Trade liberalization and institutional development

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TRADE LIBERALIZATION AND INSTITUTIONAL DEVELOPMENT*

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Abstract

Recent research shows that the majority of cross-national variation in institutions is related to history. The within variation however is somewhat less explored. Using panel data covering the periods 1865 – 1940 and 1980 – 2000 & 31 and 103 countries respectively, we find that the within variation in economic institutions (namely property rights and contracts) can be explained by trade liberalization. To address endogeneity concern we use export partner growth and rainfall as instruments for trade liberalization and log GDP per capita respectively and estimate our model using Limited Information Maximum Likelihood (LIML) Fuller estimation method. Our basic result holds after controlling for country fixed effects, time varying common shocks, and various additional covariates. It is also robust to various alternative measures of liberalization and institutions, as well as across different samples. We find weak evidence in favor of the 'critical juncture' view of history.

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I. Introduction

A growing number of economists working on comparative development identify institutional quality as an important cog in the economic success of a country. The works of Knack and Keefer (1995), Hall and Jones (1999), Acemoglu *et al.* (2001, 2002), Rodrik *et al.* (2004), Bhattacharyya (2009a), Bhattacharyya (2009c), and Bhattacharyya *et al.* (2009) are all supportive of this view.[1] Acemoglu *et al.* (2001) in particular argue that the differences in current institutional quality across nations can be traced back to its colonial origin. In brief, their story is the following. The Europeans resorted to different style of colonization in different parts of the world depending on the feasibility of settlement. In tropical climate the mortality rate among European colonizers were extremely high which prevented them from settling there and they erected extractive institutions. Whereas, in temperate climate the mortality rate among colonizers were low which made them ideal for settlement and they erected strong institutions. These institutions persisted over time and they continue to influence the current institutional and economic performance of these countries. What they find is consistent with the 'critical juncture'[2] view in political science, which shows that events during critical historical junctures can lead to extremely divergent political-economic outcome in the long run (see Acemoglu *et al.*, 2008).[3] Without doubt this is an important empirical finding. But the fact that we occasionally do notice improvements in institutional quality due to good policy suggests that good institutions are not entirely determined by history. A good illustration of this is perhaps post-independence

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[1] Bhattacharyya (2009b) show that the institutions view lacks empirical support in Africa and diseases are more important.

[2] The critical juncture idea has been central to a big literature in political science and historical institutionalism (see Gerschenkeron, 1962; Moore, 1966; Ertman, 1997). For a detailed survey of this literature see Ikenberry (1994).

[3] Acemoglu *et al.* (2001, 2002) identify European colonization of other continents around 1500 as one such critical juncture in modern history.

India.[4] India inherited relatively good institutions from the British in 1947. A democratic polity, an independent judiciary, and secure property rights were among many other positives that were enshrined into the constitution of independent India. High scores in executive constraint (consistently around 7) and democracy (consistently around 9) from Polity data during the 1950s is indicative of the fact that the institutions were strong at least on paper. India also embarked on an import substituting industrialization policy during this time relying on high tariff and quantitative restrictions to prevent imports. This led to the well-known problem of 'rent-seeking society' (see Bhagwati and Desai, 1970; and Krueger, 1974). As a consequence of the widespread culture of rent-seeking and bad policy good institutions on paper often yielded poor institutional outcomes over the next three decades. A quick comparison of the Polity score (which measures institutions on paper) and the ICRG score (which measures institutional outcome) during 1980 to 2000 support this view.[5] The Polity executive constraint index remained consistently at 7 throughout the period. In contrast the ICRG expropriation risk index was as low as 6 in 1982 when the import licensing system was fully operational and it became as high as 10 in 1993 and thereafter when India liberalized its economy.[6]

A growing number of studies have argued that the majority of the cross-national variation in institutional quality can be traced back to their historical origin (see Hall and Jones, 1999; La Porta *et al.*, 1999; Acemoglu *et al.*, 2001, 2002; Rodrik *et al.*, 2004). The within variation however is somewhat less explored. Using panel data covering the periods 1865 – 1940 and 1980

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[4] Other good examples are the Philippines and Mexico. In post-independence Philippines restrictive trade policy led to institutional decline till late 1980s when it was gradually reversed. See Shepherd and Alburo (1991) for a summary of trade policy in post-independence Philippines. Haber (2006) documents that import substitution policies were adopted in Mexico in the early 20th century to protect the businesses of the rich elite aligned with the government of the dictator Porfirio Diaz. This damaged subsequent institutions. Haber (2006) also reports similar consequences of import-substitution in Brazil.

[5] In both the Polity and the ICRG indices, higher score signify better institutional quality. For details refer to Appendix A.1.

[6] India liberalized its economy in 1991 and the positive effect on expropriation risk started to show in 1992 when the index score jumped from 6.2 to 8.2.

– 2000 & 31 and 103 countries respectively, we find that the within variation in economic institutions (namely property rights and contracts) can be explained by trade liberalization. To address endogeneity concern we use export partner growth and rainfall as instruments for trade liberalization and log GDP per capita respectively and estimate our model using Limited Information Maximum Likelihood (LIML) Fuller estimation method. Our basic result holds after controlling for country fixed effects, time varying common shocks, and various additional covariates. It is also robust to various alternative measures of liberalization and institutions, as well as across different samples. We find weak evidence in favor of the 'critical juncture' view of history as the country fixed effects are only weakly correlated with settler mortality and population density in 1500.[7] In contrast latitude seems to have stronger explanatory power.

We make four original contributions in this paper. First, using panel data for the period 1980 to 2000 and 103 countries, country and year fixed effects, and an instrumental variable method we are able to systematically estimate the causal effect of trade liberalization on economic institutions (namely property rights and contracts) within a country. To the best of our knowledge, there are no comparable panel data empirical studies estimating the causal effects of trade liberalization on institutions within a country. Second, using panel data for the period 1865 to 1940 and 31 countries we are able to show that this causal relationship holds also for the pre-war period. This in our view is also an entirely new result. Third, by looking at the partial correlation between the country fixed effects and some of the historical variables, we are able to test the 'critical juncture' view of history. Contrary to some of the recent studies (see Acemoglu

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[7]Note that our weak evidence in favor of the 'critical juncture' view is not disproving the role of history in shaping institutions. The methodology used here and other cross-section studies are a-historical in the sense that it tries to explain the variation in present-day cross-sections with inferred differences in specific characteristics in other, long-past, cross sections. Therefore a statistically insignificant relationship is weak statistical evidence but may not necessarily be a refutation of the theory.

et al., 2008) we only find weak evidence in support of this view. Fourth, the IMF (2005) raises concerns about the lack of research on macroeconomic policies to improve institutions. In this study we aim to fill in this gap.[8]

Our results are consistent with the theories of trade and institutional development propounded by North (1981), Rogowski (1989), Acemoglu *et al.* (2005), and Acemoglu and Robinson (2006).[9] North (1981) emphasizes the role of market size and technology in engendering institutional change over time. It is widely accepted that both market size and technology are influenced by trade (see Smith, 1776). Hence trade can bring about institutional change. Rogowski (1989) show that trade affects domestic political alignments through changes in factor prices. Acemoglu *et al.* (2005) show that trade induces institutional change by strengthening commercial interests. Acemoglu and Robinson (2006) show that trade induces institutional change through the transfer of skill-biased technology which increases the income share of the middle class. The 'critical juncture' results are also related to Hall and Jones (1999), La Porta *et al.* (1999), Acemoglu *et al.* (2001, 2002), Rodrik *et al.* (2004), and many others who find evidence in favor of the historical origin of institutional divergence across countries.

The remainder of the paper is organized as follows. Section II discusses the theoretical motivation. Section III introduces the empirical strategy and data. Section IV presents the results and Section V concludes.

II. Theoretical Motivation

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[8] Note that IMF (2005) conducts a panel analysis using trade openness measured by trade volume and the Cato Institute's Economic Freedom Index covering 93 countries over 1970 to 2004. They use five-year averages to smooth out noise in the annual data. They find that the impact of trade openness on the Economic Freedom Index is weak. In contrast, here we are using measures of property rights and contracting institutions.

[9] Levchenko (2007) provides an alternative view. In a theory paper he shows that institutional differences are an important determinant of trade flows across countries. Institutional differences as a source of comparative advantage imply that the less developed countries may not gain from trade.

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The theoretical literature on institutional change over time perhaps originates from North (1981). North (1981) emphasizes that a change in per capita capital stock due to population growth and technological progress brings about institutional change over time. What North does not mention is the impact of international trade on population and technological progress. International trade increases the size of the market which is equivalent to an increase in the size of domestic population (see Smith, 1776). It is also a widely accepted view that trade induces technological progress via technology transfer (see Romer, 1990; Coe and Helpman, 1995). Therefore, potentially engagement in international trade can bring about institutional change in a country. In a related research Rogowski (1989) also shows that trade affects domestic political alignments through changes in factor prices. He however does not focus on the impact of trade on institutions.

Acemoglu *et al.* (2005) documents historical evidence in favor of the trade induced institutional change view. Their hypothesis however is different from North's (1981) capital stock theory as they focus on trade's impact on the distribution of political power and subsequent institutional change. They show that Western Europe's engagement in Atlantic trade induced institutional change by strengthening commercial interests which resulted into rapid economic growth in countries where the initial political institutions were non-absolutist.

In a related study, Acemoglu and Robinson (2006) show that trade induced transfer of skill-biased technology increases the income share of the middle class. This increases their political power relative to the rest of the society and they impose checks and balances on the existing institutions to protect their property rights and contracts. With a larger share of income, the powerful middle class also favor taxation which is less redistributive. This makes the elite more willing to accept the checks and balances on institutions imposed by the middle class.

Rajan and Zingales (2003) also put forward a similar theory of 'interest group politics' with regards to financial development.

Without doubt there is enough theory floating around to believe that trade liberalization does have an impact on institutions. The key issue however is how much of it is supported by the data.[10] Here, we focus on the question whether trade liberalization can explain within variation in two types of economic institutions namely property rights and contracts. The motivations behind choosing property rights and contracts as our dependent variables are two fold. First, the empirical literature to date has focused more on the impact of liberalization on corruption and political institutions (democracy).[11] Property rights institutions and contracting institutions have received relatively less focus. Second, recent evidence shows that different institutions affect economic performance differently and these institutions are crucial to the economic performance of a country (see Rodrik, 2000a; Djankov *et al.*, 2002; Acemoglu and Johnson, 2005; Bhattacharyya, 2009a). Hence they are extremely important for policymaking.

III. Empirical Strategy and Data

We use panel data which covers 103 countries from the period 1980 to 2000. Due to data limitations, not all specifications cover 103 countries and in some cases, the panel is unbalanced. Our basic specification uses five year averages of institutions, trade liberalization and other control variables between 1980 and 2000[12]. In table 5 we also explore the causal relationship between trade liberalization and institutions using data for the period 1865 to 1940. To uncover

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[10] Note that here we solely focus on empirically estimating the causal link between trade liberalization and institutions and not the channels through which trade liberalization may affect institutions. Section II however discusses the theory of some of these channels through which liberalization may affect institutions.

[11] Some of the influential papers on trade and corruption are Krueger (1974), Ades and Di Tella (1999), Giavazzi and Tabellini (2005), and Wei (2000). Research on trade and democracy is due to Giavazzi and Tabellini (2005), Persson (2005), and Eichengreen and Leblang (2006).

[12] Note that we are constratined by the non availability of trade liberalization and contracting and property rights institutions data beyond 2000.

the causal relationship between trade liberalization and institutions within a country we estimate an equation of the form:

$$INS_{it} = \alpha_i + \beta_t + \gamma_1 po_{it} + \phi_1 y_{it} + \mathbf{X'_{it}}\Lambda + \varepsilon_{it} \tag{1}$$

where INS_{it} is a measure of institutional quality in country i averaged over years $t-4$ to t, α_i is a country dummy variable controlling for country fixed effects (namely colonial history, culture, and other unobservable country specific factors), β_t is a year dummy variable controlling for time varying common shocks, po_{it} is trade liberalization measured by Sachs and Warner openness index in country i over years $t-4$ to t, y_{it} is log income per capita in country i averaged over years $t-4$ to t, and $\mathbf{X_{it}}$ is a vector of other control variables. Note that we control for income here as it is likely that richer countries are able to afford better institutions.

We focus on two types of institutions: property rights institutions measured by ICRG expropriation risk and contracting institutions measured by ICRG repudiation of contracts. The motivation is to go beyond the frequently used "cluster" of institutions and unbundle economic institutions. According to North (1981), Rodrik (2000a), and Acemoglu and Johnson (2005), a measure of property rights institutions should capture the performance of institutions that constraints government and elite expropriation of private property; and a measure of contracting institutions should capture the performance of institutions that supports private contracts. We try to follow these definitions while selecting our measures.

As mentioned above, we use expropriation risk from the International Country Risk Guide (ICRG) published by the international agency Political Risk Services as our measure of property rights institutions ($PRINS_{it}$). The measure varies between 1 in Libya in 1980 to 10 in Australia, the United States, and many other countries. A higher value of the index indicates a

lower probability of expropriation of private property by the state and hence better quality institutions. We also notice that property rights institutions have converged globally over our sample period. The variance in expropriation risk scores across countries have declined from 3.2 in 1980 to 2.7 in 2000 which is a 16 percentage points drop from its 1980 level.

Note that Douglass North defines good property rights institutions as those that provide checks against expropriation by the government and other politically powerful groups (see North, 1981; pp. 20-27). Expropriation risk is perhaps the closest to North's definition as it captures the notion of extractive state. Furthermore, many previous studies have also used this measure (see Knack and Keefer, 1995; Acemoglu *et al.*, 2001; Acemoglu and Johnson, 2005; Bhattacharyya, 2009a). Nevertheless, we also use executive constraint from the Marshall and Jaggers (2002) dataset as an alternative and our results are robust (see table 8). For the 1865 to 1940 period we use executive constraint as a measure of institutions as this is the only option available to us.

Contracting institutions ($CONINS_{it}$) are defined as the rules and regulations that govern contracts between ordinary citizens, for example, between a creditor and a debtor or a supplier and a buyer (see Acemoglu and Johnson, 2005). Ideally one should use a measure of contract enforcement between private citizens. Djankov *et al.* (2002) legal formalism index is one such measure. This measure is also used by Acemoglu and Johnson (2005). But unfortunately this measure is only available in a cross-section and hence not suitable for our purpose. As an alternative we use the risk of repudiation of government contracts from ICRG which is available in a panel.[13] Repudiation risk measures contract enforcement between the government and a private citizen and hence admittedly not an ideal measure. However, we would expect the

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[13] Note that this measure is also used as a proxy for contracting institutions by Knack and Keefer (1995).

contracting environment between the government and a private citizen to be correlated with the contracting environment among private citizens. The measure operates on an eleven point scale ranging from 0 to 10 with a high score implying better contracting institutions. In our sample, Myanmar registers the lowest contracting institutions score of 1 in 1990.

A major conceptual challenge in separately measuring property rights institutions and contracting institutions is the issue of potential overlap. Both institutions are put in place to control opportunistic behavior of an agent or a group. The overlap is perhaps reflected by the high correlation (of the order of 0.9) between the two measures reported in Table 2. However, there are significant differences as well. Contracting institutions are contracts between two private citizens or between a state institution and a private citizen. Therefore, if contracting institutions fail, it is possible to write an alternative contract. In contrast, property rights institutions are the contract between the state and the ordinary citizens as a group and depend on the distribution of political power between the two. Hence it is difficult to write an alternative contract when property rights institutions fail (see Acemoglu and Johnson, 2005). However, having mentioned the significant differences between these two institutions we also remind the reader that our analysis do not successfully resolve the issue of potential overlap.

The trade liberalization measure (po_{it}) is from Sachs and Warner (1995). The index runs from 1950 to 1990. Wacziarg and Welch (2003) provide an update of Sachs and Warner (1995) index and they extend it till 2000. We use the Wacziarg and Welch (2003) updated figures for the 1990s. The Sachs and Warner index classify a country as not liberalized if any of the following conditions apply: (i) Its average tariff rate on imports of capital or intermediate goods is above 40 percent; (ii) Its non tariff barriers cover 40 percent or more of its import of capital and intermediate goods; (iii) Its black market premium is 20 percent or more; (iv) It has a

socialist economic system; (v) It has a state monopoly on major exports. This index suits our purpose as it covers all aspects of trade liberalization. Using this index we create a short-run and a long-run measure of trade liberalization. The short-run measure po_{it} is the fraction of years a country has remained liberalized in the last five years.[14] The long-run measure PO_{it} is the fraction of years a country has remained liberalized from 1950. These measures by construction vary between 0 and 1. We notice that Liberia remained closed all throughout our sample period whereas the United Kingdom, the United States, and many other countries are at the other end of the scale.

Recently, the Sachs and Warner liberalization index has been criticized by Rodriguez and Rodrik (2000) as they show that the index suffers from measurement problems.[15] However, this index has been used by several recent studies (see Alesina and Dollar, 2000; Wacziarg, 2001; Wacziarg and Welch, 2003; Easterly *et al.*, 2003; Giavazzi and Tabellini, 2005; Persson, 2005; Hausmann *et al.*, 2005; Bhattacharyya *et al.*, 2009) as a measure of trade liberalization or economic liberalization. We use the Sachs and Warner index because of its wide coverage (both cross-section and time series) and easy availability. It is also relatively superior in capturing trade liberalization than other measures such as trade volume (Wacziarg, 2001). Nevertheless, we also use export growth rate[16], Gwartney and Lawson (2005) free trade index, and log tariff as alternative measures of liberalization in table 8 and our results are robust. Also note that the LIML Fuller IV estimation method adopted here is robust to any potential bias in our estimates

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[14] The rationale for choosing a 5 year period for the short run measure is twofold. First, annual data is typically noisy and the aim here is to smooth out business cycle fluctuations. Second objective is to maximize the number of data points. With a 10 year period for instance, we would end up with a much smaller dataset. Alternatively reducing the interval to less than five years may introduce a lot of noise in the data.

[15] Rodriguez and Rodrik (2000) show that the index is dominated by black market premium and state monopoly in exports which are not necessarily trade related. However, Warner (2003) updates the index and comes up with a reply arguing that these factors are trade related.

[16] Export growth rate can be interpreted as a measure of liberalization. If trade is determined by policy and geography based factors then any change in exports should be due to policy and not gravity. See Dollar and Kraay (2003) for a better discussion of these concepts.

due to systematic measurement error in the liberalization index as long as the error is uncorrelated with the instruments. For the 1865 to 1940 period we use tariff and export growth rate from Clemens and Williamson (2004) as measures of trade liberalization.

The main variable of interest is po_{it} and therefore γ_1 is our focus parameter. We expect γ_1 to be positive and statistically significant. However it is not straightforward to interpret γ_1 as a causal effect. There are challenges of endogeneity and omitted variable bias that we need to address to interpret γ_1 as a causal effect. Endogeneity or reverse causality is a concern and can lead to bias in our estimates. We are arguing here that trade liberalization improves institutional quality. However it is also possible that causality runs in the opposite direction. Furthermore, time invariant country specific unobservable factors (for eg., colonial history, culture, ethnic makeup, religion, geography) may influence both institutions and trade liberalization. This will also bias our estimates. To address these concerns, we estimate our model using the LIML Fuller instrumental variable method and also control for country and year fixed effects.

To address endogeneity of trade liberalization (po_{it} and PO_{it}) we use export partner growth rate ($\hat{y}_{it}^{XP}$) as an instrument. $\hat{y}_{it}^{XP}$ is the weighted average of export partners' GDP growth rates where the weights are based on the share of each export market in a country's total exports in 1995. For $\hat{y}_{it}^{XP}$ to be a valid instrument, it has to satisfy the twin conditions of being correlated with the suspected endogenous variables (po_{it} and PO_{it} in this case) and orthogonal to the error term ε_{it}. If a proposed instrument is not strongly correlated with the suspected endogenous variables then it leads to a bias in the estimates (Staiger and Stock, 1997; Stock and Yogo, 2005). This is commonly known as the problem of weak instruments. Statistically, this can be checked by comparing the first stage F statistic with the Stock-Yogo critical value. If the first stage F

statistic is greater than the Stock-Yogo critical value, then the instruments are not weak. Our instrument $\hat{y}_{it}^{XP}$ passes this test and hence is not weak (see panel B, table 3). Nevertheless, we also use the LIML Fuller Instrumental Variable estimation method which is robust to weak instruments.

It is expected that faster growth in export partners' GDP will induce more trade and liberal trade policies in the origin. Indeed we find that export partner growth ($\hat{y}_{it}^{XP}$) is a strong positive predictor of trade liberalization (see panel B of table 3). Figures 1a and 1b plot these relationships. Moreover, it is unlikely that export partner growth would influence economic institutions in the country under scrutiny directly or through channels other than trade liberalization not explicitly controlled for in our model. As a further check, we also look at the correlation between INS_{it} and $\hat{y}_{it}^{XP}$ by using the latter as an explanatory variable. We do not find any evidence of direct relationship.[17] Even though not conclusive, the above evidence is suggestive that export partner growth ($\hat{y}_{it}^{XP}$) is a valid instrument.

To address endogeneity of log per capita income (y_{it}) we use International Panel on Climate Change (IPCC) approved log rainfall from Mitchell _et al._ (2003) as an instrument. Rainfall is geography based and therefore is likely to be orthogonal to the error term. We also find that it has no direct effect on institutions. It is also likely to be a positive predictor of income as it may affect living standards through its impact on agriculture. Indeed we find evidence that rainfall is a strong instrument as it passes the Stock-Yogo test for weak instruments (see panel B, table 3).

$$po_{it} = \alpha_i + \beta_t + \theta_1 \hat{y}_{it}^{XP} + \theta_2 \ln(RAIN)_{it} + u_{it} \qquad (2)$$

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[17] This result is not reported here to save space but is available upon request.

$$PO_{it} = \alpha_i + \beta_t + \lambda_1 \hat{y}_{it}^{XP} + \lambda_2 \ln(RAIN)_{it} + u_{it} \tag{3}$$

$$y_{it} = \alpha_i + \beta_t + \omega_1 \hat{y}_{it}^{XP} + \omega_2 \ln(RAIN)_{it} + u_{it} \tag{4}$$

The instrumental variable estimation method can be summarized as follows. At the first stage we estimate equations (2)/(3) and (4) and use the predicted values of trade liberalization and log per capita income to estimate equation (1). Note that we do not report Sargan style overidentification tests as we have an exactly identified system.

To address concerns of omitted variable bias we control for country and year fixed effects, and additional covariates. The country fixed effects are able control for colonial history which may have caused institutional divergence across countries (Acemoglu *et al.*, 2001). The year fixed effects control for global shocks that may have affected institutions in all countries in our sample. Therefore, γ_1 can be interpreted as the 'within country' effect of liberalization on institutions.

The other measures used are schooling from Barro and Lee (2000), inequality, foreign aid and FDI data from the World Bank, log settler mortality and log population density in 1500 from Acemoglu *et al.* (2001). Appendix A.1 reports all the variable definitions and Table 1 reports descriptive statistics of the major variables.

IV. Evidence

This section systematically tests whether trade liberalization leads to institutional development. In table 3, column 1 we estimate the model using po_{it} and find that trade liberalization positively affects property rights institutions. Our model predicts that one sample standard deviation (0.5) increase in trade liberalization would lead to a 2.2 points increase in the property rights institutions index. To put this into perspective let's focus on Myanmar. Myanmar is a virtually closed economy with very poor institutions during our sample period. The average

trade liberalization (po_{it}) score and the average property rights ($PRINS_{it}$) score over the sample period in Myanmar is 0 and 5.7 respectively. If trade liberalization index in Myanmar are to increase by 0.5 then the corresponding increase in the institutional score would be to 7.8 points which is 39 percentage points increase. The United States on the other hand is an open economy with strong institutions. The average trade liberalization (po_{it}) score and the average property rights ($PRINS_{it}$) score over the sample period in the United States is 1 and 10 respectively. If we are to compare between Myanmar and the United States then, ceteris paribus the model explains approximately 97 percent of the actual difference in property rights institutions in Myanmar and the United States. In other words, ceteris paribus if Myanmar's trade policy is to become like that of the United States (an increase in po_{it} by 1) then her $PRINS_{it}$ would increase by 4.2 which is approximately 97 percent of the actual difference in $PRINS_{it}$ between Myanmar and the United States. The actual difference in $PRINS_{it}$ between Myanmar and the United States is 10-5.7 = 4.3. In column 2, we focus on the long run trade liberalization index. We replace po_{it} with trade liberalization since 1950 (PO_{it}). The result remains qualitatively unchanged. Column 3 estimates the impact of po_{it} on contracting institutions ($CONINS_{it}$). A one standard deviation increase in po_{it} predicts a 0.8 standard deviation increase in the quality of contracting institutions. In Myanmar, this would imply an increase in $CONINS_{it}$ from the sample period average of 4.3 to 6.1 which is a 42 percentage point increase. Column 4 tests the model with long run trade liberalization and the result survives. The first stage regressions with F statistic and Stock – Yogo critical values are reported in panel B.

In Table 4 we estimate the effect of colonial history on the time invariant component of institutions. There exists a strong view in political science and the historical institutionalism

literature that institutions across countries diverged due to a common shock (but of variable nature) at a critical juncture in history (see Gerschenkeron, 1962; Moore, 1966; Ertman, 1997). In columns 1 – 2 we examine the effect of historical variables on the fixed effects from the regression reported in column 1, table 3. The rationale is that the fixed effect captures the fixed time invariant component of property rights institutions and the historical variables capture global shocks at critical junctures in history. If we find correlation between the two after controlling for other factors then we can conclude that the critical juncture theory explains a part of the cross-national variation in institutions. Note that to test critical juncture theory we only focus on the fixed effects that are statistically significant as otherwise they may not be an important explanator of institutions in a particular country. In column 1 we notice that among the historical variables (log settler mortality and log population density in 1500) used in the literature only log settler mortality is statistically significant. However, they are jointly insignificant which is perhaps suggestive of a very weak colonial history effect. In column 2 we add latitude to control for geography. We find that the coefficient on log population density in 1500 is significant but log settler mortality is no longer significant. Note that columns 1 and 2 include a large number of other control variables (ethno-linguistic and religious fragmentation, latitude, fraction of Catholic, fraction Protestant, fraction Muslim, fraction Buddhist, fraction Hindu, fraction Confucian, and fraction Jew). We find that latitude has a positive and statistically significant effect which implies being further from the equator exhibits better property rights institutions. This is consistent with the findings of La Porta *et al.* (1999) who also report a positive relationship between latitude and government performance. Religion remains jointly significant in column 2. This again is consistent with the findings of La Porta *et al.* (1999) who show that religion explains institutional divergence. We repeat the same exercise with the fixed

effects from contracting institutions in columns 3 and 4. We observe a similar pattern emerging but with a much weaker colonial origins effect. However, note that in all the cases we have to deal with a small sample which may have influenced the estimates.

In table 5 we ask the question whether this relationship is relevant for the historical period of 1865 to 1940. Using tariff data from Clemens and Williamson (2004) we are able to estimate our model for a 31 country sample. Executive constraint ($EXCONST_{it}$) is used as a measure of economic institutions[18] and due to data constraint we are unable to make any distinction between property rights and contracting institutions. In column 1 we estimate the model using ordinary least squares. The advantage is that this allows us to cover the entire sample period. We notice that tariff has a negative and statistically significant impact on economic institutions. In other words, trade liberalization is good for institutions. In column 2 we estimate the model using LIML Fuller IV and export partner growth and log rainfall as instruments. Our result survives. This method is able to tackle potential endogeneity in tariff and per capital income. However it restricts the sample period to 1900 to 1940. In column 3 we estimate the model using export growth rate which is an alternative measure of trade liberalization. Footnote 12 explains why this is an appropriate measure. The trade liberalization result remains unchanged. In column 4 we focus on the sample period 1865 to 1914 which is excluding the interwar period. This period is typically characterized by de-globalization, mounting protectionism, and the emergence of autarkic development policies in many peripheral nations (O'Rourke and Williamson, 1999; Thorp, 1984). Therefore, one view is to treat this period differently. We do find that our basic result survives even after leaving out the interwar period.

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[18] Note that Acemoglu *et al.* (2005) uses executive constraint as a measure of economic institutions.

In table 6 we test the robustness of our trade liberalization result using additional covariates. In columns 1 – 4 we check the robustness of γ_1 using property rights institutions as the dependent variable. We notice that our result of a positive and statistically significant effect of liberalization on property rights institutions is robust to the inclusion of schooling, inequality measured by Gini coefficient, foreign aid, and FDI (net inflow as a percentage of GDP). The choice of additional covariates is not arbitrary as previous studies have documented that these variables have impacts on institutions. Glaeser *et al.* (2004) show that schooling positively influences institutional quality; Engerman and Sokoloff (1997) show that inequality has a negative effect on institutions; and Alesina and Dollar (2000) document that foreign aid and FDI (net inflows) has an impact on institutions. In column 5 we test robustness by controlling for all statistically significant additional control variables (schooling and foreign aid in this case). Our result survives. Columns 6 – 10 repeat the experiment using contracting institutions as the dependent variable and our result survives in all occasions.

In table 7 we put our result under further scrutiny. We check whether our result is driven by any particular group of countries. Our result is robust to the omission of African countries, Asian countries, European countries, Neo-European[19] countries, and OECD member countries. When we omit American countries from the sample the coefficient on trade liberalization remains positive. However it is only marginally significant at 18 percent. One explanation for the lack of significance could be that trade liberalization and institutional development went hand in hand in the American continent due to the influence of the so-called Washington Consensus. Alternatively, it could be an outcome of protracted consequences of the autarky policies initially set up in the 1930s with negative consequences for both openness and institutions. We however

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[19] Neo-Europe includes all Anglo-Saxon countries outside Europe: Australia, Canada, New Zealand, and the United States.

are unable to confirm or rule out any possibility given the data and results that we have. Nevertheless, to rule out the risk of outliers, we also perform Li's (1985) outlier robust estimation and our results are robust (see Table 7A). Therefore, our results are at least not driven by outliers.

In table 8 we use alternative measures of trade liberalization and institutions. In column 1 we use export growth rate as an alternative measure of liberalization and our result survives. Column 2 uses the Free Trade Index compiled by Gwartney and Lawson (2005) of the Fraser Institute. The measure takes into account: (i) taxes on trade, (ii) regulatory trade barriers, (iii) actual size of the trade sector relative to expected size, (iv) black market exchange rates, (v) international capital market controls. The measure ranges from 0 to 10 with a high score implying more freedom to trade internationally. Our result survives this test in case of property rights institutions but not in case of contracting institutions. Columns 3 and 6 check the robustness using tariff. The result survives however we have to settle with a much smaller sample. Finally in column 8 we use executive constraint as an alternative measure of institutions and our result remains unaffected.

We also undertake additional robustness tests using Blundell and Bond GMM estimation method and lagged variables as instruments in an earlier version of the paper. These results survive. We are willing to share these results upon request.

V. Concluding Remarks

In this paper we look at the relationship between trade liberalization and economic institutions (in particular, property rights and contracting institutions) from a within country perspective. Using panel data covering the periods 1865 – 1940 and 1980 – 2000 & 31 and 103 countries respectively, we find that the within variation in economic institutions can be explained

by trade liberalization. To address endogeneity concern we use export partner growth and rainfall as instruments for trade liberalization and log GDP per capita respectively and estimate our model using Limited Information Maximum Likelihood (LIML) Fuller estimation method. Our result holds after controlling for country fixed effects, time varying common shocks, and various additional covariates. It is also robust to various alternative measures of liberalization and institutions, as well as across different samples. We find weak evidence in favor of the 'critical juncture' view of history as the country fixed effects are only weakly correlated with settler mortality and population density in 1500. In contrast latitude seems to have stronger explanatory power.

The IMF (2005) in chapter 3 of World Economic Outlook writes,

"If higher growth depends on better institutions, the key question must be how those better institutions can be built. To date, partly reflecting the weakness of the data on institutions, the economic literature on this topic is very limited." (p. 125)

In this study we aim to fill in this gap by providing evidence to support the role of trade policy in improving institutions. Our results are an important step towards understanding institutional change. The challenge however is to take this beyond the broad framework and work out a detailed understanding of the channels through which trade liberalization impacts institutions. Rodrik (2000b) is perhaps a step in the right direction where he identifies several channels through which trade liberalization leads to importation of institutions from abroad. But more research along this line is certainly called for.

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Appendix A

A.1 Data description

1980 to 2000 period:

Property Rights Institutions (PRINS$_{it}$): Property rights institutions are measured by expropriation risk. Expropriation risk is defined as the risk of "outright confiscation and forced nationalization" of property. This variable ranges from 0 to 10 where higher values are equals a lower probability of expropriation. It is averaged over years $t-4$ to t. Source: ICRG

Contracting Institutions (CONINS$_{it}$): Contracting institutions are measured by risk of repudiation of contracts by the government. This variable ranges from 0 to 10 and a higher value implies better contracting environment. It is averaged over years $t-4$ to t. Source: ICRG.

Executive Constraint (EXCONST$_{it}$): A measure of the extent of institutionalized constraints on the decision making powers of chief executives. The measure ranges from 1 to 7 where higher values equal a greater extent of institutionalized constraints on the power of chief executives. Source: Polity IV dataset.

Trade Liberalization (po$_{it}$): Fraction of years open between $t-4$ and t. The original variable is from Sachs and Warner (1995) and is updated by Wacziarg and Welch (2003). Source: Sachs and Warner (1995).

Trade Liberalization since 1950 (PO$_{it}$): Fraction of years open between 1950 and t. This measure is used as an index of trade liberalization in the long run. Source: Sachs and Warner (1995).

Export Growth Rate: Annualized growth rate of exports calculated using exports data from the World Bank. Source: WDI, World Bank.

Free Trade Index: Freedom to trade internationally takes into account taxes on international trade, regulatory trade barriers, relative size of the trade sector, black market exchange rates, and international capital controls. Source: Gwartney and Lawson (2005).

Log Tariff: Taxes on international trade as a percentage of revenue. Source: WDI, World Bank.

Log Per Capita Income (y_{it}): Log real GDP per capita PPP (current international dollars)/1000, averaged over the years $t-4$ to t. Source: Penn World Table, PWT 6.1.

Log Rainfall: Log of Annual Precipitation. This data is approved by the International Panel on Climate Change (IPCC). *Source:* Mitchell *et al.* (2003).

Export Partner Growth ($\hat{y}_{it}^{XP}$): Weighted average of export partners' GDP growth rates. Weights are based on the share of each export market in a country's total exports in 1995. Source: Constructed using WDI from the World Bank and IMF Direction of Trade statistics.

Log Settler Mortality: Natural log of estimated European settler mortality rate in colonies and settlements. Source: Acemoglu *et al.* (2001).

Log Population Density in 1500: Total population divided by total arable land in 1500 A.D. Source: Acemoglu *et al.* (2001).

Ethno-Linguistic and Reliogious Fractionalization: Probability that two randomly selected individuals from a population belongs to different ethnic, linguistic or religious groups. Source: Alesina *et al.* (2003).

Religion: Percentage of population Catholic, Protestant, Muslim, Buddhist, Hindu, Confucian, and Jew. Source: Hall and Jones (1999).

Latitude: Source: Hall and Jones (1999).

Schooling: Average schooling years of the aged over 25 in the total population. This is measured at five year intervals from 1980-2000. Source: Barro and Lee (2000).

Inequality: Income (in some cases consumption expenditure) Gini coefficient. Source: WDI, World Bank..

Foreign Aid: Official Development Assistance (ODA). Source: WDI, World Bank.

Foreign Direct Investments: Net inflows of Foreign Direct Investment as % of GDP. Source: WDI, World Bank.

1865 to 1940 period:

Executive Constraint ($EXCONST_{it}$): A measure of the extent of institutionalized constraints on the decision making powers of chief executives. The measure ranges from 1 to 7 where higher values equal a greater extent of institutionalized constraints on the power of chief executives. Source: Polity IV dataset.

Export Growth Rate: Annualized growth rate of exports. Source: Clemens and Williamson (2004).

Log Tariff [$\ln(tariff)_{it}$]: Log of average tariff rates calculated by the ratio of total revenue from import duties to the value of imports in the same year. Source: Clemens and Williamson (2004).

Log Per Capita Income (y_{it}): Log GDP per capita measured in 1990 US dollars. Source: Clemens and Williamson (2004).

Log Rainfall: Log of Annual Precipitation. This data is approved by the International Panel on Climate Change (IPCC). *Source:* Mitchell *et al.* (2003).

Export Partner Growth: Weighted average of trading partners' GDP growth rates. Source: Clemens and Williamson (2004).

A.2 Sample

1980 to 2000 period:

Albania, Algeria, Argentina, Australia, Austria, Bangladesh, Belgium, Bolivia, Botswana, Brazil, Bulgaria, Burkina Faso, Cameroon, Canada, Chile, China, Colombia, Congo Dem. Rep., Congo Rep., Costa Rica, Cote d'Ivoire, Cyprus, Denmark, Dominican Rep., Ecuador, Egypt, El Salvador, Ethiopia, Finland, France, Gabon, Gambia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea – Bissau, Guyana, Haiti, Honduras, Hong Kong, Hungary, Iceland, India, Indonesia, Iran, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kenya, Korea Rep., Liberia, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Mexico, Morocco, Mozambique, the Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, The Philippines, Poland, Portugal, Romania, Senegal, Sierra Leone, Singapore, Somalia, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Syria, Tanzania, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, United Kingdom, United States, Uruguay, Venezuela, Yemen Rep., Zambia, Zimbabwe.

<u>1870 to 1940 period</u>:

Argentina, Australia, Austria, Brazil, Canada, Chile, China, Colombia, Cuba, Denmark, Egypt, France, Germany, Greece, Italy, Japan, Mexico, New Zealand, Norway, Peru, the Philippines, Portugal, Russia, Serbia, Spain, Sweden, Thailand, Turkey, United Kingdom, United States, Uruguay,

Table 1: Summary Statistics

Variable	Number of obs.	Mean	Standard Deviation (within countries)	Standard Deviation (between countries)	Standard Deviation (overall)	Minimum	Maximum
Property Rights Institutions ($PRINS_{it}$)	445	6.8	1.5	1.8	2.3	1	10
Contracting Institutions ($CONINS_{it}$)	445	6.3	1.2	1.9	2.2	1	10
Trade Liberalization (po_{it})	1405	0.5	0.4	0.3	0.5	0	1
Trade Liberalization since 1950 (PO_{it})	1384	0.3	0.2	0.3	0.4	0	1
Log Per Capita Income (y_{it})	1684	7.7	0.9	1.1	1.4	4.1	10.9

Table 2. Trade Liberalization and Institutional Development: Correlation Matrix

	Property Rights ($PRINS_{it}$)	Contracts ($CONINS_{it}$)	Trade Liberalization (po_{it})	Trade Liberalization since 1950 (PO_{it})	Log Per Capita Income (y_{it})
Property Rights ($PRINS_{it}$)	1.00				
Contracts ($CONINS_{it}$)	0.90	1.00			
Trade Liberalization (po_{it})	0.64	0.68	1.00		
Trade Liberalization since 1950 (PO_{it})	0.54	0.62	0.85	1.00	
Log Per Capita Income (y_{it})	0.68	0.77	0.63	0.64	1.00

Table 3: Trade Liberalization and Institutional Development

	(1)	(2)	(3)	(4)
	Panel A: LIML Fuller IV Estimates			
	Property Rights Institutions ($PRINS_{it}$)		**Contracting Institutions ($CONINS_{it}$)**	
Trade Liberalization (po_{it})	4.2** (1.80)		3.65** (1.57)	
Trade Liberalization since 1950 (PO_{it})		15.97** (7.91)		13.91** (6.60)
Log Per Capita Income (y_{it})	-1.13 (1.57)	-0.48 (1.82)	-0.04 (1.68)	0.71 (1.35)
Endogeneity test ($p-value$)	0.007	0.008	0.015	0.017
Controls:	Country Dummies, Year Dummies			
Instruments:	Export Partner Growth ($\hat{y}_{it}^{XP}$), Log Rainfall			
Countries	103	103	103	103
Observations	366	366	366	366
	Panel B: First Stage Regressions			
	Trade Liberalization (po_{it})	**Trade Liberalization since 1950 (PO_{it})**	**Log Per Capita Income (y_{it})**	
Export Partner Growth ($\hat{y}_{it}^{XP}$),	0.03* (0.017)	0.01* (0.006)	-0.003 (0.014)	
Log Rainfall	-0.08 (0.071)	0.02 (0.034)	0.07* (0.042)	
F statistic	17.53	78.13	160.4	
Stock – Yogo critical value	15.5	15.5	15.5	
Partial R^2 on instruments	0.32	0.31	0.36	
Controls:	Country Dummies, Year Dummies			
Countries	103	103	103	
Observations	366	366	366	
Adjusted R^2	0.73	0.92	0.96	

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. All regressions are carried out with an intercept. Sample years are every fifth year from 1980 to 2000. Fuller's modified LIML estimator with $\alpha=1$ (correction parameter proposed by Hausman *et al.*, 2005) is used in Panel A which is robust to weak instruments. Endogeneity test for one or more endogenous regressors p-values are reported. The null hypothesis is that the specified endogenous variables can actually be treated as exogenous. Under the null the test statistic follows χ^2-distribution with degrees of freedom equal to the number of regressors tested. Note that Sargan overidentification test is not reported as we have an exactly identified system. Stock –Yogo critical value are based on LIML size and significance level of 5%. An F statistic below the level of Stock –Yogo critical value would indicate that the instruments are weak. Partial R^2 on excluded instruments are also reported which measures instrument relevance.

Table 4: Effects of Colonial History on Institutions

	(1)	(2)	(3)	(4)
	Fixed effects from Property Rights Institutions ($PRINS_{it}$)		**Fixed effect from Contracting Institutions ($CONINS_{it}$)**	
	[column 1, Table 3]		**[column 3, Table 3]**	
Log settler mortality	-0.62*	-0.66	-0.29	-0.05
	(0.345)	(0.434)	(0.244)	(0.383)
Log population density in 1500	-0.42	-1.02*	-0.25	-0.64*
	(0.271)	(0.431)	(0.191)	(0.380)
Colonial History F-test	[0.12]	[0.11]	[0.30]	[0.27]
Ethno-Linguistic and Religious Fragmentation	-2.41	-7.10**	-0.68	-3.11
	(1.94)	(2.43)	(1.37)	(2.14)
Latitude		8.54*		8.68**
		(3.79)		(3.34)
Religion F-test	[0.19]	[0.05]	[0.31]	[0.10]
Observations	34	18	34	18
F-stat	1.38	3.73	0.96	2.46
Adjusted R^2	0.34	0.62	0.26	0.46

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. History F-test reports the p-value for the joint significance of log settler mortality and log population density in 1500. Religion F-test reports the p-value for the joint significance of fraction of Catholic, fraction Protestant, fraction Muslim, fraction Buddhist, fraction Hindu, fraction Confucian, and fraction Jew.

Table 5: Trade Liberalization and Institutional Development over the period 1865 to 1940

	(1) OLS	(2) LIML Fuller IV	(3) LIML Fuller IV	(4) OLS
	**Executive Constraint ($EXCONST_{it}$)**			
Log Tariff [$\ln(tariff)_{it}$]	-0.18** (0.081)	-4.35** (2.06)		-0.69** (0.35)
Export Growth Rate			0.11* (0.066)	
Log Per Capita Income (y_{it})	0.74*** (0.21)	-1.67 (7.46)	0.37 (8.89)	0.21 (0.52)
Endogeneity test (p – value)		0.001	0.21	
Controls:	Country Dummies, Year Dummies			
Instruments:		Export Partner Growth, Log Rainfall		
Countries	31	31	31	31
Observations	403	190	202	254
Year	1865 – 1940	1900 – 1940	1900 – 1940	1865 – 1914
Adjusted R^2	0.74	--	--	0.72

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. All regressions are carried out with an intercept. Sample years are every fifth year from 1900 to 1940 except column 1 when it is 1865 to 1940. Fuller's modified LIML estimator with $\alpha = 1$ (correction parameter proposed by Hausman *et al.*, 2005) is used which is robust to weak instruments. Endogeneity test for one or more endogenous regressors p-values are reported. The null hypothesis is that the specified endogenous variables can actually be treated as exogenous. Under the null the test statistic follows χ^2 -distribution with degrees of freedom equal to the number of regressors tested. Note that Sargan overidentification test is not reported as we have an exactly identified system.

Table 6: Trade Liberalization and Institutional Development: Robustness with Additional Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LIML Fuller IV Estimates									
	Property Rights Institutions ($PRINS_{it}$)					Contracting Institutions ($CONINS_{it}$)				
Trade Liberalization (po_{it})	3.13*** (0.98)	3.44* (1.93)	3.28** (1.49)	1.86*** (0.39)	1.94** (0.93)	2.84*** (0.92)	3.71** (1.87)	2.95** (1.44)	1.44*** (0.36)	1.62** (0.79)
Endogeneity test (p – value)	0.01	0.01	0.007	0.01	0.01	0.008	0.006	0.01	0.01	0.01
Controls:	Log Per Capita Income (y_{it}), Country Dummies, Year Dummies									
Instruments:	Export Partner Growth ($\hat{y}_{it}^{XP}$), Log Rainfall									
Additional Controls:	Schooling*** (+)	Inequality	Foreign Aid*** (-)	Foreign Direct Investments	All Stat. Significant Additional Controls	Schooling** (+)	Inequality	Foreign Aid** (-)	Foreign Direct Investments	All Stat. Significant Additional Controls
Countries	87	93	80	101	65	87	93	80	101	65
Observations	315	293	288	355	243	315	293	288	355	243

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. All regressions are carried out with an intercept. Sample years are every fifth year from 1980 to 2000. Fuller's modified LIML estimator with $\alpha = 1$ (correction parameter proposed by Hausman *et al.*, 2005) is used which is robust to weak instruments. Endogeneity test for one or more endogenous regressors p-values are reported. The null hypothesis is that the specified endogenous variables can actually be treated as exogenous. Under the null the test statistic follows χ^2 -distribution with degrees of freedom equal to the number of regressors tested. Note that Sargan overidentification test is not reported as we have an exactly identified system.

Table 7: Trade Liberalization and Institutional Development: Robustness with Alternative Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	LIML Fuller IV Estimates											
	Property Rights Institutions ($PRINS_{it}$)						Contracting Institutions ($CONINS_{it}$)					
Trade Liberalization (po_{it})	2.08*** (0.49)	0.92*** (0.11)	8.96 (6.69)	3.02** (1.40)	4.44** (1.88)	4.01** (1.99)	1.83*** (0.52)	1.13*** (0.09)	5.9 (4.32)	2.94** (1.42)	3.81** (1.65)	3.46* (1.79)
Endogeneity test (p – $value$)	0.04	0.02	0.00	0.05	0.01	0.01	0.02	0.13	0.01	0.03	0.01	0.02
Controls:	Log Per Capita Income (y_{it}), Country Dummies, Year Dummies											
Instruments:	Export Partner Growth ($\hat{y}_{it}^{XP}$), Log Rainfall											
Omitted Observations	Base sample without Africa	Base sample without Asia	Base sample without the Americas	Base sample without Europe	Base sample without Neo-Europe	Base sample without OECD	Base sample without Africa	Base sample without Asia	Base sample without the Americas	Base sample without Europe	Base sample without Neo-Europe	Base sample without OECD
Countries	70	85	79	78	99	75	70	85	79	78	99	75
Observations	246	297	272	289	353	276	246	297	271	289	353	276

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. All regressions are carried out with an intercept. Sample years are every fifth year from 1980 to 2000. Base sample for $PRINS_{it}$ is column 1, table 3 and for $CONINS_{it}$ is column 3, table 3. Fuller's modified LIML estimator with $\alpha = 1$ (correction parameter proposed by Hausman *et al.*, 2005) is used which is robust to weak instruments. Endogeneity test for one or more endogenous regressors p-values are reported. The null hypothesis is that the specified endogenous variables can actually be treated as exogenous. Under the null the test statistic follows χ^2-distribution with degrees of freedom equal to the number of regressors tested. Note that Sargan overidentification test is not reported as we have an exactly identified system.

Table 7A: Trade Liberalization and Institutional Development: Robustness with Outliers

	(1)	(2)
	Li's (1985) Outlier Robust Estimates	
	Property Rights Institutions ($PRINS_{it}$)	**Contracting Institutions ($CONINS_{it}$)**
Trade Liberalization (po_{it})	1.69*** (0.19)	1.10*** (0.21)
Log Per Capita Income (y_{it})	1.33*** (0.25)	1.77*** (0.29)
Controls:	Country Dummies, Year Dummies	
Countries	103	103
Observations	366	366

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. All regressions are carried out with an intercept. Sample years are every fifth year from 1980 to 2000. Estimates are based on Li's (1985) outlier robust estimator.

Table 8: Trade Liberalization and Institutional Development: Robustness with Alternative Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	LIML Fuller IV Estimates						
	Property Rights Institutions ($PRINS_{it}$)			Contracting Institutions ($CONINS_{it}$)			Executive Constraint ($EXCONST_{it}$)
Export Growth Rate	0.12** (0.059)			0.11** (0.055)			
Free Trade Index		0.90* (0.56)			0.63 (0.54)		
Log Tariff			-0.47* (0.29)			-0.88*** (0.32)	
Trade Liberalization (po_{it})							3.23* (1.82)
Endogeneity test ($p-value$)	0.69	0.59	0.29	0.69	0.71	0.29	0.04
Controls:	Log Per Capita Income (y_{it}), Country Dummies, Year Dummies						
Instruments:	Export Partner Growth ($\hat{y}_{it}^{XP}$), Log Rainfall						
Countries	105	94	49	105	94	49	129
Observations	357	329	87	357	329	87	845

Notes: ***, **, and * indicates significance level at 1%, 5%, and 10% respectively against a two sided alternative. Figures in the parentheses are cluster standard errors and they are robust to arbitrary heteroskedasticity and arbitrary intra-group correlation. All regressions are carried out with an intercept. Sample years are every fifth year from 1980 to 2000 except for column 7 when it is 1965 to 2000. Fuller's modified LIML estimator with $\alpha = 1$ (correction parameter proposed by Hausman *et al.*, 2005) is used which is robust to weak instruments. Endogeneity test for one or more endogenous regressors p-values are reported. The null hypothesis is that the specified endogenous variables can actually be treated as exogenous. Under the null the test statistic follows χ^2-distribution with degrees of freedom equal to the number of regressors tested. Note that Sargan overidentification test is not reported as we have an exactly identified system.