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Preface

I thank Guido Tabellini for his guidance and advice throughout my PhD program. I also thank all the PhD faculty and students for useful discussions and hints. I am grateful to Francesco Daveri and Paolo Epifani for their time and advice on my work. I own special thanks to Carlo Altomonte, Pierpaolo Battigalli, Elisa Borghi, Antonio Ciccone, Lucia Corno, Rosario Crinò, Olaf Jan de Groot, Giovanna Labartino, Giovanni Maggi, Elisa Portale, Dani Rodrik, Ricardo Hausman and participants to seminars in Bocconi for their helpful comments. A special thought (and a big good luck!) to Gabriele Deana, who co-authored the first chapter on this dissertation and was of crucial help during the long days in front of STATA.

Introduction

Introduction

The papers that comprise this PhD dissertation deal with the role of trade and institutions as determinants of economic growth worldwide. A consensus has not been reached about their relative importance and statistical significance. This holds true both when dealing with "classical" yearly growth performance, and when analyzing the potential determinant of longer run growth regime switches.

The first essay, "Democracy, openness and jumps in growth", shows that the variables usually employed in empirical studies, such as trade intensity and indicators of the democratization status of a country, do not have impact on the probability that a certain regime trend can shift substantially over the next few years. On the contrary, indicator variables that capture abrupt changes in values of usual growth determinants do not impact yearly growth, but exert a strong effect on the probability of regime trend changes. The paper also shows that the current debate on growth jumps (or growth accelerations) can be more rigorously reassessed in view of matching techniques that allow to isolate the impact of each specific variable on the probability of an economic boom (or slowdown). Results point to the large positive effect of a substantial move towards democratization on the likelihood of an acceleration in GDP growth in subsequent years, contrasting with the findings of Jones and Olken (2008), but in line with those of Hausmann, Pritchett and Rodrik (2005).

The second essay, "Neighbors matter: evidence on trade, growth and productivity" assesses the impact of trade openness (measured as the ratio of imports plus exports over GDP) on real GDP growth. To deal with the common econometric problems that arise when studying this relationship, the paper proposes to instrument domestic trade flows with trade of neighboring

countries, properly netted of bilateral trade flows with the home country. The instrument shows good performance both in cross section and panel data estimation, and in the former context proves to be superior to the well known instrument for trade based on geography and originally proposed by Frankel and Romer (1999). The impact of trade openness on growth is found to be positive and large, with no significant variations across time. An estimation method that takes parameter heterogeneity into account is also proposed and results are very similar to the baseline estimates.

The third essay of this dissertation is titled: "Contagion in policies and institutions". In this paper, the values of trade policies and institutional quality in neighboring countries are used to instrument domestic policies and institutions in a cross-section estimation of income per capita. Contagion is found to be a strong driver of domestic policy measures and institutional arrangements, as many countries respond to what happens in these domains among countries close to their border. Institutions are then found to have a strong and positive impact on income, but trade policies do not show any significant effect on real GDP. These results apply to a variety of trade policy measures and are robust to the inclusion of additional controls and instruments. In a natural extension of the basic estimation, the direct impact of geography on income is also assessed and compared to the contribution of institutions and growth. Even if the estimation strategy is quite dissimilar, the qualitative results of this exercise are remarkably similar to the findings of Rodrik, Submarian and Trebbi (2004): institutions are a strong determinant of income differentials, but neigher trade policies nor geography have any significant direct role in explaining real GDP per capita. Of course, geography still plays a strong role in determining institutions, so that its influence on GDP is mediated.

Overall, the findings contained in the three essays that comprise this dissertation can be summarized as follows:

1. Institutions matter: they do play a large role in explaining both income differentials and (if the definition of institution is enlarged enough to include the political environment) the probability of a significant shift in the growth trend of each country.
2. Trade intensity matters: the trade share of GDP is a strong predictor of both income levels and growth. Thus, this dissertation share the views expressed by Frankel and Romer (1999), Alcala' and Ciccone (2004) and many others on the decisive role of trade flows to promote growth.
3. Trade policies do not matter: none of the three trade policy measures tested in the third chapter of this dissertation exert any significant impact on income differentials. Moreover, the widely used Sachs and Warner index for broad economic policies does not impact on the probability of jumps in growth, as explained in the first chapter.
4. There is strong evidence that what happens in the neighborhood affects domestic variables. Trade flows, institutions and policies abroad have a role in explaining their domestic counterparts.

The last finding is the "file rouge" of the second and third essay, and has been much less explored in the economic literature than the previous ones. On the one hand, "measuring contagion" on such macroeconomic variables is a tricky task, due to the spatially correlated shocks that may underlie common movements in trade, policies and institutions. On the other hand, some controls can and have been readily employed to check that the relationships found do not depend on "usual" transmission mechanisms, such as GDP, trade or investment relations between the home country and its neighbors. Of course, much has to be done yet to fully uncover the potential explanatory power of policies, institutions and other variables in close-by countries, especially when it comes to "explain" such links in a structural way. I hope that the essays contained in this dissertation will prepare the ground for more work on this subject.

Chapter 1

Democracy, openness and jumps in growth

Democracy, openness and jumps in growth[♣]

Gabriele Deana[#] Andrea Gamba*

November 4, 2008

Abstract

We identify multiple structural breaks in a growth series using an algorithm developed by Bai and Perron (1998, 2003). We then regress the indicator of detected positive and negative breaks on a number of variables. We show that smooth growth determinants that are known to impact on yearly growth do not matter on the probability of regime changes. Conversely, external shocks, abrupt shifts in policies and political regimes changes have significant effects both on yearly growth and on swings in growth trends. Program evaluation results show that democratization fosters booms. Moves towards autocracy and economic liberalizations do not impact on the probability of growth jumps, while moves towards autarky undermine the chances of booms and trigger crises. We also study reform sequencing and find that democratization is a driver for growth and subsequent liberalization ensures stability.

Jel Codes: O11, O43, E65.

Keywords: political liberalization, democracy, economic reform, long-run economic growth.

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1. Introduction

It is now widely recognized that growth series show wide variations in time, so that multiple trends may arise for each country. Abrupt variations in growth can impact dramatically on the standard of living of the people; moreover, how to ignited a prolonged increase in growth is of particular interest for policymakers and the recipes may be different from policies aimed at sustaining growth.

Our paper aims to identify the determinants of growth swings, using matching techniques to evaluate each reform impact. Our results show that political liberalization is a key move to raise one country's probability of experiencing a positive jump in growth, while economic liberalization or moving towards autocracy do not have significant impact. In contrast, closing up the economy raises the probability of downturns. These findings provide new insights about which policies might induce accelerations in growth, or prevent growth collapses. We also explicitly compare the impact of abrupt changes in policies on yearly growth and on the probability of growth regime changes. The same comparison is carried out with commonly used "continuous" growth determinants: interesting differences emerge. First, continuous growth determinants do not impact on the probability of a growth regime change. Second, "extreme policies" follow different channels: economic liberalization increases yearly growth, but does not impact on changes in its longer term trend; democratization does not affect yearly growth, but is a powerful determinant of economic booms.

The relationship between growth, institutions and economic policy has been widely studied, leading sometimes to contrasting results[1]. Recently, Hausman, Pritchett and Rodrik (HPR, 2005) and Jones and Olken (JO, 2008) noted that growth series show large

swings over time and analysed such episodes. These two seminal papers yield, interestingly, very different results. HPR show a positive relation between political-regime changes and economic booms, while JO do not find such link. In contrast, economic liberalization is not significant in HPR regressions, while JO find a strong positive association between openness and positive jumps in growth.

Hausmann, Pritchett and Rodrik (2005) first define growth accelerations as episodes of rapid, prolonged growth in real percapita GDP. Such episodes are then regressed on a number of possible determinants. They show that external shocks are important drivers of growth accelerations. Most importantly, though, they find that political changes impact on booms, but that the effects of moves towards autocracy are much stronger than the ones deriving from democratizations. In their paper, economic liberalization does not impact on the majority of high growth episodes, but shows a positive and significant impact on growth accelerations that last longer (thus, on sustained accelerations).

Jones and Olken (2008) allow for positive and negative breaks in a growth series. Breaks detection is achieved through an econometric algorithm originally proposed by Bai and Perron (1998). JO compare the means of several economic variables before and after the detected breaks and find that international trade is positively associated with upwards breaks, while negative breaks are correlated with falls in investment and monetary instability. Political variables do not change significantly around the estimated break points.

Our work focuses on the determinants of swings in growth. We detect large and persistent breaks in a growth series and split them in booms (jumps that leave growth higher than before) and crises (decelerations in growth that lead to a decrease in average growth in the following years). We then regress break indicators on variables capturing

substantial changes in the political and economic policy dimensions, controlling for a number of potential covariates. Both pooled and panel specifications show that democratization fosters booms, while moves towards autocracy do not matter. Openness has an insurance effect against crises; conversely, moves towards autarky effectively prevent growth accelerations. Sharp currency appreciations trigger economic downturns. Exogenous shocks, such as the death of a political leader, are positively associated with booms, but not with crises.

As a corollary to our baseline regressions, we investigate whether extreme policies matter also on "smooth" growth (i.e. yearly growth). Another implication of interest is whether growth determinants usually employed in the literature matter not only for yearly growth, but also for changes in the longer term growth regime.

It turns out that different "extreme" policies matter both for yearly growth and for the probability of shifting a country's growth trend. Democratization fosters booms, but does not raise a country's short term growth. On the contrary, economic liberalization does not help shifting growth regimes, but really boosts yearly growth.

More standard indicators of political and economic features, such as the share of trade over GDP and simple absolute changes in the exchange rates and in the polity score, retain their strong predictive power when employed to explain yearly growth, but fail to have significant impacts on the probability of changing growth trends.

We next focus our attention on the determinants of breaks in growth and consider that our baseline regressions suffer from both self selection bias and potential endogeneity, as other papers in this literature. We then adopt matching techniques to isolate the Average Treatment Effect of single and joint reforms.

Our results on single reforms stand in contrast with previous works. We find that democratization has a strong positive impact on the probability of a positive jump in growth and a negative but statistically insignificant impact on the probability of a negative jump. Moves towards less liberal economic policies, in contrast, have a significant and strong negative impact on the probability of positive shift in growth, and considerably increase the probability of an economic downturn. Interestingly, all other variables do not have a significant impact either on economic booms or crises. In particular, moves towards autocracy and economic liberalizations have a negligible and not statistically significant impact on jumps in growth, irrespective of their direction.

We next study the impact of reforms when they are sequenced in time. We find some evidence that liberalizing the economy after political changes yields different results than when taking liberalization alone. After a move to democracy, a liberalizing the economy reduces the probability of subsequent jumps in growth, be they positive or negative. After a democratic collapse, opening the economy depresses the probability of negative jumps, but (insignificantly) raises the probability of a boom in growth. Democratization, when undertaken after economic reforms (either pro- or anti-market) has still a positive effect on boom probability and a negative effect on downturn probability, but these are not significant any more.

The paper is organized as follows: in section 2 we provide a brief description of the steps involved in the estimation of the breaks; in section 3 we describe the dataset and provide some descriptive statistics; section 4 describes our estimation strategy and discusses the matching techniques to circumvent the problems of pooled and panel regressions. Section 5 presents results from our baseline estimates; compares the impact

of different growth determinants both on growth breaks and on yearly growth; and shows program evaluation results, dealing also with reform sequencing. Section 6 concludes.

2. Detecting breaks in a growth series

We define growth as the yearly percentage change in real per capita GDP. Data are taken from the Penn World Tables, version 6.1 (Summers and Heston, 2002). Real per capita GDP shows different patterns across time and countries. The intuition behind our analysis is that, due to this variability, there is not a single trend in a growth series for a given country. Instead, multiple "growth regimes" can be detected and explained by a variety of factors. These “regimes” are different from usual business cycles, whose time horizon is confined to a few quarters, and are associated with different average growth rates for relatively long time horizons. Obvious examples are the economic boom of western countries in the 60s followed by stagflation in the 70s, or even the advent of the new economy in the US in the 90s. Figure 1 plots the real per capita GDP growth for different countries over time. Large swings are present in all countries and the dashed vertical lines identify potential “turning points” in growth trends. For example, Mozambique shows a very volatile growth path, but before 1973 its growth trend is quite positive, while it turns negative between the mid-70s and the mid-80s. Then, a jump occurs and subsequent growth is high again.

Multiple trends are such a wide phenomenon in growth series, the natural question is whether changing trends matters or not. We believe it does: on the one hand, large

swings in growth can have a dramatic impact on poverty and on people's standard of living[2]; on the other, many policies that have a certain effect on yearly growth can have different impact on the long-run growth path of countries.

In this paper, we focus on the last statement. Before going on, though, we must:

1. find a proper way to detect breaks in the series that are persistent enough to reflect a change in the long term growth pattern of a country;
2. ensure that such detected breaks are not due to a simple increase in the variance of the series over time.

The works by Bai and Perron (1998, 2003) suit well our aims. They design a methodology to search for multiple structural breaks in a structural change model. In particular, they show how to implement a methodology that minimises the global sum of squared residuals of the model resulting from the split of a series according to the detected breaks.

More formally, they start with a pure structural change model

$$Y_t = \bar{Z}\delta_t + u_t \qquad (1)$$

where the regressors are constant over time, but the coefficient is allowed to change if breaks are detected. Suppose a maximum of *m* breaks and *(m+1)* segments is allowed. Then, in (1), $t = T_{j\text{-}1} + 1, ..., T_j$ where $j = 1, ..., m+1$. $T_1, ..., T_m$ are the unknown break dates; by convention, $T_0 = 0$ and $T_m = T$. To estimate the regression coefficient and the break dates, Bai and Perron split the series in two segments and calculate the sum of squared residuals (SSR) resulting from the regression carried out on each spell. Iterating this process to cover all possible breaks, they retain the estimated SSR for each segment and store them in a triangular matrix of order $(T \times T)$, whose rows

represent the initial dates of each segment and columns represent the terminal dates. Therefore, each matrix entry represents the estimated SSR associated to a specific segment. So, any m-partition will be a linear combination of the matrix cells. The dynamic programming algorithm proposed by Bai and Perron compares all possible combinations of the estimated SSRs to achieve a global minimum SSR for the model. Given *h,* the pre-specified length of one segment, the algorithm solves

$$SSR\left(\left\{T_{m,T}\right\}\right) = \min_{mh \le j \le T-h}\left[SSR\left(\left\{T_{m\text{-}1,j}\right\}\right) + SSR\left(j+1,T\right)\right] \qquad (2)$$

In practice, the algorithm computes all the SSR associated with one-break partitions and finds the ones that minimize the global SSR of the model. For each of these optimal partitions, the procedure then computes if and at what point an additional partition can be inserted to minimize the global SSR. After a sequence of such minimization, the last step involves choosing which optimal (m-1) breaks minimize the overall SSR of the model when an additional segment is added. The result is a model with *m* partitions and *(m+1)* segments.

Jones and Olken (2008) test the performance of the procedure described above on small samples, implementing a Monte Carlo experiment. They model a growth process spanning 40 years, allowing for autocorrelation and structural mean shifts of different sizes. They find that the method developed by Bai and Perron is "conservative in detecting breaks, capturing only major accelerations and collapses" (p.583).

In this paper, we employ the methodology described above and adapt it to detects breaks in the growth series of all countries in the Penn World Tables with at least 20 years of data[3]. We get 183 country growth series, spanning a maximum of 50 years. We run the simple regression

$$g(t)=a(R)+e(t) \quad (3)$$

where $g(t)$ is the real annual growth rate in per-capita income, $a(R)$ is the mean growth rate during regime R, and $e(t)$ is an error term drawn from a common distribution across regimes. We set the maximum number of breaks for each country at 5. The minimum interval between breaks (so the minimum length of each segment) is 10% of sample size for each series. So, if a country spans 50 years of data, the minimum length of each segment is 5[4].

As expected, our results are very similar to the ones obtained by Jones and Olken: we find a total of 71 breaks, listed in Table 2. Breaks are featured by both developed and developing countries in all continents.

To check our findings, we analyze the behaviour of the residuals of simple trend regressions around the break points. Thus, we regress the yearly growth rate on its time average and then compute the residuals. As shown in Figure 2, residuals show large and persistent swings at the breakpoints. A simple trend regression does not take into account such swings. Segmenting the growth series at the breakpoints (and running three *separate* trend regressions) leads to a much better fit, as can be easily seen considering the dashed line of Figure 2.

3. Dataset and preliminary evidence

We can now move to the next questions: is there any cause of economic booms or depressions that may be detected? Are there particular events that may increase the

probability of entering a phase of economic expansion, or act as an insurance against the probability of crises?

We start from the estimated breakpoints of section 2. We define two dummy variables that assume value 1 in the five-year time interval (a "window") centred around the year when a break has been detected, zero otherwise. One dummy captures a positive break (one which brings the country to a higher growth path than the one registered in the previous regime); the other dummy identifies negative breaks in a symmetric way. Constructing five-year window is convenient, because this allows to take into account the uncertainty related to the estimation of the breakpoints[5]. There is another reason for keeping "event windows": since we are capturing long-term switches in growth, it makes more sense to investigate what happens not only in the first year of the new regime, but also around the turning point: some shocks may take some years to display fully. Changing the variable definition in order to take as positive outcomes only the point estimates of the breakpoints does not alter the results, although it drives down the proportion of positive outcomes substantially.

We then regress the two dependents on a number of political, economic and external covariates. Our variables of interest are all based on the concept that moderate policies may not have a substantial impact on growth (Easterly, 2001). Therefore, we construct them in order to reflect significant shifts in one potential determinant of growth.

A set of variables identifies political regime changes. They are based on the Polity IV dataset (Jaggers and Marshall, 2007): a regime change is defined as either a three unit 5-year change in the *polity2* variable[6] or a regime interruption. We distinguish

between positive and negative changes, constructing two dummies that take value 1 in the period of change towards democracy or autocracy, respectively.

The economic policy variables are related to the index developed by Sachs and Warner (1995), revisited by Wacziarg and Welch (2003). This index tries to capture the changes in the level of economic openness to trade, combining structural features and macroeconomic environment: we value it as a good proxy to measure broad economic reforms. With the same strategy adopted for the other variables, we create two dummies taking value 1 in the first five years of a transition towards, respectively, "openness" or "autarky".

Exchange rate “shocks” are constructed following Hausmann, Pritchett and Rodrik (2005). we define a dummy variable that takes value 1 whenever the change in the exchange rate with respect to the preceding 5 years falls in the upper decile of the changes experienced by all countries. In the same way, we define negative shocks to the exchange rate[7] when their 5-year change falls in the lowest decile.

Jones and Olken (2002) construct a variable that captures the death of political leaders that are due solely to exogenous factors, such as accident, illness or age. We use this variable to isolate political shocks and define an indicator that takes value one in the year of a political leader’s accidental death and in the following four years, zero otherwise. We also interact such dummy with the number of years that leader had been in power at the time of death, to get a sense of his potential influence on a country’s political and economic shape.

After cleaning the dataset, we end up with 3776 valid observations. Table 3 shows the frequencies of both the dependent and the explanatory variables. Including all windows, we get 109 positive outcomes for economic booms and 171 positive

outcomes for economic slowdowns. Moves towards democracy are relatively more frequent than abrupt changes towards autocracy. Economic liberalizations are over four times more frequent than moves to autarky. Strong declines in real exchange rate are more frequent than appreciations. Finally, death of leaders are a relatively infrequent event, and the average tenure of the leader at his death is slightly over ten years.

Table 4 splits positive and negative outcomes of the explanatory variables according to the final outcome of the dependent. Here we observe a striking result: moves towards autarky are never associated with economic booms, and economy-wide liberalizations are never associated with economic slowdowns. Thus, economic reforms act as a powerful insurance against a prolonged and significant decrease in economic growth. Moreover, leaders' tenures at their death are longer when they are associated with economic booms.

To get an idea of correlations between our variables of interest, Table 5 presents two linear probability models. In column (1) the dependent variable is economic booms (poswinbreak); in column (2) the dependent is negative jumps (negwinbreak). Economic liberalization have a positive insignificant impact on economic booms and a negative one on crises; moves to autarky have the reversed effects. Democratization is strongly associated with booms, but not with negative jumps in growth. Moves towards autocracy, on the other hand, have no impact on booms, while considerably increase the probability of a fall in growth.

4. Estimation

We start running two distinct pooled logit models, one for negative and one for positive breaks, using all the variables described above as regressors:

$$y_{it} = \alpha + \beta_1 democratization_{it} + \beta_2 autocracy_{it} + \beta_3 liberalization_{it} + \beta_4 autarky_{it} + \beta_5 appreciation_{it} + \beta_6 depreciation_{it} + \beta_7 leader\_death_{it} + \beta_8 death * tenure_{it} + \gamma Z_{it} + \varepsilon_{it} \quad (4)$$

where Z_{it} is a vector of controls. Among them, we include yearly growth, since we are interested in the additional effects that covariates may have on growth regimes *on top* of yearly growth impacts. Secondly, we add the democracy score as indicated by the *polity2* score of the PolityIV dataset. Indicators of ongoing conflicts (taken from Sarkees, 2000) and dummies to identify developing countries and LDCs are also included. Finally, an expansion in world economic activity could positively influence the probability of a growth long term acceleration or decline; to take this into account, we construct a variable that proxies world growth in a given year: it is the sum of he growth rate of neighbouring countries. We also include year and decade fixed effects in separate specifications. To evaluate each outcome separately against the baseline of no jumps in growth, we also run a multinomial logit regression.

The procedures just set out would yield only statistical correlations. To get a grip on causal relationships, we have to tackle three issues:

1. A pooled analysis might miss one of the key characteristics of our data: their panel structure.
2. A well-known problem in the literature is the possibility of "selection bias": countries may self-select into certain political regime or economic

reforms, according to their characteristics. Reform impacts may also be correlated with other observable features of each country.

3. Endogeneity has to be tackled to rule out reverse causation: it could be that reforms are undertaken because the economy has entered a boom, and not the other way round.

4.1 Dynamic inconsistency

Panel data have been widely employed in the growth literature, but the emerging stream focusing on growth episodes has limited its analysis to pooled regressions. To check whether this approach is correct, we conduct a simple test of dynamic consistency (Wooldridge, 2001. We find that the lagged residuals of the pooled regressions are correlated with the dependent. Pooled estimates would then yield inconsistent results. We then turn to a panel specification. The fixed effects hypothesis delivers consistent estimates, at the cost of conditioning our analysis to the fact that one country experiences at least one boom or one crisis in our data.

4.2 Selection bias and endogeneity

Democracies may be more likely to liberalize the economy than, say, autocracies; moreover, the impact of one particular reform could affect a country in a different manner, according to its institutional or economic setting. Taking economic liberalization as an example, it would be impossible to determine how much of the

probability of jumps is due to the direct effect of economic liberalizations and how much to the indirect effect of democratization.

An additional problem arises from the potential endogeneity of our variables: an economic boom might provide a favourable setting to introduce potentially unpopular economic reforms, or to make political shifts more or less likely.

A program evaluation approach can be employed to effectively tackle both selection bias and reverse causation. Difference-in-difference estimation is appropriate to evaluate variables that are exogenous by construction, such as macroeconomic shocks[8] and the accidental death of a political leader. On the other hand, the impact of potentially endogenous treatments should be evaluated with the tool of propensity score matching.

We have four potential endogenous treatments: economic liberalizations, moves towards autarky, democratic revolutions and democratic breakdowns. We deal each treatment separately. First, we construct a variable (the propensity score) that summarizes the likelihood of each country to be treated in any given year. The determinants of this propensity score are its other observable characteristics[9].

We next split treated and non treated countries (treated ones are countries experiencing that particular reform at least once) and match them according to their propensity score. Each treated country is matched with all controls in our sample, but each control is weighted according to the inverse of its distance, in terms of propensity score, from the treated country under consideration[10]. We thus construct a "synthetic control" that shows population characteristics similar to each treated unit; comparing the latter with this control, we can infer the Average Treatment of on the Treated.

5. Results

Baseline regression findings are summarized in Table 6 and show that a change towards democratization unambiguously fosters boom does not significantly impact on crises (though the sign of the coefficient is, reassuringly, negative). Moves towards autocracy do not have significant impact on jumps in growth. The death of a leader does not have any impact *per se* on the chances of growth transition; when interacted with the number of years the political leader had been in power, though, the variable shows a strong positive effect on the probability of booms. The probability of a crisis remains unaffected even by the interaction term. This result is fully consistent with the one found by Jones and Olken (2005). Exchange rate appreciations have a negative impact on the economy, increasing the chance of a slowdown in the subsequent 5 years. This is explained with the higher price of the home country exports, that depress international demand for these goods and puts domestic firms under pressure.

One striking feature of our data is that the economic liberalization variables almost completely predict the behaviour of our dependent. In particular, an opening of the economy is never associated with a negative jump in growth, and a move towards autarky never happens close to an economic boom. Our results show the models retaining these "perfect predictors", in order to give an idea of the potential importance of economic reforms on preventing negative shocks to growth. Table 7 shows that when dropping perfect predictors, our results are confirmed and are even sharper.

From an economic viewpoint, democratization has an explosive effect on the probability of growth acceleration. While, in the pooled sample, the average probability

of a boom is only 1.37%, moving towards democracy more than quadruples it (the effect of a discrete change in the democratization variable increases the probability of booms by 310%). Similarly, the probability of an acceleration in growth increases by 5.8% for any year of power of political leaders that have just passed away. On the other hand, the sample averaged probability of entering into an economic contraction is even lower (0.04%), but an exchange rate appreciation more than doubles that value (the marginal effect is 106%).

Table 8 presents results from a multinomial logit specification: they are fully consistent with the ones in the previous Tables and confirm that democratization improves the chance of booms, while not affecting negative breaks. Moves to autocracy impact neither accelerations nor decelerations in growth, but the death of a political leader positively and significantly affects the chance of a boom. Economic liberalizations heavily insure against crises, while moves towards autarky significantly depress the probability of a positive jump in growth.

Panel fixed effect results are shown in Tables 9 and 10, and broadly resemble the main results of the pooled analysis. The positive coefficient of democratization on booms is even larger, in all specifications. Moves towards autocracy do not have impact of either boom or crises, except a weakly negative effect on crises in the specification[11] with year dummies. The economic liberalization variables keep the same sign and significance as in the pooled analysis. The accidental death of a political leader turns out to be significant in the specification with year fixed effects. There, the death of a leader *per se* is highly detrimental for the chance of growth accelerations, but any additional year of ruling of that leader has a positive coefficient. This result could be explained assuming that the more a leader holds power, the more he can prepare its

successors, so concerns about a gap in political power are mitigated. Exchange rate appreciations reduce the probability of a boom, confirming the results from the pooled regressions; depreciations do not have any impact on either booms or crises.

5.1 Smooth and "extreme" policies: different channels?

What is important to stress is that, since we control for growth in all specifications, we are effectively separating the influence of our independent variables on non-smooth growth episodes from their impact on yearly growth. It is then natural to ask whether the extreme policies used in the right hand side of (4) impact differently on yearly GDP growth than on the probability of changing longer term regime trends. We run two regressions with yearly GDP growth as the dependent variable and the same set of right hand side variables as above. Results are shown in Table 11: column 1 contains the pooled regression coefficients, column 2 shows estimates with panel fixed effects. Both specifications show a positive and very significant impact of economic liberalization on growth. Exchange rate depreciations significantly depress growth, while political change variables do not have significant effects.

These results, together with evidence provided in Tables 6-10, show that political and economic variables impact growth differently if we look at its short-run, smooth variation, or at the probability of improving (or depressing) its longer-horizon *trend*. Reforms that seem not to have an immediate impact, such as substantial political changes, turn out to be the best recipe to raise a country's long run performance. On the other hand, economic liberalizations seem to have an important and significant impact both in raising yearly growth, and in insuring against future downturns. Monetary shocks, such as sudden devaluations of the exchange rate, significantly depress yearly

growth; on the other hand, exchange rate appreciations seems not to impact on growth immediately, but increase significantly the probability of a recession in the longer run.

We next investigate the impact on growth regime changes of "smooth" policies. In Table 12, we use as regressors the variables commonly used in standard growth regressions. These variables can be regarded as the continuous version of the extreme policy indicators employed in the previous sections. We take the fraction of international trade over GDP as a proxy for economic openness; we use the simple 5-year difference in the exchange rate instead of an indicator of a shock in its change relative to other countries. Political variables are captured by the simple polity2 score and by its 5-year variation. We retain the controls of Tables 6-10 and include the indicator variables of leader deaths. Results are interesting. Openness impacts negatively on the probability of jumps in growth, either positive or negative. The level of democracy has a negative impact on growth swings of either kind, but such impact is significant only on positive jumps and with pooled estimates. A change in the polity score, on the other hand, has a positive impact on growth regime changes, but again significance is retained only with pooled regressions on positive jumps. Changes in exchange rate have negligible effects, as political leader deaths. The interaction term between a leader's death and its tenure is significant only in column (1). Dummies for developing and LDC countries are significant, showing that developing countries have a significantly higher chance of experiencing a boom, while LDC countries are much less likely to see their growth jump, either up or down. Finally, in pooled regressions, conflicts are associated with both upward and downward swings.

Looking at columns (2) and (4) of Table 12, we find a striking result: "smooth" variables capturing economic and political phenomena do not impact on the probability

of breaks in a growth series, once the time dimension is correctly taken into account. Comparing these findings with the ones in Tables 11 and 12 teaches us that, consistently with the claim of Easterly and Levine (2001), what matters for improving longer term economic growth are extreme policies. Mild deviations in economic and political behaviour do not matter.

5.2 Matching

One issue worth discussing is the nature of the treatments we want to analyze. Many countries, especially non-OECD ones, show multiple transitions in the time spanned by our dataset, with reform-reversals, democratic revolutions and subsequent coups. Examples on political variables include Ghana, Nigeria, Pakistan, Peru and Thailand. Economic reforms have been reversed at least once in Costa Rica, Ecuador, Honduras, El Salvador, Jamaica, Nicaragua, Peru, Sri Lanka and Venezuela. We deal with this problem in a simple way: we pool all our observations together and consider treated the countries that experience at least one time the treatment under consideration. Treatments are defined as periods following one particular reform and last until a subsequent reform reverses it. For example, Pakistan experiences its first move towards democracy in 1962 and progressively improves its democratization score until 1976. The period spanning 1962-1976 is then considered as a treatment period for democratization and Pakistan is considered under treatment in all these years. In 1977, though, there is a coup d'etat that pushes the country into a dictatorship that lasts until 1988. All years from 1977 to 1988 are considered treatment periods for a move to autocracy. Finally, a ten year "democratic" period follows from 1988 to 1998, before

the last coup takes place in 1999. This implies that, in case of multiple successive treatments, in computing the ATT we simply average out the effects of different treatment periods, considering as controls all countries that never experienced such a treatment.

We then proceed with matching treatment and controls; we start dealing each treatment separately. We run probit regressions for each of our potentially endogenous treatments. In order to ensure better quality of our matches, we condition the regression on observations within the common support[12], the ones for which the probability of treatment rests strictly inside the interval [0,1]. Results from the propensity score estimation are shown in Table 13. In matching observations, on the one hand, we needed to ensure that the variables on the right hand side of each equation were good treatment predictors; on the other hand, we needed to be as parsimonious as possible in order to satisfy the balancing property. The estimated probability of treatment generally rests under .7 in all our specifications, but in two cases (democratization and moves to autocracy) there are relatively few controls for the treated units close to the upper bound of the common support . Dropping these handful of observations does not alter our findings. No problems arise in matching treated and controls close to the lower bound of the common support.

To ensure that our matching strategy works well, we run a simple mean-difference test[13] for treated and controls on all covariates and we never obtain results that reject the null of no-difference in the matched pairs at conventional levels[14]. We also run a likelihood ratio test on the propensity score specification before and after matching. Table 14 shows that it is impossible to predict among two matched units which one is going to be treated and which will act as a control.

The program evaluation estimates, shown in Table 15, indicate that moves towards democracy have a positive and very significant effect on the probability of a boom; on the other hand, democratization is successful in lowering the probability of a negative jump in growth. Liberalization does not impact significantly on growth jumps, but moves to autarky significantly increase the probability of a subsequent economic crisis. Exchange rate shocks and political leader deaths, on the other hand, seem not to have any particular influence neither on booms nor on crises.

5.3 Reform sequencing and a new look at single reforms

Economic liberalization could be a good thing for democracies, but a bad one autocratic systems. As anticipated, our data show several countries adopting not just a single economic or political reform, but experiencing subsequent, and potentially intertwined, phases of liberalization, democratization and their reversal. In this section, we try to answer the questions: do joint reforms exert special effects on jumps in growth? Does the sequencing of reforms really matter?

Giavazzi and Tabellini (2005) answer similar questions on smooth growth with simple difference in difference estimation, separating countries that liberalize the economy after becoming a democracy from the ones that experience the reversed sequence. They find that a country that liberalizes the economy first and then becomes a democracy gets a higher benefit than it would if it engaged in the opposite sequence.

Here we study the effect of joint reforms on the probability of growth accelerations and collapses. Treated countries are defined as the ones that, having already experienced one transition (say, to democracy), subsequently enter another phase of reform (say, economic liberalization). In practice, we construct a variable, called *demo_open*, that is one when a country already experiencing a democratization phase

liberalizes its economy. Conversely, the variable *open_demo* captures events when one country, after liberalizing the economy, moves towards democracy. We proceed in the same way for all possible sequences of political and economic transitions, and end up with 8 mutually exclusive treatments[15]. We consider as controls only the countries that did experience the first reform in the sequence, but did not start the second one.

In this way, we effectively isolate the impact of reforms on countries that are already benefiting from a past reform (that has not been reversed). Table 16 shows the results: economies that experience a democratization and then open up their markets see the probability of incurring into jumps in growth (either positive or negative) significantly reduced. Apparently, then, liberalizing the economy after democratization stabilizes the current growth regime, preventing swings in growth to happen. Conversely, economies that move to autocracy first, and then liberalize the economy, see their probability of incurring into a crisis reduced, but their probability of a positive jump in growth is unaffected. Becoming a democracy after liberalizing or closing up the economy does not affect the probability of a change in the growth regime, in either direction.

Another advantage of considering joint reforms consists in allowing us to better study the effect of *single* reforms. In Table 15, in fact, our results could have been driven not just by the treatment under consideration, but also by a subsequent and different reform taking place in the treatment period. We are now ready to control for this potential bias, restricting our analysis of political and economic reforms only to periods when only one reform is in place. We then compute the Average Treatment effect on the Treated and obtain the results shown in Table 17. Democratization increases significantly the probability of experiencing a boom, while moves towards

autocracy do not impact the probability of changing the growth regime. Economic liberalization has no significant effect either on the probability of a boom, or on the probability of a negative jump in growth. Moves towards autarky, in contrast, significantly depress the probability of growth accelerations, and increases the probability of slumps.

The results coming from Tables 16 and 17 can be easily compounded: democratization *per se* increases the probability of a boom (column 1 in Table 17); a subsequent liberalization (column 1 in Table 16) adds an insurance against falling into a crisis, but also hinders the chance of an additional acceleration. Interestingly, compounding the effects of democratization *per se* and subsequent liberalization gives an overall negative (insignificant) impact on the probability of boom[16]. Persson and Tabellini (2006) provide estimates similar to ours of the compounded effect on yearly growth (they find a barely positive and insignificant overall impact).

Moves towards autocracy do not affect the probability of switching growth regime. Opening up the economy is again significant in lowering the probability of a negative jump if taken after moving to autocracy.

We have seen that liberalization does not have a significant impact on growth swings when taken alone, and that subsequent democratization does not alter regime-switch probabilities. We can then conclude that going democratic first and then opening up the economy is a better reform strategy than the reversed sequence. Giavazzi and Tabellini (2005) found that the best reform sequence to achieve a higher growth rate is to liberalize the economy first, and then move to democracy. Our findings suggest that to increase the probability of a positive jump in long term growth what matters is going

democratic first. Subsequently opening up the economy adds stability, but does not promote any further acceleration in growth.

Finally, closing up the economy has a negative and significant impact on the probability of booms, and subsequent democratization does not alter the chance of switching growth regime further.

The evidence set out above is striking, if we consider the growing anecdotal evidence on fast-growing, market-oriented countries whose government are far from democratic. But these anecdotes are limited to a few relatively large or resource-rich countries, such as China, Russia and some central-Asian economies. The south-east Asian tigers, with the exception of Taiwan, had either been always open in our time span (Thailand, in our sample), or have liberalized their economy during a period of relative democratization, as South Korea did.

5.4 A summary of results

Throughout the paper many results have been shown. To clarify our findings, it is better to collect them in a single picture. This is done in Table 18. In Columns (1)-(4) the dependent variable is the probability of a positive shift in growth. Democratization impacts positively and very significantly throughout the specifications. A move to autocracy has mixed results, and the Program Evaluation estimates indicate an insignificant positive relationship. Economic liberalization has a negligible and insignificant impact on booms; its sign changes when matching is employed. Moves towards autocracy significantly depress the chance of an acceleration in growth. The evidence on other variables is more mixed and generally not significant.

Turning to economic slowdowns, it seems that political liberalization have a negative but insignificant impact on them. The same holds true for changes towards autocracy. Economic liberalization acts as an insurance in cross section and panel estimates, but the sign changes when more rigorous matching is adopted. Conversely, changes to autarky raise the threat of a growth collapse, and program evaluation yields moderately significant estimates. Other variables significance does not withstand the matching estimator.

6. Conclusion

Cross country and panel growth regressions usually consider continuous explanatory variables for yearly growth. We show that such "smooth" variables do not impact on a country's probability to change its longer-term growth regime.

We then turn to more "extreme reforms" indicators and find that they have a strong impact both on yearly growth and on the probability of growth regime changes. Their effects, though, are substantially different, depending on the investigated "horizon" of growth. Liberalization increases year on year growth, but apparently does not raise the probability of a boom; democratization does not influence smooth growth, but plays an important role in making future booms more likely.

A program evaluation approach allows to considerably reduce the potential bias due to self selection, heterogeneity and reverse causation when studying growth regime changes. We find that big shifts towards democracy can be regarded as catalysers of prolonged economic expansion. Liberalization does not matter on growth jumps, but move to autarky increase the threat of a negative swing in growth.

We also find that the sequencing of reform matters and that it is advisable to promote democratization first, and then liberalize the economy, rather than implementing the opposite sequence.

In this paper we have found robust empirical evidence that the channels through which continuous and "extreme" determinants impact growth are different. The next step would be to understand what are the mechanisms behind each channel.

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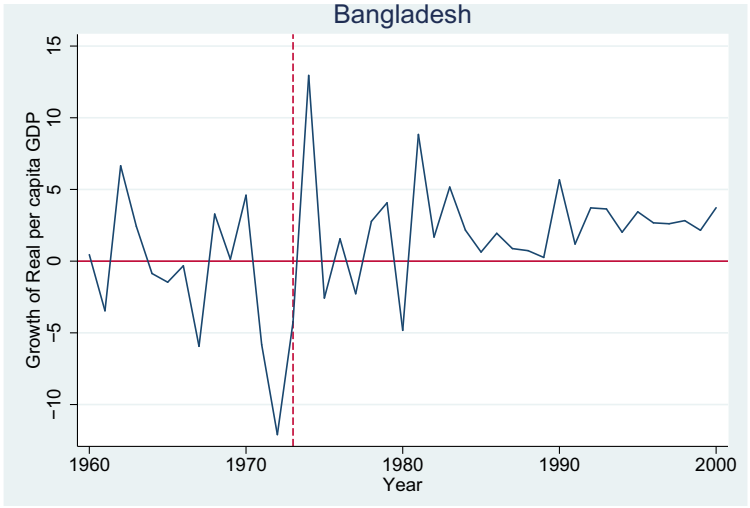
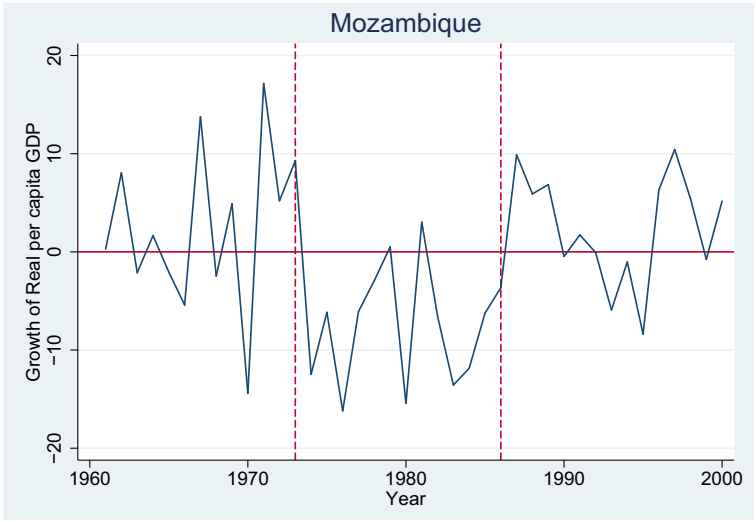
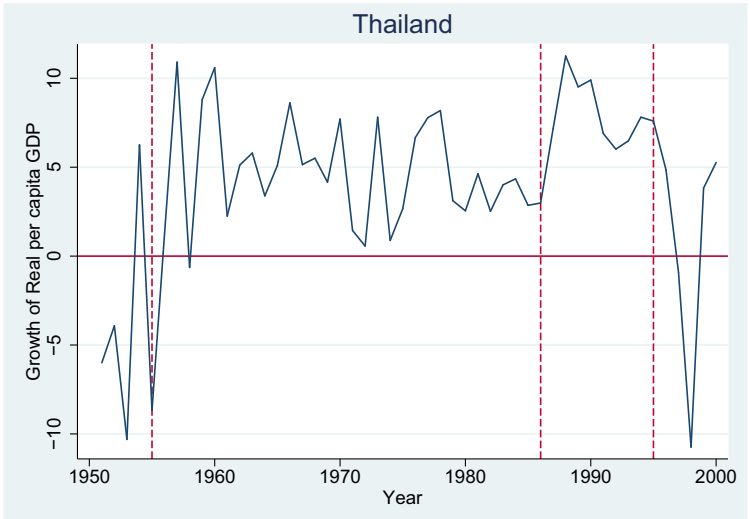
Figure 1: Growth series for a number of countries

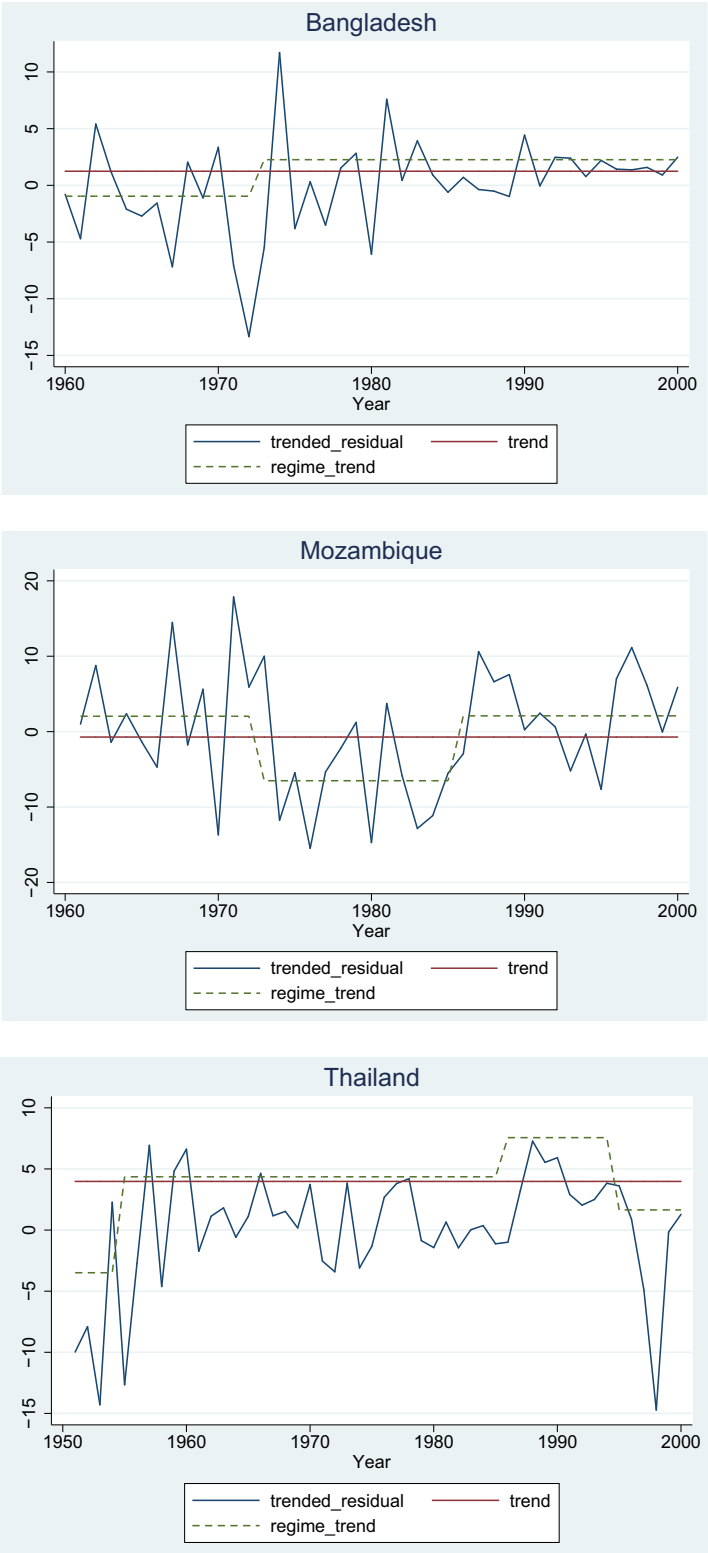






Note: dashed lines denote breakpoints in the series.

Figure 2: Residuals behaviour around the breakpoints



Note: *trend* is the coefficient of the simple trend model; *regime trend* depicts the different coefficients from the breaking of the growth series at the estimated breakpoints. *Trended residuals* stands for residual from the simple trend regression.

Table 1: Length of each country growth series

country	starting year	ending year	sample length	country	starting year	ending year	sample length
Algeria	1967	2000	34	Kenya	1968	2000	33
Argentina	1955	2000	46	Korea, Repub	1958	1998	41
Australia	1955	2000	46	Madagascar	1965	2000	36
Austria	1955	1998	44	Malawi	1969	2000	32
Bangladesh	1977	2000	24	Malaysia	1962	2000	39
Belgium	1955	1996	42	Mali	1965	2000	36
Benin	1965	2000	36	Mauritania	1965	2000	36
Bolivia	1955	2000	46	Mauritius	1973	2000	28
Botswana	1971	2000	30	Mexico	1955	1996	42
Brazil	1955	1998	44	Morocco	1961	2000	40
Burkina Faso	1965	1998	34	Mozambique	1980	2000	21
Burundi	1967	2000	34	Nepal	1965	2000	36
Cameroon	1965	1996	32	Netherlands	1955	2000	46
Canada	1955	2000	46	New Zealand	1955	2000	46
Chad	1965	2000	36	Nicaragua	1955	1998	44
Chile	1956	2000	45	Niger	1965	2000	36
China	1957	1998	42	Nigeria	1965	2000	36
Colombia	1955	2000	46	Norway	1955	2000	46
Costa Rica	1955	2000	46	Pakistan	1955	2000	46
Cote d'Ivoir	1965	1998	34	Paraguay	1956	2000	45
Cyprus	1965	1997	33	Peru	1955	1999	45
Denmark	1955	2000	46	Philippines	1955	1996	42
Dominican Re	1956	2000	45	Poland	1975	1998	24
Ecuador	1956	1996	41	Portugal	1955	1996	42
Egypt	1955	1994	40	Rwanda	1966	2000	35
El Salvador	1955	1994	40	Senegal	1965	2000	36
Ethiopia	1955	2000	46	Sierra Leone	1966	1998	33
Finland	1955	1998	44	Singapore	1967	2000	34
France	1955	1998	44	South Africa	1955	1998	44
Gabon	1965	2000	36	Spain	1955	1998	44
Ghana	1965	2000	36	Sri Lanka	1955	2000	46
Greece	1956	1998	43	Sweden	1956	1998	43
Guatemala	1955	1996	42	Switzerland	1955	1998	44
Guinea	1964	2000	37	Syria	1966	2000	35
Guyana	1971	2000	30	Taiwan	1956	1999	44
Haiti	1965	1995	31	Tanzania	1966	2000	35
Honduras	1955	2000	46	Thailand	1955	1996	42
Hungary	1975	2000	26	Togo	1965	2000	36
India	1955	2000	46	Tunisia	1966	1996	31
Indonesia	1965	1996	32	Turkey	1955	2000	46
Iran	1960	1996	37	U.K.	1955	2000	46
Ireland	1955	1998	44	U.S.A.	1955	2000	46
Israel	1955	2000	46	Uganda	1967	1999	33
Italy	1955	1998	44	Uruguay	1955	2000	46
Jamaica	1964	1997	34	Venezuela	1955	1998	44
Japan	1957	1994	38	Zambia	1969	2000	32
Jordan	1959	2000	42	Zimbabwe	1975	2000	26

Table 2: Estimated break points

Positive breaks				Negative breaks					
country	**year**	**country**	**year**	**country**	**year**	**country**	**year**	**country**	**year**
Bangladesh	1973	Iran	1981	Austria	1974	Hungary	1979	Poland	1980
Belgium	1958	Ireland	1994	Belgium	1974	Indonesia	1996	Portugal	1973
Botswana	1966	Japan	1959	Brazil	1980	Iran	1976	Romania	1985
Burkina Faso	1966	Korea, Republic of	1962	Cameroon	1987	Italy	1974	South Africa	1981
Cameroon	1993	Luxembourg	1983	Congo, Dem. Rep.	1974	Jamaica	1972	Spain	1974
China	1978	Mauritius	1960	Cote d'Ivoire	1979	Jamaica	1976	Sweden	1970
Ecuador	1971	Mexico	1995	Ecuador	1977	Japan	1970	Switzerland	1973
Egypt	1975	Mozambique	1986	Egypt	1970	Japan	1991	Thailand	1995
El Salvador	1983	Papua New Guinea	1991	Egypt	1980	Mexico	1981	Tunisia	1972
El Salvador	1991	Philippines	1986	El Salvador	1978	Mozambique	1973	Venezuela	1970
Equatorial Guinea	1995	Portugal	1966	Equatorial Guinea	1974	Nicaragua	1977	Zambia	1964
Guatemala	1955	Thailand	1955	Finland	1973	Papua New Guinea	1994	Zimbabwe	1976
Guatemala	1987	Thailand	1986	France	1973	Philippines	1956		
Haiti	1991	Tunisia	1967	Greece	1973	Philippines	1981		
Indonesia	1967			Guatemala	1980	Poland	1977		

Total positive breaks: 29
Asia 10
Africa 8
South America 7
Europe 4
Developing countries 20
Developed countries 9

Total negative breaks: 42
Asia 8
Africa 11
South America 9
Europe 14
Developing countries 24
Developed countries 18

Grand total: 71

Table 3: Number of positive and negative outcomes for dependent and explanatory variables

	positive outcome	negative outcome
Positive jump in growth	109	3667
Negative jump in growth	171	3605
Democratization	326	3450
Move to autocracy	191	3585
Economic liberalization	348	3330
Move to autarky	80	3598
Exchange rate depreciation shock	369	3407
Exchange rate appreciation shock	282	3494
Death of leader	49	3727
Conflict	390	3379
Leader's average tenure at death	10.06	
N	3776	

Table 4: Number of positive and negative outcomes for each regressor, conditional on jumps in growth.

	Positive jumps		Negative jumps	
	positive outcome	**negative outcome**	**positive outcome**	**negative outcome**
Democratization	25	84	8	163
Move to autocracy	4	105	6	165
Economic liberalization	7	98	0	169
Move to autarky	0	105	4	165
Exchange rate depreciation shock	9	100	10	161
Exchange rate appreciation shock	9	100	27	144
Death of leader	2	107	4	167
Conflict	27	82	29	142
Leader's average tenure at death	20.02		12.4	
N	109		171	

Table 5: Linear probability model of a growth regime change on economic and political liberalizations

	positive jumps	negative jumps
	(1)	(2)
Democratization	0.064***	-0.011
	(0.009)	(0.011)
Move to autocracy	-0.005	-0.031**
	(0.011)	(0.015)
Economic liberalization	-0.012	-0.023*
	(0.009)	(0.012)
Move to autarky	-0.037**	0.019
	(0.019)	(0.024)
N	3678	3678
Adjusted R-squared	0.013	0.049

Note: Standard errors in parenthesis
*Significant at 10%; ** significant at 5%; ***significant at 1%

Table 6: Pooled logit

	Booms			Crises		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization	1.590***	1.687***	1.687***	-0.173	-0.132	-0.174
	(0.262)	(0.269)	(0.301)	(0.322)	(0.323)	(0.345)
Move to autocracy	-0.599	-0.633	-0.709	-0.639	-0.755	-0.775
	(2.285)	(2.293)	(576109)	(0.882)	(0.909)	(0.751)
Economic liberalization	-0.217	-0.214	-0.339	-17.76***	-16.95***	-17.78***
	(0.705)	(0.734)	(0.674)	(0.486)	(0.473)	(0.478)
Move to autarky	-17.69***	-17.97***	-18.10	-0.196	0.158	0.396
	(0.569)	(0.632)	(2279247)	(2.257)	(2.375)	(2.407)
Exchange rate depreciation shock	-0.367	-0.340	-0.368	-0.567	-0.596	-0.593
	(0.432)	(0.422)	(0.474)	(0.363)	(0.385)	(0.400)
Exchange rate appreciation shock	0.0176	0.00509	0.0623	0.754***	0.714***	0.662***
	(0.888)	(0.911)	(0.830)	(0.229)	(0.236)	(0.255)
Death of leader	-0.528	-0.599	-0.874	-0.201	-0.244	-0.325
	(0.514)	(0.518)	(0.733)	(0.603)	(0.616)	(0.666)
Tenure at leader death	0.0597***	0.0739***	0.101***	0.00676	0.00004	0.00725
	(0.0203)	(0.0213)	(0.0376)	(0.0503)	(0.0461)	(0.0506)
Real percapita GDP growth	0.0111	0.0116	-0.0782	-0.0304*	-0.0265*	0.0840
	(0.0165)	(0.0179)	(10309)	(0.0171)	(0.0161)	(4197)
Rest of world growth	-0.0004	0.0006	-0.0912	0.002***	-0.00007	0.111
	(0.0008)	(0.001)	(10309)	(0.0008)	(0.0008)	(4197)
POLITY2 score	-0.0538***	-0.0563***	-0.0522***	-0.0508***	-0.0348***	-0.0310**
	(0.0179)	(0.0181)	(0.0192)	(0.0133)	(0.0133)	(0.0145)
Developing country	0.513*	0.566**	0.676**	-0.331	-0.226	-0.188
	(0.272)	(0.281)	(0.307)	(0.207)	(0.219)	(0.233)
Ldc	-1.368***	-1.252***	-1.328***	-18.37***	-18.08***	-17.98***
	(0.386)	(0.382)	(0.391)	(0.672)	(0.351)	(0.483)
Conflict	0.974***	0.994***	1.020***	0.604**	0.520**	0.548**
	(0.278)	(0.287)	(0.307)	(0.239)	(0.251)	(0.264)
Constant	-3.988***	-4.449***	-9.648	-2.956***	-3.588***	-37.13
	(0.262)	(0.440)	(1648268)	(0.222)	(0.331)	(1042297)
Observations	3653	3653	3653	3653	3653	3653
Pseudo R-squared	0.110	0.133	0.163	0.113	0.196	0.250

Note: (1) and (4) baseline regressions. (2) and (5) with decade fixed effects. (3) and (6) with year fixed effects. Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Pooled logit dropping perfect predictors

	Booms			Crises		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization	1.315***	1.427***	1.453***	-0.524	-0.402	-0.475
	(0.262)	(0.273)	(0.251)	(0.346)	(0.350)	(0.335)
Move to autocracy	-0.930*	-0.961*	-1.053**	-0.123	-0.318	-0.334
	(0.560)	(0.566)	(0.486)	(0.349)	(0.365)	(0.326)
Economic liberalization	-0.252	-0.251	-0.374			
	(0.461)	(0.464)	(0.431)			
Move to autarky				0.210	0.606	0.863
				(0.602)	(0.605)	(0.572)
Exchange rate depreciation shock	-0.356	-0.349	-0.369	-0.527	-0.529	-0.549
	(0.417)	(0.416)	(0.375)	(0.359)	(0.371)	(0.356)
Exchange rate appreciation shock	0.036	0.027	0.058	0.874***	0.841***	0.781***
	(0.403)	(0.422)	(0.426)	(0.225)	(0.232)	(0.250)
Death of leader	-0.379	-0.444	-0.354	0.584	0.783	0.693
	(0.692)	(0.694)	(0.817)	(0.637)	(0.676)	(0.627)
Tenure at leader death	0.048***	0.059***	0.071***	-0.001	-0.014	-0.009
	(0.017)	(0.018)	(0.019)	(0.035)	(0.036)	(0.024)
Real percapita GDP growth	0.008	0.007	0.008	-0.020	-0.014	-0.011
	(0.018)	(0.019)	(0.019)	(0.016)	(0.015)	(0.015)
Rest of world growth	-0.000	0.001	0.000	0.003***	-0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
POLITY2 score	-0.052***	-0.055***	-0.054***	-0.037***	-0.017	-0.012
	(0.018)	(0.018)	(0.018)	(0.014)	(0.015)	(0.014)
Developing country	0.512*	0.554**	0.625**	-0.632***	-0.532**	-0.512**
	(0.272)	(0.276)	(0.266)	(0.224)	(0.240)	(0.227)
Ldc	-1.328***	-1.226***	-1.303***			
	(0.376)	(0.376)	(0.355)			
Conflict	0.966***	0.989***	0.993***	0.640***	0.585**	0.653***
	(0.272)	(0.280)	(0.255)	(0.230)	(0.239)	(0.234)
Constant	-3.956***	-4.412***	-4.834***	-3.171***	-3.904***	-2.284***
	(0.255)	(0.439)	(0.698)	(0.235)	(0.338)	(0.377)
Observations	3653	3653	3391	3653	3653	2869
R-squared	0.093	0.114	0.130	0.039	0.129	0.130

Note: (1) and (4) baseline regressions. (2) and (5) with decade fixed effects. (3) and (6) with year fixed effects. Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Multinomial logit

	Positive jumps (1)	Negative jumps (2)
Democratization	1.283***	-0.441
	(0.260)	(0.354)
Move to autocracy	-0.921	-0.288
	(3.478)	(0.348)
Economic liberalization	-0.338	-33.800***
	(1.230)	(2.027)
Move to autarky	-33.851***	-0.289
	(1.982)	(4.674)
Exchange rate depreciation shock	-0.384	-0.576
	(0.418)	(0.358)
Exchange rate appreciation shock	0.137	0.755***
	(1.630)	(0.229)
Death of leader	-0.585	-0.224
	(0.509)	(0.613)
Tenure at leader death	0.065***	0.013
	(0.020)	(0.052)
Real percapita GDP growth	0.006	-0.030*
	(0.018)	(0.017)
Rest of world growth	-0.0002	0.003***
	(0.0008)	(0.0008)
POLITY2 score	-0.053***	-0.049***
	(0.018)	(0.013)
Developing country	0.536*	-0.280
	(0.278)	(0.207)
Ldc	-1.442***	-34.424***
	(0.384)	(2.061)
Conflict	1.002***	0.677***
	(0.280)	(0.239)
Constant	-3.883***	-2.930***
	(0.257)	(0.223)
Observations	3653	
Pseudo R-squared	0.109	
Log likelihood	-1026.072	

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Panel fixed effects

	Positive jumps			Negative jumps		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization	2.258*	2.492*	2.915***	0.105	-0.283	-0.415
	(1.237)	(1.436)	(0.459)	(0.538)	(0.531)	(0.436)
Move to autocracy	-0.273	0.524	0.479	-0.792	-1.024	-0.914*
	(4.547)	(4.519)	(0.676)	(2.149)	(2.246)	(0.553)
Economic liberalization	-0.429	-0.504	-0.663	-16.36***	-15.46***	-16.99
	(1.614)	(1.649)	(0.494)	(0.787)	(1.051)	(1408)
Move to autarky	-14.52***	-13.72***	-16.95	-0.38	-0.156	0.556
	(2.055)	(2.446)	(1672)	(8.083)	(7.545)	(0.945)
Exchange rate depreciation shock	0.327	0.243	0.622	-0.377	-0.525	-1.001
	(3.276)	(3.187)	(0.528)	(3.198)	(3.192)	(0.651)
Exchange rate appreciation shock	-1.568	-1.651	-2.050**	0.502	0.627	0.396
	(3.391)	(3.542)	(0.797)	(0.451)	(0.464)	(0.413)
Death of leader	-6.583	-8.199	-8.521***	-0.604	-0.596	-0.688
	(45)	(43.97)	(2.97)	(17.23)	(12.78)	(0.585)
Tenure at leader death	0.564	0.755	0.780***	0.0603	0.036	0.0384
	(3.603)	(3.504)	(0.217)	(1.07)	(1.339)	(0.035)
Real percapita GDP growth	-0.0228	-0.0266	-0.028	-0.0466*	-0.0442	-0.0334
	(0.034)	(0.038)	(0.029)	(0.026)	(0.03)	(0.023)
Rest of world growth	-0.0013	0.0007	-0.0004	0.0029**	-0.0002	0.0015
	(0.001)	(0.002)	(0.003)	(0.001)	(0.0009)	(0.001)
POLITY2 score	-0.0943	-0.1	-0.104**	-0.135**	-0.0993	-0.0775**
	(0.098)	(0.12)	(0.045)	(0.057)	(0.063)	(0.032)
Conflict	0.574	0.754	0.879**	0.61	0.485	0.618*
	(1.029)	(1.237)	(0.41)	(0.512)	(0.493)	(0.359)
Observations	809	809	809	1271	1271	1271
Number of countries	20	20	20	31	31	31
Log-Likelihood	-224.336	-208.909	-188.301	-383.260	-325.098	-286.618

Note: (1) and (4) baseline regressions. (2) and (5) with decade fixed effects. (3) and (6) with year fixed effects. Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Panel fixed effects - dropping perfect predictors

	Positive jumps			Negative jumps		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization	2.344***	2.584***	2.973***	0.194	-0.204	-0.311
	(0.384)	(0.402)	(0.452)	(0.369)	(0.410)	(0.425)
Move to autocracy	-0.061	0.702	0.766	-0.782	-0.968*	-0.890
	(0.613)	(0.632)	(0.671)	(0.484)	(0.515)	(0.544)
Economic liberalization	-0.298	-0.304	-0.441			
	(0.438)	(0.465)	(0.498)			
Move to autarky				-0.250	-0.002	0.810
				(0.594)	(0.740)	(0.927)
Exchange rate depreciation shock	0.290	0.228	0.711	-0.508	-0.608	-1.053*
	(0.474)	(0.505)	(0.537)	(0.505)	(0.553)	(0.638)
Exchange rate appreciation shock	-1.087**	-1.149**	-1.389**	0.530*	0.595*	0.401
	(0.518)	(0.537)	(0.598)	(0.285)	(0.347)	(0.412)
Death of leader	-0.695	-1.013	-0.953	0.715	1.134	0.971
	(0.878)	(0.890)	(1.046)	(0.665)	(0.761)	(0.810)
Tenure at leader death	0.159***	0.213***	0.241***	0.019	-0.003	-0.002
	(0.034)	(0.038)	(0.040)	(0.025)	(0.029)	(0.030)
Real percapita GDP growth	-0.031	-0.037	-0.043	-0.058***	-0.048**	-0.038
	(0.025)	(0.026)	(0.029)	(0.021)	(0.023)	(0.024)
Rest of world growth	-0.001	0.001	0.001	0.003***	-0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
POLITY2 score	-0.096***	-0.098**	-0.104**	-0.147***	-0.105***	-0.096***
	(0.034)	(0.038)	(0.042)	(0.027)	(0.031)	(0.032)
Conflict	0.513	0.648*	0.740*	0.616**	0.442	0.534
	(0.334)	(0.355)	(0.389)	(0.294)	(0.327)	(0.349)
Observations	809	809	809	1271	1271	1271
Number of groups	20	20	20	31	31	31
Log-likelihood	-235.72	-222.85	-202.33	-400.24	-332.51	-239.96

Note: (1) and (4) baseline regressions. (2) and (5) with decade fixed effects. (3) and (6) with year fixed effects. Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 11: "Extreme policies" on smooth growth

	Linear model on smooth growth - pooled OLS	Linear model on smooth growth - panel fixed effects
Democratization	-0.245	-0.439
	(0.372)	(0.394)
Move to autocracy	0.252	0.477
	(0.440)	(0.561)
Economic liberalization	0.981***	1.088***
	(0.242)	(0.274)
Move to autarky	-0.0970	0.729
	(0.629)	(0.704)
Exchange rate depreciation shock	-0.942***	-0.553
	(0.284)	(0.355)
Exchange rate appreciation shock	-0.0620	-0.390
	(0.221)	(0.293)
Death of leader	0.424	0.720
	(0.455)	(0.648)
Tenure at leader death	-0.0163	-0.0586
	(0.0274)	(0.0362)
Rest of world growth	0.0088***	0.009***
	(0.0009)	(0.0009)
POLITY2 score	-0.0308*	-0.0019
	(0.016)	(0.041)
Developing country	-1.358***	
	(0.209)	
Ldc	-1.435***	
	(0.343)	
Conflict	-1.270***	-1.631***
	(0.383)	(0.494)
Constant	1.733***	0.673***
	(0.221)	(0.232)
Observations	3653	3653
R-squared	0.071	0.049

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 12: "Smooth policies" on growth jumps

	(1)	(2)	(3)	(4)
	Positive jumps – pooled	**Positive jumps – panel fixed effects**	**Negative jumps – pooled**	**Negative jumps – panel fixed effects**
Polity2	-.040**	-.023	-.024	-.078
	(.016)	(.136)	(.015)	(.106)
Diffpolity2	.096***	.125	.010	.020
	(.027)	(.077)	(.015)	(.041)
Openness	-.012**	-.034	-.006***	-.001
	(.005)	(.041)	(.002)	(.033)
Exchange rate change	-.0004	.031	-.067	-.048
	(.039)	(.127)	(.187)	(.934)
Leader_death	.905	-6.860	-.422	-.621
	(.758)	(68.350)	(.623)	(24.022)
Leader death * tenure	.099***	.632	.010	.025
	(.030)	(5.625)	(.039)	(.679)
Growth	.001	-.032	-.025	-.037
	(.021)	(.043)	(.016)	(.024)
Developing country	.739***		-.263	
	(.250)		(.240)	
LDC	-1.462***		-18.008***	
	(.410)		(.453)	
Conflict	.881***	.913	.414*	.454
	(.277)	(1.309)	(.245)	(.684)
Constant	-2.799		-3.172	
	(6.610)		(7.737)	
N	3751	825	3751	1300
Groups		20		31
Pseudo R2	.133		.234	
Log Likelihood		-213.109		-303.251

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. *Growth* is yearly real per capita GDP growth; *openness* is the ratio of the sum of exports and imports over GDP at current prices. *Diffpolity2* is the 5-year difference in the polity2 index. Growth and openness data are taken from the Penn World Table, version 6.1. The polity2 score is taken from the Polity2 dataset.

Table 13: Propensity scores estimation

	(1)	(2)	(3)	(4)
	Democratization	**autocracy**	**opening**	**closing**
Economic liberalization	0.427***	-0.122		
	(0.0803)	(0.118)		
Move to autarky	-0.170	-0.080		
	(0.201)	(0.198)		
Exchange rate depreciation shock	0.867***	0.976***		-0.216*
	(0.087)	(0.098)		(0.122)
Exchange rate appreciation shock	-0.538***	0.020		-0.396**
	(0.118)	(0.161)		(0.195)
Death of leader	0.0067	-0.019**		-0.071**
	(0.007)	(0.009)		(0.032)
Tenure at leader death			-0.095	
			(0.101)	
Real percapita GDP growth		-0.0002		-0.020***
		(0.005)		(0.006)
Rest of world growth	-0.001***	-0.0008***		
	(0.0003)	(0.0003)		
POLITY2 score			0.079***	0.033***
			(0.003)	(0.005)
real per capita GDP		-0.0001***		
		(0.00001)		
Change in the real exchange rate			0.0006	
			(0.0004)	
Conflict	0.2833			0.458***
	(0.089)			(0.096)
Constant	-0.636***	-0.353***	-0.499***	-1.193***
	(0.051)	(0.067)	(0.027)	(0.043)
Observations	2928	2679	3143	2199
Pseudo R2	.062	.156	.155	0.058
Log-likelihood	-1510	-1023	-1763	-747.4

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 14: log likelihood tests for unmatched and matched samples

	Treatments							
	Democratization		Autocracy		Opening		Closing	
	(1)		(2)		(3)		(4)	
	Pseudo R2	Log-likelihood test	Pseudo R2	Log-likelihood test	Pseudo R2	Log-likelihood test	Pseudo R2	Log-likelihood test
Unmatched	0.062	198.92	0.156	378.51	0.155	645.62	0.058	91.25
Matched	0.001	1.33	0.007	8.96	0.000	1.05	0.003	1.97

Table 15: ATT estimation

Outcome	Treatments						
	Propensity score matching (kernel matching, Normal density)				Simple diff-in-diff		
	(1) Democratization	(2) Move to autocracy	(3) Economic liberalization	(4) Move to autarky	(5) Appreciation shock	(6) Depreciation shock	(7) Leader death
Positive jump	0.029***	0.022**	0.007	0.018	0.393	-0.338	0.454
	(0.008)	(0.011)	(0.005)	(0.012)	(1.69)	(0.764)	(1.603)
Negative jump	-0.022***	0.008	0.002	0.056***	0.097	-0.263	-0.397
	(0.008)	(0.011)	(0.009)	(0.019)	(0.553)	(0.871)	(1.262)
N Treated	699	450	1191	257	352	397	204
N Control	2196	1591	1863	1846	2608	2616	3196

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 16: Studying the sequence of reform

Outcome	Treatments			
	(1)	(2)	(3)	(4)
	Opening after democratization	**Democratization after opening**	**Democratization after closing**	**Opening after move to autocracy**
Positive jump	-0.041 (0.018)**	0.009 (0.015)	0.052 (0.059)	0.016 (0.030)
Negative jump	-0.032 (0.011)***	-0.015 (0.017)	-0.043 (0.037)	-0.047 (0.015)***
N. Treated	180	140	40	73
N. Control	408	1014	124	317

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 17: Program evaluation - single reforms

Outcome	Treatment			
	Propensity score matching (kernel matching, Normal density)			
	(1) Democratization	(2) Move to autocracy	(3) Economic liberalization	(4) Move to autarky
Positive jump	0.037***	0.009	0.005	-0.020***
	(0.013)	(0.011)	(0.005)	(0.003)
Negative jump	-0.013	-0.006	0.016	0.040*
	(0.012)	(0.011)	(0.011)	(0.020)
N Treated	345	327	782	182
N Control	2143	1423	1863	1825
Bootstrapped standard errors in parenthesis.				
* significant at 10%; ** significant at 5%; *** significant at 1%				

Note: Bootstrapped standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 18: summary of results

	Booms				Crises			
	Pooled logit	**Multinomial logit**	**Panel FE**	**Program Evaluation**	**Pooled logit**	**Multinomial logit**	**Panel FE**	**Program Evaluation**
Democratization	1.687***	1.283***	2.973***	0.037***	-0.174	-0.441	-0.311	-0.013
	(0.301)	(0.260)	(0.452)	(0.013)	(0.345)	(0.354)	(0.425)	(0.012)
Move to autocracy	-0.709	-0.921	0.532	0.009	-0.775	-0.288	-0.890	-0.006
	(57.610)	(3.478)	(0.671)	(0.011)	(0.751)	(0.348)	(0.544)	(0.011)
Economic liberalization	-0.339	-0.338	-0.441	0.005	-17.78***	-33.800***		0.016
	(0.674)	(1.230)	(0.498)	(0.005)	(0.478)	(2.027)		(0.011)
Move to autarky	-18.10	-33.851***		-0.020***	0,275	-0.289	0,5625	0.040*
	(22.79)	(1.982)		(0.003)	(2.407)	(4.674)	(0.927)	(0.020)
Exchange rate depreciation shock	-0.368	-0.384	0.494	-0.338	-0.593	-0.576	-1.053*	-0.263
	(0.474)	(0.418)	(0.537)	(0.764)	(0.400)	(0.358)	(0.638)	(0.871)
Exchange rate appreciation shock	0,433	0,095	-1.389**	0,273	0.662***	0.755***	0,278	0.097
	(0.830)	(1.630)	(0.598)	(1.69)	(0.255)	(0.229)	(0.412)	(0.553)
Death of leader	-0.874	-0.585	-0.953	0.315	-0.325	-0.224	0.674	-0.397
	(0.733)	(0.509)	(1.046)	(1.603)	(0.666)	(0.613)	(0.810)	(1.262)

Notes

[1] See, among many others, Levine and Renelt (1992) and Barro (1996) for cross section estimates; Glaeser *et al.* (2004), Acemoglu *et al.* (2005) for panel specifications. Tabellini and Giavazzi (2005) and Persson and Tabellini (2006) employ program evaluation techniques to identify the impact of policies and democratization on growth.

[2] About the relationship between growth and poverty, see, among many, Kraay (2004). Note that one of the preconditions for the achievement of the Millennium Development Goals (UN, 2000, 2006) is to *boost* economic growth of some countries to a rate as high as 7%. See also the analyses accompanying the *Africa Development Indicators* (World Bank, 2007).

[3] A complete list of countries analyzed, with the time span for each series, is provided in Table 1.

[4] The shortest series we have spans 21 years, so we allow for segments as short as 2 years for a few countries (Bangladesh, Hungary, Mauritius, Mozambique, Poland, Zimbabwe). Only Poland shows breaks lasting less than 5 years. However, not considering these countries does not alter our main findings.

[5] To further control for the estimates uncertainty, we obtain bootstrapped standard errors throughout.

[6] The *polity2* variable is a scaled index of democratization: it takes discrete values in the range -10 (hereditary dictatorship) to +10 (consolidated democracy).

[7] Exchange rate changes may be caused either by real exchange rate movements or simple price movements. Given the "extreme" nature of our indicator, though, we are confident of capturing a destabilizing event that cannot be controlled by monetary authorities.

[8] We consider abrupt changes in the exchange rate as exogenous shocks, as each country cannot control the distribution of shocks of the other countries. Our results do not change if we treat such

shocks as potentially endogenous and we estimate their impact using the matching techniques described below.

[9] See, among others, Rosenbaum and Rubin (1983).

[10] This procedure is known as "Kernel Matching Estimator". When specifying the kernel for weighting, we choose the Normal density.

[11] That effect is wiped out if we do not consider, in our specification, the perfect predictor liberalization variable.

[12] Persson and Tabellini (2007) provide an excellent explanation of the issues related with the common support in matching strategies.

[13] The tests presented below employ the nearest neighbour matching estimator (that uses only the most similar control for each treatment), following the procedures set out by Becker and Ichino (2002) and Leuven and Sianesi (2003). Actual estimates are retrieved employing the kernel matching estimator described in section 4.2, which compares each treated unit with all controls. Using nearest neighbour matching in the final estimates does not alter our results.

[14] Full results are available at:

http://www.cespri.unibocconi.it/whos.php?vedi=4228&tbn=albero&id_doc=1911 .

[15] We do not show results treatments that have less than 40 treated units: they are driven by two or three countries that experience the joint reforms once.

[16] If we consider results from Table 15 on single reforms when summing up the effects of a sequence, we obtain an even larger negative impact on the probability of booms.

Chapter 2

Neighbors matter: evidence on trade, growth and productivity

2.1 Introduction

Recent evidence shows that international trade, measured as the share of imports and exports over GDP, is good for growth. The crucial paper to reach this consensus is the seminal work of Frankel and Romer (1999, henceforth FR), that proposed for the first time an instrument, based on geography variables in a gravity equation, that was considered valid for trade in growth regressions. The geography-based instrument for trade has been widely employed, not only to estimate growth but also productivity (see Alcalà and Ciccone, 2003, that propose real openness as an alternative measure of international trade).

The debate on the impact of trade on growth is, however, far from over. The geography-based instrument raises some questions. The first one is about its validity: geography variables may impact growth also through channels other than openness, such as institutions and productivity. The second one is how to employ geography-based instrumental variables in panel estimation.

This paper proposes an alternative instrument for trade based on trade flows of a country's neighbors. Cross sectional evidence suggests that the new variable fares substantially better than the geography-based variable proposed by Frankel and Romer. Second-stage results are consistent with the evidence presented by Alcalà and Ciccone (2003) and show that, using either instrument, nominal openness is not a robust predictor of income per capita and labor productivity, but real openness is.

Contrary to the FR variable, the new instrument can be employed also in panel regressions and its quality is high also in that context. Structural estimation shows that both nominal and real openness impact positively and strongly on within country variations in income and labor productivity.

In panel estimation, I also deal with potential heterogeneous returns to trade across countries. Even allowing for country-specific coefficients, the Population Average Effect (PAE) of openness variables is always positive and highly significant both in income and productivity equations. Further analysis rules out that the returns to trade changed substantially as the global trade network grew with time.

Empirical findings have generally shown a positive correlation between trade, defined in various ways, and per capita GDP growth[1]. Results have been questioned, though, and there is an ongoing debate about their robustness and validity. Trade policies have been investigated by the very influential paper by Sachs and Warner (1995) that construct an "openness

[1]Temple (2003) provides an excellent review of empirical growth studies, including the ones dealing explicitly with openness variables.

index" based on five broad policy indicators and shows a strong positive correlation between this index and growth. Dollar (1992) finds a similar result highlighting a negative relationship between growth and real exchange rate departures from the law of one price. Such departures were considered symptoms of more restrictive trade policies. Rodriguez and Rodrik (2001) show the pitfalls of both papers. The index developed by Sachs and Warner relies heavily on just one indicator, the state trading monopoly power over exports, that is present only in Sub-Saharan Africa countries. The positive impact of freer trade policies disappears when this indicator is excluded. Regarding Dollar's paper, Rodriguez and Rodrik show that deviations from the law of one price may be determined by factors other than trade policy, and that the paper's results are reversed if estimation is repeated with updated data.

When we turn to more direct measures of trade, such as the fraction of imports and exports over GDP, most evidence is for a positive link between openness and growth, but questions are open on how reliable are there results. Levine and Renelt (1992) find a positive correlation between growth and openness, but claim that this result is mainly driven by the link between openness and investments and, in turn, investments and growth. In general, the authors show that most specification of growth regressions, including the ones containing proxies for trade, are very fragile with respect to small modifications of the list of the right-hand side variables.

These mixed results notwithstanding, research on trade and growth had been undermined by two severe econometric issues, made clear after Mankiw, Romer and Weil (1992) seminal paper on conditional convergence. First, reverse causation might drive the results, especially when openness measures were studied. Second, cross section estimates imposed country specific features not to be correlated with the regressors. A shift towards panel data estimation, that could take unobserved heterogeneity into account, was needed to obtain more reliable parameter estimates.

Frankel and Romer (1999) proposed an instrumental variable approach to estimate the impact of openness on growth in a cross section. They employed the predicted values of a gravity model (whose main variables are related to geography) as an instrument for the trade share of GDP. Their results show a very positive impact of openness on growth[2]. However, the debate about causation in not over. In fact, geographic variables may affect growth and productivity not only through openness, but also through other factors such as political institutions, productivity and culture. Moreover, results in Rodrik, Submarian and Trebbi (2002) show that, using Frankel and Romer's methodology, when institutions are controlled for, openness ceases

[2]Interestingly, the coefficient they retrieve is higher than the OLS estimates.

to be significant and shows even a negative sign in the structural equation.

The problem of unobserved heterogeneity was addressed by Islam (1995), that retrieved estimates of conditional convergence from a panel of 96 countries based on the Penn World Tables. He found a faster conditional converge rate than previous studies based on cross section techniques and, above all, started the interest for the determinants of country-specific productivities. Caselli, Esquivert and Lefort (1996) offered a solution for both endogeneity and correlation between "country effects" with the regressors. They employed a Generalized Method of Moments (GMM) estimation for dynamic panel data using lagged values of the endogenous variables as instruments. Their results (a faster convergence rate than previously thought, particularly for open economies) were questioned by Bond, Hoeffler and Temple (2001). They show that the standard GMM may be affected by weak instrument when series are persistent, compromising estimates reliability. They propose an extension of GMM ("system GMM") originally developed by Arellano and Bover (1995) and Blundell and Bond (1998). The system GMM adds a set of moment condition to the first-difference GMM that makes it consistent even when series are persistent. Dollar and Kraay (2003) employ GMM directly to the openness variable, instrumenting it with its past values. They show a positive and significant impact of trade openness on growth. The problem with GMM estimation is that the moment conditions are strong assumptions: past values of openness may well be correlated with current growth and the resulting estimates would be inconsistent.

The instrument proposed in this paper offers a solution for reverse causation that is not vulnerable to the critiques moved to geogrpahy or GMM. In addition, it can be gainfully employed both in cross section and panel data contexts. Structural estimates show that openness does matter for income and productivity, and that its impact is strong also on within-country growth. Moreover, these results hold even allowing for heterogeneous returns to trade across countries.

The paper is structured as follows: section 2 describes the instrument and discusses some of its features; section 3 describes the estimation strategy and the data; section 4 presents the results; section 5 concludes.

2.2 Mind thy neighbors

The intuition behind this paper is that world trade flows are correlated with the extent of each country's trade. The openness of each country's neighbors can serve as a good proxy for world trade. Looking at a country's borders is also sensible if we think of the cultural and economic ties that usually link

neighbors.

To ensure instrument validity, I do not employ neighbors' openness *per se*, but use the sum of their trade flows with the rest of the world only. In practice, my instrument is constructed as follows:

1. For each country i, take a neighboring country j
2. Sum up all country j's imports and exports
3. Take out j's imports and exports with country i
4. Repeat the same steps for all of i's neighbors and sum up the resulting "netted flows"

More formally, we can define openness as the sum of imports and exports over GDP: $openness_{it} = \frac{X_{it}+M_{it}}{GDP_{it}}$, where X_{it} is the value of country i's exports in time t and M_{it} is the value of its imports in time t. The instrument I propose is constructed as follows:

$$z_{it} = \sum_{J} (X_{j,-i} + M_{j,-i})$$

Where J is the set of country i's neighbors. $X_{j,-i}$ is the exports of country j towards all countries in the world but i. Similarly, $M_{j,-i}$ is the sum of imports of j from all countries but i.

This measure has the obvious advantage that is relatively easy to construct. It is a good proxy for world trade flows: the correlation between the sum of world trade and my instrument is .89 in my sample. Moreover, it is a very sensible instrument for trade: on the one hand, trade flows may be the channel that relates some other variables at home and abroad; on the other, it is difficult to think of trade flows outside the borders to impact a country's growth through channels different than trade itself. Think of trade diversion. When it occurs, trade diversion might make my instrument *weaker*, but it is unlikely that it raises questions about its *validity*. The same applies if there is a country-specific productivity shocks abroad.

Is it possible to think of plausible explanations for the relationship between trade at home and abroad? This paper does not analyze systematically this issue, but I would like to suggest some tentative hypotheses.

In a cross sectional context, this instrument captures "regional trade clusters". In fact, it allows to check whether countries in regions where international trade is high are more open than countries close to areas where trade is low.

In a panel context, if we think of global trade as a complex network, the intensity and width of trade flows may embody a "network effect" (that can

be distinguished from "scale effect", controlling for market size at home and abroad). When global trade is low, there is no significant gain in joining the international trade network. As the global trade network grows, countries may be induced to join in to reap the rising benefits of globalization.

An alternative explanation may arise because of learning effects. As open economies grew more, governments perceived that trade was beneficial and opened up their frontiers. Opening to trade may be part of a learning process by governments and firms (see Buera *et al.* 2008 and the vast literature of "learning by exporting").

Finally, globalization has come in "waves", when many countries decided to boost their exposure to international trade almost contemporaneously and, sometimes, in a coordinated way. One can think of the GATT/WTO Rounds as good examples of such events.

The hypothesis sketched here are just tentative. However, the estimation strategy outlined below provides a straightforward way to check for the existence of "network effects". Consider that world trade has increased steadily over time, so that trade in the 90s is considerably higher than it used to be in the 60s. Therefore, if network effects are present, cross section growth regressions repeated over time would show higher coefficients of trade in more recent samples.

2.3 Estimation and data

I estimate the following equation:

$$y_{it} = \alpha_i + \beta open_{it} + \boldsymbol{\gamma} X_{it} + \delta N_{it} + w_t + \varepsilon_{it} \tag{2.1}$$

Where y_{it} is the log of per capita income of country i at time t, $open_{it}$ is the log of openness and X_{it} and N_{it} are two matrices of controls. a_i and w_t are, respectively, fixed country and time effects.

It is well known that estimates of all coefficients in (1) will be inconsistent, due to the correlation between $open_{it}$ and the error term ε_{it}.

A standard two-stage procedure is therefore applied. I first run the regression:

$$open_{it} = \alpha_i + \theta_1 z_{it} + \theta_2 X_{it} + \theta_3 N_{it} + w_t + u_{it} \tag{2.2}$$

Where z_{it} is the excluded instrument, constructed as described in the previous section: it is the sum of imports and exports of country i's neighbors, netted of trade between them and country i. The linear projection of (2) is then plugged in (1) in place of $open_{it}$. The resulting second stage estimation

yields consistent results for all coefficients, provided that z_{it} is uncorrelated with the ε_{it}.

In both stages, the same controls are applied. X_{it} captures time-varying characteristics of country i, such as (the log of) population, the level of its nominal exchange rate against the dollar, the price level relative to the US, how democratic are its institutions, and an indicator for conflict. Additional variables capture if the country is a GATT member and the number of trade agreements, economic integration areas and custom unions country i is engaged in at time t. N_{it}contains a similar set of controls for the *neighbors* of country i. In particular, the (log of) sum of populations and the neighbors GDP growth rates are included. Controls are also added to account for the total number of trade agreements, integration areas and custom unions participated by country i's neighbors.

2.3.1 Heterogeneity

The potential presence of "network effects" induces to put extra caution when doing inference. If global trade changed the benefits one country exerts from opening, estimates in the structural equations would be inconsistent. An intuitive test of the "network" hypothesis is done by running the same regressions on sub-samples of my data, sorted by the size of my instrument. As noted above, it is convenient to exploit the high correlation between world trade flows and time, so that testing for "network effects" equates to looking for heterogeneous impacts of trade in time.

A second, important issue is whether there are heterogeneous responses to trade across countries. Wooldridge and Murtazashvili (2005) propose a simple procedure to deal with this issue. Let us assume that the relations between trade and growth is best depicted with a Correlated Random Coefficient (CRC) model, where the impact of trade on growth is $b_i = \beta + d_i$. The authors show that, under weak conditions, we can retrieve consistent estimates of the Population Average Effects even neglecting individual specific slopes. This is possible if standard IV techniques are applied only after all the variables have been appropriately detrended. De-trending can be achieved simply regressing each variable separately on a common time trend (that can be arbitrary) and retrieving the residuals to use them in the final estimation.

More formally, let us simplify (1) and rewrite it as a CRC model:

$$y_{it} = w_t a_i + b_i x_{it} + \varepsilon_{it} \tag{2.3}$$

As above, here $b_i = \beta + d_i$. Here, x_{it} stands for the openness measure. Aggregate time variables are captured by the vector w_t and the individual-

specific slopes on these variables are a_i. Both b_i and a_i are not assumed to be mean independent of x_{it}. We can use the expression for b_i to rewrite (3) as:

$$y_{it} = w_t a_i + \beta x_{it} + (d_i x_{it} + \varepsilon_{it}) \tag{2.4}$$

We can eliminate the a_i by regressing, for each i, y_{it} and x_{it} on w_t and using the residuals from these regressions, $\ddot{y}_{it}$and $\ddot{x}_{it}$, in the following equation:

$$\ddot{y}_{it} = \beta \ddot{x}_{it} + (d_i \ddot{x}_{it} + \ddot{\varepsilon}_{it}) = \beta \ddot{x}_{it} + \ddot{\nu}_{it} \tag{2.5}$$

Where $\ddot{\nu}_{it} \equiv d_i \ddot{x}_{it} + \ddot{\varepsilon}_{it}$. Now take the proposed instrument, z_{it}, and apply the same procedure described above to obtain the detrended values $\ddot{z}_{it}$. We can employ the latter variable as an instrument to estimate (5) and obtain consistent results, provided the following moment condition holds:

$$E\left[\ddot{z}'_{it}, \ddot{\nu}\right] = 0, \ \forall t \tag{2.6}$$

Condition (2.6) requires a set of assumptions. The first one is a standard strict exogeneity assumption of the instrument variable:

$$E\left[\varepsilon_{it} | z_{it}, a_i, b_i\right] = 0, \ \forall t \tag{2.7}$$

Now consider the error component in (2.5) The crucial point here is that its second term is $d_i \ddot{x}_{it}$. To ensure that the detrended instrument is uncorrelated with $d_i \ddot{x}_{it}$, we have to impose that b_i is mean independent of all $\ddot{x}_{it}$.

$$E\left[b_i | \ddot{z}_{it}\right] = E\left[b_i\right] = \beta, \ \forall t \tag{2.8}$$

Assumption (2.8) is much weaker than full independence between the instrument and the slopes, since it allows b_i to be arbitrarily correlated with systematic components of z_{it}. In practice, mean independence is maintained only between the heterogeneous slopes and the deviations of the instrument from its long-run level[3]. To obtain consistent IV estimates, we need one more assumption:

$$Cov\left(\ddot{x}_{it}, b_i | \ddot{z}_{it}\right) = Cov\left(\ddot{x}_{it}, b_i\right), \ \forall t \tag{2.9}$$

Condition (2.9) allows the *unconditional* covariance between detrended variables and the individual specific slopes to be nonzero and even to change

[3]The richer is w_t, the more likely will (2.8) hold, but there is a trade off with efficiency, because of loss in variation in the detrended instrument as more structure is put on w_t.

over time. In contrast, covariance *conditional* on the detrended instrument is assumed not to depend on $\ddot{z}_{it}$. Conditions (7)-(2.9), together with the usual rank condition on the detrended instrument, are employed by Wooldridge and Murtazashvili to show that the fixed-effect IV estimator is consistent for β, provided a full set of time dummies is included in (2.5).

The procedure just described allows for controls in (2.5), provided the variables to be detrended are roughly continuous. I then apply the technique outlined here in section 4.3. I detrend all roughly continuous variables in my data using time and total world trade in each period. I obtain the yearly deviation of each variable from its long-term average. Only instrument's deviations from its systematic component should be mean independent from the individual slopes, and this looks a very plausible assumption.

2.3.2 Data

Trade data are taken from the publicly available NBER-United Nations Database (Feenstra and Lipsey, 2001), that contains bilateral trade flows covering the period 1962-2000. Values are in nominal thousands of dollars. GDP per capita is taken from the Penn World Tables, mark 6.2 (Heston *et al.*, 2006). I also evaluate my instrument against *real* openness, as defined by Alcalà and Ciccone (2003). They claim that nominal openness may be a biased measure of trade, due to cross country differnces in the price of non-traded goods. To overcome this problem, they suggest to correct the nominal opennes measure taking out the differences in prices of the nontradable. In practice, real openness is obtained multiplying openness at current prices by the ratio of the price levels (PPP) between the country of interest and a reference country (the US in the PWT).

Population, price level, exchange rate and growth data also come from the Penn World Tables. The political variable is the polity2 score contained in the Polity IV dataset (Marshall and Jaggers, 2007). The scores takes values from -10 (absolute autocracy) to +10 (completely democratic institutions). Conflict variables are taken from the Correlates of War (CoW) dataset (Sarkees, 2000). Data include interstate, extra-state and intrastate wars up to the year 2007. For my purposes, I classify any kind of war as a conflict for a given country and year.

International trade agreements (and their distinction in preferential trade agreements, economic integration areas and customs unions) are drawn from the WTO database of notified regional agreements as of 20 May 2008. A complete list of participating countries for each agreement can be found at http://www.wto.org/english/tratop_e/region_e/regfac_e.htm.

Border data are taken from the Direct Contiguity database (version 3.1)

available on the website of the Correlates of Was Project (http://correlatesofwar.org.). The classification system for contiguous dyads is comprised of five categories, one for land contiguity and four for water contiguity. Land contiguity is defined as the intersection of the homeland territory of the two states in the dyad, either through a land boundary or a river, such as the Rio Grande in the case of the US-Mexico border. Water contiguity is divided into four categories, based on distances of 12, 24, 150, and 400 miles. Version 3.1 of the dataset spans the period 1816-2006. In my analysis, I employ contiguity measures such that two countries are considered neighbors if they share a land border or their reciprocal distance is less than 25 miles.

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2.4 Results: growth, trade and productivity

To fix ideas, table 1 shows simple correlations between real per capital GDP, openness and real openness (both in log and levels). Correlations are computed for observations in 1995. Values are generally high. The proposed instrument (in logs) has a correlation of .32 with nominal openness in levels and of .52 with the same variable in logs. Correlations with real openness in logs and levels follows a similar pattern. Results are basically unchanged when correlations are computed between the various openness measures and the instrument in levels.

(INSERT TABLE 1 HERE)

OLS results of a cross sectional regression of growth on trade are presented in Table 2. The dependent variable is the log of income per capita in 1995. Column (1) depicts the strong correlations between the log of trade over GDP and real percapita GDP growth. Column (2) adds controls referred to the internal features of each country, as population, the degree of democratization, and the occurrence of conflicts. A dummy variable captures whether a country is a GATT (WTO) member in 1995. Of these covariates, only conflict is weakly significant (and negative). Column (3) includes the price level relative to the United States and the exchange rate level (relative to the dollar), to control for potential price and currency distortions (or, put differently, to keep the real exchange rate fixed). It turns out that exchange rate appreciations depress growth. More importantly, the exchange rate and price variables considerably cut the point estimates of openness and weaken its significance. In Column (4), controls referring to each country's neighbors, such as their growth and population, are added. Continental dummies are included in Column (5). The final column adds discrete variables that capture the economic integration both at home and "at the borders". They are the

number of Free Trade Agreements, Economic Integration Areas and Customs Unions into force for each country of interest and its neighbors. Being part of an Economic Integration Area significantly increases growth, but Customs Unions seem to act in the opposite way. Note also that the GATT variable, always positive, gains significance in the richer specifications. The key message up to now, though, is that nominal openness does not predict growth when monetary variables, such as exchange rates and prices, are included in the specification.

Columns (7)-(12) show similar specifications, but use the log of real openness instead of nominal openness as the main variable of interest. As expected, real openness is a predictor of growth that is more robust to monetary swings. The estimated coefficient is always positive and very significant. Its size is slightly reduced as additional controls are included, but overall the point estimates are stable across specifications.

(INSERT TABLE 2 HERE)

2.4.1 IV: Cross sectional evidence

As previously noted, OLS estimates might be severely biased. The instrument based on neighbors openness, proposed above, is now implemented in a cross sectional context and compared to the "geographic" instrument constructed by Frankel and Romer (1999). To begin with, we show first stage results in Table 3: in Panel A the dependent variable is the log of imports plus exports over GDP in 1995. Columns (1) to (5) use my new instrument as the main explanatory variable; its coefficients are positive, very large in magnitude and highly significant: countries are more likely to trade more if their neighbors are more open. Estimates are stable across specifications, increasing slightly when geography and trade agreements controls are added. Country size, measured in terms of population, has the expected negative and significant coefficient on openness: larger countries benefit more from domestic market size and trade less outside their borders. The same holds true for neighbors market size.

(TABLE 3 AROUND HERE)

Columns (5) to (10) repeat the estimation using the geographic instrument for trade developed by Frankel and Romer. Their variable is positive and very significant in the "basic" specification, where the coefficient is very large in magnitude. Adding controls, though, drives the estimates down: the coefficient of the instrument drops from over 5 to around 1.4. Geographic controls and trade policy variables do not impact point estimates further but their significance drops at the 10% level. Overall, the performance of the FR instrument is considerably lower than the instrument based on neighbors

trade. As we will see, this translates in questionable performance in under- and weak identification tests.

Panel B shows results on real openness as defined by Alcalà and Ciccone (2003). Again, Columns (1)-(5) present results using the new instrument; in Columns (6)-(10) the FR instrument is employed. Here, both instruments are very significant in all specifications: the magnitude of the coefficient on neighbors openness is a bit more stable across columns, while the point estimates of the FR instrument for openness drops as new controls are added, and loses some significance in the broader specification. Note that the market size variable (both at home and abroad) is significant only when the new instrument is employed; in the FR specification, they have the expected sign but are, in general, not significant.

Table 4 shows results on the structural equation, estimated with 2SLS. The dependent variable is log of real per capita income in 1995. Panel A shows specifications that use nominal openness as the main explanatory variable. In Columns (1)-(5), the log of nominal openness is instrumented with the log of neighbors trade. The point estimates are somewhat higher than OLS results, but their magnitude and significance drop substantially when controls for exchange rates and prices (relative to the US) are added. This is consistent with Alcalà and Ciccone's claim: nominal openness does not take into account shifts in nontradable goods sector. When these are controlled for, the collinearities that are created invalidate the estimates.

Nominal appreciations seem to have a negative impact on income, even if they do not affect openness in the first stage regression. In the most complete specification of column (5), the polity2 score is negative and not significant: having a more democratic regime in 1995 did not matter for income. Conflict is negative and insignificant, while being a WTO member considerably increases income. Growth "at the border" has a negative and significant impact on GDP at home. Again, being part of a EIA is good for growth, but Customs Unions seem to be detrimental.

Columns (6)-(10) show the same set of specifications, instrumenting nominal openness with the FR variable. Results are similar: nominal openness is not a robust determinant of income. The coefficient on trade is always higher than in Columns (1)-(6), so that estimates with our new instrument stand somewhere in between OLS and Frankel and Romer's findings.

(TABLE 4 AROUND HERE).

Panel B presents evidence with real openness. Columns (1)-(4) contain estimates on log of GDP per capita using the new variable as excluded instrument; Columns (7)-(10) show results based on the FR instrument. Consistently with recent literature (see Alcalà and Ciccone, 2003) real openness performs better in predicting growth and, particularly, productivity. This is

evident when comparing Columns (5) and (6). They show structural estimates when the dependent variable is labor productivity (log of real GDP per worker): nominal openness is not significant, while real openness has a larger coefficient and is always significant. As expected, due to the high correlation between labor productivity and income, results are similar to specifications where log of real percapita GDP is the dependent variable.

A key argument in favor of the new instrument for trade is that it fares substantially better in a variety of tests shown at the bottom of both Panels of Table 4. The Underidentification-Test is the Kleibergeen-Paap rank test, whose statistic is robust to clustering within countries. The Weak Identification test is the Kleibergeen-Paap F-statistic, in turn robust to heteroskedasticity and serial correlation within countries. With nominal openness, Under-Identification tests range between 3.8 and 8.4, and are generally slightly better using the geographic instrument. Overall, they reject the null at the 1% or 5% level. But the Weak-Identification test is usually much higher using neighbors openness (ranging from 11.140 to 36.584) than using the FR instrument (in that case, they are high in the simple specifications, but drop below 4, when controls are added). When the endogenous variable is real openness, both Under- and Weak-Identification tests show higher statistics, regardless of which instrument is used. Nonetheless, focusing on the specifications with more controls, the Weak-ID statistics is considerably higher when using the new instrument (33.5) rather then the FR one (11.3).

2.4.2 Panel estimates

As noted earlier, a key advantage of my instrument is its time-varying nature, that makes it useful also when studying within-country variations of income in time. In a panel context, neighbors trade is a natural proxy for world trade, thus it can picture the "trailing" of a country openness on world trade flows. But the specificity of each country's neighbors are still crucial in determining its openness. I start showing pooled-OLS and fixed effects results in Table 5, without instrumenting trade. Openness is always positively impacting growth and its coefficient is very significant; POLS estimates are naturally larger than the ones obtain employing fixed-effects. In turn, fixed-effects estimates are stable across specifications. Real openness shows a systematically lower coefficient than nominal openness, but it is still very significant in all trials. Time dummies are included throughout. The political variable seem not to matter, but conflict significantly depresses growth. The same holds true for exchange rate appreciations. Interestingly, in this Table the log of population turns negative and significant in all specifications with fixed effects.

(TABLE 5 AROUND HERE)

Let us now turn to Panel-IV estimation, using neighbors trade as excluded instrument. Table 6 depicts First-stage results. The dependent variable is nominal openness in Columns (1)-(4) and real openness in Columns (5)-(9). The instrument is a strong predictor of both nominal and real openness; the coefficient is very large in size (about 1.5 for nominal openness and 1.9 for real openness) and remarkably stable when adding controls. The polity2 score has a weakly positive impact on openness. Previous results that a country's population is inversely related with its openness are confirmed. Conflict is negative but not significant; being a GATT-WTO member is positive and insignificant. Entering a FTA is significantly positive, while Trade Agreements and Customs Unions joined by others depress home trade, perhaps due to trade diversion. Interestingly, though, the more Economic Integration Areas are joined by neighbors, the more the home country trades with the world.

(TABLE 6 AROUND HERE)

Table 7 presents estimates of the structural equation. Columns (1)-(5) use nominal openness as the main explanatory variable. Its estimated coefficients are always positive and very significant. Their size is much lower than simple POLS estimates in Table 5, but slightly higher than fixed effects estimates without instrumentation. Openness coefficients are also very stable across specifications. The negative impact of conflict, exchange rate appreciation and domestic population are confirmed. The political regime at home and population and growth abroad are not significant. Being part of the GATT impacts positively but not significantly on growth. The entry into force of an additional Free Trade Agreement does not impact growth, while being member of a Customs Union positively does. The creation of Economic Integration Areas that do not include the home country are detrimental to its growth.

Columns (6)-(10) repeat the same specifications with real openness in place of nominal openness. The estimated coefficients on this alternative measure of trade are always positive and very significant. They are also very stable and remarkably similar to the estimates with nominal openness. Results on controls broadly confirms the ones in preceding Columns.

(TABLE 7 AROUND HERE)

The last rows of Table 7 provide some tests on the quality of my instrument and on the reliability of inference based on it. Results for nominal openness are very encouraging also in this panel context: the LM Paap test of Under-identification always rejects the null at the 1% confidence level, and values of the tests statistics actually increase as more controls are added. The same holds true for the Kleibergeen-Paap F test for weak identification: the

test statistics is always above the highest critical value, ranging from above 19 in the bare specification to over 29 when additional controls are added. Tests on real openness are equally reassuring, though the statistics are somewhat lower. Under-identification is always rejected at the 1% level and Weak Identification tests always exceeds the "second highest" critical value, being always well above the "rule of thumb" value of 10.

Table 8 presents the impact of openness on labor productivity in a panel framework. Recall that Table 4 indicated that cross section results are consistent with previous findings by Alcalà and Ciccone (2003): nominal openness shows a positive insignificant coefficient, and real openness is always a very good predictor of labor productivity. In panel estimates, the coefficient on nominal openness is generally larger than the one in real openness specifications, and it increases as additional controls are included. Both measures of openness are strongly and positively significant; real openness estimates are, again, stable across specifications. Instrument tests are passed with no problem: when regressing on real openness, the statistics of the weak identification tests falls somewhat, but remains well above the rule of thumb value of 10.

2.4.3 Heterogeneity

The results shown in previous sections may have to be checked with respect to two issues. On the one side, correlation between the instrument and the coefficients estimates must be ruled out. On the other, single countries may react differently to trade openness, invalidating the previous findings.

Now, the correlation between time and the instrument is very high in my sample (.95). The mean value of my instrument reaches 22.72 in 2000, up from 19.41 in 1965, and the increase in monotonic in time. The sum of world trade flows shows a similar behavior and its correlation with the instrument is .89, as previously noted[4]. This implies that, if there are positive "network effects" of the global trade web, coefficients on openness should rise with time. In Table 9, I run cross sectional IV-regressions for each five-year time interval in my data. In Panel A the dependent variable is the log of real GDP per capita, in Panel B it is the log of labor productivity. Results show that the coefficients on nominal and real openness are always positive and significant; their size varies with time, but not dramatically. Importantly, values referring to more recent years are not higher than the ones obtained for older samples. Spikes occur in the years 1980-1984, but even

[4]By contrast, the correlation between time and country-specific measure of openness is only .01.

then coefficients are not significantly different from their minimum values (incidentally, such minima occur in 1995). Coefficients vary more in the specifications where the log of GDP per worker is the dependent, but the relative stability of my estimates across time suggests that the correlation between the instrument and the structural coefficient is negligible. What's more, it seems that "network effects" of global trade flows are not in place in my sample.

Heterogenous responses to trade across countries are also possible. To investigate this issue, I follow the procedure proposed by Wooldridge and Murtazashvili (2005) and outlined in section 3.1. I detrend all variables[5] separately for each country, I estimate (5) instrumenting the detrended opennes measure with the detrended values of neighbors openness. This yields consistent estimates of the PAE even allowing for country-specific slopes and for correlation between the systematic component of the instrument and the individual coefficients. Results are presented in Table 10 and indicate that, even allowing for heterogeneous responses to trade across countries, the openness variable is always positive and highly significant.

2.5 Conclusion

Trade, measured both by nominal and real openness, impacts positively and very significantly on income, growth and labor productivity. This is true both in cross sectional regressions and in panel estimates. Estimation takes advantage of a new instrument that proxies world openness in a given year with the openness of one country's neighbors. First-stage results show that this instrument performs much better in cross section estimation than the geography-based instrument proposed by Frankel and Romer. Moreover, the quality of the instrument is high even in panel-fixed effects estimation. At the structural level, trade exerts a positive and significant effect on within-country variations in income and productivity. These results hold even if we allow the coefficient on openness to be heterogeneous across countries.

A tentative analysis based on cross-section regressions across time rules out that the benefits of trade increase as the size of world trade increases: "network effects" seem not to be in place.

[5] With this procedure, I can employ only roughly continous variables.

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Table 1

Correlation between income, openness and instrument

Cross section in 1995

obs=81	Real pca GDP	log ofReal pca GDP	openness	log of openness	Real openness	log of real opennes	log of neighbours trade	neighbours trade
Real pca GDP	1.0000							
log ofReal pca GDP	0.9026	1.0000						
openness	0.6812	0.6260	1.0000					
log of openness	0.6542	0.6661	0.8028	1.0000				
Real openness	0.7046	0.6093	0.9349	0.6793	1.0000			
log of real opennes	0.7584	0.7518	0.8306	0.9694	0.7532	1.0000		
log of neighbours trade	0.4101	0.5591	0.3263	0.5224	0.3245	0.5286	1.0000	
neighbours trade	0.4357	0.5939	0.3474	0.5142	0.3540	0.5348	0.9923	1.0000

Table 2

OLS results on trade and income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
COEFFICIENT	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch	lrgdpch
Log of nominal openness	0.737***	0.680***	0.237*	0.204	0.176*	0.159					
	(0.144)	(0.172)	(0.139)	(0.143)	(0.0965)	(0.0985)					
Log of real openness							0.577***	0.547***	0.546***	0.464***	0.399***
							(0.0675)	(0.0839)	(0.0867)	(0.0817)	(0.0908)
polity2		0.0247	0.0122	0.0153	-0.00346	-0.00745		0.0180	0.0179	-0.00642	-0.0177
		(0.0185)	(0.0176)	(0.0160)	(0.0142)	(0.0172)		(0.0184)	(0.0174)	(0.0159)	(0.0196)
log of population		0.104	0.0539	0.00265	0.00255	-0.0200		0.103	0.0426	0.0359	0.00590
		(0.0712)	(0.0599)	(0.0588)	(0.0538)	(0.0821)		(0.0656)	(0.0609)	(0.0591)	(0.0777)
conflict		-0.684*	-0.470**	-0.356*	-0.360	-0.268		-0.551*	-0.431*	-0.424	-0.356
		(0.355)	(0.204)	(0.180)	(0.277)	(0.241)		(0.311)	(0.245)	(0.345)	(0.271)
log exchange rate			-0.131***	-0.122***	-0.0952***	-0.0969**					
			(0.0388)	(0.0327)	(0.0348)	(0.0379)					
price level relative to US			0.0138***	0.0146***	0.0137***	0.0139**					
			(0.00359)	(0.00358)	(0.00363)	(0.00564)					
gatt		0.172	0.0897	0.261	0.374*	0.429*		0.0729	0.286	0.439*	0.382
		(0.246)	(0.198)	(0.189)	(0.200)	(0.220)		(0.230)	(0.230)	(0.233)	(0.243)
neighbours' growth				-0.0181***	-0.0113***	-0.00989**			-0.0147***	-0.00737*	-0.00684
				(0.00337)	(0.00358)	(0.00415)			(0.00474)	(0.00437)	(0.00420)
log of neighbours' population				0.0910**	0.0363	0.0264			0.131***	0.0717	0.0624
				(0.0402)	(0.0429)	(0.0452)			(0.0437)	(0.0492)	(0.0491)
FTA						-0.193					-0.0672
						(0.125)					(0.131)
EIA						0.386*					0.472*
						(0.202)					(0.271)
CU						-0.532**					-0.375*
						(0.207)					(0.222)
neighbours' FTA						0.0368					-0.0162
						(0.0567)					(0.0593)
neighbours' EIA						0.00841					0.0680
						(0.0871)					(0.103)
neighbours' CU						0.0324					0.0825
						(0.0953)					(0.0909)
Geo controls					YES	YES				YES	YES
Observations	81	79	79	79	79	79	81	79	79	79	79
R-squared	0.444	0.509	0.678	0.748	0.794	0.816	0.565	0.603	0.673	0.744	0.776

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3

First stage results - Cross section in 1995

Panel A - Dependent variable is log of nominal openness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
COEFFICIENT	logopen	logopen	logopen	logopen	logopen	logopen	logopen	logopen	logopen	logopen
log of neighbour's trade	3.386***	3.882***	3.821***	4.163***	4.134***					
	(1.025)	(1.075)	(1.093)	(1.130)	(1.214)					
FR geo insrument						5.221***	1.399***	1.047	1.372*	1.461*
						(0.751)	(0.526)	(0.643)	(0.686)	(0.770)
polity2		0.00618	0.0114	0.0145	0.0160		0.0208	0.0276	0.0232	0.0267
		(0.00932)	(0.00937)	(0.0134)	(0.0145)		(0.0160)	(0.0167)	(0.0239)	(0.0284)
log of population		-0.375***	-0.339***	-0.380***	-0.394***		-0.160***	-0.140***	-0.147***	-0.161**
		(0.0726)	(0.0699)	(0.0771)	(0.0824)		(0.0528)	(0.0504)	(0.0520)	(0.0617)
conflict		0.147	0.131	0.153	0.209		-0.143	-0.120	-0.123	-0.0213
		(0.291)	(0.277)	(0.297)	(0.310)		(0.258)	(0.213)	(0.212)	(0.248)
log exchange rate		0.00919	-0.00450	0.0101	0.00270		-0.0684	-0.0817*	-0.0609	-0.0661
		(0.0241)	(0.0244)	(0.0228)	(0.0265)		(0.0434)	(0.0443)	(0.0563)	(0.0612)
price level relative to US		0.0127***	0.0113***	0.0102***	0.00945**		0.0139***	0.0128***	0.0127***	0.0151***
		(0.00236)	(0.00236)	(0.00271)	(0.00436)		(0.00216)	(0.00228)	(0.00254)	(0.00417)
gatt		-0.197	-0.277*	-0.320*	-0.290		0.0281	-0.0351	-0.0557	0.0569
		(0.165)	(0.164)	(0.172)	(0.186)		(0.262)	(0.250)	(0.240)	(0.274)
neighbours' growth			-0.00246	-0.00136	-0.00137			-0.00887*	-0.00673	-0.00549
			(0.00264)	(0.00290)	(0.00342)			(0.00461)	(0.00671)	(0.00675)
log of neighbours' population			-0.0877**	-0.120***	-0.125**			-0.0784*	-0.116**	-0.122**
			(0.0359)	(0.0451)	(0.0488)			(0.0430)	(0.0531)	(0.0604)
FTA					0.00185					-0.134
					(0.118)					(0.131)
EIA					0.0797					0.0519
					(0.174)					(0.230)
CU					-0.0390					-0.378*
					(0.216)					(0.220)
bfta					0.0357					0.0804
					(0.0530)					(0.0607)
beia					-0.00842					-0.0478
					(0.0828)					(0.0981)
bcu					-0.0439					-0.0597
					(0.0645)					(0.0992)
Geo controls				YES	YES				YES	YES
Observations	82	80	80	80	80	76	75	75	75	75
R-squared	0.266	0.750	0.769	0.795	0.800	0.262	0.576	0.606	0.620	0.641

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Panel B - Dependent variable is log of real openness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
COEFFICIENT	lreal	lreal	lreal	lreal	lreal	lreal	lreal	lreal	lreal	lreal
log of neighbour's trade	4.967***	6.659***	6.565***	6.533***	5.958***					
	(1.135)	(0.994)	(1.050)	(0.949)	(1.024)					
FR geo insrument						7.835***	6.027***	5.387***	5.364***	3.689**
						(1.136)	(1.059)	(1.318)	(1.582)	(1.762)
polity2		0.0347*	0.0421**	0.0321	0.0219		0.0652**	0.0742***	0.0421	0.0374
		(0.0181)	(0.0184)	(0.0213)	(0.0198)		(0.0248)	(0.0248)	(0.0334)	(0.0360)
lpop		-0.561***	-0.488***	-0.539***	-0.577***		-0.141	-0.104	-0.135	-0.241**
		(0.0902)	(0.0870)	(0.0759)	(0.0834)		(0.0864)	(0.0756)	(0.0817)	(0.101)
conflict		-0.103	-0.131	-0.0453	0.119		-0.756*	-0.739*	-0.692*	-0.455
		(0.518)	(0.511)	(0.555)	(0.514)		(0.447)	(0.435)	(0.353)	(0.370)
gatt		-0.00675	-0.210	-0.143	-0.362		0.242	0.0964	0.0973	-0.0572
		(0.279)	(0.292)	(0.298)	(0.280)		(0.369)	(0.352)	(0.354)	(0.386)
sumg			-0.00379	-0.000106	-0.00319			-0.0115**	-0.00622	-0.00837
			(0.00479)	(0.00447)	(0.00518)			(0.00563)	(0.00748)	(0.00882)
lsumpop			-0.178***	-0.195**	-0.208***			-0.138*	-0.147	-0.205**
			(0.0634)	(0.0790)	(0.0623)			(0.0751)	(0.104)	(0.0860)
FTA					0.374***					0.317*
					(0.138)					(0.163)
EIA					0.350					0.479
					(0.273)					(0.316)
CU					-0.00890					-0.563
					(0.287)					(0.361)
bfta					0.0231					0.0438
					(0.0799)					(0.0964)
beia					0.0223					-0.00698
					(0.126)					(0.151)
bcu					0.175*					0.262*
					(0.102)					(0.153)
Geo controls				YES	YES				YES	YES
Observations	82	80	80	80	80	76	75	75	75	75
R-squared	0.277	0.586	0.626	0.706	0.796	0.286	0.403	0.438	0.520	0.609

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4

Structural estimation - Cross section in 1995

Panel A - Dependent variable is log real GDP per capita

	Instrument is neighbours' openness					Instrument is FR geo				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log of nominal openness	1.184***	0.879***	0.522*	0.303*	0.264	1.065***	1.156***	0.916*	0.489	0.404
	(0.435)	(0.281)	(0.297)	(0.180)	(0.170)	(0.134)	(0.213)	(0.552)	(0.412)	(0.317)
polity2		0.0139	0.00719	-0.00690	-0.00867		-0.00387	-0.00203	-0.0127	-0.0131
		(0.0186)	(0.0162)	(0.0134)	(0.0151)		(0.0244)	(0.0225)	(0.0185)	(0.0196)
log of population		0.152*	0.107	0.0232	0.00295		0.189**	0.153	0.0392	0.0194
		(0.0853)	(0.0740)	(0.0563)	(0.0730)		(0.0866)	(0.106)	(0.0754)	(0.0919)
conflict		-0.571	-0.428*	-0.346	-0.265		-0.439	-0.412	-0.327	-0.285
		(0.407)	(0.249)	(0.263)	(0.225)		(0.385)	(0.321)	(0.260)	(0.228)
log exchange rate			-0.111***	-0.0856***	-0.0912***			-0.0562	-0.0525	-0.0598
			(0.0361)	(0.0307)	(0.0319)			(0.0621)	(0.0488)	(0.0444)
price level relative to US			0.00910	0.0119***	0.0124**			0.00385	0.0111*	0.0122*
			(0.00591)	(0.00422)	(0.00581)			(0.00913)	(0.00669)	(0.00726)
gatt		0.121	0.0821	0.378**	0.421**		0.0675	0.0717	0.420**	0.420**
		(0.236)	(0.205)	(0.183)	(0.188)		(0.312)	(0.273)	(0.206)	(0.214)
neighbours' growth				-0.0104***	-0.00969***				-0.00982*	-0.00986**
				(0.00341)	(0.00363)				(0.00526)	(0.00443)
log of neighbours' population				0.0535	0.0364				0.0810	0.0641
				(0.0451)	(0.0429)				(0.0800)	(0.0654)
FTA					-0.162					-0.119
					(0.114)					(0.122)
EIA					0.385**					0.374*
					(0.183)					(0.193)
CU					-0.522***					-0.356*
					(0.185)					(0.208)
bfta					0.0320					0.0168
					(0.0498)					(0.0564)
beia					-0.00584					-0.0298
					(0.0756)					(0.0816)
bcu					0.0390					0.0388
					(0.0821)					(0.0841)
Geo controls				YES	YES				YES	YES
Observations	81	79	79	79	79	75	74	74	74	74
R-squared	0.280	0.484	0.650	0.789	0.809	0.400	0.395	0.533	0.768	0.793
Robust standard errors in parentheses										
*** p<0.01, ** p<0.05, * p<0.1										
UnderID test (Paap)	4.820	8.457	4.308	3.895	3.780	8.675	7.596	4.172	4.713	4.179
Kleibergeen-Paap F	11.140	36.584	12.543	13.131	11.273	48.415	32.754	8.007	3.795	3.918

Panel B - Dependent variable is log of GDP per capita (lrgdpch) or log of GDP per worker (logwok)

	Instrument is neighbours' openness						Instrument is FR geo			
	(1)	(2)	(3)	(5)	(7)	(8)	(6)	(9)	(10)	(11)
Dependent	lrgdpch	lrgdpch	lrgdpch	lrgdpch	logwok	logwok	lrgdpch	lrgdpch	lrgdpch	lrgdpch
log of nominal openness					0.279					
					(0.212)					
log of real openness	0.811***	0.652***	0.590***	0.454***		0.468***	0.713***	0.761***	0.778***	0.651***
	(0.220)	(0.162)	(0.157)	(0.129)		(0.168)	(0.0838)	(0.131)	(0.105)	(0.111)
polity2		0.00962	0.0140	-0.0161	-0.0158	-0.0251		-0.00257	-0.00672	-0.0152
		(0.0180)	(0.0180)	(0.0170)	(0.0153)	(0.0181)		(0.0210)	(0.0211)	(0.0179)
log of population		0.135*	0.0526	0.0302	0.0251	0.0427		0.140**	0.0749	0.0659
		(0.0723)	(0.0620)	(0.0755)	(0.0848)	(0.0926)		(0.0691)	(0.0605)	(0.0609)
conflict		-0.452	-0.393	-0.341	-0.181	-0.260		-0.365	-0.239	-0.290
		(0.362)	(0.280)	(0.260)	(0.234)	(0.289)		(0.329)	(0.234)	(0.283)
gatt		0.0205	0.272	0.371*	0.274	0.234		0.00440	0.283	0.415*
		(0.222)	(0.217)	(0.211)	(0.206)	(0.228)		(0.255)	(0.248)	(0.214)
neighbours' growth			-0.0139***	-0.00715*	-0.0118***	-0.00941**			-0.0108*	-0.00718
			(0.00498)	(0.00376)	(0.00363)	(0.00374)			(0.00560)	(0.00508)
log of neighbours' population			0.138***	0.0656	0.0281	0.0649			0.162***	0.107**
			(0.0471)	(0.0507)	(0.0459)	(0.0548)			(0.0518)	(0.0520)
FTA				-0.0469	-0.164	-0.0555				
				(0.120)	(0.123)	(0.134)				
EIA				0.460*	0.487***	0.556**				
				(0.246)	(0.184)	(0.255)				
CU				-0.394**	-0.485***	-0.370*				
				(0.200)	(0.187)	(0.192)				
bfta				-0.0149	0.0196	-0.0268				
				(0.0511)	(0.0548)	(0.0544)				
beia				0.0310	-0.00159	0.0342				
				(0.0877)	(0.0779)	(0.0890)				
bcu				0.0783	0.0462	0.0868				
				(0.0784)	(0.0846)	(0.0838)				
Geo controls			YES	YES	YES	YES			YES	YES
Observations	81	79	79	79	77	77	75	74	74	74
R-squared	0.472	0.589	0.670	0.766	0.794	0.744	0.580	0.580	0.644	0.725
Robust standard errors in parentheses										
*** p<0.01, ** p<0.05, * p<0.1										
UnderID test (Paap)	7.330	12.020	11.976	6.503	3.333	5.300	8.347	7.572	9.233	11.789
Kleibergeen-Paap F	19.235	44.949	39.009	33.554	11.101	31.211	47.447	31.292	16.199	11.336

Table 5

POLS e Panel-FE: dependent is log of real GDP per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
COEFFICIENT	POLS	POLS	FE	FE	FE	FE	FE	POLS	POLS	FE	FE	FE	FE
Log of nominal openness	0.726***	0.610***	0.168***	0.120***	0.123***	0.122***	0.137***						
	(0.0361)	(0.0499)	(0.0398)	(0.0319)	(0.0358)	(0.0349)	(0.0370)						
Log of real openness								0.528***	0.439***	0.0832***	0.0494**	0.0485**	0.0578***
								(0.0281)	(0.0367)	(0.0210)	(0.0206)	(0.0205)	(0.0204)
polity2		0.0404***		-0.00208	-0.0000666	0.000106	-0.00241		0.0396***		-0.000174	-0.0000905	-0.00156
		(0.00610)		(0.00502)	(0.00496)	(0.00497)	(0.00456)		(0.00637)		(0.00499)	(0.00501)	(0.00488)
log of population		-0.0201		-0.743***	-0.739***	-0.765***	-0.882***		-0.0439		-0.797***	-0.817***	-0.891***
		(0.0269)		(0.141)	(0.144)	(0.160)	(0.176)		(0.0288)		(0.150)	(0.163)	(0.185)
conflict		-0.187**		-0.101***	-0.101***	-0.101***	-0.102***		-0.223**		-0.102***	-0.102***	-0.0998***
		(0.0883)		(0.0347)	(0.0337)	(0.0339)	(0.0335)		(0.0947)		(0.0356)	(0.0357)	(0.0360)
log exchange rate		-0.0534***			-0.0165**	-0.0167**	-0.0231***						
		(0.00617)			(0.00777)	(0.00786)	(0.00778)						
price level relative to US		-0.000424			-0.000209	-0.000215	-0.000232						
		(0.000457)			(0.000279)	(0.000272)	(0.000241)						
gatt		0.0125		0.0296	0.0252	0.0268	0.0389		0.0531		0.0357	0.0367	0.0467
		(0.0851)		(0.0622)	(0.0587)	(0.0585)	(0.0561)		(0.0887)		(0.0637)	(0.0635)	(0.0636)
neighbours' growth		0.00463				-0.00170	-0.00206		0.00340			-0.00195	-0.00244
		(0.00507)				(0.00164)	(0.00162)		(0.00524)			(0.00183)	(0.00188)
log of neighbours' population		0.0662***				0.0295	0.0311		0.0798***			0.0222	-0.00467
		(0.0199)				(0.109)	(0.108)		(0.0190)			(0.110)	(0.115)
FTA		0.00488					-0.0198		-0.0176				-0.0179
		(0.0380)					(0.0303)		(0.0364)				(0.0302)
EIA		0.143					0.0122		0.165*				0.0198
		(0.0985)					(0.0473)		(0.0921)				(0.0445)
CU		-0.0530					0.0923		-0.0328				0.0955
		(0.0890)					(0.0648)		(0.0849)				(0.0679)
neighbours' FTA		0.0212					0.00794		0.0165				0.0106
		(0.0148)					(0.00999)		(0.0149)				(0.0111)
neighbours' EIA		0.0137					-0.0487**		0.00687				-0.0419*
		(0.0419)					(0.0235)		(0.0417)				(0.0249)
neighbours' CU		0.00291					0.0175		0.0210				-0.0185
		(0.0353)					(0.0227)		(0.0356)				(0.0230)
Observations	545	541	545	541	541	541	541	545	541	545	541	541	541
Number of countries			83	82	82	82	82			83	82	82	82
R-squared	0.412	0.575	0.297	0.411	0.431	0.433	0.460	0.437	0.537	0.257	0.387	0.390	0.407

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6

IV Panel-FE: first stage regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent	log nominal openness	log nominal openness	log nominal openness	log nominal openness	log real openness	log real openness	log real openness	log real openness	log real openness
log of neighbour's trade	1.588***	1.642***	1.526***	1.520***	1.876***	1.950***	1.962***	1.968***	1.909***
	(0.389)	(0.382)	(0.306)	(0.309)	(0.649)	(0.647)	(0.631)	(0.650)	(0.637)
polity2		0.0176**	0.0190**	0.0191**		0.0100	0.0147	0.00984	0.0188*
		(0.00708)	(0.00865)	(0.00861)		(0.0108)	(0.0119)	(0.0107)	(0.0109)
log of population		-1.012***	-0.952***	-0.886***		-1.218***	-1.211***	-1.159**	-0.592
		(0.238)	(0.211)	(0.231)		(0.417)	(0.392)	(0.483)	(0.474)
conflict		-0.0242	-0.0303	-0.0318		-0.0934	-0.0941	-0.0942	-0.114
		(0.0603)	(0.0567)	(0.0574)		(0.0973)	(0.0986)	(0.0979)	(0.0961)
gatt		0.102	0.0955	0.0893		0.126	0.116	0.121	0.0386
		(0.108)	(0.110)	(0.110)		(0.164)	(0.159)	(0.168)	(0.148)
neighbours' growth				0.000206				0.00217	0.00395
				(0.00225)				(0.00287)	(0.00315)
log of neighbours' population				-0.0904				-0.0820	-0.0387
				(0.145)				(0.266)	(0.265)
FTA									0.0964*
									(0.0515)
EIA									0.0464
									(0.0962)
CU									0.0156
									(0.124)
bfta									-0.0356**
									(0.0138)
beia									0.150***
									(0.0361)
bcu									-0.0808**
									(0.0363)
log exchange rate			-0.00291	-0.00296			-0.0365		
			(0.00922)	(0.00928)			(0.0225)		
price level relative to US			0.00231*	0.00231*					
			(0.00126)	(0.00125)					
Observations	551	547	547	547	551	547	547	547	547
Number of ccode	83	82	82	82	83	82	82	82	82
R-squared	0.285	0.362	0.418	0.419	0.252	0.298	0.313	0.299	0.350

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7

IV Panel-FE: structural equation (dependent is log of real GDP per capita)

COEFFICIENT	(1) lrgdpch	(2) lrgdpch	(3) lrgdpch	(4) lrgdpch	(5) lrgdpch	(6) lrgdpch	(7) lrgdpch	(8) lrgdpch	(9) lrgdpch	(10) lrgdpch
log of nominal openness	0.242***	0.269***	0.294***	0.290***	0.290***					
	(0.0880)	(0.0841)	(0.0903)	(0.0922)	(0.0909)					
log of real openness						0.206**	0.228***	0.229***	0.223***	0.235***
						(0.0808)	(0.0792)	(0.0797)	(0.0797)	(0.0844)
polity2		-0.00530	-0.00390	-0.00377	-0.00646		-0.00265	-0.00149	-0.00131	-0.00431
		(0.00399)	(0.00414)	(0.00420)	(0.00424)		(0.00379)	(0.00395)	(0.00396)	(0.00412)
log of population		-0.607***	-0.593***	-0.639***	-0.800***		-0.597***	-0.592***	-0.635***	-0.860***
		(0.136)	(0.138)	(0.143)	(0.143)		(0.156)	(0.157)	(0.164)	(0.158)
conflict		-0.0900***	-0.0879***	-0.0874***	-0.0895***		-0.0752**	-0.0753**	-0.0754**	-0.0693*
		(0.0294)	(0.0289)	(0.0289)	(0.0281)		(0.0359)	(0.0356)	(0.0353)	(0.0357)
log exchange rate			-0.0166***	-0.0167***	-0.0251***			-0.00914*	-0.00939*	-0.0170***
			(0.00466)	(0.00469)	(0.00509)			(0.00544)	(0.00546)	(0.00538)
price level relative to US			-0.000687*	-0.000681*	-0.000657*					
			(0.000366)	(0.000363)	(0.000354)					
gatt		0.0140	0.00858	0.0124	0.0294		0.0123	0.00955	0.0130	0.0416
		(0.0422)	(0.0400)	(0.0397)	(0.0377)		(0.0531)	(0.0529)	(0.0526)	(0.0507)
neighbours' growth				-0.000974	-0.00145				-0.00132	-0.00229
				(0.00129)	(0.00128)				(0.00144)	(0.00143)
log of neighbours' population				0.0539	0.0571				0.0463	0.0248
				(0.0697)	(0.0687)				(0.0712)	(0.0697)
FTA					-0.0247					-0.0340
					(0.0216)					(0.0224)
EIA					0.000732					-0.000718
					(0.0361)					(0.0356)
CU					0.105*					0.0705
					(0.0576)					(0.0601)
bfta					0.00871					0.0167**
					(0.00617)					(0.00767)
beia					-0.0536***					-0.0780***
					(0.0157)					(0.0210)
bcu					0.0260					0.0185
					(0.0188)					(0.0201)
Observations	543	539	539	539	539	543	539	539	539	539
Number of ccode	81	80	80	80	80	81	80	80	80	80
R-squared	0.281	0.354	0.363	0.369	0.408	0.170	0.214	0.217	0.231	0.266
Clustered standard errors in parentheses										
*** p<0.01, ** p<0.05, * p<0.1										
Underidentification LM (Paap)	10.966	10.843	14.349	14.216	13.991	8.193	8.228	8.499	8.658	8.214
Weak ID test (Kleibergeen-Paap F)	19.473	20.406	29.293	29.009	28.591	12.790	13.171	13.741	13.950	12.749

Table 8

IV Panel-FE: structural equation (dependent is log of real GDP per worker)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log of nominal openness	0.193**	0.219***	0.242***	0.243***	0.246***					
	(0.0825)	(0.0775)	(0.0841)	(0.0865)	(0.0852)					
log of real openness						0.163**	0.184***	0.186***	0.185**	0.197**
						(0.0740)	(0.0713)	(0.0718)	(0.0728)	(0.0767)
polity2		-0.00414	-0.00252	-0.00260	-0.00577		-0.00203	-0.000556	-0.000554	-0.00398
		(0.00380)	(0.00390)	(0.00396)	(0.00403)		(0.00356)	(0.00369)	(0.00370)	(0.00387)
log of population		-0.582***	-0.569***	-0.622***	-0.799***		-0.575***	-0.570***	-0.620***	-0.852***
		(0.139)	(0.140)	(0.144)	(0.146)		(0.156)	(0.158)	(0.166)	(0.163)
conflict		-0.0922***	-0.0901***	-0.0888***	-0.0906***		-0.0803**	-0.0802**	-0.0791**	-0.0737**
		(0.0289)	(0.0283)	(0.0283)	(0.0276)		(0.0339)	(0.0335)	(0.0334)	(0.0337)
log exchange rate			-0.0177***	-0.0177***	-0.0256***			-0.0115**	-0.0115**	-0.0185***
			(0.00399)	(0.00404)	(0.00453)			(0.00486)	(0.00492)	(0.00489)
price level relative to US			-0.000626*	-0.000631*	-0.000597*					
			(0.000344)	(0.000344)	(0.000334)					
gatt		-0.0464	-0.0510	-0.0453	-0.0294		-0.0452	-0.0473	-0.0420	-0.0154
		(0.0478)	(0.0445)	(0.0440)	(0.0417)		(0.0579)	(0.0576)	(0.0577)	(0.0555)
neighbours' growth				-0.000344	-0.000823				-0.000606	-0.00151
				(0.00128)	(0.00127)				(0.00141)	(0.00139)
log of neighbours' population				0.0706	0.0656				0.0647	0.0382
				(0.0663)	(0.0654)				(0.0706)	(0.0691)
FTA					-0.0309					-0.0384*
					(0.0198)					(0.0211)
EIA					-0.0111					-0.0130
					(0.0322)					(0.0322)
CU					0.119**					0.0886
					(0.0527)					(0.0544)
bfta					0.00887					0.0157**
					(0.00583)					(0.00736)
beia					-0.0506***					-0.0715***
					(0.0151)					(0.0200)
bcu					0.0163					0.00949
					(0.0171)					(0.0181)
Observations	537	533	533	533	533	537	533	533	533	533
Number of ccode	80	79	79	79	79	80	79	79	79	79
R-squared	0.240	0.327	0.344	0.346	0.392	0.147	0.207	0.215	0.219	0.261

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Underidentification LM (Paap)	10.764	10.688	14.129	14.022	13.848	8.134	8.193	8.462	8.626	8.216
Weak ID test (Kleibergeen-Paap F)	19.095	20.097	28.769	28.549	28.278	12.714	13.134	13.698	13.913	12.774

Table 9
Cross section estimates on sequenced five-year averages

Panel A: dependent variables are log of per capital GDP (lrgdpch) or log real GDP per worker (logwok)

	1970		1975		1980		1985		1990		1995	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
COEFFICIENT	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok
log of nominal openness	0.524***	0.682***	0.555***	0.675***	0.751***	0.861***	0.625***	0.681***	0.714***	0.813***	0.514**	0.510**
	(0.183)	(0.215)	(0.119)	(0.142)	(0.184)	(0.227)	(0.0982)	(0.116)	(0.132)	(0.140)	(0.213)	(0.257)
polity2	0.0250	0.0231	0.0287**	0.0270*	0.0158	0.0138	0.0300*	0.0284*	0.00478	0.00357	-0.00634	-0.0186
	(0.0174)	(0.0186)	(0.0144)	(0.0143)	(0.0140)	(0.0142)	(0.0153)	(0.0172)	(0.0125)	(0.0145)	(0.0164)	(0.0171)
log of population	-0.0699	-0.0454	-0.0767	-0.0481	0.00133	0.0385	0.00376	0.0363	0.0431	0.0861	0.0350	0.0287
	(0.0759)	(0.0935)	(0.0651)	(0.0729)	(0.0618)	(0.0768)	(0.0490)	(0.0605)	(0.0534)	(0.0645)	(0.0740)	(0.0815)
conflict	-0.120	-0.00580	-0.0671	-0.0295	0.0709	0.117	-0.0577	-0.0453	0.189	0.265*	-0.0619	-0.0231
	(0.177)	(0.211)	(0.197)	(0.224)	(0.220)	(0.234)	(0.167)	(0.180)	(0.140)	(0.143)	(0.197)	(0.232)
log exchange rate	-0.0403***	-0.0447***	-0.0367***	-0.0419***	-0.00843	-0.00816	-0.0320*	-0.0411**	-0.0668**	-0.0601**	-0.0707***	-0.0720***
	(0.0125)	(0.0131)	(0.0127)	(0.0132)	(0.0175)	(0.0172)	(0.0190)	(0.0207)	(0.0268)	(0.0277)	(0.0248)	(0.0272)
price level relative to US	-0.00153	-0.00145	-0.00192***	-0.00198***	-0.00258***	-0.00262***	-0.00222***	-0.00203***	0.00306***	0.00427***	0.0116*	0.0119*
	(0.00154)	(0.00185)	(0.000685)	(0.000697)	(0.000698)	(0.000765)	(0.000613)	(0.000636)	(0.00104)	(0.00143)	(0.00654)	(0.00642)
gatt	0.0614	-0.0515	0.00473	-0.102	0.0629	-0.0390	-0.0446	-0.132	0.425**	0.248	0.449**	0.377*
	(0.215)	(0.244)	(0.190)	(0.206)	(0.190)	(0.215)	(0.171)	(0.190)	(0.188)	(0.198)	(0.179)	(0.197)
neighbours' growth	0.0193	0.0255	0.00238	0.00567	-0.0251*	-0.0321**	-0.0297*	-0.0345*	0.00811	0.0123	-0.00234	0.00128
	(0.0149)	(0.0157)	(0.0161)	(0.0163)	(0.0134)	(0.0159)	(0.0173)	(0.0189)	(0.0106)	(0.0118)	(0.0135)	(0.0140)
log of neighbours' population	-0.0542	-0.0517	-0.0728	-0.0801	0.0685	0.0825	0.0221	0.0119	0.0434	0.0242	0.0555	0.0543
	(0.0792)	(0.0899)	(0.0528)	(0.0576)	(0.0637)	(0.0666)	(0.0481)	(0.0553)	(0.0471)	(0.0512)	(0.0520)	(0.0527)
FTA	0.221	0.291	0.00232	0.0807	-0.0549	0.00363	0.103	0.119	-0.00426	-0.0694	-0.0929	-0.0782
	(0.189)	(0.213)	(0.373)	(0.378)	(0.258)	(0.240)	(0.221)	(0.207)	(0.0961)	(0.102)	(0.0735)	(0.0792)
EIA	0.413	1.143*	0.556	1.017	1.012**	1.016**	0.214	0.209	0.294	0.371	0.167	0.184
	(0.552)	(0.606)	(0.569)	(0.662)	(0.504)	(0.499)	(0.361)	(0.362)	(0.246)	(0.254)	(0.143)	(0.148)
CU	-0.733	-1.376**	-0.914	-1.370**	-1.194***	-1.191***	-0.545**	-0.480**	-0.421*	-0.367	-0.0432	-0.0490
	(0.543)	(0.601)	(0.599)	(0.675)	(0.416)	(0.429)	(0.214)	(0.227)	(0.248)	(0.257)	(0.114)	(0.128)
bfta	0.267***	0.264**	0.191	0.205*	0.152	0.169*	0.154	0.228**	-0.00576	-0.00440	0.0411*	0.0424
	(0.0990)	(0.106)	(0.118)	(0.119)	(0.108)	(0.102)	(0.0996)	(0.0916)	(0.0611)	(0.0675)	(0.0241)	(0.0280)
beia	-0.340	-0.672*	-0.188	-0.457	-0.323	-0.442	-0.145	-0.317	0.0296	0.00428	-0.0316	-0.0324
	(0.305)	(0.343)	(0.366)	(0.395)	(0.265)	(0.291)	(0.263)	(0.260)	(0.0952)	(0.0979)	(0.0697)	(0.0740)
bcu	0.189	0.443	0.195	0.369	0.304	0.321	0.0310	0.0448	0.0149	0.0323	-0.0471	-0.0611
	(0.259)	(0.289)	(0.289)	(0.318)	(0.230)	(0.240)	(0.143)	(0.152)	(0.0697)	(0.0674)	(0.0494)	(0.0510)
Geo controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	69	69	77	77	77	76	78	77	80	79	79	78
R-squared	0.724	0.720	0.699	0.711	0.726	0.735	0.775	0.774	0.797	0.787	0.803	0.784

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B: dependent variables are log of per capital GDP (lrgdpch) or log real GDP per worker (logwok)

	1970		1975		1980		1985		1990		1995	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
COEFFICIENT	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok	lrgdpch	logwok
log of real openness	0.552**	0.707***	0.487***	0.592***	0.764***	0.936***	0.549***	0.603***	0.568***	0.640***	0.505***	0.505***
	(0.223)	(0.261)	(0.122)	(0.146)	(0.217)	(0.284)	(0.108)	(0.130)	(0.108)	(0.118)	(0.140)	(0.169)
polity2	0.0146	0.0104	0.0188	0.0155	0.00435	-0.000379	0.0261*	0.0252	0.00451	0.00284	-0.00152	-0.0129
	(0.0226)	(0.0252)	(0.0153)	(0.0156)	(0.0198)	(0.0224)	(0.0157)	(0.0170)	(0.0129)	(0.0146)	(0.0169)	(0.0174)
log of population	-0.0694	-0.0505	-0.0542	-0.0229	0.0247	0.0815	-0.00690	0.0322	0.0464	0.0854	0.0355	0.0291
	(0.0836)	(0.102)	(0.0750)	(0.0853)	(0.0766)	(0.0984)	(0.0608)	(0.0689)	(0.0557)	(0.0670)	(0.0733)	(0.0823)
conflict	0.0827	0.245	-0.203	-0.182	-0.339	-0.356	-0.247	-0.231	0.153	0.223	-0.0606	-0.0123
	(0.256)	(0.308)	(0.259)	(0.294)	(0.408)	(0.477)	(0.217)	(0.229)	(0.142)	(0.143)	(0.191)	(0.219)
log exchange rate	-0.0211	-0.0215	-0.0161	-0.0181	0.00701	0.00673	0.00227	-0.00641	-0.0673**	-0.0618**	-0.0782***	-0.0809***
	(0.0152)	(0.0169)	(0.0156)	(0.0168)	(0.0240)	(0.0262)	(0.0228)	(0.0243)	(0.0303)	(0.0313)	(0.0289)	(0.0314)
gatt	0.0757	-0.0392	0.0757	-0.0219	0.267	0.225	0.185	0.108	0.402**	0.220	0.454**	0.389*
	(0.252)	(0.291)	(0.198)	(0.214)	(0.258)	(0.307)	(0.191)	(0.204)	(0.192)	(0.201)	(0.177)	(0.200)
neighbours' growth	0.0106	0.0151	0.00103	0.00375	-0.0118	-0.0110	0.00880	0.00482	0.00242	0.00612	-0.00182	0.00171
	(0.0220)	(0.0249)	(0.0170)	(0.0177)	(0.0215)	(0.0282)	(0.0371)	(0.0385)	(0.0103)	(0.0113)	(0.0129)	(0.0133)
log of neighbours' population	-0.00156	0.0142	-0.0479	-0.0509	0.129	0.151	-0.000386	-0.0163	0.0470	0.0251	0.0480	0.0450
	(0.101)	(0.118)	(0.0594)	(0.0672)	(0.101)	(0.118)	(0.0621)	(0.0685)	(0.0464)	(0.0510)	(0.0539)	(0.0546)
FTA	0.318	0.408	-0.0577	0.00659	-0.362	-0.350	-0.0428	-0.0379	-0.0576	-0.116	-0.0535	-0.0376
	(0.223)	(0.256)	(0.381)	(0.390)	(0.303)	(0.309)	(0.259)	(0.244)	(0.110)	(0.115)	(0.0615)	(0.0691)
EIA	0.968	1.818**	0.692	1.181	1.359**	1.485*	0.379	0.387	0.195	0.291	0.151	0.164
	(0.618)	(0.773)	(0.583)	(0.719)	(0.671)	(0.764)	(0.376)	(0.364)	(0.253)	(0.264)	(0.146)	(0.147)
CU	-1.010*	-1.703**	-0.779	-1.212*	-1.035**	-1.071*	-0.563***	-0.494**	-0.345	-0.304	-0.0520	-0.0588
	(0.518)	(0.670)	(0.579)	(0.694)	(0.507)	(0.578)	(0.202)	(0.209)	(0.250)	(0.259)	(0.105)	(0.115)
bfta	0.280**	0.277**	0.107	0.113	0.0106	0.0195	0.0145	0.0863	-0.00627	-0.00515	0.0351	0.0373
	(0.111)	(0.119)	(0.161)	(0.169)	(0.197)	(0.215)	(0.113)	(0.112)	(0.0597)	(0.0658)	(0.0247)	(0.0286)
beia	-0.649*	-1.046**	-0.197	-0.475	-0.190	-0.355	-0.175	-0.343	0.0553	0.0351	-0.00845	-0.0116
	(0.371)	(0.447)	(0.410)	(0.452)	(0.416)	(0.476)	(0.273)	(0.274)	(0.0923)	(0.0945)	(0.0703)	(0.0735)
bcu	0.293	0.568*	0.220	0.395	0.413	0.478	0.198	0.209	-0.0316	-0.0163	-0.0332	-0.0464
	(0.278)	(0.338)	(0.298)	(0.342)	(0.293)	(0.348)	(0.184)	(0.194)	(0.0644)	(0.0659)	(0.0487)	(0.0489)
Geo controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	69	69	77	77	77	76	78	77	80	79	79	78
R-squared	0.597	0.567	0.625	0.624	0.460	0.390	0.701	0.704	0.795	0.786	0.805	0.789

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10
Panel-FE IV estimation allowing for individual-specific coefficients on openness

Dependent variable is detrended real GDP per capita (d_y) or detrended real GDP per worker (d_wok)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent	d_y	d_y	d_y	d_y	d_y	d_y	d_wok	d_wok	d_wok	d_wok	d_wok	d_wok
log of nominal openness	0.104***		0.0875**		0.0813**		0.120***		0.104***		0.0964***	
	(0.0348)		(0.0348)		(0.0352)		(0.0313)		(0.0305)		(0.0305)	
log of real openness		0.0834***		0.0741***		0.0687**		0.0966***		0.0880***		0.0812***
		(0.0259)		(0.0274)		(0.0276)		(0.0237)		(0.0242)		(0.0240)
log of nominal exchange rate			-0.0148***	-0.0108*	-0.0157***	-0.0118*			-0.0158***	-0.0110	-0.0168***	-0.0121*
			(0.00510)	(0.00640)	(0.00514)	(0.00623)			(0.00522)	(0.00669)	(0.00524)	(0.00642)
neighbours' growth			0.000351	0.000376	0.000465	0.000489			0.000508	0.000542	0.000652	0.000685
			(0.000917)	(0.000919)	(0.000925)	(0.000923)			(0.000772)	(0.000770)	(0.000778)	(0.000771)
log of neighbours' population			-0.0583	-0.0500	-0.0601	-0.0517			-0.0259	-0.0161	-0.0273	-0.0175
			(0.151)	(0.152)	(0.143)	(0.143)			(0.150)	(0.151)	(0.141)	(0.141)
polity2					-0.00263	-0.00283					-0.00336*	-0.00359*
					(0.00184)	(0.00196)					(0.00184)	(0.00200)
Observations	580	580	580	580	575	575	577	577	577	577	572	572
R-squared	0.141	0.090	0.163	0.113	0.176	0.131	0.175	0.115	0.201	0.139	0.221	0.167

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: all variables are detrended on (t, world openness at time t)

Chapter 3

Policy and institutional contagion

3.1 Introduction

What are the real drivers of growth? Scholars have long tried to answer this question, but findings have been mixed. The race for the best growth determinant has been usually between institutions, trade integration and geography. Rodrik, Subramanian and Trebbi (2004) provide evidence that among those three, only the former matters, but the debate is still open. Even more controversial is the assessment of trade policies, and not just trade intensity, in growth regressions. Rodrik and Rodriguez (1999) have argued it is meaningless to look for a systematic relationship between policies and growth, as one cannot verify the real objectives of the governments that implement them. From a less theoretical perspective, it is hard to think of procedures or instruments capable of solving the well-known endogeneity problem associated with policy evaluation in growth regressions.

In this paper, I run a number of growth regressions where policies and institutions are instrumented with their values in neighboring countries. In fact, the evidence suggests there is a systematic correlation between trade measures introduced by one particular country and similar policies implemented by its neighbors. Similarly, the quality of foreign institutions impacts strongly on domestic institution quality. The structural estimation shows that institutions are a powerful driver of growth, but trade policies (and geography) are not.

There are many real world examples that suggest that international correlation in trade policies and institutional settings is indeed relevant. Historically, when protectionism was the leading policy option, tariffs have peaked as a result of subsequent retaliation rounds, where each country responded to the imposition of import duties abroad by raising its custom levies. Shifting

to the post-war era, many Preferential Trade Agreements (PTAs) have been shaped by the desire of not being left behind in the building of a global network of preferential channels for home firms' abroad. In the 50s, the EFTA was created largely to respond to the birth of the European Union; more recently, many PTAs have been signed in a rush to ensure a better market position with key trade partners and neighbors vis-a-vis third countries (think of the agressive US trade policy in the early 2000s and the dynamism of Central and Eastern Asia countries in shaping bilateral agreement with sensitive neighbors). Another explanation for this geographical correlation is the proximity of neighboring countries to a large common third country or trading bloc. The increasing activism of the European Union in shaping Association and Free Trade Agreements with MENA and CEECs countries is a good example. Indeed, the data suggest that the impact of foreign variables on domestic ones is strong regardless of the different indicators of trade policies employed.

Turning to institutions, it is evident that many shifts in rule of law and right enforcement have spread from some leading country (think of the fall of the USSR as the cathalyzer of many institutional improvements in Russia and many Eastern European and Asian states). In western Europe, the EU plays again a crucial role in dictating the "approximation" to its institutions to neighbors and potential candidates for accession to the Union. In Asia, many countries followed China path of reforms and improvement of the rule of law, as they witnessed its impressive economic performance starting from the 1980s. In Latin America, Asia and elsewhere, developing countries are striving to improve their rule of law in order to qualify for official development assistance.

The validity of the instrument proposed is not to be taken for granted: the question is whether it is plausible that rule of law and trade policies abroad affect the income level at home only through their effect on rule of law at home and policies. Potential other channels include some form of trade or investment diversion induced by improved rule of law or better policies abroad. Moreover, and perhaps most importantly, stronger institutions or more liberal policies abroad may impact positively on income abroad, and this would improve home growth through increased domestic exports. Ultimately, the potential channels that could question instrument validity are mediated by foreign income and investment and trade flows. Section 5 deals with the potential drawbacks of such instrumentation strategy and provides evidence that controlling for foreign growth, investment and trade does not change the main findings of the paper.

Structural estimates show unambiguously that trade policies do not impact on growth, but rule of law does, positively and very significantly. Thus,

the evidence provided in this paper is a point in favor of the stream of literature that views institutions as the main driver for growth. In fact, much like the paper by Rodrik, Subramanian and Trebbi (2004), institutions seem to "trump" policies and geography when it comes to the direct effect of each single variable on growth.

Many other studies have examined the relationship between trade, insitutions and growth. Dollar and Kraay (2003) find a positive effect of trade on growth, but warn that their findings are unreliable due to weakness of their instruments. Alcalà and Ciccone (2004) study cross-country productivities and find that real openness (measured as the share of imports plus exports over PPP GDP) is a strong determinant of productivity, while institutions are not.

Trade policies have been examined by a number of other studies. The very influential paper by Sachs and Warner (1995) proposes an "openness index" based on five broad policy indicators, that shows a strong positive correlation with growth[1]. Dollar (1992) finds a similar result highlighting a negative relationship between growth and real exchange rate departures from the law of one price. Such departures were considered symptoms of more restrictive trade policies. Rodriguez and Rodrik (2001) heavily criticized both papers, claiming that the SW index is driven by two of its five components and that, with updaed data, Dollar's findings disappear. More recently, matching techniques have been gainfully employed in political economics to assess the relationship between democracy, market liberalization and growth[2], and the evidence given supports a positive role of market liberalization in fostering growth. A growing body of literature (Temple, 2003, provides an excellent review) claims that a definite answer to the question "how do policies affect growth" would be possible only if an appropriate instrument were found.

The paper is structured as follows: Section 2 presents the main empirical strategy and the data; Section 3 shows reduced form results on contagion; Section 4 present the relation between policies and growth; Section 5 provides some robustness checks and Section 6 concludes.

3.2 Data and empirical strategy

The core variables in my investigation are those related to trade policies and institutions. The latter are proxied by the rule of law index taken from the World Governance Indicators (Kaufmann, Kraay and Mastruzzi, 2008). The

[1] The Sachs and Warner (SW) index, later updated by Wacziarg and Welch (2003), is my baseline indicator for trade related policies

[2] See Giavazzi and Tabellini (2005) and Persson and Tabellini (2007)

index ranges from -2.5 to +2.5, with higher scores reflecting a sounder and better enforcement of the rule of law.

I chose three indicators of trade policy. First, I employ the Sachs-Warner index (Sachs and Warner, 1995), updated by Wacziarg and Welch (2003). Despite some criticism (Rodrik, 2001) and its disproportionate reliance on black market premium, it is still widely used as a broad indicator of market oriented policies.

Another straightforward measure of trade policy is the average tariff level imposed by each country on its imports. Bouet (2004) provides an excellent dataset with the average applied tariff in 2001 in 110 countries. The advantage of this dataset is that it incorporates the tariffs agreed on the basis of bilateral agreements and reports not just the "bound rate", the declared tariff to the WTO, but the duties effectively applied on each good from each country.

Finally, a direct proxy of strategic trade policy is given by PTAs signed by each country. I use the PTAs notified to the WTO as of May 2008, available at the WTO website[3].

To measure contagion, I first retrieve border data from the contiguity database v.3.1. The classification system for contiguous dyads is comprised of five categories, one for land contiguity and four for water contiguity. Land contiguity is defined as the intersection of the homeland territory of the two states in the dyad, either through a land boundary or a river, such as the Rio Grande in the case of the US-Mexico border. Water contiguity is divided into four categories, based on distances of 12, 24, 150, and 400 miles. Version 3.1 of the dataset spans the period 1816-2006. I select as neighbors all countries sharing a land or river border with home, or situated at a coastal distance of no more than 24 miles from the domestic coast. I then sum the relevant policy and institutional values of all resulting neighbors and use them as excluded.

To assess the existence of contagion in trade policies, I estimate the equation:

$$P_i = \alpha + \beta P_j + \gamma GEO_i + \delta POP_i + \zeta INST_j + \varepsilon_i \tag{3.1}$$

WhereP_i is the relevant trade policy under study, P_j is the value of the same policy in neighbouring countries, GEO_i is a set of geographical controls (namely log of area and absolute latitude), POP_i is the log of population and $INST_j$ is the measure of institutional quality in neighboring countries (the simple sum of the rule of law indexe of such countries). Population, trade

[3]A complete list of participating countries for each agreement can be found at http://www.wto.org/english/tratop_e/region_e/regfac_e.htm.

intensity and investment data are taken from the Penn World Tables, mark 6.2 (Summer, Heston and Betina, 2007); geographical data are taken from the dataset constructed by Frankel and Romer (1999), who in turn use World Reference data. Similarly to equation 3.1, contagion in institutional quality is given by:

$$INST_i = \theta + \lambda INST_j + \mu GEO_i + \nu POP_i + \xi P_j + \eta_i \tag{3.2}$$

I then use the predicted values of $INST_i$and $P_i$as regressors in the structural equation

$$GDP_i = a + b_1\hat{P}_i + b_2\hat{INST}_i + b_3 GEO_i + b_4 POP_i + e_i \tag{3.3}$$

Equation 3.2estimates the direct contribution to growth of the different potential determinant: policies, institutions and geography. In the section devoted to the robustness checks, I will add to both estimation stages additional controls, based again on the sum of each neighbor's value of real GDP, trade and investment intensity.

3.3 Contagion in policies and institutions

I start by running a basic specification for the reduced form equation with institutions as the dependent variable. The regressors are based on geograpy, with additional controls for country size. Note that the variable that captures trade policies abroad is also added, and is insignificant. Distance from the equator is a strong predictor of the rule of law index. This result is perfectly consistent with the findings of Hall and Jones (1999), who first proposed to use distance from the equator as an instrument for institutions, and Alcalà and Ciccone (2004). The log of land area and the log of population do not have any significant impact.

Column 2 considers an alternative instrument for domestic institutions based on the quality of institutions among a country's neighbors. The new instrument is the simple sum of the rule of law indexes in each country's neighbors. Such variable proves to be highly significant and positively affects domestic institutions. The impact of distance from equator is still strong, but on top of this foreign institutions do play a large role in determining domestic ones.

The instrument tests performed show that instrument quality is good in both specifications; importantly, the addition of foreign institutions does not weaken the instrumentation strategy. To the contrary, rule of law abroad proves to be a strong additional instrument for domestic institutions. The

F-test of excluded instruments takes value of 12.52 for the specification in Columns 1 and 15.86 when institutions abroad are added as exogenous instrument. The rule of thumb proposed by Staiger and Stock to be confident of instrument relevance is 10.

In Columns 3 and 4 the dependent variable is the domestic index for liberal trade policies. From inspection of the results, it is appartent that more "open" policies abroad induce more liberal policies at home. The coefficient is large and very significant. Column 3 shows that geography, measured by distance from the equator, does not play a role in shaping policies, while population has a marginally positive role. Larger countries, in terms of land area, appear to have more restrictive policies. In Column 4 the index for rule of law in neighborig countries is added. Results show that institutions abroad do not play any role in explaining domestic trade policies. The F-test of exluded instrument for openness is 57.54 in the specification without foreign institutions (Column 3) and 39.96 when the additional instrument for institutions is added (Column 4).

The results shown provide strong evidence that proxies for institutions and trade policies abroad are reliable predictors of similar variables at home. In the case of institution, foreign measures perform well even controlling for geography.

In models with multiple instruments, high significance of each single excluded instrument is not sufficiesnt to ensure reliable inference. This is due to the potential correlation between the predicted values of the endogenous regressors coming from first stage estimation. Stock and Yogo (2003) have devised a weak identification test, built as an extension of the Kleibergerg-Paap rank F-statistics. The test specificies critical values that indicate rejection at the 5% level of the null that the quality of the instruments is below a certain threshold.

The model with latitude and openness abroad as exluded instruments performs well in the weak identification test, showing that instrument quality is above the highest level at the 5% confidence level (critical value is 7.03 and test statistics is 12.021). When adding rule of law abroad to latitude and foreign policies, performance is still high in all test. The Stock-Yogo test for weak identification shows values above the ones required for the highest possible level (critical value is now 13.43 and test statistics is 15.762). The J statistics for overidentifying restriction is close to zero, implying that the null of valid overidentying restriction cannot be rejected at any level. Note also that the Shea Partial R-squared is high both when foreign institutions are omitted and included in the model: in the model with them, they take

values of [4] .54 for trade policies and .27 for rule of law.

3.4 Policies, institutions and growth

The estimates from the structural equation are reported in Table 2. The dependent variable is the log of real per capita GDP. The first Column shows estimates using trade policies abroad and latitude as excluded insruments; Column 2 shows estimates obtained adding foreign institutions to the set of exluded instruments.

The feature that immediately meets the eye is policies' absolute lack of predictive power: the coeffienct of the Sachs and Warner index is never (and by far) significantly different from zero. The rule of law index, instead, impacts positively and very significantly on the imcome level: coefficient are large and relatively precise.

The evidence provided marks a strong point in favour of those (see, among others, Rodrik, Submaharian and Trebbi, 2004) that claim that institutions are the real driver for economic growth, and that, once these are taken into account, they "trump" other potential determinants such as exposure to foreign trade or geography. Here the variables under scrutiny are institutions and trade policies, using geography as one of the instrument. Nevertheless, given that foreign institutions provide an additional instrument for domestic institutions, I can explore the direct impact on geography on growth simply treating, in te last Column of Table 2, the distance from the equator variable as *included* instrument rather than as an *excluded* one. Result show that only institutions have a significantly positive coefficient in the main equation. Geography plays a role only in shaping institutions, but does not directly affect growth. Unlike previous studies (see also Dollar and Kraay, 2003), the quality of the estimates is supported by a good performance of the proposed instruments in a variety of under- and weak-identification tests[5].

Does this mean that policies and geography do not matter, and that institutions account for all variation in growth? While the Sachs and Warner index does not have explanatory power in the growth regressions proposed, it is important to stress that it is a summary measure that cannot capture

[4] The usual partial R-squared is not reliable in models with more than one endogenous variable, due to potential correlation between the excluded instruments. Shea (1991) proposed a revised version of the R-squared that takes this problem into account.

[5] In this last specification, the F-test for excluded instrument regarding rule of law falls slightly below the rule of thumb of ten. This does not seem to affect the overall reliability of the inference, given the Stock-Yogo weak identification test is still above the highest level. Moreover, the tests for weak-ID robust inference always reject the null of joint no-significance for endogenous regressors at the 1% level.

all the different features that trade-reated policies worldwide can assume. The results shown above just point out that "blunt" liberalization do not matter per se. It is much more reasonable to think that the way policies are implemented, and their eventual outcome in promoting trade, matters much more[6].

3.5 Robustness

The validity of the instruments employed in the previous sections could be questioned, because there are other possible channels through which trade policies and institutions abroad might impact domestic growth. The most obvious one is that rule of law and trade policies abroad affect GDP abroad, and this can impact home GDP. As argued above, trade and investment are the most plausible channel for the latter influence to take place. For example, better institutions and policies abroad may boost foreign growth, investment and trade, and these could affect domestic growth. Therefore, it is essential that the estimates outlined above are robust to the inclusion of controls that take those magnitudes into account.

There could also be spatially correlated shocks affecting policies, institutions and growth in neighboring countries and at home. Apart from natural distaster that affect an entire region, many "common shocks" not really regional exogenous shocks. Many of them actually start from a specific country and then spread overseas. Given our interest in "contagion", that is exactly one of the channels through which the proposed instruments should work.

Table 3 presents evidence that the main results of these paper hold even after adding foreign GDP, openness and investment as included instruments. The first three Columns examine each potential channel (respectively, GDP, trade and investment) separately. It can be seen that the structural estimates of the rule of law index are always positive and very significant. The size of he coefficient is somewhat reduced when neighobrs' GDP is included in the regression. Instrument quality is good in all three specification, even if in Column 1 the Weak-ID test rejects the null of weak identification only at the second-highest possible level. As anticipated above, this is due to the high correlation between rule of law abroad and foreign GDP (.61) that prevents to properly isolate the effects of the two on domestic growth.

Column 4 controls simultaneously for foreign GDP and trade opennes,

[6] The evidence in favour of a positive impact of trade flows on growth is still mixed, but results so far have been considered more reliable in this area rather than when dealing with policies. Gamba (2008) provides evidence that actual trade is good for growth using an instrumentation strategy similar to the one proposed in this paper.

Column 5 for foreign GDP and investment share of GDP. Results hold in both estimation stages: policies and rule of law abroad are still strong predictors of domestic policies and institutions, and the latter promote growth, while market-oriented policies do not. Weak-ID test statistics is above or close to the highest level, even though the F-test of exluded instrument for rule of law falls to around 8 in Columns 4 and 5. Columns 6 shows a model that controls for foreign GDP, trade and investment, together with domestic trade and investment shares of GDP: results are confirmed, even though instrument quality is somewhat affected: the Weak-ID test statistics is now 7.29, and the second level critical value is 8.18. Moreover, the F-test for excluded instrument on rule of law is now only 7.78, below the rule of thumb of 10. It is important to stress that when considered separately, neither foreign income, nor trade nor investment weaken my instruments at all. Even considered together, the most threatening channels that could invalidate the proposed instruments seem to affect instrument quality only marginally.

It is also interesting to investigate whether other policy variables in addition to the two already examined, show similar cross border "contagion". In Table 4, I re-run the baseline estimations substituting the Sachs and Warner index with two alternative trade policy measures: the average applied import tariff rate in 2001 and the number of Preferential Trade Agreements (PTAs) signed and entered into force at that time. As in the previous sections, the endogenous variables are referred to the home country and the instruments are the variables' values in neighboring countries.

Results are strikingly similar to those in Table 2. The trade policy variables are never significant in the structural equation, and even enter with the "wrong" sign. The institutional variable is instead always positive and highly significant, and coefficient size is similar across specification. Instrument quality is, however, less satisfactory when using tariffs or PTAs. The Weak-ID test, for example, rejects the null only at the lowest level, and sometimes it even fails to do so. Therefore, the inference shown in this Table should be treated with caution[7]. Nonetheless, the estimates do point to the lack of explanatory power for trade policy measures, while the quality of institutions seems a strong driver of growth in all specifications.

It is worth noting that, even if using alternative measures of trade policy is not advisable due to instrument weakness, the reduced form equations all point to a strong "contagion" effect in policies: tariffs or PTAs signed abroad have a strong impact on similar measures at home. In particular,

[7]The reason behind the lack of instrument quality may lie in the strong correlation between tariffs and rule of law (-.59) and number of signer PTAs and rule of law (.56). These are much higher than the correlation between the SW index and rule of law (.30).

higher tariffs abroad imply higher tariffs at home, while more PTAs signed by neighbors (and not participated by the home country) translate into less PTAs signed by the home country itself. This last results deserves some attention. On the one hand, one would expect a "keep up with the Joneses" effect in PTAs: countries strive to achieve the best deals, competing directly with other countries, and their immediate neighbors in particular. This would imply a positive relationship between PTAs signed abroad and at home; on the other hand, if a neighbor signs a PTA with a trade partner relevant to the home country, a "trade policy diversion" effect might be in place: the third country may not be interested in signing another PTA with Home, given it has already stroken a deal with the neighbor. The negative sign on the coefficients for PTAs abroad points to this second option.

Finally, it would be interesting to see whether original settler mortality of former colonies, a proven reliable instrument for institutional quality proposed by Acemoglu, Johnson and Robinson (2001), alters somehow the results. Table 5 provides estimates of equations where the log of European settler mortality is added as an instrument. In Column 1, settler mortality is used together with distance from the equator and foreign trade policies as excluded instrument, dropping the quality of institutions abroad. Structural estimates are almost identical to those provided in Table 2 and instruments remain strong. Column 2 reintroduces the measure of institutions abroad and runs the estimation with four exluded instruments for the two endogenous variables: trade policies and rule of law. Again, structural estimates do not change, the Weak-ID test rejects the null at the highest possible level and the test of my overidentifying restrictions does not reject the null at any significance level. The Shea's partial R-squared are .65 for trade policies and .47 for institutions. In the reduced form equations (not reported), all of the instruments that influence domestic rule of law have the expected signs and are significant, but the quality of institutions abroad shows a higher t-statistics than the other two.

Column 3 mimics the last Column of Table 2, using distance from the equator as included instrument; the difference with respect of Table 2 is the addition of the log of mortality as excluded instrument. Structural estimates are very similar to the previous specifications, with latitude playing to direct role in fostering growth. The positive impact of rule of law is confirmed, as it is the lack of significance of trade policies. As in Table 2, though, instrument quality drops substantially when distance from the equator is included in the main equation: this is due to the high correlation between rule of law in neighbor countries, log of settler mortality and latitude. The Weak-ID test drops below the lowest level and inference should then be treated with caution. The same caution should apply, in general, to all results coming from

instrumentation with the settler mortality variable, given the much smaller sample available and its restriction to former colonies. Overall, though, the findings outlined above are consistent with results shown in the previous Tables. Contagion seems to be an important driver for policies and institutions: what a country's neighbors do influences domestic trade measures and rule of law. Utilizing foreign variables as instruments for domestic ones in growth regressions, the key indication that emerges is that institutions positively affect growth, but trade policies and geography do not have any significant impact.

3.6 Conclusion

This paper shows that there is international contagion both in trade policies and institutions. Foreign trade measures and institutional environment positively affect domestic trade policy and institutions, respectively. Foreign variables have subsequently been employed to instrument domestic trade policies and institutions in growth regressions. Structural estimates show that the quality of domestic institutions, measured by the rule of law index, is a strong driver for growth, but the same does not hold true for trade policies. Using the instruments introduced in this paper, the direct impact of geography on growth has been assessed. The evidence suggest that geography does not exert any direct effect on a country's income level.

These results hold even allowing for potential other channels, such as trade and investment flows or iternational growth, to contemporaneously affect domestic variables. Moreover, the same findings are reported using alternative measures of trade policies and an alternative sample of former colonies, that allows inclusion of European settler mortality as additional instrument.

Reduced form estimates are interesting per se and deserve further attention. It is particularly interesting that a larger number of trade agreements signed by its neighbors drive down the number of PTAs country is involved in. It becomes then important to see whether some structure can be added to the "contagion" spotted in the data. Recent papers (De Benedictis and Tajoli, 2008) have depicted world trade flows as a global network, and similar efforts have been made also for trade agreements (Teteryatnikova, 2008). Future research may try to investigate whether there is a "global network game" behind the complex and wide ties of world trade relations.

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Table 1
Contagion in policies and institutions

	(1)	(2)	(3)	(4)
Dependent	home rule of law	home rule of law	home SW	home SW
SW index in neighboring countries	0.0188	0.0179	0.122***	0.122***
	(0.0268)	(0.0243)	(0.0115)	(0.0113)
distance to equator	0.0339***	0.0280***	0.00163	0.000667
	(0.00699)	(0.00664)	(0.00306)	(0.00317)
log of population	0.0181	0.0337	0.0571**	0.0596**
	(0.0783)	(0.0741)	(0.0265)	(0.0260)
log of land area	-0.130*	-0.0957	-0.0851***	-0.0796***
	(0.0779)	(0.0777)	(0.0242)	(0.0248)
rule of law index in neighboring countries		0.0977***		0.0159
		(0.0258)		(0.0158)
Continent dummies	YES	YES	YES	YES
Constant	0.937	0.429	0.919***	0.836***
	(0.715)	(0.689)	(0.242)	(0.251)
Observations	107	107	107	107
R-squared	0.551	0.591	0.667	0.673

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2
Growth impact of institutions, policies and geography

Dependent variable is log of real GDP per capita in 2001

	(1)	(2)	(3)
Sachs and Warner index	0.00583	0.0114	-0.0241
	(0.180)	(0.185)	(0.197)
Rule of law index	0.940***	0.990***	1.134***
	(0.160)	(0.142)	(0.276)
distance from equator			-0.00654
			(0.0104)
log of population	-0.0206	-0.0188	-0.0224
	(0.0687)	(0.0694)	(0.0694)
log of land area	0.0168	0.0185	0.0395
	(0.0567)	(0.0570)	(0.0595)
Continent dummies	YES	YES	YES
Constant	8.643***	8.535***	8.489***
	(0.538)	(0.510)	(0.540)
Observations	107	107	107
R-squared	0.799	0.794	0.774
Robust standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			
Excluded instruments	SW index abroad, distance from equator	SW index abroad, distance from equator and rule of law abroad	SW index abroad and rule of law abroad

Table 3
Robustness check with respect to trade, investment and growth variables

Dependent variable is log of real GDP per capita in 2001

	(1)	(2)	(3)	(4)	(5)	(6)
SW index	-0.217	-0.199	-0.112	-0.220	-0.176	-0.209
	(0.145)	(0.169)	(0.164)	(0.147)	(0.147)	(0.152)
rule of law index	0.748***	1.069***	0.989***	0.758***	0.658***	0.754***
	(0.173)	(0.133)	(0.132)	(0.197)	(0.194)	(0.225)
log of sum of neighbors' GDP	0.314***			0.306**	0.436***	0.373**
	(0.103)			(0.133)	(0.144)	(0.149)
neighbors' trade share of GDP		0.00107***		0.0000518		0.000696
		(0.000336)		(0.000381)		(0.000484)
neighbors' investment share of GDP			0.00499**		-0.00525*	-0.00750**
			(0.00199)		(0.00287)	(0.00331)
domestic trade share of GDP						0.000256
						(0.00132)
investment share of GDP						0.000996
						(0.0139)
log of population	0.0457	-0.00752	-0.0228	0.0445	0.0750	0.0721
	(0.0619)	(0.0651)	(0.0685)	(0.0616)	(0.0648)	(0.0615)
log of land area	-0.0912*	-0.0331	-0.0206	-0.0907*	-0.0925*	-0.0841
	(0.0531)	(0.0586)	(0.0574)	(0.0530)	(0.0508)	(0.0555)
Continent dummies	YES	YES	YES	YES	YES	YES
Constant	6.379***	8.827***	8.857***	6.451***	5.202***	5.599***
	(0.889)	(0.539)	(0.529)	(1.150)	(1.288)	(1.514)
Observations	107	107	107	107	107	107
R-squared	0.843	0.806	0.806	0.843	0.845	0.850

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Excluded instruments: SW index abroad, rule of law abroad, distance to equator

Table 4
Alternative trade policy measures

Dependent variable is log of real GDP per capita in 2001

	(1)	(2)	(3)	(4)
applied import tariff	4.861	1.415		
	(6.295)	(7.599)		
PTAs signed			-0.101	-0.0536
			(0.0640)	(0.0528)
rule of law index	0.970***	1.015***	1.213***	1.102***
	(0.128)	(0.153)	(0.239)	(0.197)
neighbors' trade share of GDP		0.000934		0.000563
		(0.00112)		(0.000665)
neighbors' investment share of GDP		0.00109		0.00339
		(0.00687)		(0.00435)
log of sum of neighbors' GDP		-0.0355		-0.0945
		(0.0664)		(0.0628)
log of population	-0.0823	-0.0667	-0.0234	-0.0735
	(0.0785)	(0.0714)	(0.0911)	(0.0759)
log of land area	0.0657	0.0141	0.0593	0.0444
	(0.0662)	(0.0727)	(0.0682)	(0.0605)
Continent dummies	YES	YES	YES	YES
Constant	8.623***	9.137***	8.974***	10.00***
	(0.514)	(0.831)	(0.423)	(0.778)
Observations	85	85	130	130
R-squared	0.766	0.783	0.675	0.742
*** p<0.01, ** p<0.05, * p<0.1				
Robust standard errors in parentheses				
Additional excluded instrument (other are rule of law abroad and distance from equator)	tariffs abroad	tariffs abroad	PTAs signed by neighbors	PTAs signed by neighbors

Table 5

Former colonies sample - instrumentation with settler mortaility

	Dependent variable is log of real GDP per capita		
	(1)	(2)	(3)
COEFFICIENT	lrgdpch	lrgdpch	lrgdpch
SW index	-0.192	-0.193	-0.207
	(0.291)	(0.291)	(0.304)
rule of law index	1.043***	1.036***	1.251***
	(0.155)	(0.145)	(0.238)
distance from equator			-0.0141
			(0.0115)
log of population	-0.0740	-0.0745	-0.0529
	(0.0939)	(0.0921)	(0.0894)
log of land area	0.0776	0.0777	0.103
	(0.0671)	(0.0667)	(0.0860)
Continent dummies	Africa, Asia	Africa, Asia	Africa, Asia
Constant	8.725***	8.729***	8.460***
	(0.702)	(0.688)	(0.804)
Observations	52	52	52
R-squared	0.772	0.773	0.729
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			
Additional excluded instruments (other is SW index abroad)	distance from equator, log of settler mortality	distance from equator, log of settler mortality, rule of law abroad	rule of law abroad, log of settler mortality