencing. Endogenous regressors, in levels as well as in first-differences, are instrumented by their lagged levels. This approach allows (i) to cast the trade-growth relationship as a *dynamic* model, (ii) to appropriately

[3]Static regressions focus on the cross-section and relate the level of current per capita income to trade openness and other regressors. These *income regressions* can be differenced and read as growth regressions. The more conventional empirical approach regresses the rate of per capita income growth on the level of initial income, current trade and other things. These dynamic *growth regressions* admit a fixed point and can therefore be read as income regressions. In the remainder, we use the terms 'growth regression' and 'income regression' interchangeably.

deal with omitted variable bias due to time-invariant *country-specific fixed effects* such as geographical or institutional characteristics and (iii) to tackle the issue of *regressor endogeneity* and *measurement error.*

This paper argues that the proposed more general model has properties superior to those of F&R-type models. Using the system-GMM estimator, we find quantitatively plausible and statistically significant evidence for a positive effect of trade openness on income. This confirms F&R's results and shows that they are broadly robust even if *all* time-invariant country-specific effects (such as geographical or institutional features) are controlled for, *all* potentially endogenous regressors are appropriately instrumented, and the relationship is specified as a *dynamic* equation.

In the proposed framework, the coefficient on instrumented trade is somewhat smaller but certainly not larger than in the uninstrumented case, suggesting that higher income is associated with higher trade. Paradoxically, F&R find just the contrary and attribute their counterintuitive result to sampling error or attenuation bias due to measurement problems. Irwin and Terviö (2002) use F&R's instrument on historical data and find this anomaly again; hence sampling error is unlikely to be the answer. At the same time, as F&R argue, measurement error must be implausibly large to overcompensate the expected upward bias in the uninstrumented coefficient. In the present paper, where a dynamic panel data approach is used and a GMM-type instrumentation strategy is chosen, there is no evidence that instrumentation increases the effect of trade on income. This finding is robust to the exact methodology and implies that endogeneity bias is important relative to measurement or sampling error. However, when a general indicator for a country's stance of trade policy is added to the specification (the Sachs-Warner index), the benefits from openness seem to materialize even without actual trade really taking place. This suggests, that the trade share is indeed a noisy proxy for the overall effect of openness on income.

Besides advocating the use of a dynamic panel data model, the present paper answers a highly relevant question. After controlling for institutional or geographical characteristics, do initial conditions matter for the causal effect of international trade on growth? This question is of political interest, since critics of globalization often argue that market integration is inherently biased against poor countries and that institutional reform will not be able to remedy this problem. Our results yield little systematic evidence that trade is *causally* responsible for the unlucky experiences of divergence clubs members. If anything, trade seems to be particularly helpful for initially poor countries. This effect is strongest, when total factor productivity instead of real GDP per capita is used as the dependent variable. The results are not compatible with Lucas-type development traps and the feeling of many globalization critiques, that trade is on its own responsible for the poor fates of many poor countries. Hence, it seems that backwardness is no handicap for the causal effect of trade on growth. In an indirect way, the paper provides evidence for the importance of country-specific effects for the curse of economic development. To my knowledge, the present study is the first to study the *causal* effect of trade in the development process.

The remainder of the paper is organized into five sections. The first section explains the empirical model and discusses a consistent way to estimate it. The second section deals with data issues, while the third one revisits F&R's results. Section five checks whether trade is causally related to the bad fortunes of initially backward countries. Finally, the papers offers conclusions and some outlook on future research.

2 Consistently estimating the growth-openness nexus

2.1 The empirical model

The reduced form model that Frankel and Romer (1999) [F&R] and many others have estimated can be stated as

$$y_i = \alpha + \tau T_i + \mathbf{X_i'}\boldsymbol{\beta} + u_i, \qquad i = 1, ..., N. \tag{1}$$

The dependent variable is the log of real GDP per capita, y_i. T_i is the share of trade over GDP (exports plus imports over GDP), $\mathbf{X_i}$ is a vector of other covariates, and u_i is the error term. In the original F&R model, the vector $\mathbf{X_i}$ contains the size of the population and the land surface. These controls capture the idea that larger countries depend less on international trade since they benefit from larger home markets.

The equation is estimated for a cross-section of countries, with observations indexed by $i = 1, ...N$, and N is the number of countries. The coefficient of interest is τ, the semi-elasticity of income with respect to the trade share. In the present context, it has to be interpreted as follows: an increase in the trade share by 1 *percentage point*, increases per capita income by τ *percent*. Subsequent researchers have included more and different controls, but kept the basic specification. The assumption behind equation (1) is that all countries are in their respective steady states, so that there is no need to estimate a dynamic model (see below). To the extent that this assumption is violated, equation (1) is misspecified and the estimate of τ will be biased, regardless of whether and how other econometric problems, such as the endogeneity of T_i and other variables, are dealt with.

The choice of specification (1) is surprising, given the robust econometric evidence that adjustment dynamics are quantitatively important. If one wants to allow for conditional convergence dynamics, at least two time periods must be considered. Linearizing the solution to an augmented neoclassical growth model around its steady state yields the following specification (Mankiw et al., 1992, p. 423):

$$y_{it} = \gamma y_{i,t-1} + \tau T_{it} + \mathbf{X}'_{\mathbf{it}}\boldsymbol{\beta} + \boldsymbol{\delta}_{\mathbf{t}} + \eta_i + v_{it}, \qquad |\gamma| < 1, i = 1, ..., N \text{ and } t = 1, ..., \Theta, \quad (2)$$

where t indexes time and Θ is the date of the last observation. All variables are averages over five-year means to avoid business cycle effects; thus the models covers a total period of 5Θ years length. The log of real per capita GDP, y_{it}, follows an AR(1) process with a persistence parameter γ. The error term is assumed to have the standard error term components structure. The term $\tau T_{it} + \mathbf{X}'_{\mathbf{it}}\boldsymbol{\beta}$ determines the level of the steady state that country i is converging to. In order to account for global cycle effects, the oil crises etc. and to allow for continuous growth a comprehensive set of period dummies ($\boldsymbol{\delta}_\mathbf{t}$) is included. Moreover, for a steady state to exist, we require that γ be smaller than unity in absolute terms.

Subtracting $y_{i,t-1}$ on both sides of the equation shows that the above equation can also be written in its more conventional form

$$\Delta y_{it} = (\gamma - 1)\, y_{i,t-1} + \tau T_{it} + \mathbf{X}'_{\mathbf{it}}\boldsymbol{\beta} + \boldsymbol{\delta}_\mathbf{t} + \eta_i + v_{it}, \qquad |\gamma| < 1, i = 1, ..., N \text{ and } t = 1, ..., \Theta, \tag{3}$$

where Δy_{it} denotes the rate of growth of real GDP per capita and the absolute value of $\gamma - 1$ measures the rate of convergence, with the half time of the adjustment process being given by $\ln 2/(1-\gamma)$. The smaller this rate, the longer it takes for an economy to come arbitrarily close to its respective steady state. If γ is close to unity, the rate of convergence is small, and it will take a long time before the new (higher) steady state is reached, thus giving rise to a protracted period of higher growth. In equation (1), per capita income can only grow if T_i (or any other growth promoting covariate) increases over time. Specification (2) allows that the income effect of a once for all change in T_i is spread out over time. Hence, in equation (2) we can distinguish between the instantaneous growth effect τ and a long-run effect $\tau^{SS} = \tau/(1-\gamma)$. The first measures the additional growth that a one-time increase in T_i guarantees during the current period; the latter provides the effect on steady state income once the adjustment dynamic has come to an end. The larger γ is, the longer will a beneficial effect of an increase in T_i last, and the larger the effect on steady-state income will be. In the limit case, where $\gamma = 1$, a one-time increase in T_i lifts the growth for all t.

Estimating equation (2) poses some important econometric problems. First, it is almost impossible to control for all determinants of growth. Some of them, for example initial efficiency[4], are simply not observed. Others are observed, but there is a large

[4] If initial efficiency (TFP) is not controlled for, the error term will be correlated to initial GDP

degree of uncertainty on how to measure them, for example institutional quality or geographical variables. Trivially, initial efficiency and geographical variables are time invariant and may be conveniently captured by the country-specific fixed effects η_i. Many institutional features also do not change very much over the period that a typical growth model considers (e.g. 1960-2000). Moreover, the time series available for most indexes of institutional quality are very short. All this suggests, that it may be a good idea to relegate institutions to the η_i terms. However, one must make sure that the method chosen to estimate (2) is consistent even if $E\left[y_{i,t-1}\eta_i\right] \neq 0$ for some i, i.e. if at least some of the time invariant characteristics of the countries are endogenous. This rules out the random effects estimator, which is only consistent if individual effects are uncorrelated with the other regressors.

However, even if $y_{i,t-1}$ and η_i are not correlated, if the number of time periods $\Theta + 1$ does not approach infinity (which it clearly does not in the present model where $\Theta + 1 = 8$), then estimation by fixed effects or random effects is not consistent (even as N goes to infinity), see Nickel (1981). Monte Carlo simulation shows that for panels with a comparable time dimension, the bias on the coefficient on the lagged variable can be significant, although the bias for the coefficient on the other regressors tends to be minor, in particular if N is quite large. In the context of growth regressions, where N is typically not larger than 100, the bias may still be sizeable.

A major concern in the recent literature has to do with the endogeneity of regressors. Measuring the covariates at the beginning of the sample period may provide some help on this front; however, this is often not desirable (e.g. for flow variables such as the investment rate) or not practicable (when there are no observations at the beginning of the period). The literature has devoted much effort to finding clever instruments for endogenous regressors; F&R (1999) and Acemoglu et al. (2001) are prominent recent examples. However, if several regressors are to be instrumented, the requirements for

per capita and bias the estimated convergence rate and the other coefficients downward (see Mankiw et al., 1992, p. 424).

any proxy to pass as a valid instrument become more stringent (Staiger and Stock, 1997). Hence, one would need an empirical methodology which can deal with the endogeneity of a whole host of regressors at the same time.

2.2 System-GMM estimation

One prominent way to address the problems enumerated above has been through first-differenced generalized method of moments estimators applied to dynamic panel data models. The relevant estimator was originally developed by Holtz-Eakin et al. (1988) and Arellano and Bond (1991). It corrects not only for the bias introduced by the lagged endogenous variables, but also permits a certain degree of endogeneity in the other regressors. The approach was introduced into the growth literature by Caselli et al. (1996). Since then, similar techniques have been applied in growth research by Benhabib and Spiegel (2000), Forbes (2000) and Levine et al. (2000) among many others.

The basic idea of the GMM approach is to write the regression equation as a dynamic panel data model (our equation (2)), take first differences to remove unobserved time-invariant country-specific fixed effects and then instrument the right-hand-side variables in the first-differenced equation using levels of the series lagged two periods or more, under the assumption, that the time-varying disturbances in the original levels are not serially correlated. Caselli et al. (1996) argue, that this procedure has important advantages over simple cross-section regressions and other estimation methods for dynamic panel data models. First, estimates will no longer be biased by any omitted variables that are constant over time. Second, the use of instrumental variables allows parameters to be estimated consistently in models which include endogenous right-hand-side variables, such as openness or the investment rate. Moreover, as Bond et al. (2001) explain, the use of instruments allows consistent estimation even in the presence of measurement error.

The procedure used by Caselli et al. (1997) is known in the literature as first-differenced GMM. Differentiation of equation (2) leads to[5]

$$\Delta y_{it} = \gamma \Delta y_{i,t-1} + \tau \Delta T_{it} + \Delta \mathbf{X}'_{\mathbf{it}} \boldsymbol{\beta} + \Delta \boldsymbol{\delta}_{\mathbf{t}} + \Delta v_{it}, \tag{4}$$

for $i = 1, ..., N$ and $t = 3, ..., \Theta$. Arellano and Bond (1991) make the rather conventional assumptions that $E[\eta_i] = E[v_{it}] = E[v_{it}\eta_i] = 0$ for $i = 1, ..., N$ and $t = 3, ..., \Theta$. Moreover, they require that the transient errors are serially uncorrelated $E[v_{it}, v_{i,t-s}] = 0$ for all $s \geq t$ and that the initial conditions are predetermined by at least one period: $E[y_{i,1}v_{it}] = 0$ for $i = 1, ..., N$ and $t = 3, ..., \Theta$.

Together, these assumptions imply the following $m = \frac{1}{2}(\Theta - 1)(\Theta - 2)$ moment restrictions

$$E[y_{i,t-s}\Delta v_{it}] = 0 \text{ for } t = 3, ..., \Theta \text{ and } s \geq 2. \tag{5}$$

These are the moment restrictions exploited by the standard linear first-differenced GMM estimator, implying the use of lagged levels dated $t-2$ and earlier as instruments for the equations in first-differences.[6] This yields a consistent estimator of γ as $N \to \infty$ with Θ fixed. Arellano and Bond (1991) provide an appropriate test to check the crucial assumption $E[v_{it}, v_{i,t-s}] = 0$, i.e. that there is no serial correlation in the errors in levels.[7] As long as $\Theta > 4$, the model will be overidentified. Then, a Sargan test can be run to test for the validity of the overidentifying restrictions.

However, there is a major drawback with the method adopted by Caselli et al. (1996). Since time series of income per capital are typically rather persistent and the

[5] Note that in first differences predetermined variables (such as $y_{i,t-1}$) become endogenous.

[6] Typically, all available lags are used as instruments, this guarantees maximum efficiency. The procedure also implies that fewer instruments are available with earlier observations.

[7] Arellano and Bond (1991) demonstrate that a test of the null hypothesis that the errors in the differenced equation are not second order serially correlated, is equivalent to testing for first order serial correlation in the errors in levels. We refer to this test as $m2$. AR(1) is expected in first differences, because $\Delta v_{it} = v_{it} - v_{i,t-1}$ should correlate with $\Delta v_{i,t-1} = v_{i,t-1} - v_{i,t-2}$ since they share the same $v_{i,t-1}$ term. But higher-order autocorrelation indicates that some lags of the dependent variable, which might be used as instruments, are in fact endogenous, thus bad instruments: that is, $y_{i,t-s}$,where s is the lag, would be correlated with $v_{i,t-s}$, which would be correlated with $\Delta v_{i,t-s}$, which would be correlated with $\Delta v_{i,t}$ if there is AR(s).

number of time series observations in growth panels is limited, the first-differenced GMM estimator is poorly behaved. The reason is that, under these conditions, lagged levels of the variables are only weak instruments for subsequent first-differences, which would have close to random walk properties. Blundell and Bond (1998) have proposed a system GMM estimator that exploits an assumptions about the initial conditions to obtain moment conditions that remain informative even for persistent series. Bond et al. (2001) argue that the necessary restrictions are consistent with the standard empirical growth framework and recommend the system-GMM estimator as the best available unbiased panel data estimator in the context of growth regressions.

To obtain a linear GMM estimator better suited to estimating autoregressive models with persistent panel data, Blundell and Bond (1998) consider the additional assumption that $E\left[\eta_i \Delta y_{i2}\right] = 0$ for $i = 1, ..., N$. Combined with the assumptions above, this assumption yields $T - 2$ further linear moment conditions

$$E\left[u_{it} \Delta y_{i,t-1}\right] = 0 \text{ for } i = 1, ..., N \text{ and } t = 3, 4, ..., \Theta, \tag{6}$$

where $u_{it} = v_{it} + \eta_i$. These allow the use of lagged first-differences of the series as instruments for equations in levels, as suggested by Blundell and Bond (1998). As an empirical matter, the validity of the additional instruments can be examined by conventional tests of over-identifying restrictions, such as the Sargan test. This test checks whether the instruments, as a group, appear exogenous. Conditions (5) and (6) provide a stacked system of $(\Theta - 2)$ equations in first-differences and $(\Theta - 2)$ equations in levels, corresponding to periods $3, ..., \Theta$ for which observations are observed. The calculation of this system GMM estimator is discussed in more detail in Blundell and Bond (1998).[8]

Bond et al. (2001) apply the system GMM estimator to an empirical growth model.

[8] The regression results in the present paper are computed in STATA 8, using a program that David Roodman from the Center for Global Development (CGD) has generously provided. The program used to estimte the system-GMM model is available on `http://fmwww.bc.edu/repec/bocode/x/xtabond2.ado.`

They show that condition (6) requires that $E[\eta_i \mathbf{\Delta X_{it}}] = 0$ for all t and argue that it looks reasonable to assume that first differences in typical left-hand-variables such as investment rates are uncorrelated with country-specific effects. Moreover, if these first-differences were correlated with country-specific effects, this would have implausible long-run implications. Note that the assumption $E[\eta_i \mathbf{\Delta X_{it}}] = 0$ does not imply that country-specific effects do not play any role in income determination. They do play a role for the steady-state level of income per capita, conditional on initial income and other steady-state determinants like investment, schooling or openness.

3 Data issues

3.1 Definitions and data sources

In the models discussed above, the vector $\mathbf{X_{it}}$ summarizes a host of determinants that determine a country's steady state level of per capita income. To fill this vector, the empirical analysis draws on widely used standard data sets. Most variables come from the Penn World Tables 6.1 compiled by Heston et al. (2002). Data on education comes from Barro and Lee (1997, 2000). We use average years of schooling in the total population over 25. The trade share (exports plus imports over GDP) is taken from Heston et al. (2002). We sometimes use a second measure of openness, the Sachs-Warner index, as updated by Wacziarg and Welch (2003). Moreover, we need a measure of total factor productivity (TFP); section 3.3 provides the details on how this measure is computed.

The data is organized in panel format. The time dimension comprises five years averages, in order to avoid contamination by business cycle effects. Since Islam (1995) and Caselli et al. (1996) this is a standard convention in dynamic panel data studies. In order to achieve a more balanced structure of the panel, the first period used is 1960-64; the last period is 1995-99 so that there $\Theta = 8$. Moreover, only those countries are kept which we observe at least in five connected time periods.[9] This eliminates all those countries that have been newly created since 1990 and a substantial number of countries that have started reporting only recently. Incidentally, the list of countries used is almost identical to the 98 country sample investigated by Frankel and Romer (1999). They argue that this sample includes only countries with reasonable data quality and excludes those countries, whose income is determined by idiosyncratic factors. Most importantly, the sample excludes most nations whose exports are predominantly made up by oil.[10] The number of countries that enters in our regressions differs slightly from specification to specification. Table A1 in the appendix provides the summary statistics for the data, Table A2 indicates the exact sample composition that underlies the regressions.[11]

3.2 Measuring openness

Rodriguez and Rodrik (2001) distinguish between two broad measures of openness: a first continuous measure that is directly related to actual trade flows, and a second binary one, which records whether a country is in principle open or not. We refer to the first measure as *revealed* openness and distinguish between the nominal trade share and a measure called real openness. The second type of measure, the well-known Sachs-Warner index, captures *political* openness.

The measure of international trade used in almost all empirical work on the effect on trade on income or growth is nominal imports plus exports relative to nominal GDP, usually referred to as openness or simply as the *trade share*. It has been used in a large number of studies, from which the papers by Romer and Frankel (1999), Alesina et al. (2000) and Irwin and Terviö (2002) may be the most well known. Alcalá and Ciccone (2004) argue that in the context of cross-country productivity studies, there

[9] This procedure has the drawback of wasting the available observations 1950-1960.

[10] From the OPEC members, only Algeria, Indonesia and Venezuela are in the sample. We conduct sensitivity analysis to check whether those countries are important for the results.

[11] The data base used in the analysis can be obtained from the author at `gfelberm@iue.it`.

are sound theoretical reasons why this measure may result in a misleading picture of the productivity gains due to trade. If trade increases productivity but these gains are larger in the tradeable goods sector than in the non-tradeable goods sector, productivity growth due to higher trade will not be necessarily associated with higher openness. The reason is that the relatively greater productivity gains in manufacturing lead to a rise in the relative price of services, which may result in a decrease in openness. To remedy this problem, Alcalá and Ciccone propose a different measure that they call *real openness*. Real openness is defined as imports plus exports in exchange rate US$ relative to GDP in purchasing power parity US$. Using real openness instead of the nominal trade share as a measure of trade eliminates distortions due to cross-country differences in the relative price of non-tradeable goods. In the data, real openness is obtained by multiplying openness by the price level. In the present paper, real openness is the preferred index when we look at total factor productivity, and the nominal trade share is preferred when we look at GDP per capita. With slight abuse of terminology, we refer to Alcalá and Ciccone's measure as to the 'real' trade share.

Another indicator for economic openness is the Sachs-Warner (1995) index (SWI). The SWI is a binary variable that classifies an economy as closed if one of the following criteria is met: (i) the black market premium is larger than 20 percent, (ii) the government has a purchasing monopoly on a major export crop and delinks purchase prices from international prices, (iii) the country is socialist, (iv) own-import-weighted average frequency of non-tariff measures (licenses, prohibitions, and quotas) on capital goods and intermediates larger than 40 percent, and (v), the own-import-weighted average tariff on capital goods and intermediaries is greater than 40 percent. The SWI indicator has been recently extended by Wacziarg and Welch (2003) and now covers a longer time span and a larger number of countries.

Since the SWI index is a binary measure, it provides information on the degree of trade-friendliness of a country's institutions rather than on the importance of trade as such. In cross-country regressions, it is therefore no wonder that introducing other measures of institutional quality undoes the significance of the Sachs-Warner index, as other institutional variables are likely to be highly collinear to the SWI (Rodriguez and Rodrik, 2001). Compared to the continuous openness index à la Frankel and Romer, the SWI may have the advantage that its endogeneity to economic outcomes is less problematic. In the paper, we use the SWI as an alternative measure of openness to trade.

3.3 Construction of TFP measures

Growth accounting studies such as Hall and Jones (1999) show that cross-country income differences are to a large extent due to differences in total factor productivity (TFP). F&R do not find strong evidence for trade to affect income through TFP. In order to check this result in our more general framework, we need to construct an appropriate TFP measure.

Following Klenow and Rodriguez-Clare (1997) as well as Benhabib and Spiegel (2004), total factor productivity is estimated in the following way. Assuming that initially all countries have been in their respective steady states, and using the simple closed economy Solow model, initial capital stocks (as of 1960) of country i, K_{i0}, are calculated according to

$$\frac{K_{i0}}{Y_{i0}} = \frac{\overline{I/Y_i}}{\gamma_i + \delta + n_i}, \tag{7}$$

where $\overline{I/Y_i}$ is the average share of physical investment in output from 1960 to 2000, γ_i represents the growth rate of output per capita over that period, n_i is the average growth rate of population and δ is a depreciation rate, assumed common to all countries, and set equal to 0.03. Given initial capital stocks estimates, the capital stock of country i in period t satisfies

$$K_{it} = \sum_{j=0}^{t} (1-\delta)^{t-j} I_{ij} + (1-\delta)^t K_0 \text{ for all } t. \tag{8}$$

Next, total factor productivity (TFP) is typically computed using a constant returns to scale Cobb-Douglas production function with the capital share set to 1/3. For country i at time t the log of TFP, a_{it} can be written as

$$a_{it} = y_{it} - \frac{1}{3}k_{it} - \frac{2}{3}l_{it}, \tag{9}$$

where k_{it} and l_{it} denote the logarithms of the capital stock and the working population, respectively. All the relevant data for this exercise come from the Penn World Tables 6.1. Working population has been constructed by computing the ratio between real GDP per capita and real GDP per worker and then multiplying by population.[12]

Table A2 in the appendix shows the results for 102 countries. The results are almost exactly identical to Benhabib and Spiegel, who have used the same data. As in their analysis, a couple of countries have experienced negative TFP growth over the period. With the exception of Venezuela, all these countries lie in subsaharan Africa. With the exception of Nigeria, negative TFP growth is coupled with capital shallowing – i.e. a negative growth rate of the K/L ratio. At the same time, on the other extreme of the distribution, strong positive TFP growth goes hand in hand with capital deepening. Most star performers in terms of TFP growth lie in south-east Asia, but there are some noteworthy exceptions in subsaharan Africa: Botswana (BWA), the Republic of Congo (COG) or Mauritius (MUS). Some exceptions notwithstanding, most countries featuring a TFP growth rate larger than the US rate, have been able to close the gap to the US.

The following section reviews F&R results and argues that their equation may be seriously misspecified. Consequentially, an explicitly dynamic estimation procedure is needed.

[12] Note, that the farther we move away from the initial starting point, the less important will the (implausible) assumption be, that initially all economies have been in their respective steady states.

4 Revisiting Frankel and Romer (1999)

F&R estimate the causal effect of changes in the nominal trade share on real per capita income in the 1985 cross-section. The problem is that in most economic models, trade and openness are both endogenous variables so that causal inference requires the use of instruments. The contribution of F&R's paper lies in the discovery of a clever instrument for trade: first, they use bilateral trade data to estimate a gravity equation that explains trade (exports plus imports) between country i and country j as a function of geographical variables only (such as distance, the land area, population etc.), i.e. deliberately omitting the income variables. Then, F&R retrieve the predicted bilateral trade volumes and aggregate over j so as to construct total trade volumes for any country i. In this way, they arrive at a measure of trade, which is by construction orthogonal to income but still strongly correlated with the actual trade share and is therefore a valid instrument. Their findings are (i) trade *causes* income, but the statistical significance of the effect is 'modest', (ii) the OLS estimate seems to *underestimate* the effect of trade. Irwin and Terviö (2002) run the F&R model on historical data and obtain results very similar to the ones found by F&R, in particular, IV estimates are consistently larger than OLS estimates.

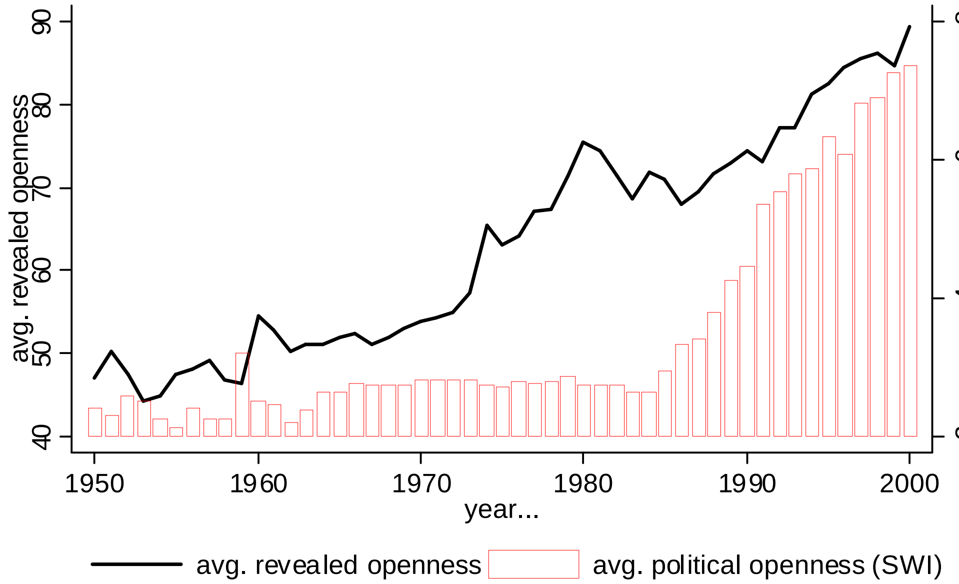
This section revisits F&R. It reproduces their baseline results for a slightly different sample of countries and the up-to-date version of the Penn World Tables but uses exactly the same instrument for the trade share. The estimates are almost identical to those found by F&R. However, when initial GDP is added to the regression, the regression turns out to perform better. In the standard F&R model, Rodriguez and Rodrik (2000) have shown that the positive causal effect of trade on income vanishes once geographical latitude is added to the regression. Irwin and Terviö (2002) show that this effect does not depend on the particular subperiod under consideration.[13]

[13] Acemoglu et al. (2001) argue that latitude is itself a good proxy for institutional quality, because European settlers chose to install good institutions only in those colonies where the climate was

This section shows, that geographical latitude does not destroy the causal effect of trade if a dynamic specification is chosen. As a consequence, the dynamic specification may not only be desirable from a theoretical point of view, but may also improve the robustness of the results.

Figure 1

The evolution of average revealed and average political openness



Source: Heston et al. (2002) and Wacziarg & Welch (2004).

While the title of F&R's paper is '*Does trade cause growth*', the authors deviate from the canonical empirical growth models surveyed e.g. by Barro and Sala-ì-Martin's (1995). In these models, growth rates are regressed on initial income and on a host of other variables which determine the steady state of an augmented neoclassical growth model. Clearly, if the date at which the countries in the cross section have started adjusting towards their respective steady states lies sufficiently far in the past, initial

convenient for them, i.e. in places relatively far from the equator. This finding has given rise to a heated debate on whether institutions or trade are the more important determinants of long run income levels. In line with their earlier result, Rodrik et al. (2002) argue that institutions are key, while Sachs (2003) replies that geography is more important. In all these papers, the original F&R framework is extended to different measures of institutional quality or, more directly, geographical variables themselves.

conditions should matter little in explaining current per capita real income. However, the evolution of the world economy featured some important disruptions prior the date (1985) at which F&R anchor their analysis and some of the shocks have occurred in the global trading system. On its right axis, Figure 1 shows that in the last half-decade the share of countries, classified as open by the Sachs-Warner index has risen dramatically from something slightly above 20% to 75%. Most of the increase has materialized after 1985, when many developing countries joined the global economy. Prior to that date, the set of open countries more or less coincided with the group of OECD countries. However, figure 1 clouds the fact, that prior to 1985 many countries in the sample switched from open to closed and vice versa, without there being a universal trend towards more liberalization. In fact, Wacziarg and Welch (2004) identify 16 countries, most of them in Central and South America, which experienced periods of temporary liberalization between 1950 and 1985, some countries even switched their openness status more than once. Hence, it would be wrong to conclude from Figure 1 that the time span before 1985 was one where no trade-related shocks occurred.

Moreover, if one turns to the nominal trade share, it is clear that the time prior to 1985 was certainly not a period of tranquility. Average openness of countries peaked in 1980 after rising quite strongly, and then slided back again. The peak was due to the strong rise in the price of oil, which in the short-run inflated the value of world trade.[14] Typically, empirical estimates of the speed of conditional convergence are rather small (Barro and Sala-ì-Martin, 1995) and lie between 2% and 3% (implying half-lives of between 35 and 23 years). Moreover, many endogenous growth theories imply rather small rates of convergence, too (see, e.g. Steger, 2003). Hence, it may be unrealistic to assume that in 1985 countries have been even close to their respective steady states.[15] It is then necessary, to cast the econometric model in a dynamic framework. Besides

[14] The average 'real' trade share shows a similar pattern.

[15] Mankiw et al. (1992) warn that the static model is *"...valid only if countries are in their steady states or if deviations from steady state are random"*.

these principal considerations, allowing for conditional convergence in the formulation of the model has important effects on the results, as the next paragraphs try to show.

To see that the F&R model is probably misspecified, we replicate their exercise with our data, check Rodriguez and Rodrik's (2000) critique, reformulate the econometric framework as a standard cross-sectional growth model and check Rodriguez and Rodrik's critique again. F&R run their regressions for a restricted sample of 98 countries, for which they believe that data quality is higher. This data excludes oil-producing countries and countries so small that their incomes are likely to be determined by idiosyncratic factors. Our sample has 101 observations; the vast majority of which are also present in F&R's 98 countries sample. However, we do not use data from the Penn World Tables 5.6. but from the corrected and updated version 6.1. Table A1 in the appendix shows the exact list of countries covered.

Column (1) and column (2) in Table 1 replicate the results of F&R's analysis. Column (1) presents the results of a simple OLS regression of the log real per capita GDP in 1985, on the nominal trade share, the log of population and the log of land area, where all regressors are measured at time 1985. F&R control for population, because firms in more populous countries depend less on the international market place to conduct their business. Jointly with the log of land area, this amounts to controlling for population density. Turning to the regression results, the coefficient on nominal openness is 0.91 and highly significant. There is also evidence for a scale effect, while the coefficient on land surface is not significant.[16] Column (2) reproduces F&R's core results, namely, (i) that component of the trade share which is orthogonal to income is positively related to income (i.e. trade causes higher income), the estimate is large, but the coefficient is only significant at the 10% level (the P-value being 7.7%) (ii) the IV estimate is by a factor of 2.5 larger than the OLS one. The scale effect remains significant. The F-test at the bottom of Table 1 tests the overall significance of the first

[16] These results are almost identical to those exhibited in F&R's Table 3, column (3).

stage regression, in which actual nominal openness is regressed on F&R's constructed trade share, on population and on land area. The results are not exactly identical to those obtained by F&R (Table 3, column (4)), because the sample and the data are slightly different. However, the estimated coefficients, the associated standard errors and the R^2 are very close.

While F&R's framework has the advantage of parsimony, it is likely to suffer from omitted variables bias. In particular, institutional and geographical characteristics are missing from the equation. Rodrik and Rodriguez (2000) add geographical latitude to the model and find that the effect of trade disappears. Column (3) shows what happens if latitude is introduced into the regression. Now, the trade share is statistically not distinguishable from zero and enters the equation with the wrong side. Strikingly, income is rather well explained by a model that contains only latitude and a constant.

Both the F&R specification and the R&R amendment assume that all countries in the sample are on their balanced growth paths. To the extent that they are in the process of convergence, the F&R and R&R equations are misspecified. The remaining columns of Table 2 show the results of prototype Barro-type growth regressions, where equation (2) is estimated for two time observations only:

$$y_{i,1985} = \alpha + \gamma y_{i,1960} + \tau T_{i,1985} + \mathbf{X}'_{\mathbf{i,1985}}\boldsymbol{\beta} + u_i, \qquad i = 1, ..., N. \tag{10}$$

The log of initial per capita income $y_{i,1960}$ (1960) is added to the equation and the set of controls now includes the log of years of schooling in 1960 to provide an additional proxy for the steady state that the respective economies are converging to. Column (4) provides a baseline OLS regression. Initial income and schooling enter with the expected signs and magnitudes. Moreover, compared to the pure cross-section, the Barro-type framework yields a precisely estimated trade coefficient which is much lower than the one in Column (1). This is no surprise, since the coefficient in Column (1) is not estimated consistently due to omitted variable bias. In the univariate case (where the vector $\mathbf{X_i}$ is empty), the bias from omitting $y_{i,0}$ is $\gamma \text{cov}(y_{i,0}, T_i) / \text{var}(T_i)$. Hence, if initial income and trade in 1985 are positively correlated, and $0 < \gamma < 1$ (as in virtually all empirical growth studies) the bias is positive. In other words, it is not high openness in the year of 1985 which is responsible for high income in 1985, but the fact that the country was already rich to start with, which, in turn, is a function of the trade share at that time. Column (5) is the IV counterpart to Column (4). Now, in contrast to F&R's baseline equation, the trade coefficient is significant at the 5% level (P-value of 2.7%). Moreover, adding latitude does no longer undo the statistical significance of the trade coefficient, nor does it change its sign. Interestingly, latitude is no longer significant. Since latitude and initial GDP are closely correlated (the unconditional correlation is about 62%), statistical inference may suffer from near collinearity. From an interpretative point of view, latitude and initial income play a similar role in explaining income differences and, to the extent that they are strongly correlated to institutional quality, they may both be valid instruments for the same object. However, while including latitude into the equation undoes the effect of trade on growth, the inclusion of initial income conserves the causal effect of trade and actually improves the precision by which it is estimated.

Hence, specifying the trade – income relationship as a dynamic equation as in (10) fits better into the theoretical and empirical literature, it also seems to produce more exact estimates and wards off R&R's critique. However, also in the dynamic regressions, the IV estimates are larger than the OLS estimates, which lacks a plausible explanation. Moreover, other omitted variables could be identified and added to the model. The literature has included numerous additional covariates, such as different measures of institutional quality or measures of geography. All these variables may affect the level of the steady state that countries are converging to and bias the trade effect when they are not completely controlled for. However, only a panel data approach is able to fully control for time-invariant individual effects. We have seen above, that the system-GMM estimator, may provide a convenient solution to all these problems.

5 Results from a consistent dynamic panel data approach

5.1 The effect of instrumentation and the baseline model

In this section, we review the income-openness relationship when a GMM-type instrumentation strategy is chosen instead of the F&R instrument. Equation (2) is estimated using different panel data techniques, with the system-GMM estimator being the preferred method. The aim of the analysis is to see (i) whether there is a robust positive causal effect of trade on income when other instruments are used and all time-invariant country-specific fixed effects are accounted for, and (ii) whether instrumenting increases the coefficient of revealed openness relative to the uninstrumented case.

We address point (ii) first and show that using GMM-type instruments does not lead to the kind of counterintuitive results as in F&R. The fact that the IV estimates exceed the OLS estimates by a large amount is surprising and in contradiction with economic theory. Normally one would expect that richer countries trade more because they are rich: high-income countries have better infrastructure which facilitates trade, demand for tradeable goods may rise faster than demand for nontradeables as countries grow rich and poor countries may have little choice other than to resort to trade taxes to finance government spending, which would tend to depress openness. Hence, in an OLS regression, trade and the error term should be positively correlated so that the estimate would be biased upwards.[17]

In principle, F&R's finding may be triggered by sampling error, i.e. that the instrument is by pure chance positively correlated with the residual leading to a bias in the IV results,. and / or by measurement error. Irwin and Terviö (2002) use F&R's instrument on a historical data set and find again that IV estimates are larger than the

[17] There are arguments in trade theory (e.g. in the dynamic Heckscher-Ohlin model), why countries may trade less when they grow richer (and in that model, more similar). However, these models are generally not corroborated by the data.

OLS ones. Hence, it is unlikely, that the wrong sign of the bias is due to sampling error. Measurement error may be an alternative explanation, because it biases the OLS estimates downwards and instrumentation is the appropriate way to cure this problem. F&R argue that nominal openness ... "*is only a noisy proxy for the many ways in which interactions between countries raise income – specialization, spread of ideas, and so on.*" (F&R, p. 393) For example, openness may induce productivity-enhancing technology spillovers that are not so much related to the volume of trade but rather to the existence of an open trade relation between two countries. Similarly, trade theory predicts that one major source for gains from trade resides in the dilution of market power; again this effect does not require actual trade flows but depends on a credible threat of entry (contingent markets). Hence, while the endogeneity bias leads OLS to overestimate, measurement error leads to underestimation so that the sign of the bias in the OLS estimates is ambiguous and may well be negative if measurement error is large enough. However, as F&R admit, measurement error must be implausibly large to overcompensate the endogeneity bias.

A conclusion of all these considerations is that the literature has not come up with a convincing explanation of F&R's results yet. One aim of the present study is to check whether this anomaly persists when a different instrumentation strategy is chosen and of country-specific fixed effects are controlled for.

Table 2 reports different panel data estimates for (2). Data is available for a total of 93 countries, see Table A2 for details. The panel is almost balanced: from the eight five-years intervals available between 1960 and 1999, the first interval is lost due to differentiation of log income; the average number of observations per country is 6.78 which is very close to the maximum of 7. In all regressions reported in Table 2, lagged income, schooling and investment are instrumented by their first order lags. In even numbered columns, also the trade share is instrumented. This allows to isolate the size and sign of the endogeneity bias which arises when the endogenous character of the trade share is not appropriately addressed.

Column (1) in Table 2 reports the results of a pooled 2SLS regression, where the variance-covariance matrix was adjusted to allow for correlation of the error term within the group of observations pertaining to the same country. The last line in the table reports τ^{SS}, the effect of openness on steady state income. All coefficients are estimated with considerable precision. The results imply that an increase of nominal openness by 1 percentage point increases the growth rate by 0.05 percentage points along the adjustment path, while steady state income is increased by 1.2 percent. This is somewhat larger than the estimate in table 1. The reason is that the dynamic specification allows for delayed effects of increased trade on income. In column (1), the trade share is still treated as if it were strictly exogenous. Column (2) runs a two stage least squares regression, where *all* the regressors have been instrumented by their lagged values; again within country correlation in the error terms has been controlled for. As before, all coefficients are estimated with considerable precision. However, the instantaneous effect of openness on the growth rate and the long run effect on income are now smaller than before, so that that the endogeneity bias shows the expected sign. This result is in accordance with intuition, but in stark contrast with F&R who find that the instrumented effect is by a factor of 2.5 larger than the uninstrumented effect.

Columns (3) and (4) repeat this exercise for a fixed effects (within) model. In this model, the coefficients on schooling and population are no longer significantly different from zero. The point estimate for the instantaneous effect of openness on the growth rate is larger than the instrumented equation, but statistically indistinguishable.[18]

Columns (5) and (6) show the results of a linear GMM regression. In column (5) nominal openness is treated as an exogenous variable, while in column (6) it is appropriately instrumented, along with all the other regressors. In both columns, the coefficient of population becomes significantly different from zero again, and the instantaneous

[18] Note that the procedure applied to compute columns (3) and (4) is inefficient.

effect of openness on the growth rate is estimated with acceptable precision. If instrumented, the coefficient is halved and the steady state income effect falls by an even larger amount. In both cases, the GMM model seems appropriately specified since the Sargan test of the overidentifying restrictions is passed without any difficulties, and the Arellano-Bond (1991) test for first-order serial correlation in the (level) residuals ($m2$) cannot reject the Null of no correlation by any standard levels of significance (the P-values are 0.22 and 0.21 percent, respectively).[19]

All in all, comparing the unevenly numbered non-instrumented regressions with the evenly numbered instrumented regressions, it appears that instrumentation does not increase the coefficient of openness, contrary to F&R's findings. This suggests that attenuation bias due to the fact that trade shares are only weak proxies for the full effect of openness may not be important enough to undo the endogeneity bias. To say the least, if one compares between different estimation methods (which is of course problematic), the steady state effects shown in the last line of table 2 do not exhibit the enormous variation between instrumented and non-instrumented equation that F&R have found. Hence, if the counterintuitive sign of the bias in F&R's OLS estimates is due to sampling error, it seems that our instruments do not suffer from this problem. However, even if measurement error does not offset the endogeneity bias, it may well still be present (see subsection 5.3).

However, in accordance with F&R, the SYS-GMM estimator finds a statistically significant and positive causal effect of trade openness on steady state income. The effect is only half as large as the one found by F&R (table 3, column (4)), namely 1.49 instead of to 2.96. Consider increasing the trade share of a country by, say, 50 percentage points. Ceteris paribus, this experiment has a quantitatively important effect on the instantaneous growth rate, which is increased by 2.865 percentage points.

[19] Note that our standard error estimates are consistent in the presence of any pattern of heteroskedasticity and serial correlation within panels. Note also that in all our regressions, the $m1$ test statistic (not reported) indicates the presence of first-order serial correlation in differences, as implied by the model; see footnote 7.

Comparing the long-run equilibrium before and after the increase in openness shows that the experiment boosts steady state income by 74.42%, which is again a very considerable number. However, compared to F&R, this effect is still small, since they find that income would rise by 148%. Note that the effect obtained with SYS-GMM is quantitatively very similar to the one found in the 1985 cross section. Thus, it seems that the largest part of the difference between F&R's results and the SYS-GMM results come from choosing a dynamic specification, rather than a static one. In contrast to other methods, the GMM estimator makes sure that all time-invariant country-specific fixed effects are controlled for, that the lagged income variable on the right-hand-side of the regression is appropriately dealt with and that all potentially endogenous regressors are instrumented in a meaningful way.

The regression yields quantitatively plausible and statistically significant coefficients for all regressors. Most importantly, there seems to be evidence for a scale effect since the coefficient of population is positive and significant. This is in line with F&R and Alcalà and Ciccone.

Moreover, the regression in our Table 2, column (6), uses more than six times as many observations than F&R. Hence, it seems safe to argue that the true causal effect from trade to income is smaller than what F&R claim. For the remainder of the paper, the model reported in column (6) serves as the benchmark.

5.2 Sample sensitivity checks

Table 3 reports a number of sensitivity checks. Column (1) includes only non-OECD countries while column (1) focuses on the much smaller subsample of OECD countries.[20] Strikingly, the rate of conditional convergence in the OECD subsample is much larger than in the non-OECD subsample (3% versus 1%, respectively), while the instanta-

[20] To avoid the problem of endogenous sample selection, the OECD subsample includes only those countries that have been members from the start (1961). This leaves us with the former EU15 (without Luxemburg), plus Switzerland, Norway, Turkey, Canada, USA, New Zealand, Australia and Japan.

neous effect of an increase in nominal openness is almost the same in both subsamples. This implies that the steady-state effect is considerably larger in the non-OECD sample. Interestingly, the same observation can be made also with respect to schooling and (albeit to a smaller extent) with respect to the investment rate.

Next we segment the sample into a European subsample (which is of course a subset of the OECD sample), and a non-European subsample.[21] Columns (3) and (4) show that the coefficient on openness is statistically significant and positive in both the European and the non-European subsample, with the instantaneous effect larger and the long-run effect smaller in the European subsample. Interestingly, there is no evidence for a scale effect in the European subsample, while size still matters in the somewhat larger OECD subsample. This fact may be interpreted as evidence for the European integration process, which makes the size of the home market more and more irrelevant.

Column (5) excludes subsaharan Africa from the sample. Compared to the benchmark model, the instantaneous and the long-run effects of openness change only slightly and keep their statistical significance. Thus, excluding low growth / low openness countries from the sample does not undo the results. However, excluding subsaharan Africa undoes the significance of the scale effect. Hence, it seems that we find a positive scale effect in the full sample only because of the presence of those relatively closed African countries whose growth perspectives are hampered by very limited home markets.

Column (6) excludes the three OPEC countries that are present in the full sample (Algeria, Indonesia and Venezuela). Compared to the benchmark results, this exclusion leaves all coefficients virtually unchanged.

[21] The European sample comprises the former group of EU15 countries without Luxemburg, plus Cyprus, Switzerland, Norway and Turkey.

5.3 Political versus revealed openness

In order to see what the nominal openness index really measures, it may be useful to isolate the effects of physical shipment of goods and services from those generated by political openness. As stressed above, many benefits of openness do not need actual trade flows to materialize. Table 4 includes the Sachs-Warner Index (SWI) into the econometric analysis. For comparison reasons, column (1) reproduces the baseline result obtained in Table 4, column (6). Column (1) replaces the nominal openness measure by the SWI. While an increase of the trade share by one percentage point causes an instantaneous growth effect of about 0.06 percentage points, switching from being closed to being open causes growth to shoot up by 6.66 percentage points. This is equivalent to increasing the trade share by 110 percentage points.

Column (3) runs a regression in which both the nominal trade share and the SWI are present. Strikingly, now the trade share is no longer significant while the SWI coefficient and the associates standard error change only slightly with respect to column (2). If one controls explicitly for political openness, there is no strong evidence that increasing the trade share boosts income. Hence, it appears that the beneficial effects from trade do not come from the physical delivery of goods as such but more from the general fact that a country is open and participates in the international economy.

The message of including the SWI index is twofold. First, there is a causal effect of openness on instantaneous growth regardless of the exact definition of openness. Second, the trade share may indeed be a noisy proxy for openness. This implies that the pure endogeneity bias is probably larger than what the results in Table 2 suggest.

5.4 Do initially poor countries benefit less from globalization?

Now, we take up the politically interesting question whether initially poor countries benefit from globalization on equal terms than initially rich countries. To check this question, we divide our sample into two subsamples: of initially 'rich' and one of initially 'poor' countries. Poor countries are those whose log income per capita in 1960 was smaller than the median level, the subsample of rich countries is just the remainder. In order to provide a check on whether our results depend on this particular segmentation of the data, we work with a second definition, whereby a country is poor if its log income per capita in 1960 was smaller than the 25% percentile and rich otherwise. Only countries for which per capita income is observed in 1960 are in the sample, hence our panel is perfectly balanced. Table A2 in the appendix informs which countries are in the sample and which are classified as rich or poor according to our first segmentation.

Table 5 compares the median growth rates measured in the sample as a whole and in the two subsamples. It turns out that the median growth rate in the poor subsample was substantially lower rates than that in the rich subsample or in the total sample. While in the full sample, the median growth rate over the 1960-1999 period was 1.73%, countries initially poorer than the median grew by a mere 0.80% and those poorer than the 25%-percentile by an even smaller 0.70%.[22]

Is trade causally responsible for the lack of absolute convergence, as shown in Table 5, or must other factors be blamed? Our GMM panel approach is a good means to answer this question, because it controls for all time-invariant country-specific effects, therefore isolating the effect of trade. Denote the indicator of initial per capita GDP by I_{i1}. Regardless of whether I_{i1} is a binary or a continuous variable, the empirical specification now contains an interaction term

$$T_{it} * I_{i1}.y_{it} = \gamma y_{i,t-1} + \tau_1 T_{it} + \tau_2 \left(T_{it} * I_{i1}\right) + \mathbf{X}'_{\mathbf{it}}\boldsymbol{\beta} + \boldsymbol{\delta}_{\mathbf{t}} + \left(\eta_i + I_{i1}\right) + v_{it}. \qquad (11)$$

If τ_2 is strictly negative, the causal effect of trade on growth is smaller for countries who feature a higher value of I_{i1} and trade can be seen as a driving factor for convergence.

[22] Our measure of divergence probably overstates the true divergence; comparing means leads to a less dramatic picture. However, while the extent of absolute divergence is disputable, there is no evidence in favor for convergence.

First, we run our baseline regression (2) separately for both subsamples. The results are shown in columns (1) and (2) in Table 6. The instantaneous growth effect of openness appears larger for initially poor countries, albeit the effect is imprecisely estimated. However, there is evidence, that investment has a higher but shorter lived effect on the growth rate of initially rich countries. Running two separate regressions exploits only variation *within* the two subsamples. Hence, we can only conclude that compared to the other countries in the same group, openness pays off more in the sample of poor countries. However, it may well be that, as a group, poor countries benefit much less from openness than rich countries. To allow for both, within group variation and between group variation, column (3) adds the interaction term to the regression. To start with, I_{1i} is a dummy variable that takes the value of one if the country is in the subsample of initially rich countries and the value of zero if it is in the subsample of initially poor ones. This allows group specific variation in the coefficient of openness, but constrains the other coefficients to be identical across groups. It turns out that the interaction term has a negative sign but is statistically not different from zero. Hence, we do not find any systematic evidence, that openness causally affects initially poor countries differently than initially rich ones. The pattern of divergence that emerged from Table 5 cannot be attributed to trade openness and must be due to some different factor. To explain divergence, one would have to turn towards the country-specific fixed-effects which capture institutional and geographical characteristics.

The remaining columns show the results of some robustness checks. In column (4), the variable I_{i1} is still a binary one, and takes the value of one if a country had a 1960 log income per capita larger than the 25%-percentile and the value of zero otherwise. Again trade does not turn out to be causally responsible for the poor performance of those countries that in 1960 counted amongst the 25% poorest.

In column (5), I_{i1} is just the initial log of per capita income. In contrast to the cases discussed before, this regression does not pass the specification test $m2$, the null of serial correlation in the error terms cannot be rejected at the 10% significance level. However, this problem notwithstanding, initial income does not appear to matter for the causal effect of trade on income and the result is qualitatively similar to the earlier regressions.

Finally, column (6) checks whether the insensitivity of the trade effect with respect to initial GDP depends on the precise definition of openness. When the trade share is replaced by the Sachs-Warner index, the picture does not change qualitatively: again, there is no evidence that the interaction of initial income with the SWI index plays any role in determining growth.

Hence, to say the least, if backwardness is not a virtue, as column may (1) suggest, it certainly does not appear to be a handicap and the causal effect of trade on growth is not different for initially poor countries.

5.5 On the role of TFP in the growth-openness nexus

Table 6 reproduces our results if log TFP is used as the dependent variable. Following Alcalà and Ciccone (2004), real openness is used to measure the importance of trade. Moreover, instead of population the regression controls for the size of the working force.

Column (1) shows the results of a baseline regression, which is perfectly analogous to the one run for capita real income. However, in line with F&R's results, we do not find much evidence for a positive causal effect of trade on TFP. The estimated coefficient is much smaller than when per capita real income is used as the dependent variable and it is very imprecisely estimated. Schooling does not seem to be causally related to TFP, while the investment rate turns out to be rather important. The only interesting finding in column (1) is that of statistically significant adjustment dynamics. While this is a common finding in GDP regressions, there is less evidence in the literature for conditional convergence in TFP. Moreover, there may be a positive scale effect: increasing the work force has a (weakly) significant positive effect on the instantaneous growth rate of TFP and the long-run TFP level. However, the results must be interpreted with caution, because the Arellano-Bond test (*m2*) for second order serial correlation in the residuals almost rejects the null of no serial correlation (the P-value is 9%).

Column (2) repeats the analysis but uses the SWI dummy instead of the real trade share as a measure of openness. The *m2*-measure is somewhat more supportive, but besides the investment rate (and lagged TFP) we do not identify any other causal effects of openness on the growth rate of TFP. Column (3) puts the real trade share together with the SWI into the regression. This alters the results exhibited in column (2) only very slightly.

Now come more interesting results: if the sample is restricted to the 50% countries with the lowest 1960 TFP level, we find a quantitatively important and statistically significant causal effect of trade on the instantaneous growth rate. In contrast, focussing on the 50% countries with the highest 1960 TFP level, we fail to identify such an effect. Column (6) again considers the full sample and checks the interaction $poor * open$ where *poor* is a dummy that takes the value of unity if the country had a 1960 TFP level below that of the median. Hence, while there was no evidence either for an advantage or a handicap of backwardness in the GDP regressions, now we are led to conclude that in terms of total factor productivity, initially less productive countries tend to benefit more from an increase in trade. This result has been checked for its robustness by considering other definitions of the group of initially poor countries. However, the result remains robust.

6 Conclusions

The present paper revisits the empirical relationship between trade and growth in a panel of countries, using the system-GMM estimator proposed by Blundell and Bond (1998). This method has the advantage that it allows consistent estimation even if the growth model is specified as a dynamic AR(1) relationship and all the right-hand-side variables are potentially endogenous and/or prone to measurement error. Moreover, through first-differencing, the procedure offers a natural way to control for institutional and geographical characteristics as long as they are time-invariant.

The paper first argues that the widely cited model by Frankel and Romer (1999) may be misspecified, because it makes the implicit assumption that all countries are in their respective steady states. Since the globalization is a rather recent phenomenon, this seems a questionable requirement. If F&R's model is reformulated to allow for adjustment dynamics, the causal effect of trade on growth is estimated more precisely than in a static framework. Moreover, inclusion of geographical latitude (distance from the equator) no longer destroys the effect.

F&R's main contribution was the suggestion of a clever instrument to account for the endogeneity of the trade share in the trade-growth relationship. However, they found that the estimate obtained under 2SLS is much larger than the one found using OLS. This runs against intuition and theoretical reasoning. Using a GMM-type instrumentation strategy, the present paper finds that the effect of instrumented trade is not larger than the uninstrumented one, confirming the hypothesis that income is positively associated with trade.

Next, the empirical relationship between trade and growth is estimated using the system-GMM estimator. It turns out that the more general econometric approach confirms F&R's finding of a robust and positive causal effect of trade on growth. This result is found after controlling for country-specific fixed-effects, the endogeneity of all regressors and the presence of a lagged dependent variable on the right-hand-side of the equation. The model passes a variety of sensitivity checks, suggesting that the positive trade–growth relationship is rather robust.

Finally, the empirical model is used to check whether trade is causally responsible for the failure of initially poor countries to catch up with the richer ones. There is little evidence that the trade share has a different effect for initially poor countries; the point estimates within the subsample of poor countries and the sign of the interaction effect actually suggest the contrary, albeit without satisfactory precision. However, when the model is used to investigate the effect of trade on total factor productivity growth, we find fairly robust evidence that trade is actually more beneficial for countries that start with a lower level of TFP.

The system-GMM procedure employed in the present paper is not a panacea. It has two major drawbacks: first, to the extent that institutional features of countries are time-variant, they are not controlled for; second, the empirical framework accounts for endogenous growth only by including the time dummies δ_t. The first problem may be resolvable as time passes on. At the moment, the time series of institutional quality indicators, as published by the World Bank or elsewhere, do not cover any extensive period of time and are therefore of little use in a dynamic panel data framework. The second problem appears in any stationary econometric model. Its resolution would require a framework that allows for country-specific long-run time effects.

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Table 1

Regression results: Trade and income in the 1985 cross section

Dependent variable: log per capita income

	(1) F&R, OLS	(2) F&R, IV	(3) R&R, IV	(4) F&R Barro-type, OLS	(5) F&R Barro-type, IV	(6) R&R Barro-type, IV
Nominal trade share	0.908	2.585	-0.344	0.478	1.631	1.330
	(0.315)	(1.445)	(1.088)	(0.131)	(0.728)	(0.706)
Log population 1985	0.258	0.433	-0.024	0.129	0.267	0.218
	(0.087)	(0.190)	(0.139)	(0.037)	(0.101)	(0.100)
Log land surface in sq km	-0.088	0.001	-0.073	-0.052	-0.000	-0.008
	(0.081)	(0.132)	(0.093)	(0.030)	(0.059)	(0.050)
abs (latitude)/90			4.270			0.541
			(0.551)			(0.357)
Log real income per capita 1960				0.880	0.952	0.884
				(0.065)	(0.094)	(0.108)
Log avg. years of schooling in 1960				0.321	0.137	0.150
				(0.082)	(0.149)	(0.132)
Constant	5.349	2.191	7.582	-0.262	-2.829	-1.804
	(0.899)	(2.983)	(2.129)	(0.567)	(1.784)	(1.865)
Observations	101	101	101	101	101	101
F-test		25.98	20.61		19.10	16.05
(P-value)		(0.00)	(0.00)		(0.00)	(0.00)
R-squared	0.10	0.12	0.50	0.89	0.80	0.84

Notes: Robust standard errors in parentheses. In the IV regressions, the F-statistic tests the overall significance of the first stage regression. F&R refers to Frankel and Romer's (1999) specification, R&R refers to Rodriguez and Rodrik (2000).

Table 2
Regression results: alternate estimation techniques
Dependent variable : log per capita income

	(1) POOLED	(2) POOLED	(3) WITHIN	(4) WITHIN	(5) SYS-GMM	(6) SYS-GMM
Lagged log income	0.9562	0.9559	0.8486	0.8436	0.9687	**0.9615**
	(0.0086)	(0.0085)	(0.0183)	(0.0193)	(0.0167)	(0.0152)
Nominal trade share	0.0527	0.0476	0.1236	0.1356	0.1062	**0.0573**
	(0.0132)	(0.0131)	(0.0303)	(0.0489)	(0.0576)	(0.0246)
Log secondary schooling	0.0305	0.0306	-0.0137	-0.0142	0.0184	**0.0292**
	(0.0100)	(0.0100)	(0.0152)	(0.0152)	(0.0176)	(0.0151)
Log investment rate	0.0906	0.0920	0.0974	0.0933	0.1133	**0.1151**
	(0.0168)	(0.0168)	(0.0150)	(0.0158)	(0.0275)	(0.0278)
Log population	0.0113	0.0105	-0.0798	-0.0789	0.0285	**0.0196**
	(0.0048)	(0.0049)	(0.0427)	(0.0427)	(0.0114)	(0.0086)
Observations	631	631	631	631	631	631
R-squared	0.91	0.91	0.90			
Countries	93	93	93	93	93	93
m2					0.21	0.21
Sargan					0.90	1.00
τ^{SS}	1.2032	1.0794	0.8164	0.8670	3.3930	1.4883

Notes: Robust (asymptotic) standard errors in parentheses. In all regressions, lagged output, schooling and investment are instrumented by their first order lags. In even numbered columns, also the trade share is instrumented. In column (1) and (2) standard errors have been corrected for within group correlation (clustering). Regressions are run over 8 five-year intervals, spanning the 1960-2000 period. m1 and m2 are the Arellano and Bond (1991) tests for first-order and second-order serial correlation in levels, Sargan is a test of the overidentifying restrictions for the GMM estimators (for all tests P-values are reported). All models include constants and time-specific effects (not reported). Implied τ^{SS} reports the effect of openness on *steady-state* output in those cases, where a statistically significant effect exists.

Table 3

Regression results: sample sensitivity analysis

Dependent variable: log per capita income

	(1) Non-OECD	(2) OECD	(3) Europe	(4) Non-Europe	(5) Non-Subsaharan Africa	(6) Non-OPEC
Lagged log income	0.9522	0.8537	0.9016	0.9692	0.9492	0.9618
	(0.0207)	(0.0373)	(0.0489)	(0.0173)	(0.0184)	(0.0145)
Nominal openness	0.0633	0.0627	0.0734	0.0562	0.0509	0.0558
	(0.0269)	(0.0230)	(0.0325)	(0.0249)	(0.0237)	(0.0238)
Log schooling	0.0480	0.0376	0.0021	0.0379	0.0241	0.0309
	(0.0166)	(0.0180)	(0.0179)	(0.0152)	(0.0210)	(0.0148)
Log investment rate	0.0943	0.1724	0.1874	0.1006	0.1339	0.1101
	(0.0276)	(0.0230)	(0.0392)	(0.0285)	(0.0320)	(0.0271)
Log population	0.0115	0.0189	0.0072	0.0186	0.0143	0.0173
	(0.0086)	(0.0049)	(0.0067)	(0.0078)	(0.0087)	(0.0091)
Observations	479	152	119	512	508	610
Countries	71	22	18	75	74	90
m2	0.12	0.46	0.54	0.19	0.18	0.13
Sargan	1.00	1.00	1.00	1.00	1.00	1.00
τ^{SS}	1.3243	0.4286	0.7459	1.8247	1.0020	1.4607

Notes: Robust (asymptotic) standard errors in parentheses. All models have been estimated with SYS-GMM and contain time-specific effects and constants (not reported). OECD refers to the sample of countries that have founded the OECD in 1961. Europe refers to Western Europe. The Non-OPEC sample excludes Algeria, Indonesia and Venezuela. Subsaharan Africa is the whole of Africa without Arab countries (Morocco, Tunesia, Egypt) and Southern Africa (Namibia, South Africa, Botswana) See notes below Table 2 for further information.

Table 4

Regression results: Political versus revealed openness

Dependent variable: log real output per income

	(1) baseline	(2) SWI instead of trade	(3) SWI and trade
Lagged log income	0.9615	0.9459	0.9461
	(0.0152)	(0.0157)	(0.0145)
Nominal trade share	0.0573		0.0364
	(0.0246)		(0.0202)
SWI		0.0666	0.0549
		(0.0235)	(0.0210)
Log schooling	0.0292	0.0158	0.0250
	(0.0151)	(0.0180)	(0.0163)
Log investment rate	0.1151	0.1517	0.1261
	(0.0278)	(0.0276)	(0.0277)
Log population	0.0196	0.0089	0.0157
	(0.0086)	(0.0095)	(0.0084)
Observations	631	605	605
Countries	93	90	90
m2	0.21	0.19	0.17
Sargan	1.00	1.00	1.00
τ^{SS}	1.4883		

Notes: Robust (asymptotic) standard errors in parentheses. All regressions have been estimating using SYS-GMM and include period dummies and constants (not reported). Revealed openness is measured by the nominal trade share, political openness is measured by the Sachs-Warner dummy (SWI). See table 2 for further notes.

Table 5

Evolution of median log real output per capita depending on initial conditions

period	total sample	richer than median	poorer than median	richer than 25%pc	poorer than 25%pc
1960-64	7.87	8.61	7.26	8.17	6.85
1965-69	7.98	8.77	7.37	8.31	6.88
1970-74	8.16	8.95	7.40	8.51	6.95
1975-79	8.32	9.11	7.46	8.63	7.05
1980-84	8.41	9.25	7.56	8.69	7.10
1985-89	8.41	9.35	7.70	8.85	7.12
1990-94	8.53	9.50	7.60	8.89	7.12
1995-99	8.56	9.55	7.58	8.92	7.13
median growth	1.73	2.35	0.80	1.88	0.70

Notes: See Table A2 in the appendix for the list of countries used to compute the statistics above.

Table 6

Regression results: Do initial conditions matter?

Dependent variable: log per capita income

	(1) 50% poorest	(2) 50% richest	(3) Interaction: dummy median	(4) Interaction: dummy 25%pc	(5) Interaction: initial GDP trade share	(6) Interaction: initial GDP SWI
Lagged log income	0.9893	0.9560	0.9713	0.9796	0.9877	0.9488
	(0.0192)	(0.0278)	(0.0139)	(0.0125)	(0.0181)	(0.0167)
Nominal trade share	0.0539	0.0467	0.0444	0.0575	0.3140	
	(0.0295)	(0.0186)	(0.0184)	(0.0230)	(0.2294)	
SWI						0.2459
						(0.1328)
Log schooling	0.0274	0.0372	0.0209	0.0199	0.0199	0.0156
	(0.0132)	(0.0237)	(0.0130)	(0.0122)	(0.0125)	(0.0143)
Log investment rate	0.0896	0.1191	0.1165	0.1057	0.1056	0.1313
	(0.0239)	(0.0261)	(0.0220)	(0.0203)	(0.0201)	(0.0238)
Log population	0.0238	0.0034	0.0177	0.0176	0.0155	0.0058
	(0.0143)	(0.0092)	(0.0096)	(0.0101)	(0.0098)	(0.0085)
interaction term			-0.0254	-0.0431	-0.0333	-0.0201
			(0.0299)	(0.0422)	(0.0295)	(0.0164)
Observations	294	301	595	595	595	569
Countries	42	43	85	85	85	82
m2	0.11	0.64	0.25	0.18	0.09	0.17
Sargan	1.00	1.00	1.00	1.00	1.00	1.00

Notes: Robust (asymptotic) standard errors in parentheses. All regressions have been estimated using SYS-GMM. Note that the model in column (5) fails to pass the specification tests. See Table 2 for further notes.

Table 7
Regression results: The causal effect of trade on TFP
Dependent variable: log TFP

	(1) baseline	(2) SWI	(3) real openness plus SWI	(4) 50% poorest	(5) 50% richest	(6) interaction
Lagged TFP	0.9685 (0.0235)	0.9612 (0.0237)	0.9519 (0.0228)	0.9899 (0.0289)	0.9206 (0.0585)	0.9714 (0.0171)
Real trade share	0.0216 (0.0187)		0.0053 (0.0188)	0.0675 (0.0296)	0.0194 (0.0197)	0.0229 (0.0169)
SWI		0.0350 (0.0203)	0.0308 (0.0196)			
Log schooling	0.0127 (0.0144)	0.0095 (0.0163)	0.0105 (0.0166)	0.0203 (0.0112)	0.0080 (0.0156)	0.0146 (0.0113)
Log investment rate	0.0663 (0.0242)	0.0820 (0.0238)	0.0801 (0.0264)	0.0437 (0.0182)	0.0518 (0.0214)	0.0592 (0.0195)
Log work force	0.0134 (0.0061)	0.0047 (0.0079)	0.0110 (0.0069)	0.0124 (0.0094)	0.0016 (0.0086)	0.0116 (0.0060)
poor*open						-0.0312 (0.0132)
Observations	590	564	564	287	287	574
Countries	86	83	83	41	41	82
m2	0.09	0.11	0.11	0.14	0.66	0.18
Sargan	1.00	1.00	1.00	1.00	1.00	1.00
τ^{SS}				6.6832		1.0909$^{(**)}$

Notes: Robust standard errors in parentheses. All regressions have been estimated using SYS-GMM. They contain time-specific fixed effects and constants (not reported). See table 2 for further notes.
$(**)$ applies to the 50% poorest countries.

Table A1
Summary statistics

Variable	Definition	Source	Period	Mean	Standard deviation	Minimum	Maximum
Log income per capita	Real GDP per capita in PPP, chained index.	Heston et al. (2002)	60-64	7.75	0.91	5.98	9.70
			65-69	7.88	0.94	6.13	9.82
			70-74	8.05	0.99	5.91	9.98
			75-79	8.15	1.01	6.34	9.93
			80-84	8.21	1.04	6.27	10.01
			85-89	8.29	1.09	6.28	10.12
			90-94	8.34	1.12	6.13	10.19
			95-99	8.38	1.13	5.99	10.26
Total factor productivity (TFP)	Computed following Klenow and Rodriguez-Clare (1997).		60-64	5.57	0.56	4.02	6.58
			65-69	5.68	0.57	4.22	6.72
			70-74	5.79	0.59	4.02	6.77
			75-79	5.83	0.59	4.33	6.79
			80-84	5.84	0.59	4.26	6.79
			85-89	5.85	0.61	4.29	6.88
			90-94	5.86	0.66	4.11	6.90
			95-99	5.88	0.68	4.12	6.93
Nominal trade share	exports plus imports over GDP.	Heston et al. (2002)	60-64	0.49	0.35	0.07	2.83
			65-69	0.50	0.33	0.06	2.52
			70-74	0.54	0.34	0.07	2.56
			75-79	0.62	0.41	0.09	3.46
			80-84	0.65	0.46	0.13	3.94
			85-89	0.64	0.45	0.14	3.67
			90-94	0.71	0.47	0.16	3.65
			95-99	0.75	0.48	0.18	3.40
'Real' trade share	Computed following Alcala and Ciccone (2004).		60-64	0.26	0.25	0.04	2.09
			65-69	0.25	0.22	0.03	1.62
			70-74	0.32	0.33	0.03	2.64
			75-79	0.44	0.41	0.05	2.66
			80-84	0.41	0.36	0.05	2.91
			85-89	0.35	0.34	0.04	2.56
			90-94	0.39	0.41	0.04	3.18
			95-99	0.42	0.44	0.05	3.04
SWI	Sachs-Warner index	Wacziarg & Welch (2003)	60-64	0.35	0.46	0.00	1.00
			65-69	0.33	0.46	0.00	1.00
			70-74	0.33	0.47	0.00	1.00
			75-79	0.33	0.46	0.00	1.00
			80-84	0.33	0.47	0.00	1.00
			85-89	0.41	0.47	0.00	1.00
			90-94	0.60	0.45	0.00	1.00
			95-99	0.66	0.43	0.00	1.00
Log schooling	Average years of secondary education in total population aged over 25.	Barro and Lee (2000)	60-64	-1.08	1.29	-4.47	1.52
			65-69	-0.92	1.28	-4.42	1.52
			70-74	-0.67	1.20	-3.77	1.45
			75-79	-0.41	1.09	-3.37	1.47
			80-84	-0.19	1.02	-3.17	1.60
			85-89	-0.01	0.96	-2.69	1.63
			90-94	0.16	0.92	-2.42	1.58
			95-99	0.30	0.86	-2.18	1.61
Log investment rate	Ratio of real investment over real GDP.	Heston et al. (2002)	60-64	2.53	0.84	0.36	3.66
			65-69	2.59	0.80	0.52	3.76
			70-74	2.68	0.79	0.29	4.04
			75-79	2.75	0.71	0.08	3.95
			80-84	2.66	0.63	0.58	4.04
			85-89	2.56	0.60	0.90	3.83
			90-94	2.53	0.65	0.86	3.75
			95-99	2.54	0.64	0.77	3.74
Log population		Heston et al. (2002)	60-64	1.80	1.49	-0.99	6.51
			65-69	1.91	1.49	-0.85	6.63
			70-74	2.06	1.49	-0.70	6.76
			75-79	2.16	1.48	-0.54	6.85
			80-84	2.26	1.47	-0.47	6.92
			85-89	2.34	1.49	-1.08	6.99
			90-94	2.41	1.47	-0.93	7.06
			95-99	2.45	1.46	-0.89	7.09
Log work force	Population times GDP per capita over GDP per worker.	Heston et al. (2002)	60-64	0.91	1.52	-2.13	5.87
			65-69	1.00	1.51	-1.96	5.97
			70-74	1.11	1.50	-1.81	6.11
			75-79	1.23	1.50	-1.66	6.23
			80-84	1.33	1.49	-1.52	6.34
			85-89	1.44	1.48	-1.43	6.46
			90-94	1.57	1.48	-1.37	6.56
			95-99	1.58	1.48	-1.58	6.58

Table A2. TFP measures and sample composition

	Country	TFP iso	TFP 1960	avg. TFP 1999	growth	Table 1	Table 2	SWI available	initially rich
1	Angola	AGO	157,60	100,09	-0,65%	1			
2	Argentina	ARG	268,69	348,34	0,81%	1	1	1	1
3	Australia	AUS	324,78	578,61	1,47%	1	1	1	1
4	Austria	AUT	244,66	520,29	1,92%	1	1	1	1
5	Burundi	BDI	68,23	59,05	0,10%	1	1	1	
6	Belgium	BEL	257,95	533,39	1,85%	1	1	1	1
7	Benin	BEN	106,18	112,98	0,23%	1	1	1	0
8	Burkina Faso	BFA	75,82	84,09	0,34%	1			
9	Bangladesh	BGD	92,75	127,40	0,88%	1	1	1	0
10	Bolivia	BOL	149,60	165,16	0,31%	1	1	1	0
11	Brazil	BRA	134,48	267,31	1,80%	1	1	1	1
12	Botswana	BWA	91,15	336,48	3,57%	1	1	1	0
13	Central African Rep.	CAF	155,57	88,74	-1,26%	1	1	1	0
14	Canada	CAN	334,53	600,28	1,50%	1	1	1	1
15	Switzerland	CHE	359,90	515,99	0,94%	1	1	1	1
16	Chile	CHL	194,14	356,70	1,67%	1	1	1	1
17	China	CHN	66,07	196,36	2,85%	1	1	1	
18	Cote d'Ivoire	CIV	135,22	145,00	0,29%	1			
19	Cameroon	CMR	141,48	150,83	0,33%	1	1	1	0
20	Rep. of Congo	COG	45,50	124,47	3,12%	1	1	1	0
21	Colombia	COL	163,44	256,98	1,16%	1	1	1	1
22	Costa Rica	CRI	190,20	255,89	0,79%	1	1	1	1
23	Cyprus	CYP	149,83	457,03	3,40%	1	1	1	1
24	Denmark	DNK	317,37	568,77	1,50%	1	1	1	1
25	Dominican Rep.	DOM	129,20	267,21	1,92%	1	1	1	0
26	Algeria	DZA	148,70	212,44	1,19%	1	1	1	0
27	Ecuador	ECU	114,98	159,73	0,92%	1	1	1	0
28	Egypt	EGY	138,94	280,12	1,86%	1	1	1	
29	Spain	ESP	192,33	449,51	2,20%	1	1	1	1
30	Finland	FIN	245,49	532,80	2,00%	1	1	1	1
31	Fiji	FJI	152,09	242,92	1,33%	1	1	0	
32	France	FRA	259,70	503,37	1,68%	1	1	1	1
33	Gabon	GAB	182,50	348,61	2,13%	1			
34	Utd. Kingdom	GBR	313,90	542,39	1,39%	1	1	1	1
35	Germany	GER					1	1	
36	Ghana	GHA	81,04	119,32	1,30%	1	1	1	0
37	Guinea	GIN	155,87	168,21	0,26%	1			
38	Gambia, The	GMB	108,85	114,21	0,33%	1			
39	Guinea-Bissau	GNB	41,10	55,83	1,94%	1			
40	Greece	GRC	171,70	385,21	2,10%	1	1	1	1
41	Guatemala	GTM	173,04	236,07	0,80%	1	1	1	0
42	Guyana	GUY	102,04	165,96	1,60%	1	1	1	0
43	Hong Kong	HKG	161,46	628,63	3,56%	1	1	1	1
44	Honduras	HND	119,74	124,62	0,18%	1	1	1	0
45	Haiti	HTI				1	1	1	
46	Hungary	HUN					1	1	
47	Indonesia	IDN	88,37	188,53	1,97%	1	1	1	0
48	India	IND	80,36	165,11	1,86%	1	1	1	0
49	Ireland	IRL	236,10	706,17	2,82%	1	1	1	1
50	Iran	IRN	149,29	248,84	1,60%	1	1	1	1
51	Israel	ISR	214,33	445,39	1,91%	1	1	1	1
52	Italy	ITA	235,72	500,63	1,92%	1	1	1	1
53	Jamaica	JAM	128,29	154,45	0,56%	1	1	1	1
54	Jordan	JOR	158,51	202,51	0,85%	1	1	1	0
55	Japan	JPN	183,91	505,12	2,61%	1	1	1	1

Table A2. TFP measures and sample composition, ctd.

	Country	iso	TFP 1960	TFP 1999	avg. TFP growth	Table 1	Table 2	SWI available	initially rich
56	Kenya	KEN	76,25	104,37	0,93%	1	1	1	0
57	Rep. of Korea	KOR	101,35	422,64	3,70%	1	1	1	0
58	Sri Lanka	LKA	109,99	188,67	1,39%	1			
59	Lesotho	LSO	65,05	91,59	1,09%	1	1		0
60	Morocco	MAR	105,62	195,38	1,69%	1			
61	Madagascar	MDG	139,63	107,36	-0,62%	1			
62	Mexico	MEX	192,94	311,05	1,24%	1	1	1	1
63	Mali	MLI	92,27	93,22	0,22%	1	1	1	0
64	Mozambique	MOZ	168,21	122,09	-0,48%	1	1	1	0
65	Mauritania	MRT	113,05	111,41	0,68%	1	1	1	
66	Mauritius	MUS	192,40	520,76	2,68%	1	1	1	1
67	Malawi	MWI	47,72	76,13	1,48%	1	1	1	0
68	Malaysia	MYS	133,71	343,31	2,41%	1	1	1	0
69	Namibia	NAM	160,32	197,69	0,77%	1			
70	Niger	NER	125,60	86,07	-0,75%	1	1	1	0
71	Nigeria	NGA	93,64	63,75	-0,51%	1			
72	Nicaragua	NIC	162,67	104,63	-0,91%	1	1	1	1
73	Netherlands	NLD	291,11	552,28	1,63%	1	1	1	1
74	Norway	NOR	249,92	550,40	2,01%	1	1	1	1
75	Nepal	NPL	74,73	105,19	0,91%		1	1	0
76	New Zealand	NZL	338,72	460,63	0,82%	1	1	1	1
77	Pakistan	PAK	66,35	142,74	1,97%	1	1	1	0
78	Panama	PAN	132,46	231,56	1,49%	1	1		1
79	Peru	PER	151,52	189,56	0,71%	1	1	1	1
80	Philippines	PHL	128,07	175,08	0,82%	1	1	1	0
81	Papua New Guinea	PNG	142,82	163,31	0,56%	1	1	1	0
82	Poland	POL					1	1	
83	Portugal	PRT	165,76	434,14	2,49%	1	1	1	1
84	Paraguay	PRY	165,29	232,63	0,91%	1	1	1	0
85	Romania	ROM	69,32	160,97	2,92%	1			
86	Rwanda	RWA	121,67	105,56	0,32%	1	1	1	0
87	Senegal	SEN	142,06	133,33	-0,04%	1	1	1	0
88	Singapore	SGP	119,97	559,29	4,74%		1	1	1
89	Sierra Leone	SLE					1	1	0
90	El Salvador	SLV	217,36	252,36	0,44%	1	1	1	1
91	Sweden	SWE	305,86	534,96	1,43%	1	1	1	1
92	Syria	SYR	117,06	234,86	2,39%	1	1	1	0
93	Chad	TCD	94,99	79,91	0,36%	1			
94	Togo	TGO	90,31	78,20	0,01%	1	1	1	0
95	Thailand	THA	76,87	236,25	2,90%	1	1	1	0
96	Trinidad & Tobago	TTO	239,23	426,81	1,74%	1	1	1	1
97	Tunisia	TUN				1	1	1	0
98	Turkey	TUR	159,68	271,98	1,39%	1	1	1	1
99	Taiwan	TWN	115,67	539,18	4,17%		1	1	0
100	Tanzania	TZA	35,60	40,65	0,71%	1			
101	Uganda	UGA	106,85	138,72	0,93%	1	1	1	0
102	Uruguay	URY	252,48	353,56	0,95%	1	1	1	1
103	USA	USA	392,21	728,42	1,58%	1	1	1	1
104	Venezuela	VEN	283,28	239,43	-0,34%	1	1	1	1
105	South Africa	ZAF	239,15	319,16	0,74%	1	1	1	1
106	Congo, Dem. Rep.	ZAR	86,17	33,69	-2,20%	1	1	1	0
107	Zambia	ZMB	75,60	64,81	-0,22%	1	1	1	0
108	Zimbabwe	ZWE	78,70	133,77	1,59%	1	1	1	0

Notes: 'poor' takes the value of 1 if the country has log income per capita lower than median in 1960, the value of 0 if the contrary holds, and is missing if 1960 data is not available.