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Institutions, Trade, and Growth

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Abstract: Countries with better institutions and countries that trade more grow faster. Countries with better institutions also tend to trade more. These three stylized facts have been documented extensively. Here we investigate the partial effects of institutions and trade on growth. We argue that cross-country regressions of the log-level of per capita GDP on instrumented measures of trade and institutional quality are uninformative about the relative importance of trade and institutions in the long run, because of the very high correlation between the latter two variables. In contrast, regressions of changes in decadal growth rates on instrumented changes in trade and changes in institutional quality provide evidence of a strong effect of trade on growth, with a much smaller role for improvements in institutions. These results are suggestive of an important joint role for both trade and institutions in the very long run, but relatively larger role for trade over shorter horizons.

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1. Introduction

Throughout the past two centuries countries that were relatively rich in 1800 have generally grown faster than those that were relatively poor. This growth pattern has resulted in "divergence, big time": that is, a steady increase in the ratio of per capita GDP in rich countries compared to that of poor countries (Pritchett 1997). However, this pattern of growth has begun to change around 1980, after which several of the fastest growing countries in the world have also been poor countries. Figure 1 divides countries into income quintiles based on 1980 per capita GDP, and plots subsequent population-weighted growth rates. Growth in the poorest quintile averaged 4 percent per capita, while growth in the richest quintile (mostly OECD countries) was less than 2 percent. Weighting growth rates by country size is clearly important here, because this result depends primarily on accelerated growth in Asian countries that were large and poor in 1980: Bangladesh, China, India, and Vietnam.

This accelerated growth in poor locations is an important development in the world economy. Because of it two long-term trends have come to an end, at least temporarily, perhaps permanently. First, since changes in the distribution of income among individuals in the world have been driven primarily by differences in growth rates among countries, the steady rise in global inequality between 1800 and 1980 has changed into a modest, although probably not statistically significant decline (Bourguignon and Morrisson (2001), Sala-i-Martin (2002)). Second, the number of extreme poor in the world (living on less than $1 per day at PPP) also peaked around 1980, at an estimated 1.4 billion. While world population has grown by 1.6 billion since 1980, the number of extreme poor declined by 200 million.[1] In 1980 the vast majority of the world's poor lived in the four Asian countries noted above, and that is where the bulk of this poverty reduction has occurred.

Our objective in this paper is to try to shed some light on the relative importance of trade and institutions in driving these patterns of divergence over most of the past 200 years, and also the acceleration of growth in some of the poorest countries in the past 20 years. A growing literature has documented the importance of good institutions that protect property rights for

growth in the very long run (Acemoglu, Johnson and Robinson (2001), Hall and Jones (1999), Engermann and Sokoloff (1997), and many others). This literature has identified the effects of institutions by tracing back their origins to more fundamental determinants such as (i) the incentives of colonial powers to invest in institution-building, as proxied by settler mortality (Acemoglu, Johnson and Robinson (2001)), (ii) colonial origin itself (Hall and Jones (1999)), and (iii) natural resource endowments (Engermann and Sokoloff (1997)). A parallel literature has documented the importance of trade for growth in the very long run (notably Frankel and Romer (1999)). This literature has identified the effects of trade by exploiting the deep geographical determinants of trade – countries that are landlocked and/or remote from major markets tend to trade less than those that are not.

In the first section of this paper, we draw together these two strands of the literature in order to attempt to isolate the partial effects of trade and institutions on growth in the very long run. When we do so, we are confronted by a fundamental identification problem. Countries that trade more are also countries with better institutions. This to a large extent reflects the common origins of both that have been stressed in the literature. Countries in which colonial powers invested in developing strong institutions are also countries that tended to trade more. Countries with geographical characteristics that were conducive to more trade are also countries in which better institutions developed.

This means that when we apply the identification strategy of the existing literature to try to isolate the partial effects of trade and institutions, we are notably unsuccessful. We find that ordinary least squares regressions of current log-levels of per capita income on trade shares and measures of institutional quality deliver economically large and statistically significant coefficients on both variables. However, when we instrument for both trade and institutions using the instruments suggested by the literature discussed above, we find that both sets of instruments have very strong explanatory power for both endogenous variables. As a result, we encounter overwhelming problems of multicollinearity in the second-stage regressions which preclude the estimation of meaningful partial effects of either variable. From this we conclude

[1] Bourguignon and Morrisson (2001) estimate that the number of poor declined by 100 million from 1980 to 1993, when their analysis ends. Chen and Ravallion (2001) estimate that there was a further reduction of 100 million between 1993 and 1998.

that the cross-sectional evidence is not very informative about the relative importance of trade and institutions in the long run, although existing evidence on their individual effects suggests that both are important.[2]

We next turn to the acceleration of growth in poor countries in recent decades, and examine the extent to which it can be attributed to improved institutional quality and greater participation in international trade. We do so by estimating dynamic regressions of decadal *changes* in real per capita GDP growth on lagged growth, and *changes* in decadal averages of trade shares and measures of institutional quality, using lagged levels of institutional quality and trade as instruments to control for possible reverse causation from changes in growth to changes in trade and changes in institutional quality.

Here we do not encounter the same severe identification problems we saw in the cross-section of countries. In the cross-section of countries, we found that trade and institutions are both strongly linked to a common set of historical and geographical determinants. However, when we consider decadal changes in trade and institutions, we find that lagged levels of trade are good instruments for changes in trade but not changes in institutional quality, and similarly, lagged levels of institutional quality predict changes in institutions but not changes in trade. As a result, our instrumented decadal regressions are not plagued by the same multicollinearity problems as our cross-sectional regressions. Using these regressions, we find a strongly significant and economically relevant effect of changes in trade on changes in growth, but only modest evidence of the partial effects of improvements in institutional quality. Of course, we cannot rule out the possibility that even the decadal changes in measures of institutional quality that we rely on are simply too noisy to be informative, or that our instruments for changes in institutional quality are too weak to identify the effects of interest. However, we do find this to constitute evidence of an important role of trade in the shorter run.

[2] In a closely-related paper, Alcalá and Ciccone (2001) estimate cross-sectional regressions of log per capita incomes on trade and measures of institutional quality, and find a strongly significant effect of trade. As discussed further below, there are a number of differences in the variables, time period and sample of countries that they cover relative to our paper. While their finding is consistent with our priors, we are unable to reproduce similar findings in the dataset we consider. We speculate however that the problem of multicollinearity leading to fragile results is likely to be common in specifications similar to ours.

The rest of this paper is organized as follows. Section 2 documents our cross-sectional evidence on trade and institutions in the very long run. Section 3 is a brief digression to motivate our dynamic regressions. Easterly, et. al. (1993) noted that country growth rates from the 1970s to the 1980s were not very persistent, and we show that this remains true from the 1980s to the 1990s. We also show this is true for trade shares and measures of institutional quality, so that the decadal variation in these variables is potentially informative. In Section 4 we address the question of why we use trade volumes rather than a direct measure of trade policy in our analysis. China provides a good example of how trade liberalization actually proceeds in poor countries. In the 1980s China decentralized trade decisions to enterprises, created special trade zones in a variety of locations, opened a foreign exchange market, but only modestly reduced the average official import tariff. This pattern suggests that it is difficult to create a single convincing index of trade policy from such diverse measures. We therefore continue to rely on trade volumes in our empirical work, and note that it is likely that at least some of the variation in trade volumes can be attributed to changes in trade policy, even if we cannot measure the latter very well. Section 5 reports the dynamic regressions, and Section 6 offers some concluding remarks.

2. Institutions, Trade, and Income in a Cross Section of Countries

We start by examining the effects of trade and institutions on per capita income in a large cross-section of countries. We follow several previous papers in regressing the log-level of per capita GDP on measures of institutional quality and market size. Under the assumption that initial incomes in the distant past were not too different across countries, current levels of per capita GDP capture the growth performance of countries in the very long run. By using the historically- and/or geographically-determined component of current institutions and trade, these regressions can therefore be interpreted as capturing the effects of trade and institutions on growth in the very long run.

A number of previous papers have examined the effects of institutional quality on per capita incomes using this framework, including Hall and Jones (1999), Kaufmann, Kraay and Zoido-Lobatón (1999), Acemoglu, Johnson and Robinson (2001), and Easterly and Levine (2002). In addition, Frankel and Romer (1999) use this framework to estimate the effects of market size on growth. These respective authors make a good theoretical case for the importance of both institutional quality and market size, which we will not repeat at length. Property rights and rule of law can obviously effect the incentives to invest and innovate. And market size, determined by either population and/or access to foreign trade, may also affect those incentives, and hence growth.[3]

We measure institutional quality using an index of rule of law constructed by Kaufmann, Kraay, and Zoido-Lobaton (2002), which refers to the period 2000-01. They take a large number of available subjective measures of rule of law and protection of property rights, and combine them into a composite indicator. Their index covers 168 countries and by construction has a mean of zero and a standard deviation of 1. We measure real per capita GDP in 1995 using the Summers and Heston Penn World Tables Version 5.6, extended into the 1990s using constant price local currency growth rates of real per capita GDP. Finally, we measure market size using the logarithm of population and the logarithm of trade as a fraction of GDP. The numerator of

[3] A few of the many other papers that have examined the link between either trade policy or trade volumes and growth over shorter horizons are Ades and Glaeser 1999; Dollar 1992; Edwards 1992; and Sachs and Warner 1995.

this trade share is measured in constant US dollars, and the denominator is measured in constant US dollars at PPP. Under the assumption that traded goods prices are roughly equalized across countries, this gives a PPP-adjusted trade to GDP ratio.[4]

We start with an OLS regression of log per capita GDP on rule of law (Table 1, column 1). The coefficient is 1.01 with a t-statistic of 24.61, indicating a very strong correlation between per capita incomes and institutional quality in this large sample of countries. However, there are number of obvious potential problems with the OLS regression. The first is the endogeneity of rule of law: subjective measures of institutional quality may be subject to "halo effects" : countries are perceived to have good institutions because they are rich. Second, there is likely to be considerable measurement error in our measure of institutional quality. Third, there are surely many omitted variables possibly correlated with both per capita incomes and institutional quality in a very parsimonious regression such as this one.

We follow the previous literature in using instruments to address these problems. In order to preserve as large a sample as possible, we use as instruments the share of the population that speaks English and the share that speaks a major European language (following Hall and Jones (1999)). These instruments are intended to capture the influences of colonial origin on current institutional quality. A more compelling instrument is the measure of settler mortality suggested by Acemoglu, Johnson and Robinson (2001), which unfortunately is available only for a much smaller sample of countries. At the end of this section we show that our conclusions are similar using this instrument in a sample that is roughly half the size of our benchmark sample. In the instrumented regression with the language variables as instruments, rule of law remains highly significant (column 2). This simply verifies the results of several previous papers that institutional quality has important causal effects on per capita income in a large cross-section of countries.

[4] Here we depart from Frankel and Romer (1999) who use the current price local currency trade-GDP ratio reported in the Penn World Tables. Since the ratio of GDP at PPP to GDP at market exchange rates falls as per capita incomes increase, the PPP trade shares we use tend to be more strongly correlated with log-levels of per capita income than the variable used by Frankel and Romer (1999). Alcalá and Ciccone (2001) provide a careful theoretical justification for this PPP-adjusted trade ratio as a measure of trade openness.

In columns 3-4 of Table 1 we verify that the main findings of the Frankel-Romer paper also hold in our sample. Per capita GDP is highly correlated with domestic market size (as measured by log population) and access to foreign markets (as measured by trade as a share of GDP). As with the results on institutional quality, there are good reasons to doubt the simple OLS regressions. There may be reverse causation from per capita incomes to trade. Trade as a fraction of GDP may be an imperfect proxy for access to foreign markets. And again, the possibility of omitted variables correlated with both trade and per capita incomes cannot be ignored. To address these concerns, Frankel and Romer (1999) suggest an instrument consisting of the fitted values of trade predicted by a gravity model. We have reconstructed this instrument for our sample of countries and for our reference year of 1995, and use it to instrument trade in column 4 of Table 1.[5] As in the Frankel-Romer paper, we find a highly-significant positive effect of the geographically-determined component of trade on per capita incomes, with a larger coefficient than in the OLS regression.

We are now ready to examine the partial effects of institutions and trade in a cross-section of countries. Combining these two specifications and estimating by OLS gives significant coefficients with the intuitive signs on all variables (column 5 of Table 1). The coefficients on both rule of law and trade diminish in magnitude, which is not surprising because these variables are highly correlated with a correlation coefficient of 0.63 in this cross-section of countries. However, when we instrument for both rule of law and trade (column 6), the significance of the trade coefficient disappears, while rule of law remains significant. However, the significance of rule of law depends entirely on the inclusion of the four "neo-Europes" in this sample — the United States, Canada, Australia, and New Zealand. When those four countries are dropped, the significance of rule of law vanishes. The importance of these four countries in driving the results for rule of law is visually striking in the partial scatters from the instrumented regression in Column 6, shown in the top and bottom panels of Figure 2.

Before interpreting this finding on the insignificance of trade and rule of law, we first verify that it is robust. In columns 8 and 9 of Table 1, we add measures of geography. This is

[5] In particular, we re-estimate the gravity equation reported in Table 1 of Frankel and Romer (1999), using PPP-adjusted bilateral trade shares for 1995. The results of the gravity equation and the data used to estimate it are available from the authors on request.

because both rule of law and trade, and also their fitted values from the first-stage regressions, are correlated with a number of geographic characteristics such as distance from the equator or landlockedness which might have direct effects on per capita incomes. This criticism has been applied to the trade and growth literature by Rodriguez and Rodrik (2000), and to the institutions and growth literature by McArthur and Sachs (2000). Estimating by OLS, we find that rule of law, trade, and geography are all significant in the intuitive direction. In the IV regression, however, almost everything again becomes insignificant, and the significance of rule of law is again entirely due to the presence of the four neo-Europes (compare columns 9 and 10). We also verify that this result does not depend on the large sample or the choice of instruments. In column 11 we use settler mortality as an instrument, reducing the sample size to 68. Looking only at institutions, we find the same strong causal effect of rule of law on per capita incomes as do Acemoglu, Johnson and Robinson (2001). Once we add the market size variables and instrument for trade shares, however, we again find that everything becomes insignificant.

What is going on here? The basic problem is that the fitted values of rule of law and trade from the first-stage regressions are extremely highly correlated with each other, with a correlation coefficient of 0.72. This is both because the instruments themselves are correlated, and also because both sets of instruments have strong explanatory power for both endogenous variables. Table 2 shows the first-stage regressions corresponding to the IV regressions in Table 1, columns 6 and 9. In the first-stage regressions corresponding to the specification in column 6 of Table 1, both the linguistic origin and fitted trade instruments are highly significant in both first-stage regressions. In the specification in column 9 of Table 1 in which we allowed for direct effects of geography, we also find that distance from the equator strongly predicts both institutions and trade. Yet another way of seeing this problem is to look at a regression of fitted rule of law on fitted trade and geography (in the last column of Table 2). We find an R-squared of 0.96, indicating an almost perfect linear relationship between the right-hand-side variables in the second-stage regression of the instrumental variables procedure. All of this means that in the instrumented regressions of per capita income on institutions and trade, we have very severe multicollinearity which prevents us from identifying the partial effects of either variable on per capita incomes. In other words, the cross-sectional data we use is simply not very informative about the separate roles of trade and institutions.

There are several ways to proceed from this rather negative result. The first is to search for alternative instruments for trade and/or institutions that are more orthogonal in the sense of both being independent of eachother and also having explanatory power only for the endogenous variable to which they are assigned. The second is to search for alternative measures of institutional quality that are not significantly associated with the instruments for trade. A third possibility is to exploit the time series variation in trade, growth and institutions, rather than the cross-sectional variation on which we have relied so far. We are not very sanguine about the prospects for success in the first strategy as good instruments are hard to find. In the remainder of this section we follow the second strategy of considering alternative measures of institutional quality, and in the rest of the paper we consider the third strategy.

It is possible that other measures of institutional quality may not pose the same multicollinearity problems as the rule of law measure we have used so far. We therefore consider five other measures of institutional quality, which also exhibit some variation over time and which we will use in the final section of this paper when we turn to the dynamic regression evidence. Table 3 shows the IV regression when the rule of law index is replaced with other measures of institutional quality. In each case we instrument for trade and institutions, using the linguistic origin and fitted trade instruments, and include but do not report the coefficients on distance from the equator and landlockedness. The institutional measure in the first column is 1 minus the ratio of currency in circulation to M2. This variable, coined as "contract-intensive money" (CIM) by Clague, Keefer, Knack and Olson (1999), measures the extent to which property rights are sufficiently secure that individuals are willing to hold liquid assets via financial intermediaries. These authors document a strong positive cross-country relationship between this variable and both investment and growth. Column 2 includes the average number of revolutions per decade since the 1960s. Column 3 has a measure of rule of law reported in the International Country Risk Guide (ICRG). Column 4 uses the Freedom House measure of political freedom (higher values indicate less freedom). Finally column 5 uses the fraction of the population killed in wars. The justification for this is that the capacity of the state to protect property rights is severely undermined in periods of conflict. We average each of these variables over all available years since 1950.

The main message of Table 3 is that we have the same problems of multicollinearity with these other measures. This is most apparent in the bottom row of Table 3, where we report the correlation between fitted trade and fitted institutional quality. In all cases we find that the correlations are large in absolute value. We do find some evidence of the modest significance of trade but not institutions (in the specifications with revolutions and war casualties as objective proxies for institutional quality), and we also find some evidence of the significance of institutions but not trade (in the specifications with the ICRG and Freedom House ratings as subjective proxies for institutional quality). However, we stress that these results are not very robust, largely due to the serious multicollinearity problem we encounter. When we drop the four neo-Europes from the sample, we find that all but one of these few significant coefficients become insignificant, with only the ICRG rating entering significantly.[6]

We conclude from these cross-sectional results that it is difficult to separately identify the effects of institutions, trade, and geography. We find that rule of law is significant in our baseline instrumented regression, but this depends on four countries — the U.S., Canada, Australia, and New Zealand. Dropping these countries, and also for virtually all other measures of institutional quality we consider, we find that both institutions and trade are insignificant in a cross-section of countries, due to the severe multicollinearity problems we have documented. In short, while we find the cross-section revealing about the *joint* significance of institutions and trade, we are pessimistic that this approach can distinguish the *partial* effects of institutions and trade.

[6] Results not reported for reasons of space.

3. Persistence of Institutions, Trade, and Growth

Since institutions, trade, and geography are so collinear in the cross-section, a natural next step is to consider the relationship between changes over time (in our case, decade-to-decade changes) in these variables. Before proceeding to dynamic regressions, it is useful to document the persistence of the different variables in our analysis. If all of them are highly persistent, then the dynamic analysis is not going to add much. The geographic and historical characteristics of countries obviously do not change over time. However, we show that the other variables of interest – growth, trade, and institutional quality – do exhibit a surprising amount of variability over time. This suggests that the within-country variation in trade, institutions, and growth may be useful in shedding light on the relative importance of these factors.

Decadal growth exhibits surprisingly low persistence. Easterly, et. al. (1993) made this point with respect to the 1970s and 1980s, and it remains true for the 1980s and 1990s. The correlation among decadal growth rates is 0.37 for the 1970s and 1980s, 0.39 for the 1980s and 1990s, and 0.29 for the 1970s and 1990s. The top panel of Figure 4 shows the latter relationship and illustrates a point made in the introduction. A majority of countries are below the 45° line (slower growth in the 1990s than the 1970s). However, the group above the line is an interesting cluster of countries, including China, Thailand, India, and Bangladesh in Asia; Uganda, Mozambique, and Ethiopia in Africa; and Chile and Argentina in Latin America. (Vietnam, with growth above 6% in the 1990s is almost certainly in this group, but there is no comparable data on growth in the 1970s.) Because of the presence of these large Asian developing countries, the vast majority of the world's poor live in these countries whose growth rates have accelerated and were well above OECD growth rates in the 1990s. More generally, the large number of growth accelerations and decelerations from decade to decade suggests that a dynamic analysis could provide insights that we do not get from the cross-section.[7]

[7] Pritchett (2000) also points out this high volatility of growth rates in developing countries. He makes a good argument that high-frequency econometrics are not likely to provide much insight into determinants of long-term growth. Our approach is something of an intermediate strategy between cross-sectional and high frequency growth studies, in that we rely on decadal average growth rates.

Trade as a fraction of GDP is more persistent than growth, but still exhibits substantial variation over decades. The bottom panel of Figure 4 shows this. There are countries such as China that have seen a more than a 100% increase, and other developing countries have seen significant trade growth as well. However, many developing countries trade less of their GDP today than 20 years ago. The trade is measured at constant prices so that terms of trade changes do not have a direct effect on these measures, though of course they can have an indirect effect if there are supply responses to them.

What about measures of institutional quality? Figure 5 plots each of the five measures of institutional quality in the 1990s against its corresponding value in the 1970s or 1980s, depending on data availability. This figure shows what to us is a surprisingly high degree of variability in each of these measures over time. One interpretation of this is that true institutional change occurs only slowly, and our proxies are very noisy indicators. While there is certainly considerable truth to this view, at least some of this variation is likely to also reflect true underlying changes in institutional quality, often in the form of sudden dramatic changes. This is most evident in the objective indicators such as revolutions and war casualties. For example, the dramatic decline in political instability in countries like Argentina and Bolivia in the 1990s relative to the 1970s, and the dramatic worsening in countries as diverse as Liberia, Afghanistan and Yugoslavia over the same period is visually striking in the second panel of Figure 5. This suggests that there is sufficient variation in measured institutional quality within countries over time that dynamic regressions of changes in growth on changes in trade and changes in institutions are potentially informative about the partial effects of both these variables.

4. Trade Volumes or Trade Policies?

A final issue before turning to the dynamic regressions concerns the interpretation of changes over time in trade as a fraction of GDP. We noted earlier that our cross-sectional results were based on variation in the geographically-determined component of trade. Consequently, the results on the role of trade in growth in the very long run are not informative about the role of trade *policy* for growth.[8] Here we discuss the extent to which we can interpret changes over time in trade shares as a very imperfect proxy for changes in trade policy.

In this context, a natural question is: why not use direct measures of trade policy? We have explored this route but are unpersuaded that it is possible to construct very convincing measures of overall trade policy. The most immediate candidates (average tariff rates or non-tariff barrier coverage ratios) have obvious drawbacks. If one uses simple averages (across goods) of tariff rates, it is possible to give inordinate weight to categories of goods that are relatively unimportant for a country. If, on the other hand, one averages weighting by imports, then the effects of prohibitive tariffs which choke out all imports are lost. In the case of non-tariff barriers, the best available data simply report the number of tariff lines on which one of a small number of easily identifiable NTBs is in force. This type of data again provides no information on how binding the NTBs are, and obviously excludes a wide range of less-easily quantifiable barriers to trade, such as local procurement requirements. And for all measures, there are important gaps between statutory rates and actually collected tariffs, due to both legal exemptions as well as poor enforcement of tariffs or outright corruption in the customs administration. These types of concerns have led some to despair of the possibility of measuring trade policy at all (see, for example, Pritchett (1996)).

A further concern is that these measures have little correlation with observed trade volumes. There is almost no relationship, for example, between reductions in reported average tariff rates and changes in trade volumes (Figure 6). Bangladesh, India, and Brazil had large reported declines in tariffs and large increases in trade over the past two decades. But there are

[8] This point is stressed by Frankel and Romer (1999) and is reiterated in Rodriguez and Rodrik (2000) who also provide a detailed critique of several of the measures of trade policy that have been used in the literature.

other countries with significant reported tariff declines and decreases in trade (Pakistan, Kenya). And there are countries with huge increases in trade but only moderate reported tariff reductions (China, Mexico).

We interpret Figure 6 as illustrative of the weaknesses of available direct measures of trade policy. It is not hard to find country-specific examples of cases where measures such as average tariffs are a very poor indicator of trade policy. In the cases of Mexico and China one can point to specific administrative changes that significantly liberalized trade. The growth of Mexico's trade has been spectacular in the 1990s, which is clearly related to the reduction in a wide range of administrative barriers to trade as part of its free-trade agreement with Canada and the U.S. Yet measures of average tariffs show little change throughout the 1990s for Mexico. In China's case, it began its economic reform in 1978 with trade completely monopolized by a single ministry. The government began to liberalize trade in 1979. It decentralized trade decisions to enterprises and established special zones, some of which were very large (e.g., the whole city of Shanghai), within which foreign firms were allowed to invest and to directly import and export. Duties on imported equipment and materials were low for exporting firms (Lardy 1992). Emerging from the planned economy, a dual exchange system was a disincentive to export, but the problem was reduced with the introduction of a foreign currency market in 1986 (Naughton 1996). The point here is that, as China took a number of discrete administrative steps toward liberalization of foreign trade and direct investment between 1979 and 1986, there was an extremely rapid increase in trade (Eckaus 1997). These administrative measures were followed in the 1990s by reductions in the reported average tariff rate. China's case illustrates the point that it is difficult to find a single measure that captures trade policy. The fastest growth of China's trade occurred when it was removing administrative barriers, not when it was cutting tariffs (Figure 7).

From a policy perspective, ideally we would like to relate changes in country growth performance to changes in a wide range of policy variables, including measures of trade policy. However, the previous discussion suggests to us that existing measures of trade policy are unlikely to be very informative about actual policy changes. In the absence of such measures, we continue to rely on trade as a fraction of GDP as our measure of openness when we turn to

the dynamic regressions, and we believe that we can at least cautiously ascribe some of the growth effects of trade to underlying trade liberalizing policies that countries have undertaken.

5. Institutions, Trade, and Growth in Dynamic Regressions

In Section 2 we estimated cross-sectional regressions of growth in the very long run on institutions and trade, and found that it was difficult to separately identify the effects of institutions and trade. A natural response to this problem is to shift to a dynamic framework relating changes in growth rates within countries over time to changes in variables of interest. To the extent that (properly-instrumented) changes in trade and changes in institutional quality are less correlated than the corresponding levels of these variables, dynamic regressions may be more informative about the partial effects of trade and institutions on growth. Moreover, by focusing on changes in growth rates over relatively short periods, we are able to abstract from many of the unobserved factors that explain cross-country differences in growth rates but do not themselves vary much over time.

Consider the following "standard" cross-country growth regression:

$$(1) \qquad y_{ct} = \beta_0 + \beta_1 \cdot y_{c,t-k} + \beta_2' X_{ct} + \eta_c + \gamma_t + v_{ct}$$

where y_{ct} is log-level of per capita GDP in country c at time t, $y_{c,t-k}$ is its lag k years ago (k=10 years in our application using decadal data) and X_{ct} is a set of control variables which are measured as averages over the decade between t-k and t. We will consider trade volumes and measures of institutional quality among the variables in X. Subtracting lagged income from both sides of the equation gives the more conventional formulation in which the dependent variable is growth, regressed on initial income and a set of control variables. The disturbance term in the regression consists of an unobserved country effect that is constant over time, η_c, an unobserved period effect that is common across countries, γ_t, and a component that varies across both countries and years which we assume to be uncorrelated over time, v_{ct}.

Here we adopt the approach of Caselli, Esquivel and Lefort (1996), which is to estimate equation (1) in differences, using appropriate lags of the right-hand side variables as instruments. In particular, they advocate estimating the following regression:

$$(2) \qquad y_{ct} - y_{c,t-k} = \beta_1 \cdot \left(y_{c,t-k} - y_{c,t-2k}\right) + \beta_2' \left(X_{ct} - X_{c,t-k}\right) + \left(\gamma_t - \gamma_{t-k}\right) + \left(v_{ct} - v_{c,t-k}\right)$$

This is nothing more than a regression of growth on lagged growth, and on changes in the set of explanatory variables. Or, subtracting lagged growth from both sides of the equation, we have changes in growth from one decade to the next as a function of initial growth and changes in the explanatory variables.[9]

This approach has several desirable features for us, and in particular helps us address problems of measurement error, omitted variables, and endogeneity:

- *Measurement Error.* In the previous sections we have discussed how changes in trade shares and changes in measures of institutional quality are likely to be noisy indicators of true underlying changes in trade policy and institutional reforms. Also, errors in the measurement of GDP will tend to bias downwards the OLS estimates of the relationship between trade and growth: a positive error in the measurement of GDP in one period provides an overestimate of growth in that period and a simultaneous underestimate of the ratio of trade to GDP for that period.
- *Omitted Variables.* Many of the possible omitted variables in a growth regression that may be correlated with trade or institutional quality, such as a country's ethnic makeup, its colonial history, and its geographic characteristics change very little over time. By differencing we can at least be sure that the estimated coefficient on trade is not simply picking up a correlation with these omitted time-invariant country characteristics. Also, since we include time effects in the regression, these will pick up omitted shocks that affect trade and growth in all countries similarly, such as increases in world demand or widespread declines in transport costs. Of course, there are other determinants of growth that may change over time, and we will attempt to control for them in what follows. While we can of course never be entirely sure that we have adequately controlled for all omitted factors, we have arguably removed the effects of an important class of time-invariant omitted factors and time-varying factors that are common across countries.

[9] Elaborations of this technique involve jointly estimating a system of two equations, in levels (Equation 1) and in differences (Equation 2), and using lagged changes of endogenous variables as instruments for levels in the former (Arellano and Bover (1995)). This approach can yield important efficiency gains (Blundell and Bond (1998)) but is less appropriate in our application where we want to identify the effects of interest using within country *changes* in growth.

- *Endogeneity*. A further advantage of this differenced growth equation is that it presents a natural set of internal instruments to control for the possible problem of reverse causation from growth to trade, and from growth to institutions. There are a variety of plausible reasons to be concerned about endogeneity. If, for example, an economy enjoys a surge in growth, more firms may attain the size necessary to break into export markets, so that exports also increase. At the microeconomic level, there is convincing evidence of reverse causation in the sense that much of the observed correlation between firm performance and exports is driven by larger and more productive firms self-selecting into export markets (see for example Clerides, Lach and Tybout (1997)). Similarly, growth collapses may undermine the quality of institutions, or alternatively countries may be better able to invest in institutional infrastructure during periods of rapid growth.

Our identifying assumption is that while trade volumes and institutional quality may be correlated with the contemporaneous and lagged shocks to GDP growth ($E[X_{ct} \cdot v_{c,t-s}] \neq 0$ for $s \geq 0$), they are uncorrelated with future shocks to GDP growth, ($E[X_{ct} \cdot v_{c,t+s}] = 0$ for $s > 0$). In practice, this means that when we regress growth in the 1990s on growth in the 1980s and the change in trade volumes and the change in institutions between the 1980s and 1990s, we can use the level of trade volumes and the level of institutions in 1980 as instruments, and similarly for growth in the 1980s and the 1970s.[10]

Table 4 presents our results using this technique. We start with an unbalanced panel of 274 observations on growth in the 1990s, 1980s and 1970s for roughly 100 countries. The dependent variable is average annual growth, and the explanatory variables are average annual growth in the previous decade and average annual change in trade volumes (we have in effect divided Equation (2) by k=10 years before estimation). As a result, the coefficient on the trade variable can be interpreted as the cumulative percentage change in the level of per capita GDP over a decade of a 100 percent increase in the trade share.

In the first column we present the results simply estimating Equation (2) by ordinary least squares. While this estimation method is inconsistent, it is a helpful way of summarizing the partial correlations in the data. The striking feature of this first column is that changes in trade volumes are strongly correlated with changes in growth, with an estimated coefficient of 0.18 and a t-statistic well above 3. Of more interest are the results in the second column, where we instrument for initial income and trade volumes as described above. The coefficient on trade jumps to 0.25 and remains highly significant. The point estimate indicates that a 100 percent increase in the trade share would have the cumulative effect of raising incomes by 25 percent over a decade, or an increase of 2.5 percent per year growth. Figure 8 reports the partial scatter between trade and growth corresponding to this instrumented regression, and verifies that the results are not driven by any obvious outliers in the data.

It is worth reiterating that these estimates reflect the effect of *changes* in trade on *changes* in growth. As a result, they do not reflect the effect of geography-induced differences in trade, nor are they tainted by the omission of any variables that matter for growth but change little over time. Our instrumentation strategy also address the possibility of reverse causation from growth to trade. Furthermore, as long as any time-varying omitted variables are uncorrelated with the level of trade openness at the beginning of the previous decade, our instrumented coefficients will not reflect the spurious omission of these variables. Finally, it is worth noting that the coefficient on lagged growth, 0.73, is both highly significant and of a plausible magnitude consistent with slow conditional convergence.

We next turn to the partial effects of trade and institutions in this dynamic framework. In the remaining columns of Table 4 we introduce each of the five time-varying measures of institutional quality discussed above, and instrument for each using the same lags as with the trade variable. With contract-intensive money (columns 3-4), both trade and this institutional measure are significant in the OLS, but only trade is significant in the instrumented regression. There is a qualitatively similar result with revolutions (columns 5-6) and war deaths (columns

[10] We also instrument for lagged growth using the level of income in the 1970s, as is necessitated by the combination of a dynamic panel and unobserved country-specific effects. See Caselli, Esquivel and Lefort (1996) for details. We have also produced results using the average values of trade and institutions in the previous decade as instruments. The drawback of this is that it dramatically reduces the size of our sample, since we lack trade and institutions data for the 1950s to use as instruments for changes in these variables from the 1960s to the 1970s.

11-12): in the instrumented regressions, trade is significant, but not the institutional measure. As has been found in other work, democracy does not have much relationship to growth in either the OLS or the IV (columns 7-8), and the trade result remains quite robust. In fact, the only case in which we do not find a significant effect of trade on growth is when we include the ICRG rule of law measure and instrument (column 10). Here there are only 79 observations, and none of the variables in the IV regression has a t-statistic above 0.21. It is also striking that the estimated coefficient on the trade variable is quite stable across specifications, ranging from 0.16 to 0.38.

The first-stage regressions for change in trade perform reasonably well, as shown in Table 5. (We have labeled the columns of Table 5 with the column numbers from the corresponding IV regressions in Table 4, for ease of comparison). There is a very strong negative correlation between initial levels of trade and subsequent growth in trade. This negative correlation is consistent with the interpretation that many developing countries had suppressed trade volumes through protectionist measures in the 1960s and 1970s, and that significant numbers of countries have liberalized trade and seen large increases in trade since then. When we add changes in institutional quality to the regressions, we augment the instrument set with the decade-initial levels of these variables. The results of these first-stage regressions are reported in the remaining columns of Table 5 (we have labeled the columns of Table 5 with the column numbers from the corresponding IV regressions in Table 4, for ease of comparison). Including the five institutional measures one at a time does not have much effect on the first-stage regressions for trade. With the exception of Revolutions, the initial values of the institutional variables do not enter significantly. Note the marked contrast with Table 3, where we found that the instruments for institutional quality had strong explanatory power for trade and vice versa.

The first-stage regressions for the institutional measures are also interesting (Table 6). These regressions relate the change in institutions (say, between the 80s and 90s) to the level in 1980 of the institutional measure, trade, and per capita income. For all five institutional measures, there is a strongly significant negative coefficient on decade-initial institutional quality, indicating that these instruments have explanatory power for changes in institutional quality. The important point however is that lagged levels of trade as a fraction of GDP have very little explanatory power for changes in institutional quality, just as in Table 5 where we

found that lagged levels of institutions had little explanatory power for changes in trade. As a result, our within-country dynamic regressions do not suffer from the same problem of excessive multicollinearity that we saw in the cross-sectional regressions.

One unsatisfying feature of the results in Table 4 is that the IV estimates of the coefficients on the institutional variables behave rather peculiarly, in several cases changing sign or fluctuating wildly in magnitude relative to the corresponding OLS estimates. One possibility, despite the results in Table 6, is that the decade-initial values of the institutional variables are not sufficiently strong for the IV estimates to be meaningful. One way to address this concern is to re-estimate the regressions in Table 5 treating the change in institutional quality as exogenous (results not reported for reasons of space). In this alternative specification, we continue to find a robust relationship between changes in trade and changes in growth. All of the institutional variables entered with the same sign and similar magnitude as the corresponding OLS estimates. However, with the exception of war casualties, none were significant at conventional levels.

In this alternative specification the first-stage regression for changes in trade includes change in the institutional quality measure as a regressor, rather than the initial level of the measure. Although this it is not the focus of this paper, these regressions are interesting as the shed some light on the argument of Rodrik (2000) that increases in trade may reflect improvements in institutional quality. According to this argument, improvements in institutional quality make countries more attractive as trading partners, and also have direct effects on growth. This argument is neither implausible, nor is it inconsistent with trade also having a direct effect on growth. The first-stage regressions treating changes in institutions as exogenous generally show a positive correlation between changes in trade and changes in institutional quality, but one that explains very little of the variation in trade volumes. Most important for our purposes, the instrumented growth of trade has a very strong relationship to growth accelerations, even after controlling for changes in institutional quality (treating the latter as either endogenous or exogenous).[11]

[11] In both the cross-section and dynamic regressions we explored the intuitive idea that institutions and market size may interact. In OLS regressions we found a positive coefficient on the interaction of institutions and trade. However, it was not robust in instrumented regressions, which may simply reflect that we do have enough different cases to pick up such a subtle relationship.

We now proceed to a number of other robustness checks. First, we include in the analysis proxies for changes over time in the quality of the macroeconomic policy environment: changes in inflation, government consumption, and the black market premium. In the very long run, one can certainly view macroeconomic policies as endogenous and reflective of the underlying quality of institutions. However, in the shorter run these macroeconomic policies show considerable variability that may not be fully explained by changes in institutional quality. When we include the three policy variables and treat them as exogenous, in general they are not significant. But what is important for our purposes is that they do not alter the relationship between trade and growth. In Table 7 we repeat the IV regressions with each of the five institutional quality measures with the policy variables included (but not reported). The coefficient on trade is quite stable and is significant in all but the regression with CIM, while none of the institutional quality variables has any explanatory power.[12]

A different kind of robustness check is reported in Table 8. In our discussion of China we pointed out that some of the specific trade liberalization measures were closely related to liberalization of foreign direct investment. Nearly half of China's exports today come from foreign-invested firms. So, we want to consider whether it is FDI more than trade that is responsible for growth accelerations in the developing world. In Column 1 we add the ratio of FDI to GDP to the dynamic OLS regression. The coefficient on trade declines in magnitude, and the coefficients on both trade and FDI are significant. In the IV regression (Column 2), however, the coefficient on FDI turns negative, and the coefficient on trade is no longer significant. Because of data limitations these regressions with FDI have only 168 observations, which makes it difficult to tease out the different effects of trade and foreign investment. In the IV regression predicted trade and predicted FDI are correlated 0.40, indicating again that foreign trade and foreign investment tend to go hand-in-hand.

It is also interesting to inquire whether we get similar results with investment relative to GDP, as we get with trade. Even though we have tried to control for changes in institutional

[12] We also experimented with treating the policy variables as endogenous, using decade-initial values as instruments. In this case the instrumented regressions performed very poorly with large increases in the standard errors. This is suggestive of the same problems of multicollinearity in the second stage regressions that we saw in the first section of the paper.

quality and other policy changes, it is possible that there are unobserved domestic policy changes or other shocks that lead to both higher investment and more trade as an endogenous result. In Column 3 we include the change in the investment rate in the OLS regression and find that both trade and investment have significant correlations with growth. However, when we instrument for the three variables in the regression (column 4), using twice-lagged levels of each variable, only trade is positive and significant, with a coefficient of .26, similar to the earlier point estimates. Investment has a negative and insignificant coefficient, which is consistent with the view that investment is an endogenous response to policy and shocks and not an exogenous source of growth.

6. Conclusions

In a large cross-section of countries, we find that rapid growth in the very long run, high levels of trade, and good institutions go together. However, since both greater participation in international trade and better institutional quality can be traced back to common geographical and historical factors, it is difficult to disentangle the partial causal effects of institutions and trade separately, using these factors as instruments. This suggests to us that both trade and institutions are important in understanding cross-country differences in growth rates in the very long run, but the available cross-country variation is not very informative about the relative importance of each.

Over shorter horizons, decadal growth rates exhibit remarkably little persistence, and trade shares as well as measures of institutional quality vary substantially as well. In contrast with the cross-sectional results, we find a substantial partial effect of changes in trade shares in predicting changes in growth rates, while changes in measures of institutions play a smaller role. These results are suggestive of an important joint role for both trade and institutions in the very long run, but relatively larger role for trade in the shorter run.

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Table 1. Income Regressions with Institutions and Trade

Dependent Variable: Ln (Per Capita GDP at PPP) in 1995

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*
	OLS	***IV***	***OLS***	***IV***	***OLS***	***IV***	***IV***	***OLS***	***IV***	***IV***	***IV***	***IV***
Rule of Law	1.01	1.30			0.79	1.26	3.52	0.66	1.03	2.64	1.53	2.14
	(24.61)	(9.57)			(10.40)	(3.65)	(1.18)	(7.98)	(2.87)	(1.23)	(8.42)	(0.79)
Ln (Trade/GDP)			1.09	1.67	0.41	0.18	-3.40	0.39	0.79	-1.67		-1.37
			(12.40)	(4.41)	(3.91)	(0.31)	(0.71)	(3.73)	(1.05)	(0.47)		(0.23)
Landlock								-0.45	-0.19	0.14		
								(4.30)	(1.09)	(0.25)		
Distance from Equator								0.0082	-0.01	-0.01		
								(2.34)	(1.47)	(0.58)		
Ln(Population)			0.23	0.35	0.16	0.13	-0.53	0.12	0.26	-0.18		-0.25
			(5.84)	(4.04)	(4.61)	(1.09)	(0.56)	(3.51)	(1.53)	(0.26)		(0.17)
R^2	0.69		0.48		0.73			0.76				
# Observations:	154	153	144	144	134	134	130	134	134	130	68	63
Instruments:												
Engfrac		X				X	X		X	X		
Eurfrac		X				X	X		X	X		
Predicted Trade				X		X	X		X	X		X
Settler Mortality											X	X
Omitted Observations:							USA CAN AUS NZL			USA CAN AUS NZL		

Note: All regressions include a constant (not reported). Absolute value of t-statistics calculated with White-corrected standard errors are in parentheses.

Table 2. First-Stage Regressions

Dependent Variable:	*Rule of Law*		*Ln (Trade/GDP)*		*Fitted Rule of Law*
	(1)	(2)	(3)	(4)	(5)
Ln (Population)	0.07 (1.24)	-0.05 (1.21)	-0.15 (3.67)	-0.20 (4.77)	0.32 (14.51)
Engfrac	0.94 (2.24)	0.62 (2.43)	0.37 (1.84)	0.22 (1.45)	
Eurfrac	0.69 (3.10)	0.59 (3.29)	0.36 (2.40)	0.35 (2.74)	
Predicted Trade	0.64 (5.73)	0.14 (1.05)	0.47 (4.24)	0.22 (1.63)	
Landlock		-0.25 (1.45)		-0.03 (0.15)	-0.21 (6.12)
Distance from Equator		0.03 (7.98)		0.02 (5.63)	-0.003 (1.62)
Fitted Ln (Trade/GDP) from first-stage regression in (4)					1.68 (20.53)
R^2	0.30	0.55	0.34	0.45	0.96
# Observations	134	134	134	134	134

Note: All regressions include a constant (not reported). Absolute valure of t-statistics calculated with White-corrected standard errors are in parentheses.

Table 3. Other Measures of Institutions

Dependent variable: Ln (GDP per capita)

	(1) *IV*	*(2)* *IV*	*(3)* *IV*	*(4)* *IV*	*(5)* *IV*
Ln (Trade/GDP)	0.95	2.42	0.25	1.23	3.25
	(0.29)	(1.80)	(0.84)	(1.15)	(1.74)
Ln (Population)	0.24	0.60	0.15	0.32	0.69
	(0.32)	(2.17)	(0.83)	(1.36)	(1.89)
Contract-intensive money	6.26				
	(1.37)				
Revolutions		-2.82			
		(0.67)			
ICRG			1.29		
			(1.92)		
Freedom House				-0.87	
				(2.00)	
War deaths					463
					(0.41)
# Obs.	121	138	103	139	141
Corr(Fitted Trade, Fitted Institutions)	0.56	-0.86	0.77	-0.74	-0.82

Note: Engfrac, Eurfrac and Predicted Trade are used as instruments in all regressions. All regressions include a constant, landlock and distance from equator as explanatory variables and in the instrument set. Absolute value of t-statistics calculated with White-corrected standard errors are in parentheses.

Table 4. Dynamic Regressions with Institutions and Trade

Dependent Variable: Decadal average real per capita GDP growth

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*
	OLS	***IV***	***OLS***	***IV***	***OLS***	***IV***	***OLS***	***IV***	***OLS***	***IV***	***OLS***	***IV***
Average real per capita GDP growth in previous decade	0.31 (5.44)	0.73 (3.30)	0.28 (4.37)	0.64 (0.88)	0.29 (4.78)	0.89 (2.34)	0.36 (5.42)	0.97 (2.58)	0.37 (4.56)	3.31 (0.21)	0.32 (5.55)	0.73 (3.15)
Change over Previous Decade in Average:												
Ln(Trade/GDP)	0.18 (3.86)	0.25 (2.66)	0.17 (3.54)	0.23 (2.20)	0.18 (3.68)	0.29 (2.69)	0.19 (3.77)	0.38 (3.14)	0.08 (1.08)	0.16 (0.12)	0.19 (4.02)	0.21 (2.15)
Contract-intensive money			0.53 (2.41)	-0.17 (0.12)								
Revolutions					-0.08 (1.63)	0.18 (0.80)						
Freedom House Rating							0.02 (0.87)	0.0003 (0.002)				
ICRG									0.05 (2.26)	0.56 (0.20)		
War Deaths											-17.6 (2.45)	25.2 (0.54)
# Obs.	274	274	193	193	243	243	189	189	79	79	264	264
Decades	70s, 80s, 90s	70s, 80s, 90s	70s, 80s, 90s	70s, 80s, 90s	70s, 80s, 90s	70s, 80s, 90s	80s, 90s	80s, 90s	90s	90s	70s, 80s, 90s	70s, 80s, 90s

Notes: All regressions include decadal dummies (coefficients not reported). Lagged growth is instrumented using the initial log-level of per capita income at the beginning of the previous decade. Instruments for changes in each of the other right-hand-side variables are the levels of the same variable at the beginning of the previous decade. Absolute value of t-statistics computed with heteroskedasticity- and first-order serial correlation-consistent standard errors in parentheses.

Table 5. First-Stage Regressions for Trade

Dependent variable: Decadal change in ln(Trade/GDP)

Table 5, Column:	***(2)***	***(4)***	***(6)***	***(8)***	***(10)***	***(12)***
Level at beginning of previous decade of:						
Ln(Per Capita GDP)	0.012 (7.51)	0.010 (3.82)	0.012 (7.29)	0.013 (6.13)	0.009 (2.07)	0.012 (7.36)
Ln (Trade/GDP)	-0.014 (8.19)	-0.019 (8.02)	-0.016 (8.09)	-0.016 (7.77)	-0.017 (5.12)	-0.015 (7.96)
CIM		0.02 (1.51)				
Revolutions/Coups			-0.008 (2.72)			
Freedom House				0.0003 (0.13)		
ICRG Rule of Law					0.002 (0.79)	
War Deaths						0.10 (0.31)
R^2	0.27	0.35	0.31	0.33	0.28	0.27
# Obs	274	193	243	189	79	264
Decades	70s, 80s, 90s	70s, 80s, 90s	70s, 80s, 90s	80s, 90s	90s	70s, 80s, 90s

Note: All regressions include decadal dummies (coefficients not reported). Absolute value of t-statistics computed with heteroskedasticity-consistent standard errors in parentheses.

Table 6. First-Stage Regressions for Institutional Measures

Dependent Variable is Decadal Change in:	**CIM**	**REV**	**War Deaths**	**Freedom**	**ICRG**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Level at beginning of previous decade of:					
GDP Per Capita	0.002 (2.96)	-0.004 (2.07)	-0.44 E-6 (0.04)	-0.013 (3.23)	0.05 (4.01)
Ln (Trade/GDP)	-0.0007 (1.31)	-0.002 (0.93)	0.92 E-5 (0.75)	0.20 E-3 (0.05)	-0.005 (0.54)
CIM	-0.02 (6.47)				
Revolutions		-0.016 (4.38)			
War deaths			-0.017 (7.46)		
Freedom				-0.03 (6.78)	
ICRG					-0.05 (7.64)
R^2	0.28	0.10	0.20	0.21	0.51
# Obs	193	243	264	189	79

Note: All regressions include decadal dummies (coefficients not reported). Absolute value of t-statistics computed with heteroskedasticity-consistent standard errors in parentheses.

Table 7. Dynamic Regressions Including Policies

Dependent Variable: Decadal average real per capita GDP growth

	(1) *IV*	*(2)* *IV*	*(3)* *IV*	*(4)* *IV*	*(5)* *IV*
Average real per capita GDP growth in previous decade	0.17 (0.04)	0.78 (1.51)	0.61 (2.44)	0.82 (1.63)	0.64 (0.21)
Change over Previous Decade in Average:					
Ln (Trade/GDP)	0.34 (0.32)	0.25 (1.83)	0.23 (2.24)	0.33 (2.22)	0.43 (2.63)
CIM	-1.03 (0.15)				
Revolutions		0.21 (0.85)			
War deaths			-3.58 (0.16)		
Freedom				0.03 (0.28)	
ICRG					0.04 (0.12)
# Obs	178	222	235	172	73

Note: All regressions include decadal dummies and changes in Ln (1 + inflation), government consumption relative to GDP, and black market premium (coefficients not reported). Absolute value of t-statistics computed with heteroskedasticity-consistent standard errors in parentheses.

Table 8. Dynamic Regressions with Investment and FDI

Dependent Variable: Decadal average real per capita GDP growth

	(1) ***OLS***	*(2)* ***IV***	*(3)* ***OLS***	*(4)* ***IV***
Average real per capita GDP growth in previous decade	0.318 (1.86)	1.03 (1.04)	0.296 (5.25)	0.458 (0.56)
Change over Previous Decade in Average:				
Trade Volume	0.140 (1.93)	0.241 (1.00)	0.151 (3.22)	0.259 (2.33)
FDI/GDP	4.38 (1.99)	-4.05 (0.24)		
Investment/GDP			0.885 (2.96)	-1.69 (0.43)
# Obs	168	168	273	273

Note: All regressions include decadal dummies (coefficients not reported). Absolute value of t-statistics computed with heteroskedasticity-consistent standard errors in parentheses.

Figure 1. Growth rates of rich and poor countries (population weighted)

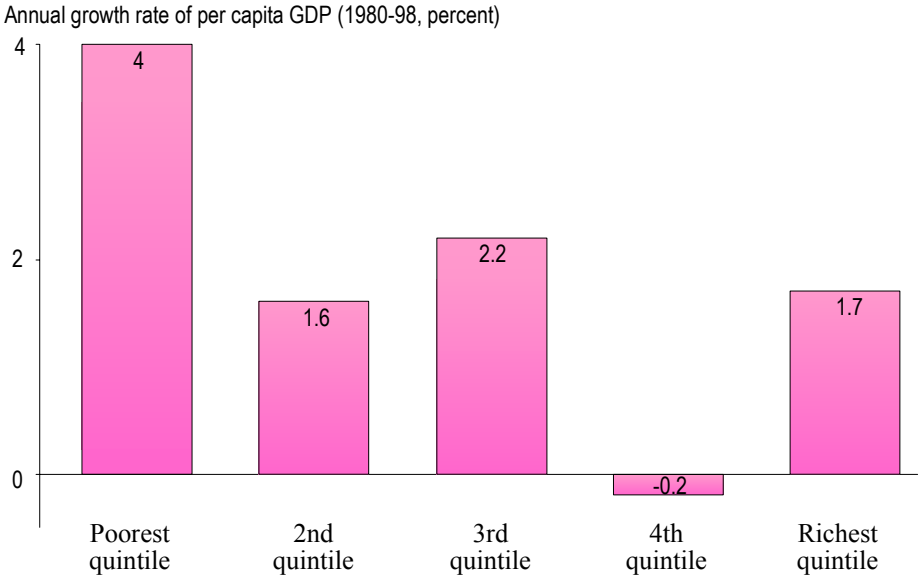
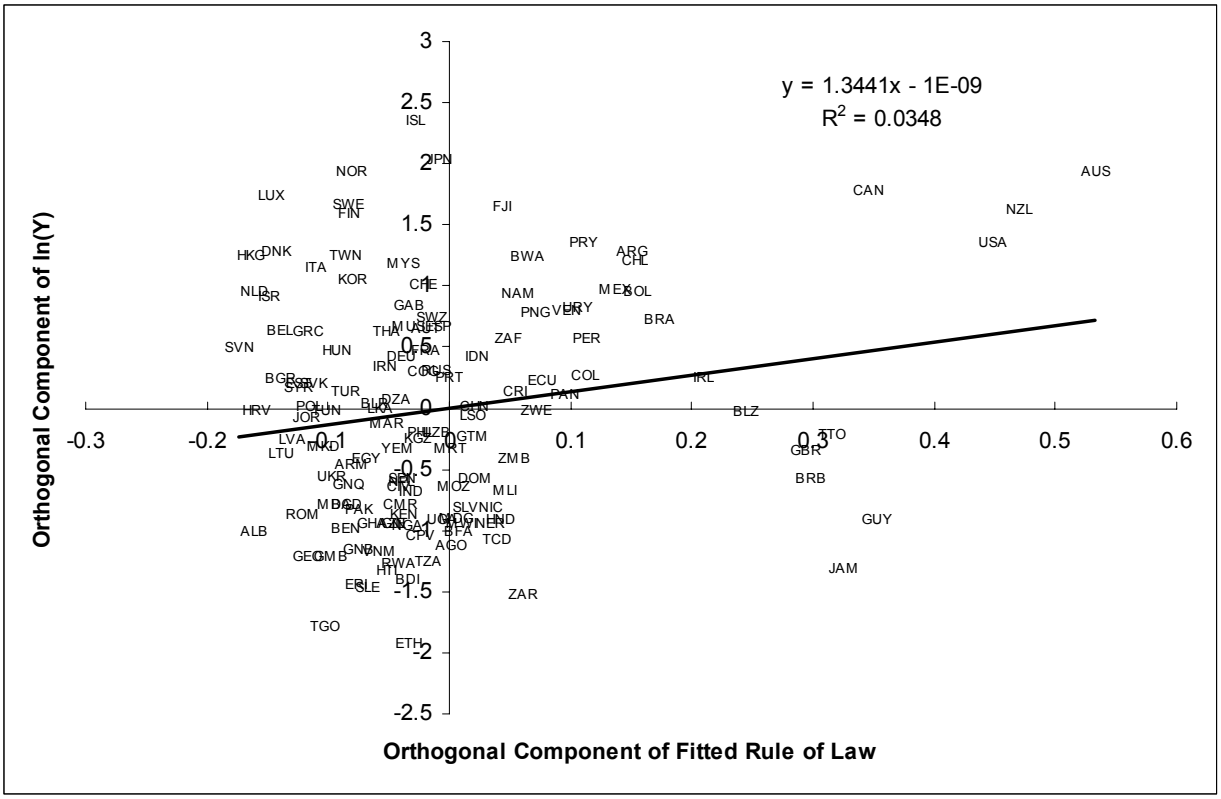


Figure 2: Partial Effects Institutions and Trade



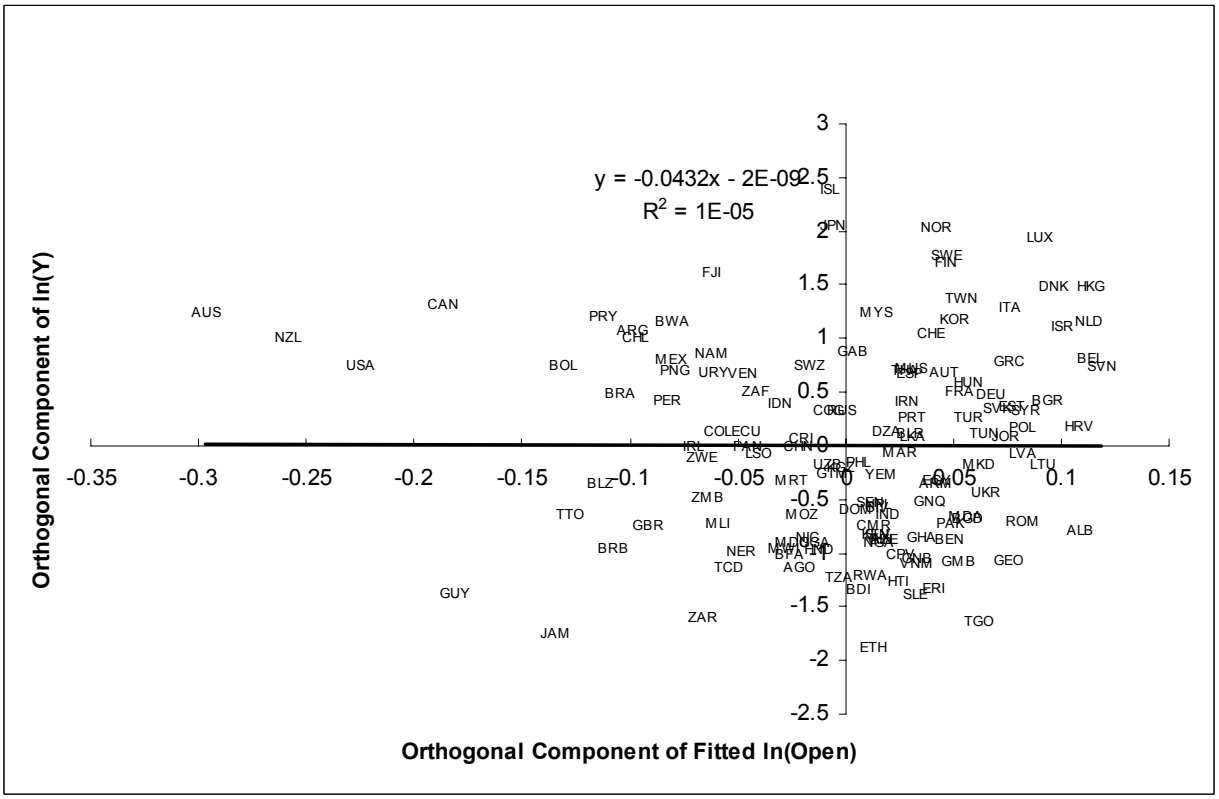


Figure 3: Predicted Trade and Predicted Rule of Law

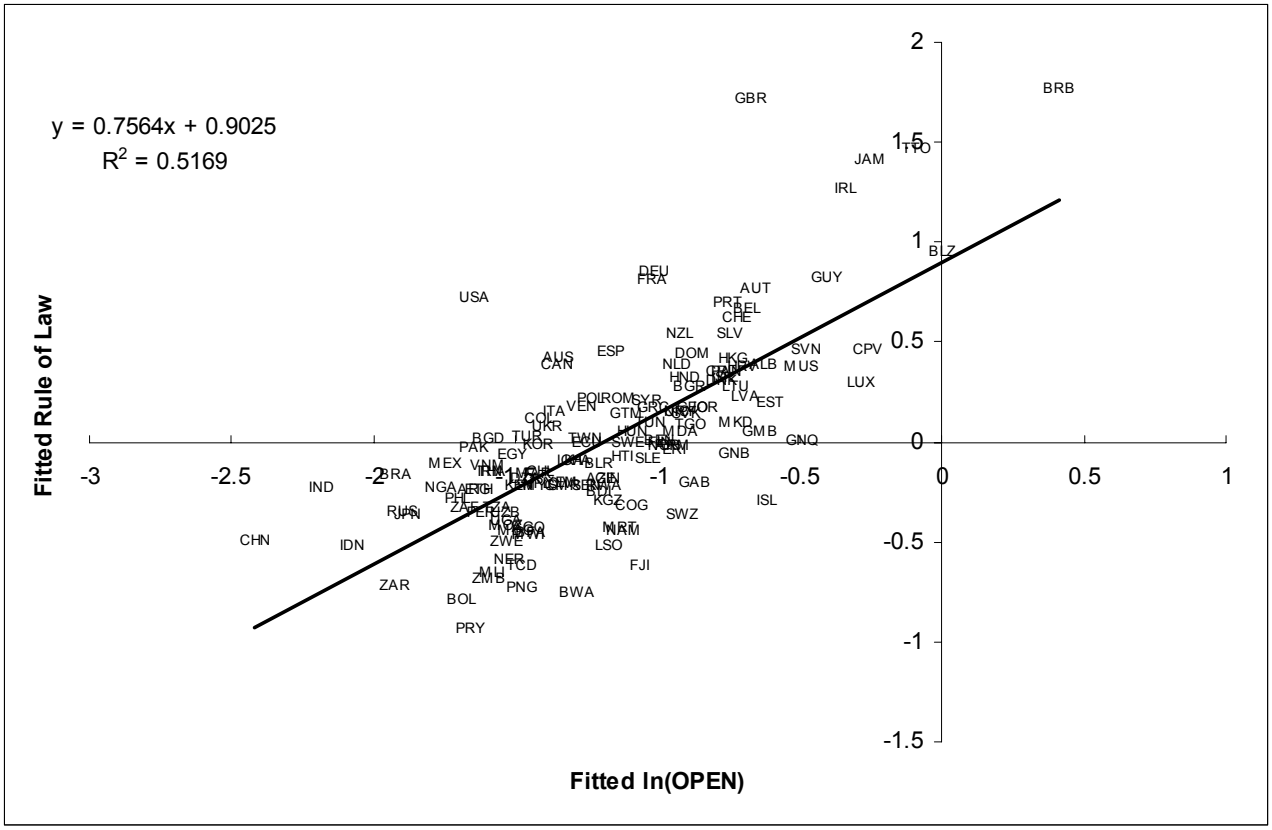
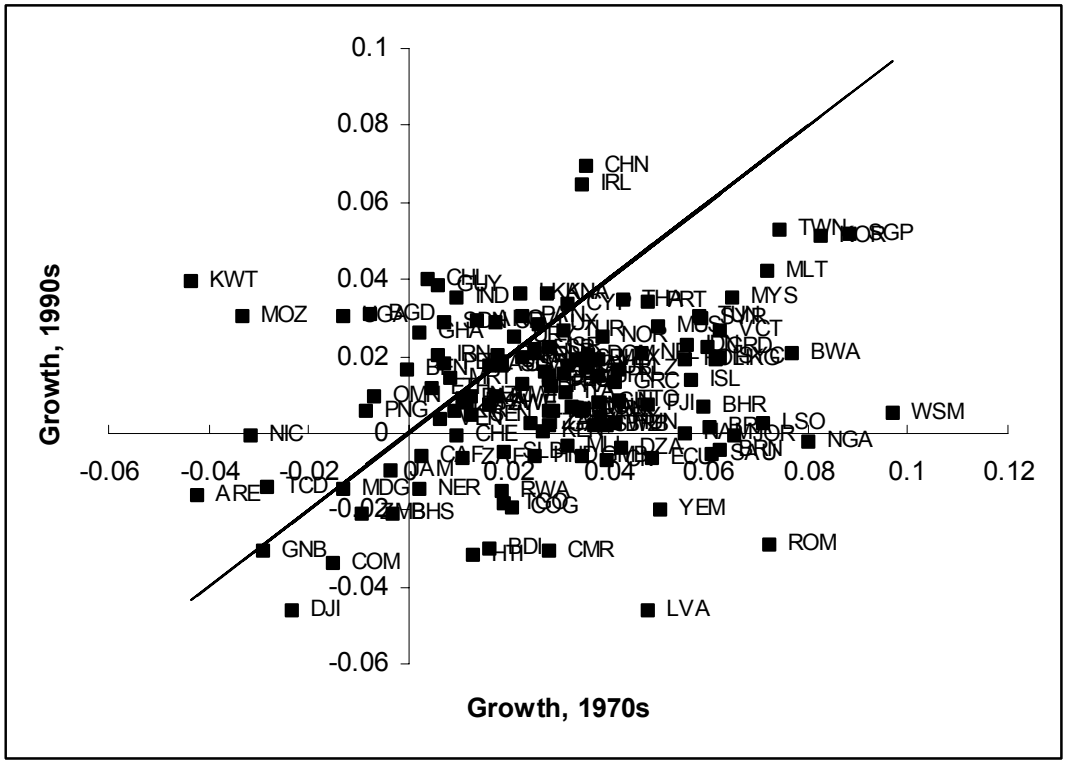


Figure 4: Persistence of Growth and Trade



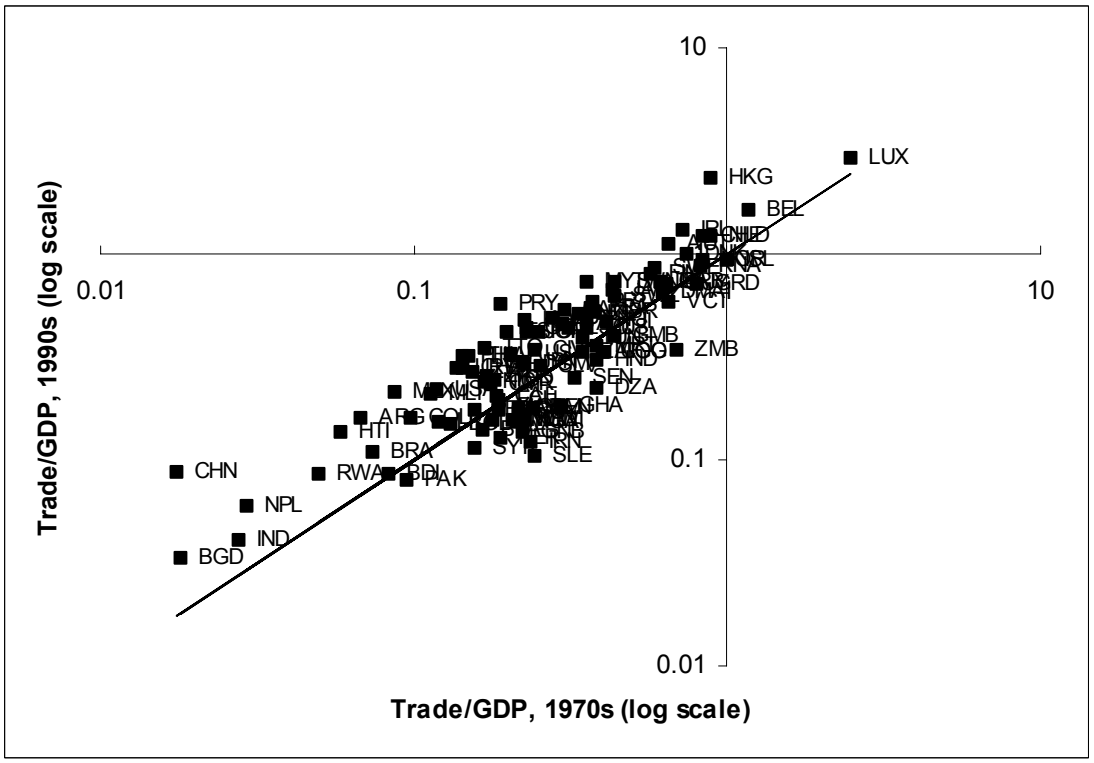
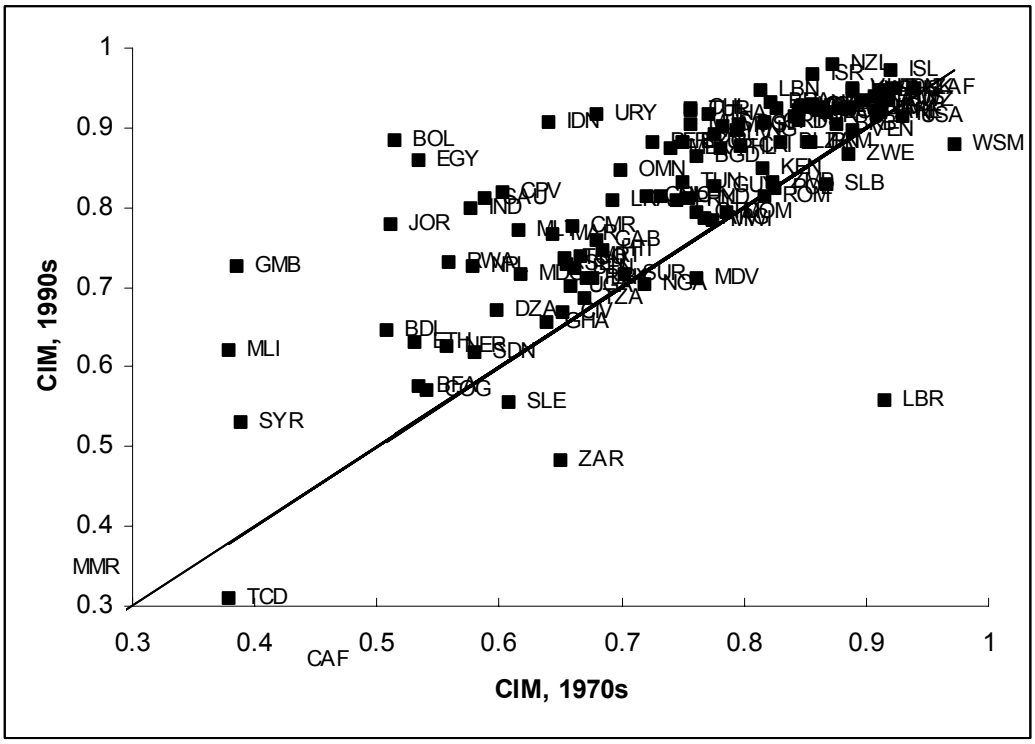


Figure 5: Persistence of Institutional Variables



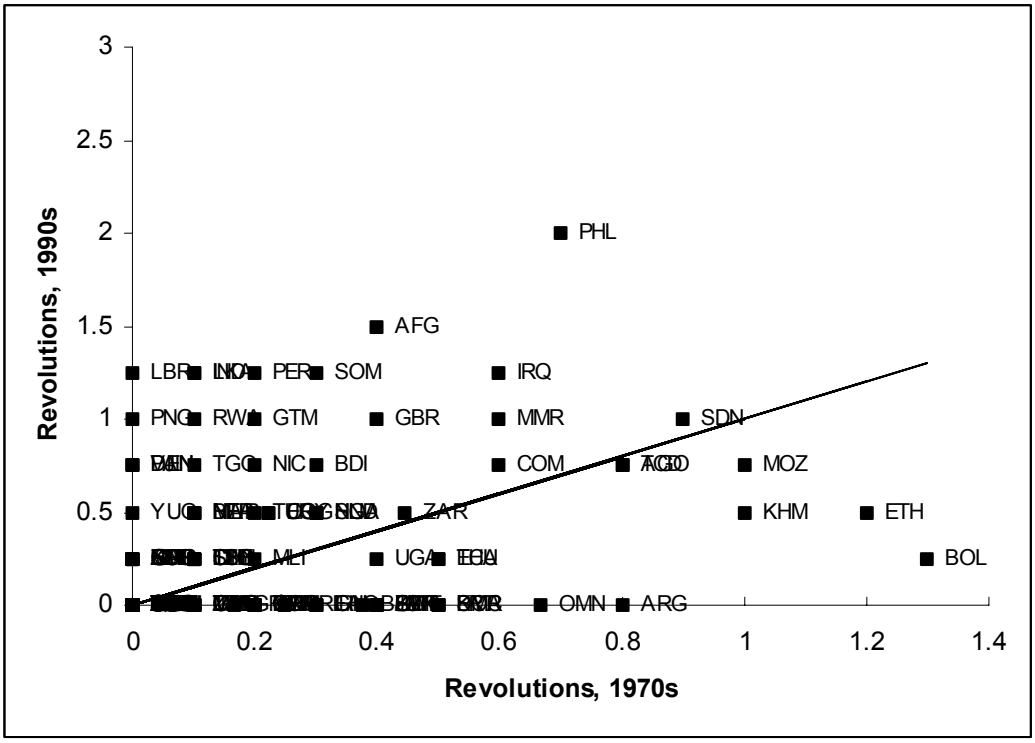
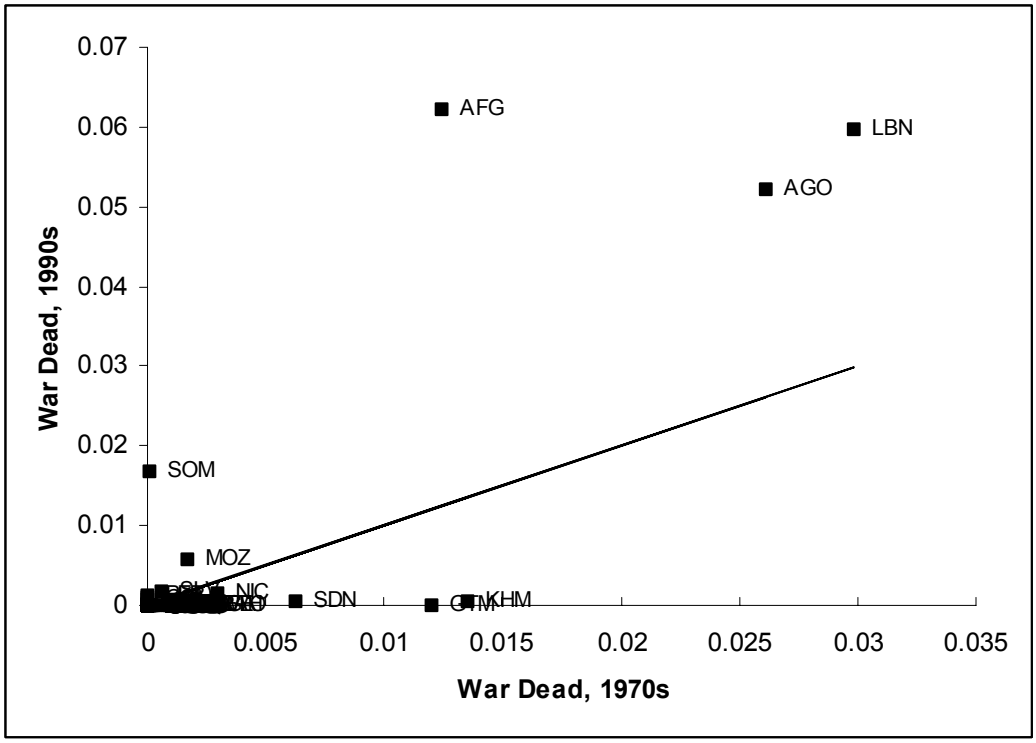


Figure 5: Persistence of Institutional Variables, Cont'd



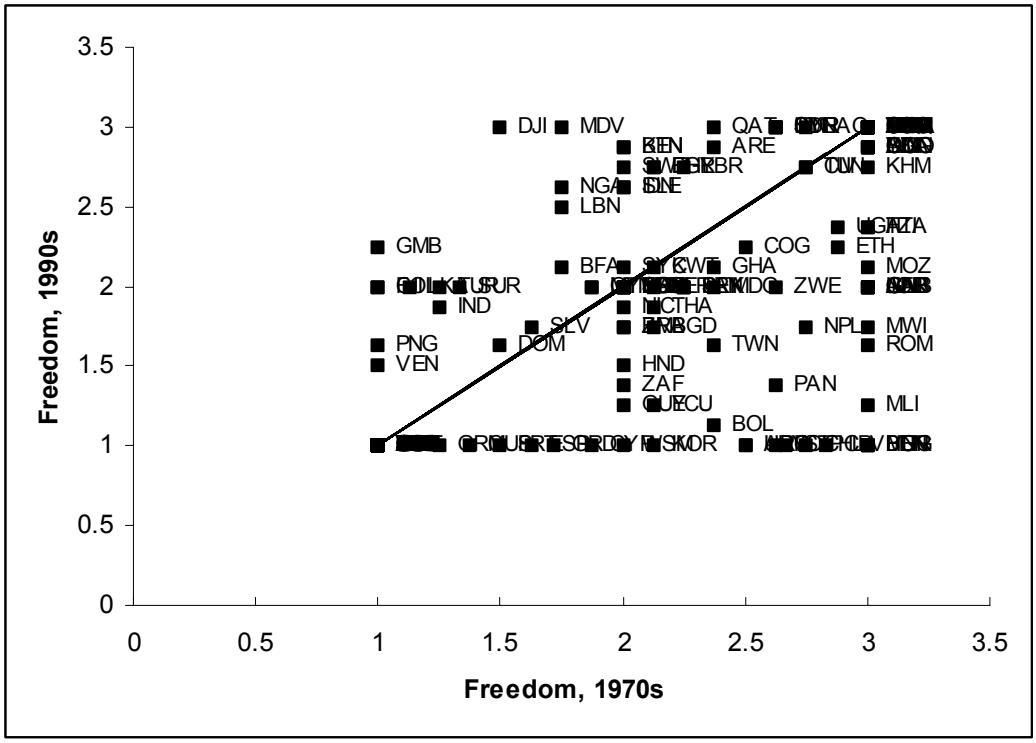


Figure 5: Persistence of Institutional Variables, Cont'd

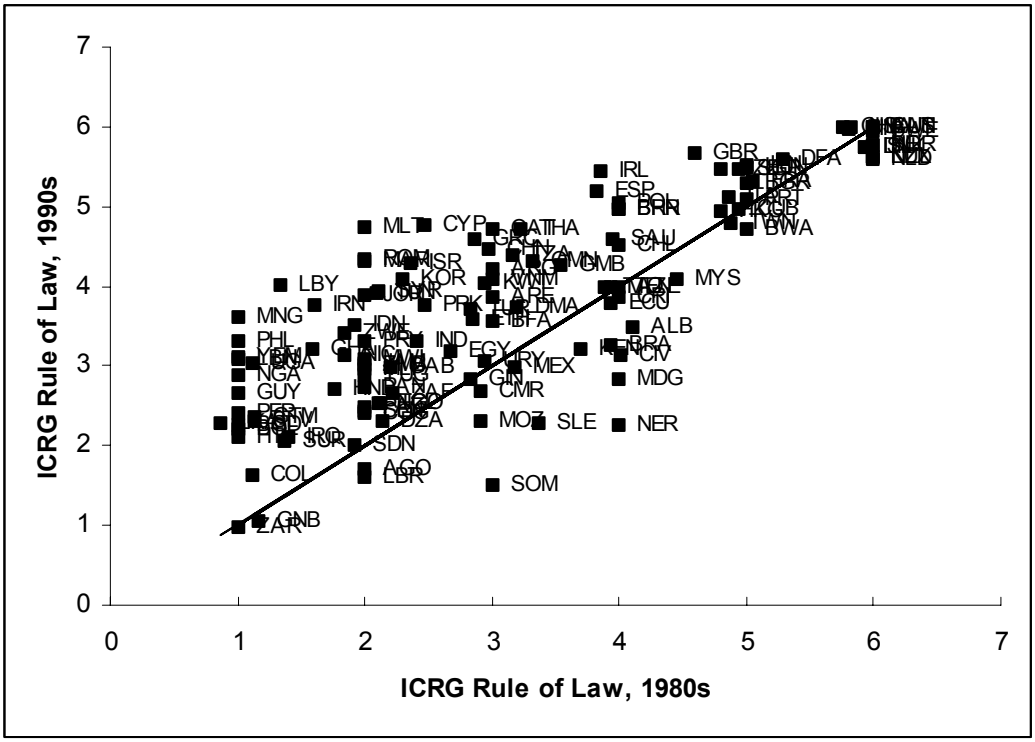


Figure 6: Tariffs and Trade Volumes

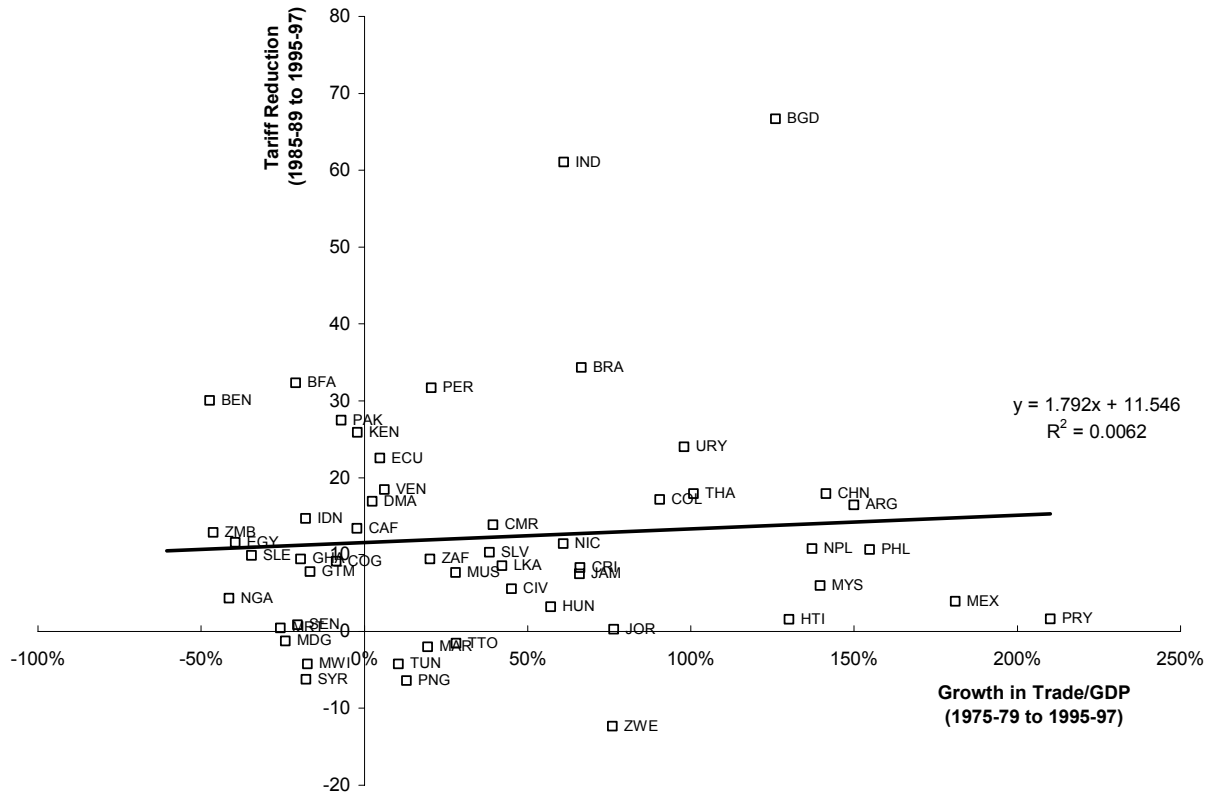


Figure 7: Trade Reforms and Trade Volumes in China 1980-2000

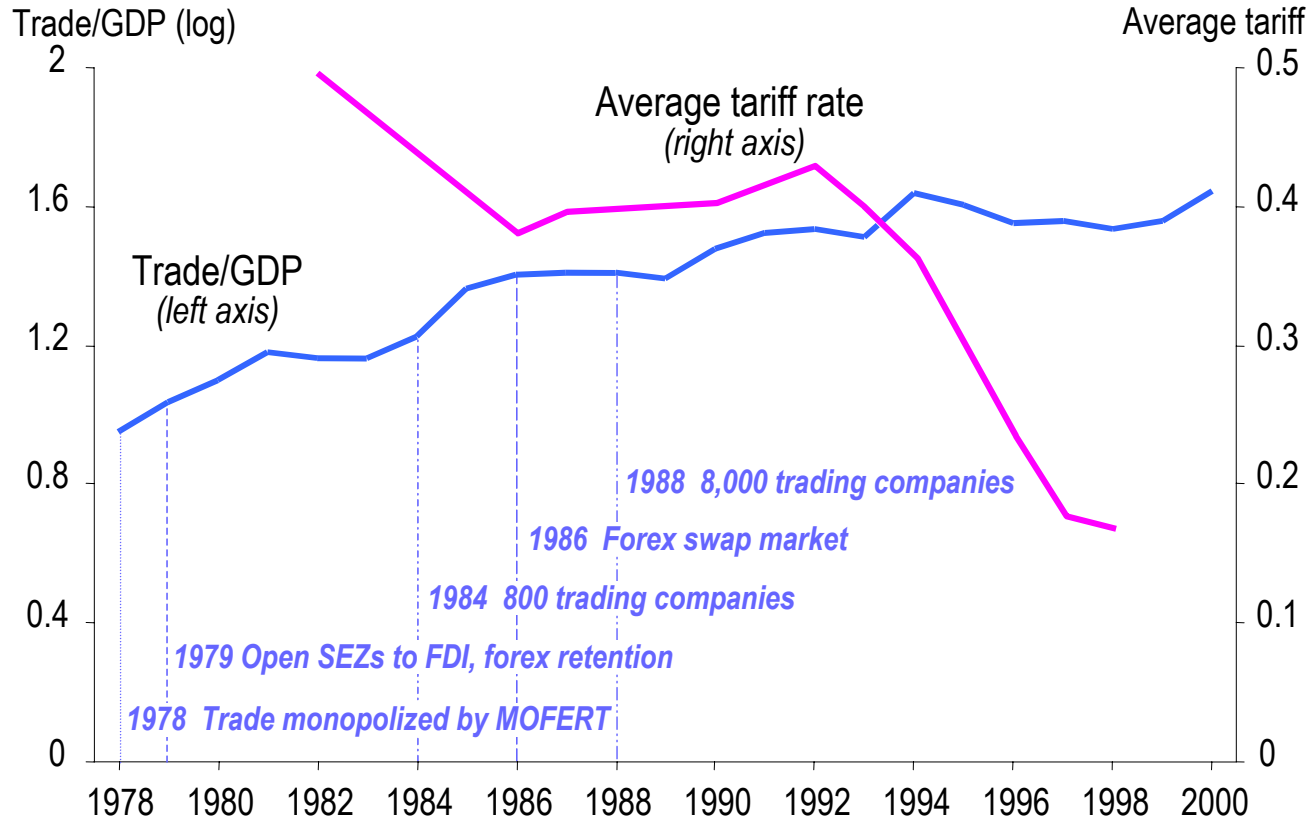


Figure 8: Partial Effect of Trade on Growth, Decadal Regressions