Trade and Cities

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Abstract

Many developing countries display remarkably high degrees of urban concentration, incommensurate with their levels of urbanization. The cost of excessively high levels of urban concentration can be very high in terms of overpopulation, congestion, and productivity growth. One strand in the theoretical literature suggests that such high levels of concentration may be the result of restrictive trade policies that trigger forces of agglomeration. Another strand in the literature, however, points out that trade liberalization itself may exacerbate urban concentration by favoring the further growth of those large urban centers that have better access to international markets. The empirical basis for judging this question has so far been weak: in the existing literature, trade policies are poorly measured (or not measured as when trade volumes are used spuriously). Here, we use new disaggregated tariff measures to empirically test the hypothesis. We also employ a treatment-and-control analysis of pre- versus post-liberalization performance of the cities in liberalizing and non-liberalizing countries. We find evidence that, controlling for, among others, largest cities that have ports and, thus, have better access to external markets, liberalizing trade does lead to a reduction in urban concentration. Finally, by using a cross-country level of analysis we provide some external validity to the more careful empirical studies that rely on single country data.

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1 Introduction

How does trade liberalization affect urban concentration? This is an important question because many developing countries display a remarkable degree of urban concentration and protectionist trade policy has been suggested as one possible cause, resulting in one or two cities overshadowing all other urban areas in a given country. Figures 1a and 1b offer some suggestive examples using two measures of urban concentration, namely percentage of urban population in the largest city and Herfindahl index of city populations, for some developing (and developed) economies in 1985. The concentrations observed are by no means recent phenomena. Around 1930, when developing market economies had an average level of urbanization of around twelve percent, sixteen percent of their urban population lived in fourteen large cities that had populations of more than half a million. Similar levels of urban concentration in the developed world had been attained in 1880, when its average level of urbanization stood much higher at twenty three percent. The number of the large cities in the developing world as well as their share of the total urban population increased radically between 1930 and 1980, by which date they had 43% of the urban population, a number which paralleled that of the developed countries. However, the level of urbanization in the latter stood at 65% whereas developing market economies had an urbanization level half of that.[1] Furthermore, as a recent survey puts it "[s]ince primate cities are invariably national capitals, they are centers of decision-making and opinion-forming. They are thus able to dominate their countries both economically and politically" (Balchin et al. 2000, p. 64).

Policymakers and international agencies are concerned about the cost of overpopulation, congestion, crime, and "unbalanced urban hierarchies" in these megacities.[2] The literature in urban and development economics points out that though a high degree of urban concentration might be useful in early stages of development by conserving on economic infrastructure and enhancing information spillovers at precisely the point when infrastructure and information are at a premium, it results in a misallocation of resources at later stages of development.[3] This is because once a certain level of urban concentration is attained, economies of scale get exhausted and mega-cities transform into sites that are excessively congested with high infrastructure costs. The consequences of this misallocation are not only static but dynamic. For instance, Henderson (2003) provides evidence that supports the notion that excessive urban concentration has significant

[1] For these numbers, see Bairoch (1988).

[2] See, for instance, UN (1993) and the World Development Report (2000).

[3] See Williamson (1965) and Hansen (1990).

negative effects on productivity growth.

Given the importance of the consequences of excessive urban concentration, the natural question to explore is its causes. We now have an extensive literature that argues that observed levels of urban concentration arise from the nature of political institutions and the policy choices that follow (Ades and Glaeser, 1995; Krugman and Livas, 1996; Henderson and Becker, 2000; Davis and Henderson, 2003). Here, one argument is that national governments may favor certain cities over others. The favorites may be capital cities (Mexico City, Seoul, London or Paris) or the traditional seats of the elites (Istanbul or Sao Paulo). Such favoritism may take the form of underinvestment in provincial transport or telecommunications networks, restrictions in financial markets and transactions, preferential treatment of elites in favored cities in the allocation of licenses, quotas, production and trading rights, as well as the disproportionate provision of local public services.[4]

Another argument proffered along these lines, and the one which we empirically test in this paper, is that mega-cities may arise from the restrictive trade policies adopted. The literature on the effects of trade policy on urban concentration consists of two generations of models. The "new" generation of models differs in two respects from the older generation. It relaxes the assumption of perfectly competitive markets favored by the older generation and endogenizes regional scale economies that remain exogenous in the older models. Both generations contain models that either assume locations within countries to be identical or introduce some sort of nonhomogeneity in inherent characteristics across locations.

With identical locations across the national space, the effects of trade on urban concentration work through different channels depending on the specifications adopted in a given model. The early literature as exemplified by Henderson (1982) finds that with perfect competition and external regional economies of scale, protection applied to industries in large cities raises urban concentration by attracting resources to these industries. In the New Economic Geography (NEG) literature where markets are taken to be monopolistically competitive and economies of scale are endogenized, whether trade liberalization leads to more or less urban concentration depends on the relative strength of the agglomeration and dispersion forces introduced. In our context it is useful

[4]Through several political economy channels, excessive urban concentration may in turn have negative consequences on economic outcomes. Karayalcin and Ulubasoglu (2011) provide evidence that the stifling of political competition in economies with high urban concentrations lead to low developmental outcome measures.

to think of the agglomeration forces coming into play in the following manner. When trade barriers are high, monopolistically competitive firms that produce for the domestic market prefer to locate as close to a large number of consumers (backward linkages) found in a metropolis. Firms would also prefer the metropolis as it would offer better access to other firms that supply inputs for the production process and consumption goods for their workers (forward linkages). Trade liberalization would then increase the share of goods bought from and sold to abroad and thus reduce the strength of the backward and forward linkages. To the extent that different cities have similar access to foreign markets and goods, trade would then lead to a weakening of the logic of agglomeration and to the dispersion of firms and consumers across urban centers. Other things being equal, dispersion forces impose a limit on how far urban concentration would be able to go. In Krugman and Livas (1996) these take the form of exogenous urban congestion costs which are independent of the level of trade and are dominated by agglomeration forces. Behrens et al. (2007) introduces two additional forces of dispersion. One arises from the assumed immobility of some workers ("farmers") across regions that would induce firms and mobile industrial workers to spread out to be close to the farmers to avoid the costly long-distance shipment of food or manufactured goods. This is the dispersion force of the original Krugman (1991) model. A second one arises from the assumption that markups fall with the intensity of local competition.[5] Thus, firms would prefer to spread out spatially to avoid reduced profits caused by lower markups in cities with high firm concentrations. Papers, such as Monfort and Nicolini (2000) and Paluzie (2001), that predict that trade liberalization, once it exceeds a certain threshold, would induce higher levels of urban concentration rely on the intensity of the dispersion forces falling faster than that of the agglomeration forces. Papers, such as Krugman and Livas (1996) and Behrens et al. (2007), that reach the opposite conclusion have built in to their structure the reverse configuration of the two opposing forces.

With locations that differ in some dimension from others additional considerations arise. Rauch (1991), working in the perfectly competitive setup introduces differential trade costs across cities. In autarkic equilibrium the location of cities would be inconsequential with the result that all cities would be of equal size. When trade costs are at an intermediate level, cities with lower trade costs (border cities, port cities) would be bigger than the internal cities. Further trade liberalization would lead to even larger cities at the border and a higher level of urban concentration. Mansori (2003) obtains a similar result within the NEG framework as the cost of access to foreign markets

[5]This is the assumption introduced in Ottaviano et al. (2002).

provides another channel through which agglomeration forces reveal themselves. Bruelhart et al. (2004) and Crozet and Konig (2004) build models that show that trade liberalization may attract domestic firms to the border (or port cities) which have while lower trade costs, these firms may also move to the interior regions where they face less competition from foreign firms. Thus, once again whether trade liberalization increases urban concentration becomes an empirical question.[6]

The mechanisms discussed so far operate in static setups. Trade liberalization, however, has dynamic consequences mainly because it raises the rate of growth of GDP. The seminal work of Williamson (1965) argued that we should expect there to be a non-monotonic relationship between rising income levels and urban concentration. At low levels of income urban concentration would be high as this would help conserve expenditure on infrastructure and enhance information spillovers at a point when the economy suffers from a severe scarcity of infrastructure and information. With higher incomes, it becomes possible to spread the infrastructure and information into the hinterland, while rising costs in congested urban areas push producers and consumers out of these erstwhile centers. This pattern of income growth, resulting initially in higher and later in lower urban concentration, is supported by a number of empirical studies (El-Shaks 1972; Rosen and Resnick 1980; Wheaton and Shishido 1981; Mutlu 1989; Ades and Glaeser 1995; Junius 1999; Davis and Henderson 2003; and Moomaw and Alwosabi 2004).

The question as to whether trade liberalization intensifies the forces of urban agglomeration or dispersion, then, becomes an empirical one. The empirical literature on the subject may be divided into two groups.[7] The first group relies on cross-country regressions, while the second one studies heterogeneous responses of different regions within a country. One remarkably consistent finding that emerges from the first group is that trade openness has no statistically significant effect on urban concentration. The results obtained by the studies in the second group are mixed, with half of the fourteen papers surveyed in Bruelhart (2011) finding support for the hypothesis that trade openness is associated with spatial divergence and three papers suggesting the opposite. A more careful recent study in this group by Redding and Sturm (2008), which looks at the effects of the loss of trading partners triggered by the division of Germany on urban

[6]See also Hanson (1998, 2001). There is now also a small literature (see Cosar and Fajgelbaum (2012) and Allen and Arkolakis (2013)) that explores the link between trade and the spatial distribution of population with forces that are distinct from the agglomeration ones in play in the new economic geography literature.

[7]Here we follow the recent survey of this literature by Bruelhart (2011).

concentration, finds that trade reduces urban concentration.

To understand these results it is useful to start with the second group that relies on within-country data using a single country as its focus (and, thus faces the standard external-validity problem). Here the typical measure of spatial concentration is either the level or the rate of growth of regional GDP per capita (and in some cases the region-industry share of employment). As for the measure of trade openness, it needs to be noted that half of the papers in this group use Mexican data and use the trade liberalization episode associated with NAFTA to identify the change in policy. The finding of spatial divergence in the Mexican case is easily explained by the observation that liberalization shifted economic activity to regions bordering the USA. As these regions were relatively more industrialized and richer than the rest of Mexico prior to liberalization, it is not surprising to find that trade exacerbated regional inequalities in general. The instructive exception in this group is Sanguinetti and Martincus (2009) who find that those Argentinian manufacturing sectors that received the largest tariff reductions in the 1985–1994 period tended to have their employment grow faster in regions that are not usually associated with the traditional sites of manufacturing activity in and around the main port and largest city, Buenos Aires. This result is also important because unlike most of the non-Mexican papers in this group, Sanguinetti and Martincus use changes in tariff rates and do not depend on such endogenous measures of trade openness as trade-to-GDP ratios. The same cannot be said for the vast majority of the papers in the first group. There, starting with Rosen and Resnick (1980) and Ades and Glaeser (1995), the standard measure of trade openness is the trade-to-GDP ratio. As pointed out in Rodriguez and Rodrik (2000) using an outcome variable such as trade-to-GDP (or imports-to-GDP) is inappropriate if we want to go beyond general correlations and explore the causal effects of trade liberalization on urban concentration (spatial convergence). This is because both trade (or imports) and GDP are endogenous variables and causal economic identification of the effect of changes in trade policy requires exogenous instruments that are correlated with trade but not with urban concentration. This is recognized in Ades and Glaeser (1995) where trade openness (as measured by trade-to-GDP ratio) loses its significance in IV regressions, thereby placing its causal effect on urban concentration in question.

In this paper, we take the question of causality seriously and differ from the existing literature by avoiding the use of endogenous "outcome" measures (like trade volume) that do not correspond to any trade policy measure that is directly controlled by policymakers. We tackle these issues by adopting an improved methodology and data set to study the effects of trade liberalization on urban concentration (spatial convergence). We look for tariff measures that are controlled by policy-makers and implement tests using continuous treatment measures. We try to answer the right policy question and attend to problems of causality and identification while avoiding biases by using a difference-in-difference approach. To put it differently, we are concerned with a treatment-and-control partition of countries based on their engagement in trade liberalization, and we test whether the liberalizers experienced a reduction in urban concentration.[8] Our "policy experiment" approach relies on identification in the time dimension rather than in cross section. Our trade openness data are the new and detailed Estevadeordal and Taylor (2013) tariff data on consumption, capital, and intermediate goods gathered from primary sources (based on digital sources for recent years, but also on archival sources for the 1980s that have not been used so far). Based on an empirical identification strategy where we first use a continuous treatment measure (changes in various tariffs) with a difference-in-difference design and then construct two instrumental variables to address endogeneity concerns, we find a significant correlation between tariff reductions and declines in urban concentration following the "Great Liberalization" experiment of the Uruguay Round. The results we obtain are robust to many alternative estimation methodologies and consideration of alternative explanatory variables and can perhaps be best visualized as in Figure 2. In that figure we trace the level of urban concentration (using the same measure as in Figure 1a) over the last 30 years for both the liberalizers and the non-liberalizers. As the figure shows ex ante (before the Uruguay round), the level of urban concentration of the treatment group (liberalizers) tracks that of the control group (non-liberalizers) very closely, with there being barely any discernible difference. If our argument is valid we should see a significant divergence after the treatment and this is exactly what we observe in Figure 2. With the Uruguay round of liberalization there starts a dramatic divergence in the levels of urban concentration of the two groups, with the treatment group of liberalizers seeing a significant decline in its level of urban concentration relative to that of the control group of non-liberalizers.

In the next sections, we first develop our estimation methodology and discuss the data in detail, where we rely on statistical methods of the treatment-control type that are designed to avoid the typical problems that arise in cross-section methods; we also address endogeneity concerns using novel arguments, given the fact that standard instruments are not useful in this context. In the final main section, we discuss our estimation

[8] We should emphasize that our focus here is squarely on urban concentration and not the more general question of regional disparities.

results. A concluding section ends the paper.

2 Estimation Methodology and Data

In this section, we present the estimation methodology and data, which take a different route from the previous empirical literature on the subject. Here, we take the question of the relation between trade openness and urban concentration as being a question of the causal effects of a change in policy. In other words, we are interested in the consequences of the policy of trade liberalization on urban concentration. To answer this question, we have an empirical design in mind that considers post-1990 trade liberalization as a treatment. Following Estevadeordal and Taylor (2013; ET hereafter), we implement this design by employing two methods. The first of these methods takes openness as a continuous treatment and uses tariff rates as a proxy for openness in regressions in differences. The advantage of using difference estimators is well-known: they avoid the problems associated with omitted variables as long as the omitted regressors do not change over time. To the extent that these regressors are time-invariant country characteristics, for example institutions that remain little changed over the medium run, this method is helpful in addressing the bias associated with omitted variables. The second method we use is an instrumental variables approach that enables us to address potential endogeneity issues.

2.1 Openness as a continuous treatment

The literature so far has asked the question: do higher levels of trade increase or decrease urban concentration, all else being equal? Given the impossibility of including all the relevant controls, it is not surprising that the results obtained in the literature are fragile and indeterminate, being marred by omitted variable bias. To this, one also needs to add the fact that the vast majority of the papers use endogenous measures, such as trade-to-GDP ratios, for trade openness that renders causal economic identification of the effects of trade policies impossible.

Here, we follow an alternate strategy that takes post-1990 trade liberalization as a treatment. The question we consider is: do the rates of growth of population of cities in a given country accelerate relative to that of the largest city in liberalizing countries (the treatment group) as compared to non-liberalizing ones (the control group)? This way of posing the question not only leads to a cleaner empirical design but also naturally points to an estimation that involves differences in growth rates of cities, which, in turn, has the advantage of dealing with omitted-variable bias by eliminating country-specific fixed effects through differencing.

Of course, for this empirical design to work, there needs to be a group of countries that were subject to treatment. ET cogently argue that the Uruguay round 1986-1994 provided exactly this kind of treatment: prior to the Uruguay round there were very few developing countries that underwent any serious trade liberalization, whereas the 1986-1994 round involved 125 countries (developed and developing) that chose to reduce tariff barriers substantially. Another group of countries (the control group) had either low tariffs to begin with and left them low, or had high tariffs and kept them high or imposed even higher ones.[9]

Using the empirical design described, together with the data to be defined below, we use the fact that changes in tariffs during the Uruguay round provide a continuous treatment and run the following regression:

$$\Delta \ln p^{i,j} = \alpha \Delta \ln \left(1 + t^j\right) + \beta \ln p^{i,j}_{1985} + c \tag{1}$$

where the dependent variable is the change in a city-specific urban concentration measure calculated as the rate of growth of the population of the i^{th}most populous city relative to the rate of growth of population of the largest city in country j during the trade liberalization period of 1985-2000 defined as:

$$\Delta \ln p^{i,j} = \left(\ln p^{i,j}_{2000} - \ln p^{i,j}_{1985}\right) - \left(\ln p^{1,j}_{2000} - \ln p^{1,j}_{1985}\right)$$

where p^i_m is the population of the i^{th} largest (i.e., most populated) city in country j in year m. In order to capture convergence effects, we include the log initial population of the i^{th} largest city, $\ln p^{i,j}_{1985}$, as an independent variable in the regression. We also include a constant c to capture the scale effects.

We want to measure the effects of a change in openness measured by a tariff change defined as:

$$\Delta \ln \left(1 + t^j\right) = \ln \left(1 + t^j_{2000}\right) - \ln \left(1 + t^j_{1985}\right)$$

where the tariff measure t^j for country j is the average of the tariffs for imports of capital and intermediate inputs.

The regression equation suggests that if smaller cities (i.e., cities other than the largest city) have grown faster than the largest city in their country (i.e., if $\Delta \ln p^{i,j} > 0$

[9] See ET for a detailed discussion and list.

on average across i) due to a decrease in tariff rates (i.e., if $\Delta \ln(1+t^j) < 0$), we would expect to have a negative and significant α estimate. The log initial population of the i^{th} largest city $\ln p^{i,j}_{1985}$ has been included in the regression to capture the convergence effects among small cities, because a small city may grow faster than a bigger city (where a bigger city is not necessarily the largest city); hence, the coefficient in front of log initial population β has an expected negative sign as well.[10]

We employ two alternative estimation methods, Ordinary Least Squares (OLS) and Two Stage Least Squares (TSLS). While OLS is our benchmark method, we employ TSLS to consider possible endogeneity issues. These issues arise because it may be the case that, for instance, tariff policy and urban concentration might just be reflections of a deeper causal variable such as institutions. In this view, economic and political institutions would have a causal effect on urban concentration and trade (and other) policies, which would then causally affect urban concentration further. Though it is hard to deny the purchase of such arguments when one is concerned with levels (cross section), given the slow rate of change in and persistence of institutions over time commonly found in the recent empirical literature (see Acemoglu et al., 2001 and 2011), one would expect that these concerns would not be valid in differences (time series). In fact, ET show that in the sample used here, there exists neither a clear, nor a robust relationship between institutional changes and changes in trade policy.

However, given the fact that trade policy is a choice variable and therefore endogenous, there still remains the need for a source of exogenous variation in the trade policies of 1980s and 1990s. Here we again follow ET in taking the view that the biggest exogenous shock to trade policy for the last century was the shifts in these policies in the 1930s triggered by the Great Depression. As a whole, the argument goes, the world moved away from liberal economic policies in the interwar period. Thus not only were tariffs much higher in 1945 than in 1913 in most countries, but quotas, which had hardly been used prior to World War I, were in wide use by the end of World War II. The creation of GATT in 1947 and much later WTO in 1995 introduced two international institutions charged with the reinstatement of the world trading system. Most developing countries, however, remained highly protectionist and only a small minority of these took part in any serious sustained trade liberalization until the Uruguay round, maintaining until

[10]One important detail here is that the i'th largest city in the pre-liberalization period may turn out not to be the i'th largest city in the post liberalization period. This does not present a problem for our analysis as we are interested in the overall ranking of cities rather than the identities of particular cities in the ranking.

that point with tariffs the levels of which dated back to the policy shift of the 1930s.

To see how this history helps us in addressing the possible endogeneity of our treatment variables, note that, following ET, we would argue that an exogenous component can be constructed in the following manner. We first observe that the interwar shocks led all countries towards more protectionist policies. The degree and the duration of protectionism each country adopted, however, depended on the size of the exogenous shock they were subjected to by the Great Depression. Thus, those countries that suffered less from the Great Depression had relatively lower tariffs and less persistent protection later on. Furthermore, for a country to be able to see a big cut in tariffs later on, it had to not only be willing to cut them, it also had to have high tariffs to cut in the first place. These considerations are taken into account in the construction of two alternative country-specific instruments (I_1^j and I_2^j) called "GATT Potential," to be used as predictors of the ability and willingness of a country reduce tariffs under the Uruguay round. The first of these instruments is defined as:

$$I_1^j = \ln\left(1 + t_{1985}^j\right) \times [\text{GATT member in 1975}]$$

This is an indicator variable that is the product of two measures that would likely promote trade liberalization. It is defined as the interaction of the country's ability (proxied by pre-Uruguay level of tariffs) and willingness (proxied by 1975 GATT membership) to cut tariffs in the Uruguay round. For a country to institute a significant reduction in tariffs it had to have high tariffs to begin with and had to enter the Uruguay round with the willingness to actually cut the tariffs. One could perhaps question the validity of this instrument by arguing that the decision to enter GATT by 1975 might be correlated with the decision to reduce tariffs in the Uruguay round later. If this were the case the exclusion restriction might not hold. We would then have to search for a deeper and perhaps historically more distant determinant of the policy stance towards trade reform. Based on the political economy literature, ET argue that this deep determinant can be found in the variance of the intensity of the shock suffered by different economies during the Great Depression. Reading the historical record as providing evidence for the depth of the Great Depression shock predicting the speed of trade liberalization roughly five decades later, we construct our second instrument as the interaction of the intensity of Great Depression (as measured by the average deviation of 1930-35 GDP level from 1929 level) with again the pre-Uruguay tariff level.

$$I_2^j = \ln\left(1 + t_{1985}^j\right) \times \left[\begin{array}{c}\text{Average deviation of 1930-35} \\ \text{GDP level from 1929 level}\end{array}\right]$$

The exclusion restriction for this instrument is expected to be valid a priori because of two reasons: (1) the distance in time between the 1930s and the 1990s is long enough, and (2) there is no direct link between urban concentration levels of the 1930s (which were affected by several factors, such as terms of trade shocks, specific to that era) and those of 1980s.

Given the logic behind our instruments, we run the following regressions as the first stage of TSLS:

$$\Delta \ln \left(1+t^{j}\right) = \gamma I_{k}^{j} + \varphi \ln p_{1985}^{i,j} + c \text{ for } k = 1,2$$

where the log initial population $\ln p_{1985}^{i,j}$ is the exogenous variable in the analysis. The coefficient γ in front of the instruments representing the "GATT Potential" has a negative expected sign, because higher "GATT Potential" leads to higher tariff reductions. The $R-$squared value of this first-stage regression, together with the corresponding F-test, can be used as an indicator for the strength of our instruments.

Our benchmark regression does not control for other confounding changes that could be taking place within countries and could potentially affect urban concentration. Accordingly, in our first robustness analysis, we consider additional explanatory variables (namely country-specific economic growth, country-specific economic growth squared, dummy variables capturing the largest city being the capital city and/or a port city, country-specific log initial domestic transportation infrastructure, and country-specific regime change) that we will further define, below, and the regression equation is revised as follows:

$$\Delta \ln p^{i,j} = \alpha \Delta \ln \left(1+t^{j}\right) + \mu_{y} \Delta \ln \left(y^{j}\right) + \mu_{y^{2}} \left(\Delta \ln \left(y^{j}\right)\right)^{2} + \mu_{x} X_{p}^{i,j} + \beta \ln p_{1985}^{i,j} + c \quad (2)$$

where y^{j} represents GDP per capita, and μ_{x} is a vector of coefficients capturing the effects of exogenous explanatory variables (i.e., additional explanatory variables other than growth and growth squared) denoted by the matrix of $X_{p}^{i,j}$. We included economic growth squared besides economic growth in order to capture any nonlinear relation between the change in urban concentration and economic growth.[11] In particular, Henderson (2000) shows that urban concentration increases with per capita income up to a certain level, declining thereafter.

Within these additional explanatory variables, the only concern is the possible endogeneity of the country-specific economic growth. Therefore, in the TSLS estimation

[11] It is important to emphasize that we also considered only the rate of growth itself, however it was econometrically insignificant. Such results are available upon request.

of the robustness analysis, besides instrumenting the tariff change according to the first stage regression of:

$$\Delta \ln \left(1 + t^j\right) = \gamma I_k^j + \varphi_x X_p^{i,j} + \varphi_p \ln p_{1985}^{i,j} + c \text{ for } k = 1, 2$$

we also instrument country-specific economic growth $\Delta \ln (y^j)$ according to the following first stage regression:

$$\Delta \ln \left(y^j\right) = \theta_y X_y^{i,j} + \theta_x X_p^{i,j} + \theta_p \ln p_{1985}^{i,j} + c \tag{3}$$

where θ_y is a vector of coefficients capturing the effects of standard explanatory variables in growth regressions (i.e., instruments in this paper) denoted by the matrix of $X_p^{i,j}$ that include log initial per capita income, log initial schooling, log initial institutions[12], and log initial tariff rate; $X_p^{i,j}$ and $\ln p_{1985}^{i,j}$ enter the equation as exogenous variables.

In our benchmark regressions, in order to have a healthy comparison across the regression results, we use information from all cities in our sample where the number of cities differ across countries and some countries are ignored due to the availability of the data for instruments; for sure, we also consider a robustness analysis in which we use all the available information in the data set. In an alternative robustness analysis, we treat all countries symmetrically by using the same number of cities from each of them. Since each country has a different number of cities in our sample, there is a tradeoff between the maximum number of countries and the maximum number of cities from each country; accordingly, in this robustness analysis, we consider all possible number of cities (up to 80) from each country. We also consider another robustness analysis in which we weight the information coming from each city of a particular country by the inverse of the number of cities from that country.

It is important to emphasize that, in our regressions, we also account for within-group dependence in estimating standard errors of regression parameter estimates at the country level. We achieve this by using (and providing the p-values for) the wild cluster bootstrap-t method developed by Cameron et al. (2008) who show that the wild cluster bootstrap-t method is superior to its alternatives, such as using the cluster-robust standard errors, especially when the number of clusters is low with respect to the sample size as in this paper.

[12]These are among the exogenous control variables that are robustly partially correlated with economic growth as suggested by Barro (1991) and Sala-i-Martin et al (2004).

2.2 Data

Since we would like to test whether the liberalizers have experienced a reduction in urban concentration, we need measures of liberalization and urban concentration. We measure liberalization by the change in tariffs between pre-liberalization and post liberalization periods (i.e., by $\Delta \ln(1 + t^j)$, above). For urban concentration, earlier literature has typically used the population in largest city (and its share of urban population). This measure tends to ignore useful information about the dynamics of urban concentration at lower levels of the distribution. Here, we consider an urban concentration measure at the city level to capture the interactions among urban centers. Our (change in) urban concentration measure employs the differences in growth rates of a given number of cities from that of the largest city (i.e., $\Delta \ln p^{i,j}$, above). For example, for the U.S., in our benchmark case, we look at the differences between the rates of growth of populations of all other cities from that of New York City. This is similar to the measure recently used by Redding and Sturm (2008) who study the effects of the loss of trading partners triggered by the division of Germany on urban concentration by focusing on the differences in the rates of growth of population of border and internal cities.[13]

We use the following data for our empirical analysis.[14]

Tariffs: The country-specific tariff data are from ET, who have compiled data on disaggregated Most Favored Nation (MFN) applied tariffs for two eras that we use as benchmarks: a pre-liberalization period circa 1985 (in practice, between 1985 and 1993), and a post liberalization period circa 2000 (in practice, between 1999 and 2004).[15] For robustness, we consider three different tariff measures for imports of capital, intermediate inputs, and consumption. The corresponding tariff rates, before and after liberalization, are given in Figures 3-5.

City Populations: The city-level population data refer to populations of agglom-

[13]Redding and Sturm (2008) find that loss of trade leads to more urban concentration as this led to a slower rate of growth of population for border cities. This finding is similar to ours in that we find that creation of trade leads to less urban concentration.

[14]The list of countries is as follows: Algeria, Argentina, Australia, Austria, Bangladesh, Belgium, Bolivia, Brazil, Canada, Chile, China, Colombia, Cote d'Ivoire, Denmark, Ecuador, Finland, France, Germany, Ghana, Hong Kong, Iceland, India, Indonesia, Israel, Italy, Japan, Korea, Malaysia, Mexico, Morocco, Nepal, Netherlands, New Zealand, Pakistan, Paraguay, Peru, Philippines, Spain, Sri Lanka, Sweden, Taiwan, Thailand, Trinidad and Tobago, Turkey, United Kingdom, United States, Uruguay, Venezuela.

[15]ET show that tariff rates in liberalizing countries have started to decline prior to the signing of the agreement and that the decline has accelerated with it. See Figure 3 in ET.

erations/metropolitan areas that include a central city and neighboring towns (suburbs) forming a connected region of dense, predominately urban population that is economically and culturally linked to the central city (e.g. by commuters).[16] The data have been downloaded from http://www.populstat.info/, http://world-gazetteer.com/, and http://www.citypopulation.de/ for the pre-liberalization period circa 1985 (in practice, between 1980 and 1994) and the post liberalization period circa 2000 (in practice, between 1995 and 2004).[17]

Instruments: In order to create country-specific instruments for tariff reductions under the Uruguay round of GATT, we use (i) GATT membership data of Rose (2004)[18], and (ii) historical GDP data of Angus Maddison covering GDP of countries (in our sample) between 1929 and 1935.[19]

We use the following additional data in our robustness analysis.

GDP Per Capita: The country-specific GDP per capita data have been obtained from PWT (rgdpch) for the years of 1985 and 2000.

Schooling: The country-specific measure of human capital has been proxied by the total years of schooling obtained from Barro and Lee (2000). We use the log initial version of the data in the first-stage growth regression.

Institutions: The country-specific institutional quality is measured by the EFW legal and property rights score (variable area 2). We use the log initial version of the data in the first-stage growth regression.

Capital City Dummy: The capital city dummy takes a value of 1 when the largest city in a country is also the capital city of the country as in Ades and Glaeser (1995) and Storeygard (2012).

Port Dummy: The port dummy takes a value of 1 when the largest city in a country has a seaport. This dummy variable has been constructed by the authors by checking the existence of a port in the largest city of each country in the sample. If this is the case, the Rauch (1991) argument suggests that trade liberalization would shift resources and population to the largest city as it benefits from its increased access to

[16]Given the nature of an urban agglomeration, there is an unavoidable measure of arbitrariness in the determination of its boundaries in any data set.

[17]Country-specific details of the data set are given in the Appendix where we depict the exact dates and sources of data for tariff rates and city-level populations for each country in our sample for the periods of pre liberalization and post liberalization. In the Appendix, we also included a table showing the representativeness of our country sample.

[18]GATT membership data of Rose (2004) has been obtained from faculty.haas.berkeley.edu/arose/.

[19]Historical GDP data of Angus Maddison has been obtained from http://www.ggdc.net/MADDISON/.

foreign markets as a port city. Consequently, we would expect that urban concentration as we measure it will rise with trade liberalization.

Initial Domestic Transportation Infrastructure: We use the percentage of roads paved (obtained from World Development Indicators) in 1985 to measure the initial quality of the transportation infrastructure in each country in the sample. This variable allows us to control for the ease with which resources can move across the cities in a given country. Higher transportation costs associated with poorer infrastructure create incentives for the concentration of economic activity in a smaller number of cities.

Regime Change Dummy: This is a dummy variable taking a value of 1 when dictatorship ends in a country before 1985. Following Ades and Glaeser (1995), we accept a country as a dictatorship when its Gastil index is higher than 3. Therefore, countries switch from a dictatorship to democracy when the Gastil index of a country decreases from above 3 (in 1970-1974) to below 3 (in 1980-1984). We use the Gastil index as documented in Barro and Lee (1994). This variable is important for our purposes because the literature (see Ades and Glaeser (1995), for instance) has documented a significant and robust positive relationship between levels of urban concentration and dictatorships.

3 Estimation Results

The regression results for our benchmark case are given in Table 1 where the sample is the same across different regressions. Estimates of α are negative and significant using any estimation methodology for all types of tariffs (except for the tariff change in consumption goods when TSLS using the first instrument is employed[20]). For instance, when the tariff change in capital goods is considered, the significantly estimated α by OLS is about -0.71, suggesting that when tariffs are reduced by 1%, on average, the cities that are smaller than the biggest city grow 0.71% faster than the biggest city in the same country over the fifteen year period between 1985 and 2000. Since the average tariff change in capital goods is about 12%, on average, smaller cities have grown about 8.4% faster than the biggest city in their countries between 1985 and 2000 (which comes to 0.56% per annum). Similar comparisons can be calculated for alternative tariff rates and

[20]Changes in the tariffs for consumption goods would in general be expected to affect urban concentration differently than changes in the tariffs for intermediate and capital goods. This is because access to intermediates is more relevant for urban agglomerations where backward and forward linkages between firms matter as in Krugman and Livas (1996).

estimation strategies. The estimates remain significant when the wild cluster bootstrap-t method (to account for within-group dependence at the country level) is considered for which the p-values are depicted. Overall, these results suggest that trade liberalization has led smaller cities to grow faster than the largest city across countries in our sample.

The coefficient estimate β for the log of initial population is also negative and significant, as expected in Table 1. The explanatory power of the regressions measured by R-squared is low mostly because here we ignore other channels that might affect city population growth. We obtain higher values in the following tables that report results of our robustness analysis where we consider additional explanatory variables. For TSLS, we can also test the strength of the instruments that we use to instrument the tariff change by looking at the details of the first-stage regressions, which are given in Appendix Tables A2-A4. In these tables, it is evident that the instruments significantly enter the regressions with their expected negative signs. Moreover, for the first-stage regressions, the R-squared takes values up to 0.70, and the F-test results all have a p-value of 0.00, which are both indicators of having strong instruments.

The regression results for our first robustness analysis are given in Table 2, where we have included per capita GDP growth, per capita GDP growth squared, capital city dummy, port dummy, initial domestic transportation infrastructure, and a regime change dummy in our regressions. As in the benchmark case, estimates of α are negative and significant using any estimation methodology for all types of tariffs. Therefore, our results are robust to the consideration of additional explanatory variables. Per capita GDP growth enters the regressions significantly with a negative sign, while per capita GDP growth squared significantly enters with a positive sign. Therefore, there is in fact evidence of a nonlinear relation between the change in urban concentration and economic growth; i.e., in countries that have grown faster, the largest city has grown faster than other smaller cities (i.e., urban concentration has increased).[21] It is important to emphasize that the results for the first-stage regressions to instrument both the tariff change and the economic growth are given in Appendix Tables A5-A6; as is evident, all considered instruments enter the first-stage regressions significantly, and the R-squared values are relatively high, showing the strength of our instruments. Turning back to Table 2, both the capital city dummy and the port dummy have negative and significant coefficient estimates suggesting that when the largest city of a country is also a port or the capital city, smaller cities have converged less to (or diverged from) that

[21] It is important to emphasize that we also considered only the economic growth itself, however it was econometrically insignificant. Such results are available upon request.

largest city in terms of population. This result reflects the fact that when the largest city is also the capital city or a port city, increased trade shifts resources and population to it and away from competing urban centers, increasing urban concentration. Initial domestic transportation infrastructure has a positive and significant effect suggesting that when transportation costs are lower within a country, smaller cities tend to benefit from the incentive to disperse economic activity. Finally, the regime change dummy has mixed effects on urban concentration, depending on the estimation methodology, and the coefficient estimateβ for the log initial population is again negative and significant as expected. The explanatory power of regressions has increased compared to the benchmark case.[22]

The regression results for our second robustness analysis are given in Figures 6-8 where we have treated countries symmetrically by considering equal numbers of cities from each of them. Since the number of cities differs across countries in our sample, for additional robustness we consider all possible numbers of cities from each country; therefore, each point at the horizontal axes of Figures 6-8 corresponds to a particular regression that we have run. The results show that estimates of α are almost always negative and significant using any estimation methodology for all types of tariffs (except for the case using the tariff change in consumption goods together with the first instrument in Figure 8). Hence, our main result that trade liberalization leads to lower urban concentration (in the sense that smaller cities growing faster than the largest city) across countries is robust to many alternative estimation methodologies and consideration of alternative explanatory variables. The explanatory power of the regressions as measured by R-squared is also high and gets higher as we increase the (equal) number of cities from each country (although the number of countries decreases in such a case).[23]

4 Conclusion

In this paper, we explore the effects of trade liberalization on the change in urban concentration. Theoretical literature on the subject identifies two relevant and opposing mechanisms. The first of these suggests that trade liberalization may diminish the

[22]The regression results based on the full sample, where the sample changes across regressions due to some missing observations of instruments, are given in Appendix Tables A7-A8; as is evident, the estimates of α are negative and significant in almost all cases.

[23]When we consider another robustness analysis in which we weight the information coming from each city of a particular country by the inverse of the number of cities from that country, we obtain the results in Appendix Tables A9-A10, where the estimates of α are negative and significant in all cases.

effect of the agglomeration forces leading to the creation of megacities and thus lead to reduced urban concentration. The second postulates that trade liberalization may lead to the expansion of those megacities that have better access to world markets, thereby increasing urban concentration. Empirical literature so far has been marred by the use of endogenous measures of trade. The innovation in this paper is the careful use of exogenous tariff policy changes and instruments. We show that, controlling for, among others, largest cities that have ports and, thus, have better access to external markets, trade liberalization has reduced urban concentration. We also improve upon the existing more careful empirical studies that have focused on a single country, providing some valuable external validity by working at the cross-country level of analysis. The results are robust to the consideration of alternative empirical methodologies and sub-samples.

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Table 1 – Estimation Results with the Same Sample across Regressions - Benchmark Analysis

Estimation Methodology	Tariff Change in Capital Goods	Tariff Change in Intermediate Inputs	Tariff Change in Consumption Goods	Log Initial Population	R-Squared	Sample Size
OLS	-0.71* (0.00) [-0.82,-0.61]			-0.09* (0.00) [-0.06,-0.05]	0.12	2878
		-0.97* (0.00) [-1.07,-0.87]		-0.10* (0.00) [-0.11,-0.10]	0.16	2878
			-0.33* (0.00) [-0.42,-0.23]	-0.09* (0.00) [-0.10,-0.08]	0.10	2878
TSLS using First Instrument	-1.12* (0.01) [-1.25,-0.98]			-0.10* (0.00) [-0.11,-0.10]	0.14	2878
		-1.27* (0.02) [-1.41,-1.13]		-0.11* (0.00) [-0.12,-0.10]	0.15	2878
			-0.50* (0.00) [-0.64,-0.36]	-0.09* (0.00) [-0.10,-0.08]	0.10	2878
TSLS using Second Instrument	-1.08* (0.01) [-1.32,-0.83]			-0.10* (0.00) [-0.11,-0.09]	0.10	2878
		-1.03* (0.01) [-1.24,-0.81]		-0.11* (0.00) [-0.12,-0.10]	0.11	2878
			-1.26* (0.00) [-1.46,-1.05]	-0.12* (0.00) [-0.13,-0.11]	0.12	2878

Notes: All regressions include a constant. * represents significance at the 10% level. The p-values (for the null hypothesis of no effect) associated with the wild cluster bootstrap-t method developed by Cameron et al. (2008) are given in parenthesis to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table 2 – Estimation Results with the Same Sample across Regressions - Alternative (Robustness) Analysis

Estimation Methodology	Tariff Change in Capital Goods	Tariff Change in Intermediate Inputs	Tariff Change in Consumption Goods	Per Capita GDP Growth	Per Capita GDP Growth Squared	Capital City Dummy	Port Dummy	Initial Domestic Transportation Infrastructure	Regime Change	Log Initial Population	R-Sqd	Sample Size
OLS	-1.46* (0.00) [-1.58,-1.33]			-4.40* (0.00) [-4.67,-4.13]	4.61* (0.00) [4.34,4.88]	-0.35* (0.00) [-0.37,-0.32]	0.02 (0.05) [-0.00,0.04]	0.88* (0.00) [0.83,0.94]	0.18* (0.00) [0.14,0.23]	-0.09* (0.00) [-0.10,-0.09]	0.41	2878
		-1.38* (0.00) [-1.50,-1.25]		-3.63* (0.00) [-3.92,-3.34]	3.72* (0.00) [3.43,4.01]	-0.34* (0.00) [-0.36,-0.31]	0.01 (0.15) [-0.01,0.03]	0.80* (0.00) [0.75,0.85]	0.18* (0.00) [0.14,0.23]	-0.10* (0.00) [-0.10,-0.09]	0.40	2878
			-1.14* (0.00) [-1.26,-1.01]	-4.48* (0.00) [-4.76,-4.20]	4.77* (0.00) [4.49,5.05]	-0.37* (0.00) [-0.39,-0.34]	0.04* (0.00) [0.01,0.06]	0.92* (0.00) [0.86,0.98]	0.18* (0.01) [0.14,0.23]	-0.09* (0.00) [-0.10,-0.09]	0.38	2878
TSLS using First Instrument	-1.70* (0.00) [-1.87,-1.52]			-3.04* (0.00) [-3.34,-2.74]	3.30* (0.00) [2.95,3.65]	-0.26* (0.00) [-0.28,-0.23]	-0.09* (0.00) [-0.12,-0.07]	0.72* (0.00) [0.66,0.79]	0.18* (0.00) [0.14,0.23]	-0.09* (0.00) [-0.10,-0.08]	0.28	2878
		-1.68* (0.00) [-1.85,-1.51]		-3.04* (0.00) [-3.33,-2.75]	3.33* (0.00) [2.99,3.67]	-0.26* (0.00) [-0.28,-0.23]	-0.05* (0.00) [-0.07,-0.03]	0.75* (0.00) [0.68,0.81]	0.20* (0.00) [0.15,0.25]	-0.10* (0.00) [-0.10,-0.09]	0.29	2878
			-0.72* (0.02) [-0.91,-0.54]	-2.69* (0.00) [-3.00,-2.38]	2.88* (0.00) [2.51,3.25]	-0.28* (0.00) [-0.31,-0.25]	-0.07* (0.00) [-0.09,-0.04]	0.53* (0.00) [0.45,0.61]	0.18* (0.00) [0.14,0.23]	-0.08* (0.00) [-0.09,-0.08]	0.22	2878
TSLS using Second Instrument	-1.57* (0.00) [-1.86,-1.29]			-2.36* (0.00) [-2.68,-2.05]	2.75* (0.00) [2.38,3.11]	-0.25* (0.00) [-0.27,-0.22]	-0.10* (0.00) [-0.13,-0.08]	0.62* (0.00) [0.54,0.70]	0.17* (0.00) [0.12,0.22]	-0.09* (0.00) [-0.10,-0.09]	0.23	2878
		-1.20* (0.00) [-1.44,-0.97]		-2.46* (0.00) [-2.76,-2.15]	2.96* (0.00) [2.60,3.32]	-0.24* (0.00) [-0.27,-0.21]	-0.05* (0.00) [-0.07,0.03]	0.56* (0.00) [0.49,0.63]	0.19* (0.00) [0.14,0.24]	-0.10* (0.00) [-0.11,-0.09]	0.24	2878
			-2.24* (0.00) [-2.52,-1.94]	-2.23* (0.00) [-2.54,-1.91]	2.52* (0.00) [2.15,2.88]	-0.31* (0.00) [-0.34,-0.28]	-0.15* (0.00) [-0.18,-0.13]	0.97* (0.00) [0.87,1.08]	0.14* (0.00) [0.09,0.19]	-0.10* (0.00) [-0.11,-0.10]	0.25	2878

Notes: All regressions include a constant. * represents significance at the 10% level. The p-values (for the null hypothesis of no effect) associated with the wild cluster bootstrap-t method developed by Cameron et al. (2008) are given in parenthesis to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Figure 1a - Percentage of Urban Population in the Largest City

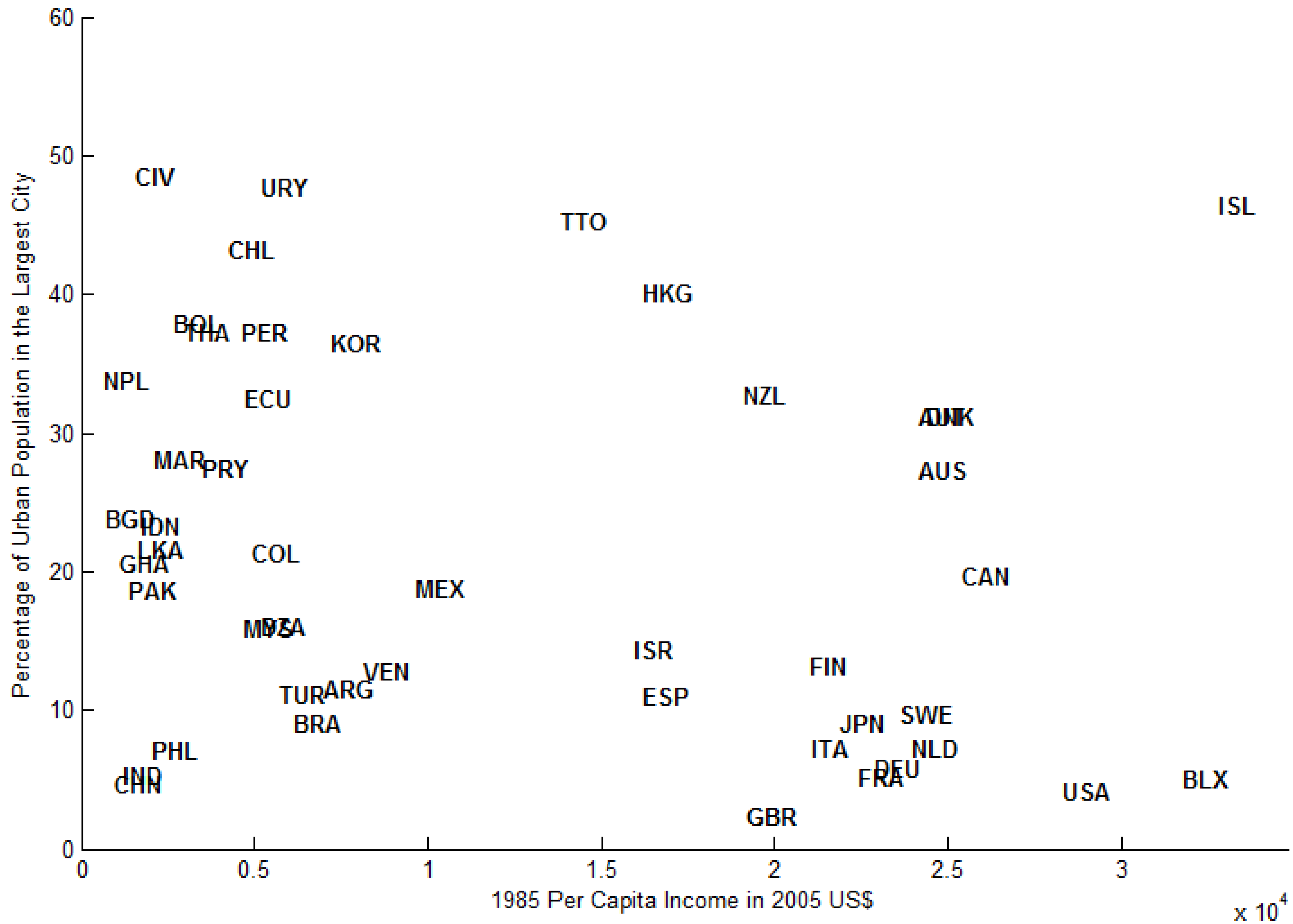


Figure 1b - Herfindahl Index of City Populations

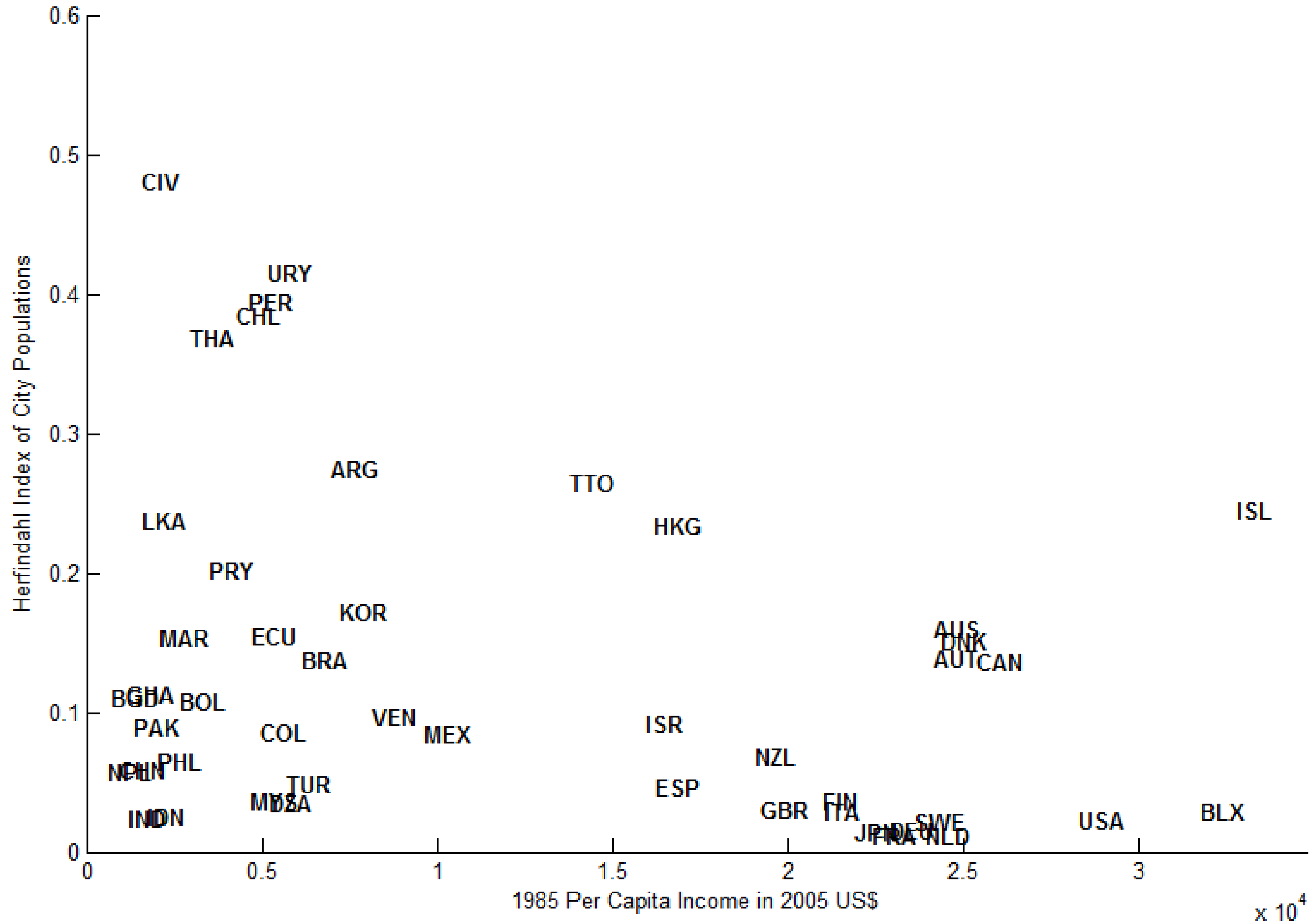
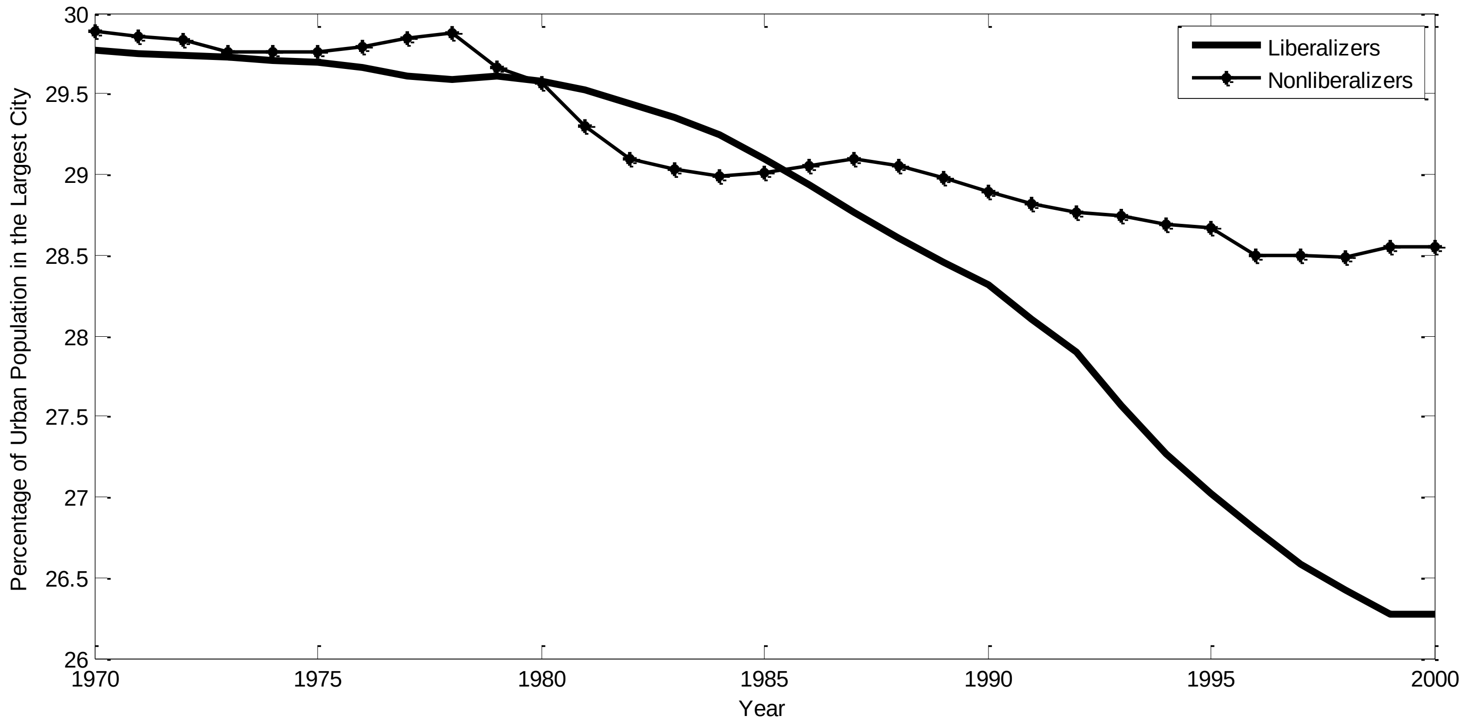


Figure 2 - The Great Liberalization and the Percentage of Urban Concentration in the Largest City

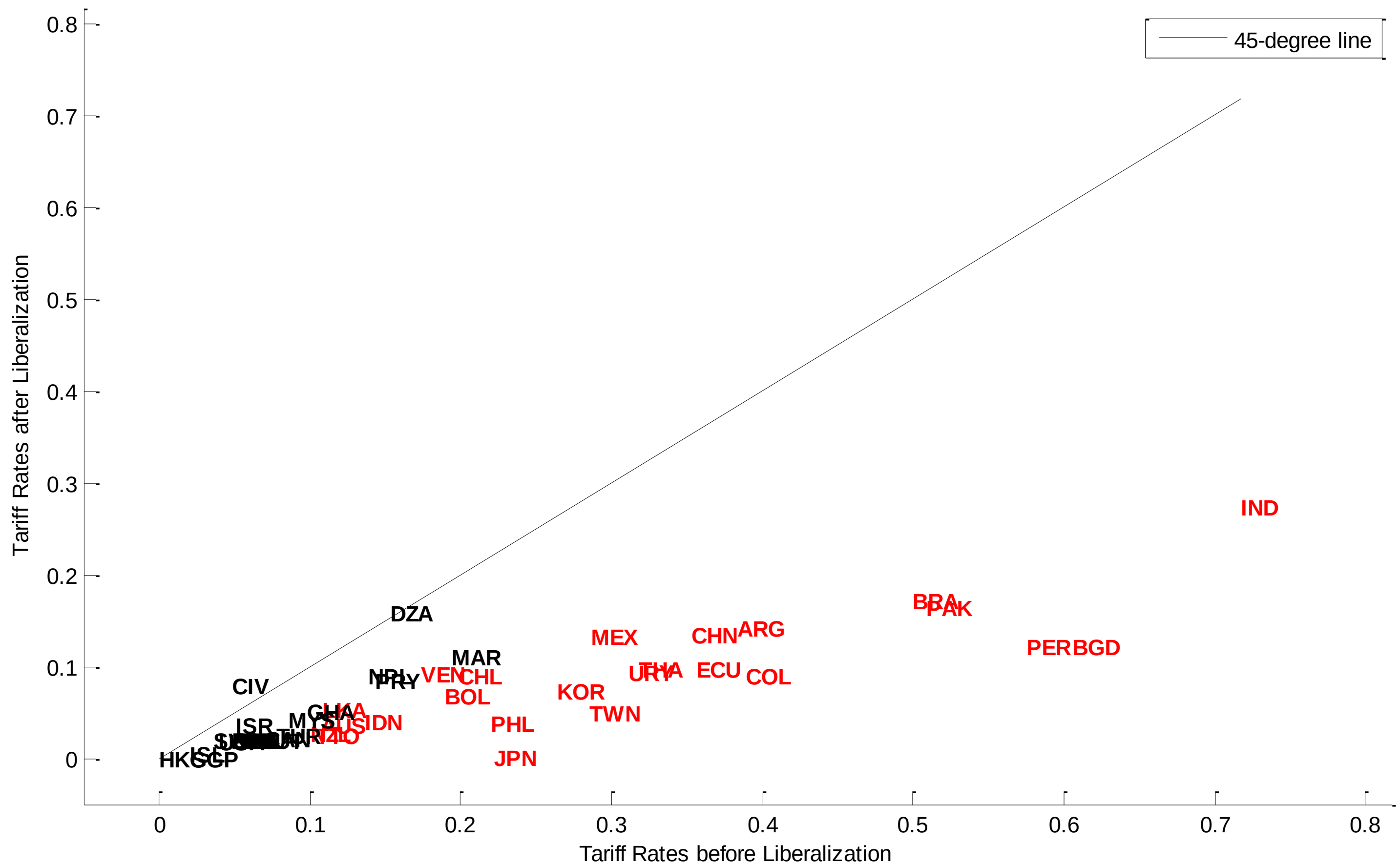


Notes: The average percentage of urban population in the largest city for nonliberalizers has been normalized to the corresponding average value for liberalizers between 1970-1985 for comparison purposes. The samples are as follows:

Liberalizers: Argentina, Australia, Bangladesh, Bolivia, Brazil, Chile, China, Colombia, Ecuador, Indonesia, India, Japan, South Korea, Sri Lanka, Mexico, New Zealand, Pakistan, Peru, Philippines, Thailand, Trinidad and Tobago, Taiwan, Uruguay, Venezuela.

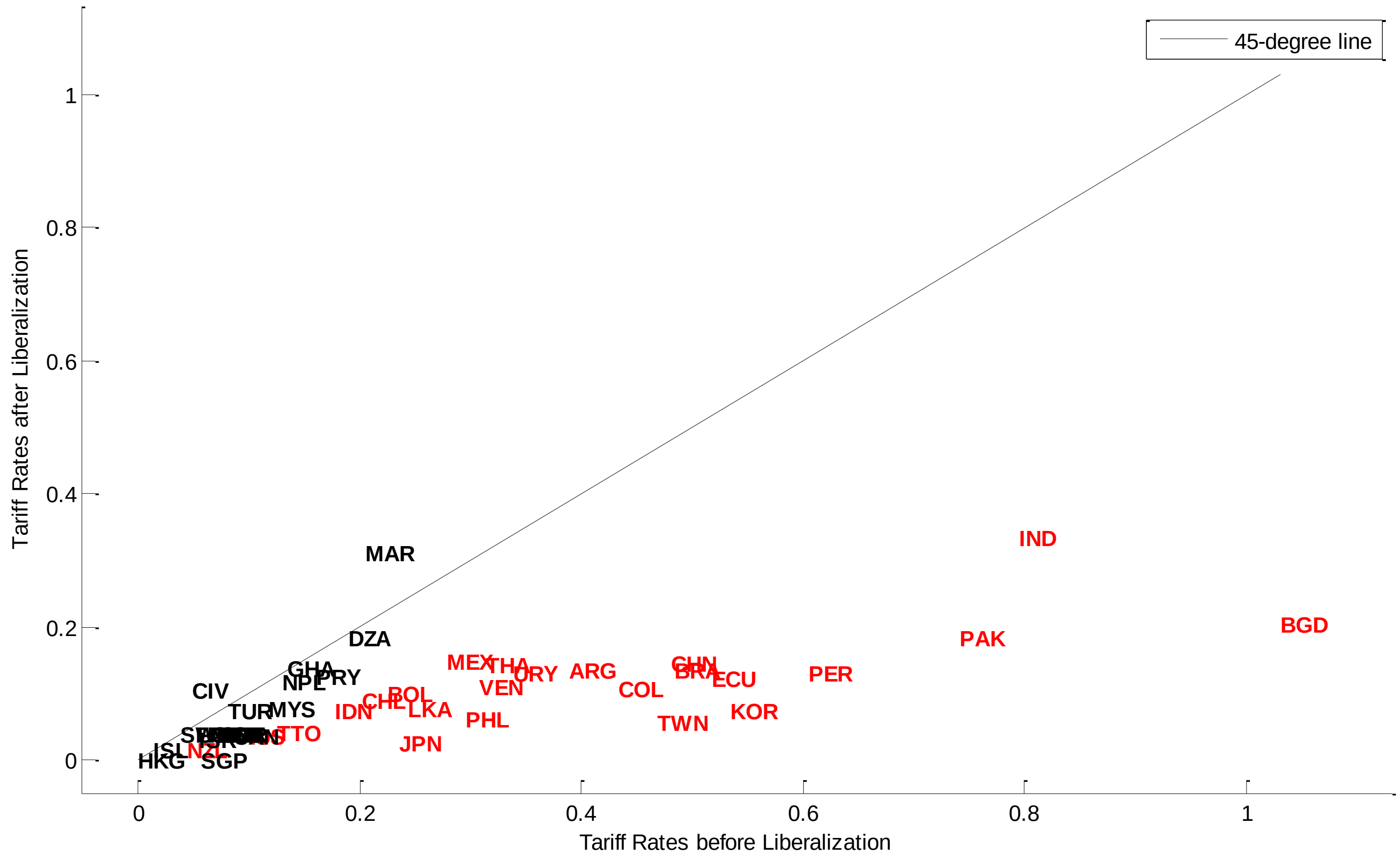
Nonliberalizers: Algeria, Austria, Belgium, Canada, Cote d'Ivoire, Denmark, Finland, France, Germany, Ghana, Hong Kong, Iceland, Israel, Italy, Malaysia, Morocco, Nepal, Netherlands, Paraguay, Singapore, Spain, Sweden, Turkey, United Kingdom, United States.

Figure 3 - Tariffs on Capital Goods - After versus Before



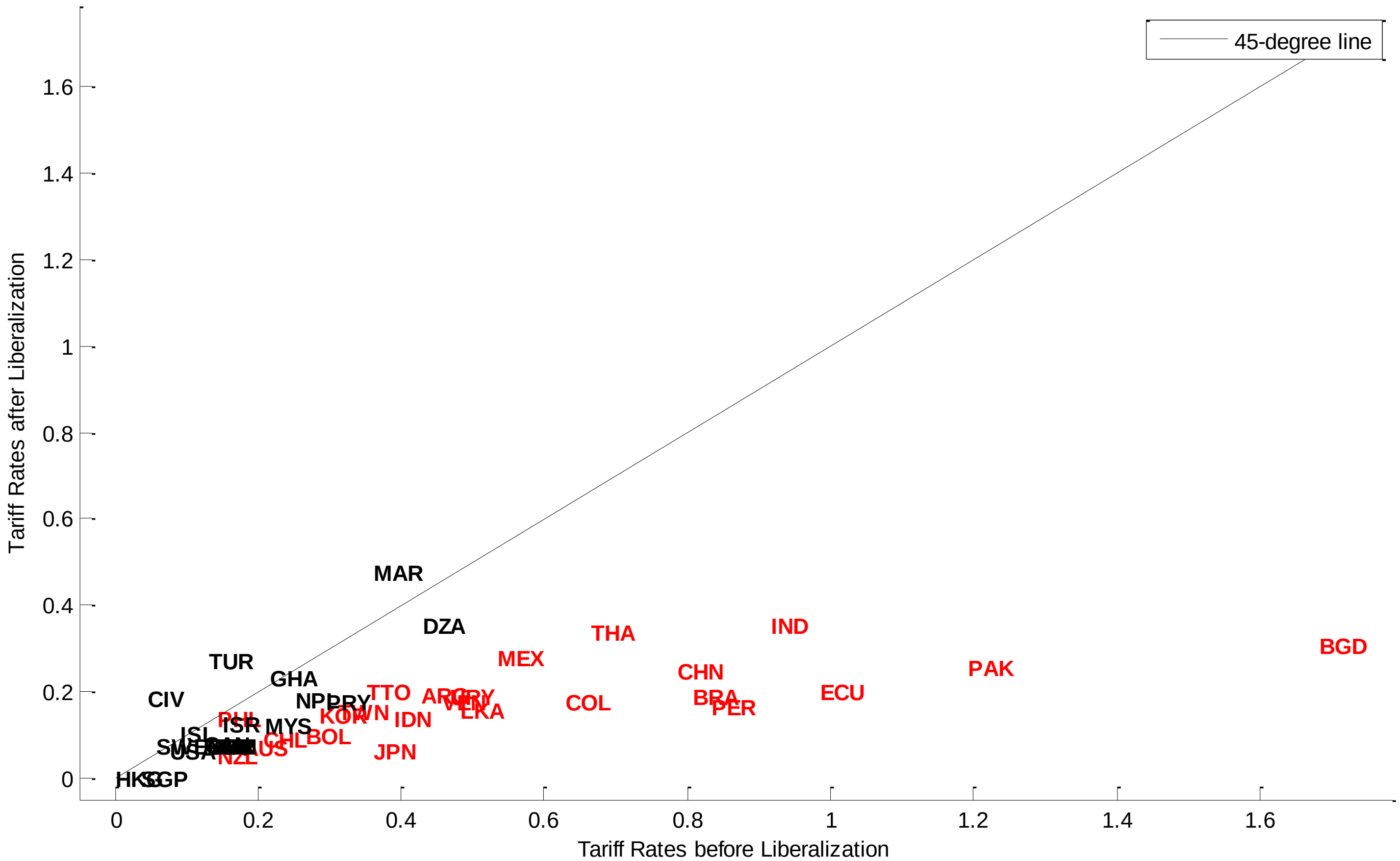
Notes: The country codes in red represent liberalizers. See underneath Figure 2 for the exact list of countries.

Figure 4 - Tariffs on Intermediate Inputs - After versus Before



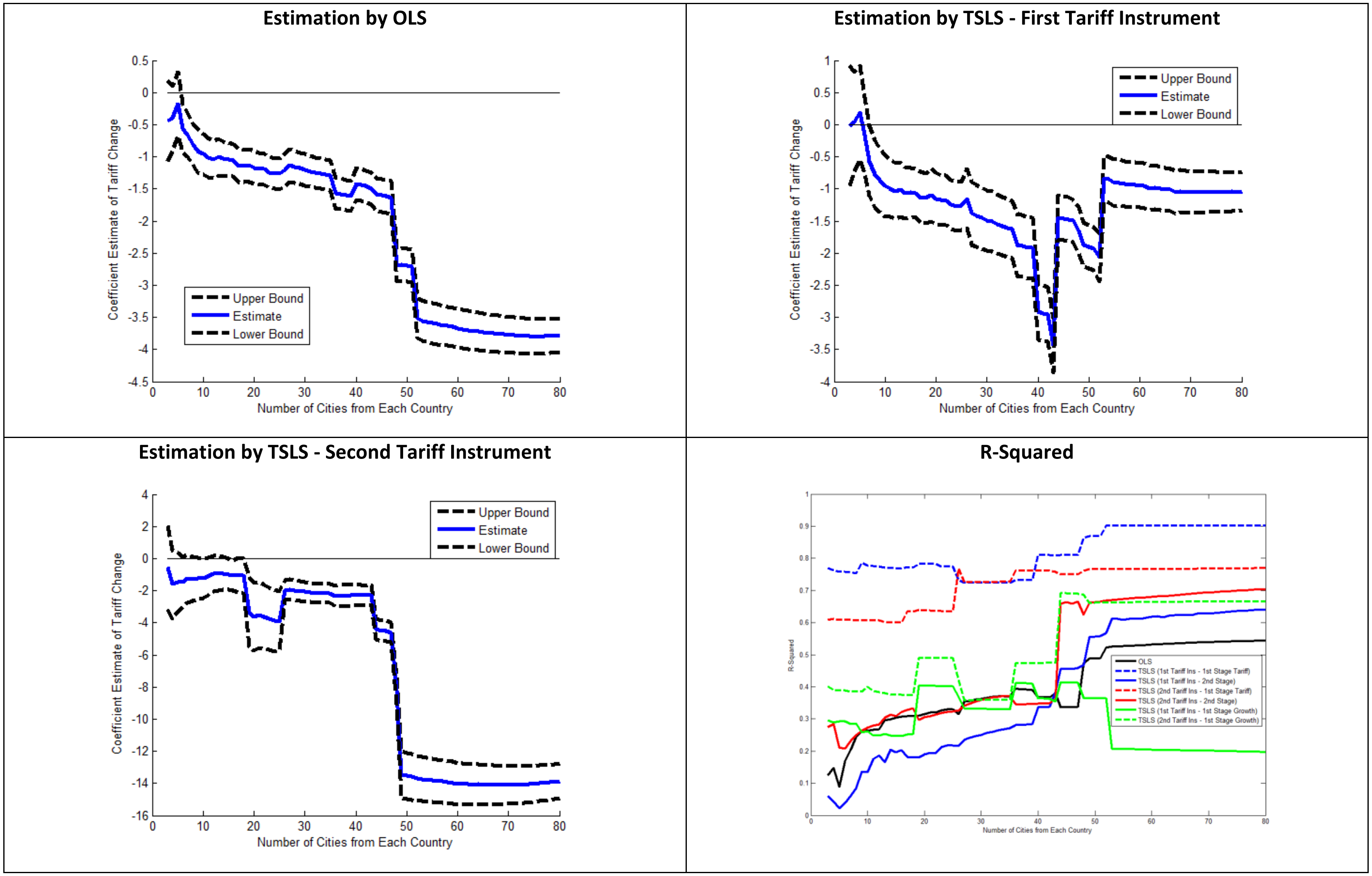
Notes: The country codes in red represent liberalizers. See underneath Figure 2 for the exact list of countries.

Figure 5 - Tariffs on Consumption Goods - After versus Before



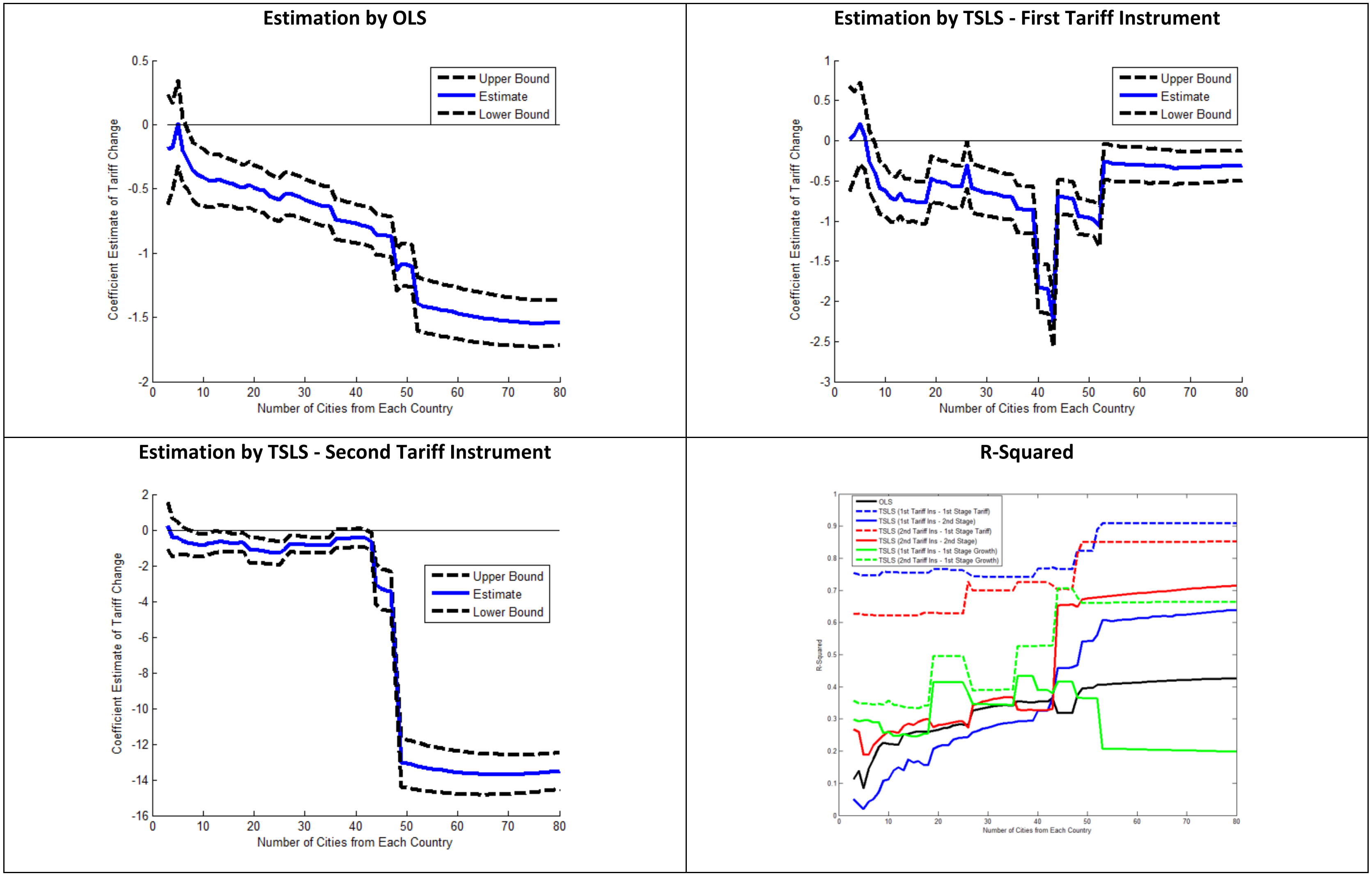
Notes: The country codes in red represent liberalizers. See underneath Figure 2 for the exact list of countries.

Figure 6 - Results with Equal Number of Cities from Each Country - Tariff Change in Capital Goods



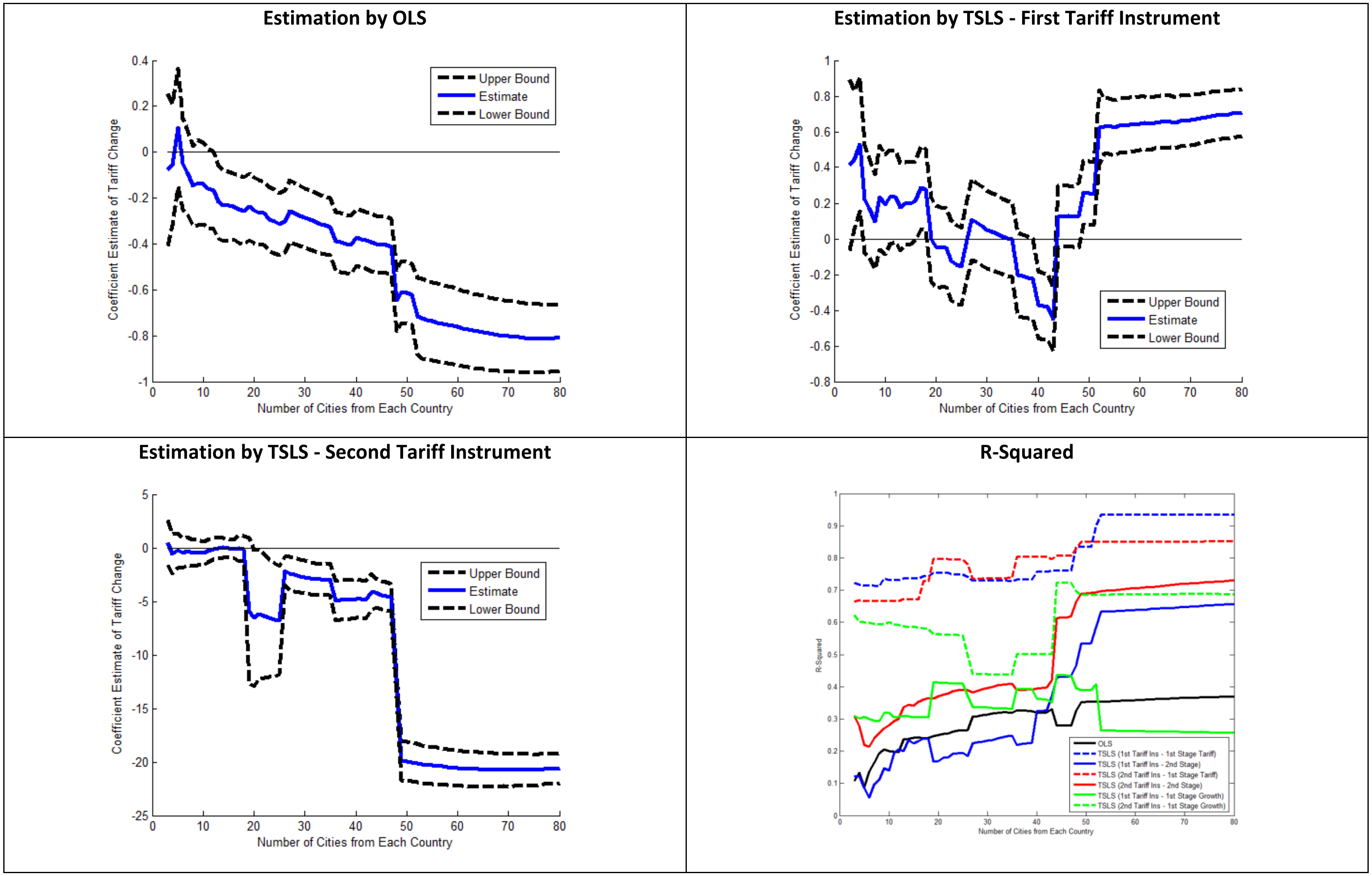
Notes: The regressions, each corresponding to a particular point on the horizontal axes, include port dummy, domestic transportation infrastructure, regime change, log initial population, and a constant. Upper and lower bounds correspond to the 90% confidence intervals.

Figure 7 - Results with Equal Number of Cities from Each Country - Tariff Change in Intermediate Inputs



Notes: The regressions, each corresponding to a particular point on the horizontal axes, include port dummy, domestic transportation infrastructure, regime change, log initial population, and a constant. Upper and lower bounds correspond to the 90% confidence intervals.

Figure 8 - Results with Equal Number of Cities from Each Country - Tariff Change in Consumption Goods



Notes: The regressions, each corresponding to a particular point on the horizontal axes, include port dummy, domestic transportation infrastructure, regime change, log initial population, and a constant. Upper and lower bounds correspond to the 90% confidence intervals.

APPENDIX (FOR ONLINE PUBLICATION)

Table A1a - Data Sources

Country Code	Country Name	Preliberalization MFN Tariff	Post Liberalization MFN Tariff	Preliberalization Population	Post Liberalization Population
ARG	Argentina	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 1999
AUS	Australia	TRAINS 1992	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
AUT	Austria	TRAINS 1991	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
BGD	Bangladesh	TRAINS 1989	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
BLX	Belgium-Luxembourg	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1988	Populstat.info 1999
BOL	Bolivia	NAT 1985	TRAINS 2000	Populstat.info 1992	Populstat.info 2001
BRA	Brazil	NAT 1985	TRAINS 2000	Populstat.info 1980	Populstat.info 2000
CAN	Canada	TRAINS 1989	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
CHL	Chile	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 2000
CHN	China	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 2001
CIV	Cote d'Ivoire	TRAINS 1993	TRAINS 2001	Populstat.info 1988	Populstat.info 1998
COL	Colombia	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 2002
DEU	Germany	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1987	Populstat.info 2000
DNK	Denmark	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1988	Populstat.info 2000
DZA	Algeria	TRAINS 1992	TRAINS 2001	Populstat.info 1987	Populstat.info 1998
ECU	Ecuador	NAT 1985	TRAINS 1999	Populstat.info 1990	Populstat.info 2002
ESP	Spain	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1988	Populstat.info 2000
FIN	Finland	TRAINS 1988	TRAINS 2000	Populstat.info 1985	Populstat.info 2001
FRA	France	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1990	Populstat.info 1999
GBR	United Kingdom	TRAINS-EU 1989	TRAINS 2000	Populstat.info 1985	Populstat.info 1998
GHA	Ghana	TRAINS 1993	TRAINS 2000	Populstat.info 1984	Populstat.info 2002
HKG	Hong Kong	TRAINS 1988	TRAINS 1998	Citypopulation.de 1991	Citypopulation.de 2001
IDN	Indonesia	TRAINS 1989	TRAINS 2000	Populstat.info 1990	Populstat.info 2002
IND	India	TRAINS 1990	TRAINS 1999	World-gazetteer.com 1991	World-gazetteer.com 2001
ISL	Iceland	TRAINS 1993	TRAINS 2001	Populstat.info 1991	Populstat.info 2001
ISR	Israel	TRAINS 1993	TRAINS 2004	Populstat.info 1992	Populstat.info 2002
ITA	Italy	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
JPN	Japan	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 2000
KOR	South Korea	NAT 1985	TRAINS 1999	Populstat.info 1985	Populstat.info 2000
LKA	Sri Lanka	TRAINS 1990	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
MAR	Morocco	TRAINS 1993	TRAINS 2000	World-gazetteer.com 1994	World-gazetteer.com 2004
MEX	Mexico	NAT 1985	TRAINS 2000	Populstat.info 1990	Populstat.info 2000
MYS	Malaysia	TRAINS 1988	TRAINS 2001	Populstat.info 1991	Populstat.info 2000
NLD	Netherlands	TRAINS-EU 1988	TRAINS 2000	Populstat.info 1988	Populstat.info 1999
NPL	Nepal	TRAINS 1993	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
NZL	New Zealand	TRAINS 1992	TRAINS 2000	Populstat.info 1991	Populstat.info 2001
PAK	Pakistan	NAT 1985	TRAINS 2001	Populstat.info 1981	Populstat.info 1998
PER	Peru	NAT 1985	TRAINS 2000	Populstat.info 1981	Populstat.info 1998
PHL	Philippines	NAT 1985	TRAINS 2000	Populstat.info 1980	Populstat.info 1995
PRY	Paraguay	NAT 1985	TRAINS 2000	Citypopulation.de 1982	Citypopulation.de 2002
SWE	Sweden	TRAINS 1988	TRAINS 2000	Populstat.info 1989	Populstat.info 2000
THA	Thailand	TRAINS 1989	TRAINS 2000	Populstat.info 1980	Populstat.info 2000
TTO	Trinidad and Tobago	TRAINS 1991	TRAINS 2001	Populstat.info 1990	Populstat.info 2000
TUR	Turkey	TRAINS 1993	TRAINS 1999	Populstat.info 1980	Populstat.info 1997
TWN	Taiwan	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 2000
URY	Uruguay	NAT 1985	TRAINS 2000	Populstat.info 1985	Populstat.info 2002
USA	United States	TRAINS 1989	TRAINS 2000	Populstat.info 1990	Populstat.info 2000
VEN	Venezuela	NAT 1985	TRAINS 2000	Citypopulation.de 1981	Citypopulation.de 2001

Notes: NAT stands for national sources, TRAINS stands for Trade Analysis and Information System, TRAINS-EU stands for the EU schedule of tariffs according to TRAINS.

Table A1b – Representativeness of the Country Sample

	GDP Per Capita	GDP Per Capita Growth	Export/GDP (%)	Import/GDP (%)	Urbanization
Country Sample in This Paper					
10th Percentile	442.34	-0.12	8.08	9.55	10.09
25th Percentile	1,313.77	1.55	13.32	14.84	17.43
50th Percentile	3,999.16	2.36	21.21	22.79	25.19
75th Percentile	19,866.90	3.09	30.31	33.19	39.06
90th Percentile	22,713.54	4.58	46.27	47.83	55.33
All Countries in WDI					
10th Percentile	333.05	-1.20	9.35	13.42	16.04
25th Percentile	768.69	0.47	15.19	19.90	22.20
50th Percentile	2,379.74	2.02	25.32	30.06	35.76
75th Percentile	9,502.26	2.97	42.69	52.87	53.49
90th Percentile	21,338.86	4.44	60.31	72.17	88.25

Notes: The percentiles compare the country sample in this paper with the complete set of countries in the WDI data set on average over the period of 1970-1985. GDP per capita measures are in 2005US$. Urbanization corresponds to the percentage of urban concentration in the largest city.

Table A2 – First-Stage Results of Benchmark TSLS Estimation Using The First Instrument

	Dependent Variable		
Instruments Used	**Tariff Change in Capital Goods**	**Tariff Change in Intermediate Inputs**	**Tariff Change in Consumption Goods**
Initial Tariff × GATT Member in 1975	-0.56 [-0.57,-0.54]	-0.58 [-0.59,-0.57]	-0.61 [-0.62,-0.59]
Log Initial Population	-0.01 [-0.01,-0.01]	-0.01 [-0.01,-0.01]	-0.01 [-0.01,-0.01]
R-Squared	0.66	0.66	0.62
Sample Size	5522	5522	5522

Notes: All regressions include a constant. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A3 – First-Stage Results of Benchmark TSLS Estimation Using The Second Instrument

	Dependent Variable		
Instruments Used	**Tariff Change in Capital Goods**	**Tariff Change in Intermediate Inputs**	**Tariff Change in Consumption Goods**
Initial Tariff × Average Deviation of 1930-35 GDP level from 1929 level	-3.76 [-3.99,-3.53]	-3.99 [-4.18,-3.80]	-2.64 [-2.83,-2.45]
Log Initial Population	-0.02 [-0.03,-0.02]	-0.03 [-0.03,-0.02]	-0.03 [-0.03,-0.03]
R-Squared	0.31	0.40	0.27
Sample Size	4400	4400	4400

Notes: All regressions include a constant. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A4 – First-Stage Results of Benchmark TSLS Estimation Using Both Instruments

	Dependent Variable		
Instruments Used	**Tariff Change in Capital Goods**	**Tariff Change in Intermediate Inputs**	**Tariff Change in Consumption Goods**
Initial Tariff × GATT Member in 1975	-0.49 [-0.50,-0.48]	-0.40 [-0.42,-0.39]	-0.44 [-0.45,-0.42]
Initial Tariff × Average Deviation of 1930-35 GDP level from 1929 level	-2.83 [-2.99,-2.68]	-3.15 [-3.30,-3.00]	-1.89 [-2.03,-1.74]
Log Initial Population	-0.01 [-0.01,-0.01]	-0.01 [-0.01,-0.01]	-0.01 [-0.02,-0.01]
R-Squared	0.70	0.66	0.57
Sample Size	4400	4400	4400

Notes: All regressions include a constant. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A5 – First-Stage Results of Alternative TSLS Estimation Using The First Instrument

	Dependent Variable			
Instruments and Exogenous Variables	**Tariff Change in Capital Goods**	**Tariff Change in Intermediate Inputs**	**Tariff Change in Consumption Goods**	**Per Capita GDP Growth**
Initial Tariff × GATT Member in 1975	-0.48 [-0.49,-0.47]	-0.54 [-0.55,-0.53]	-0.50 [-0.51,-0.49]	
Capital City Dummy	-0.04 [-0.05,-0.04]	-0.06 [-0.07,-0.06]	-0.05 [-0.06,-0.05]	0.01 [0.00,0.03]
Port Dummy	-0.02 [-0.03,-0.02]	-0.00 [-0.01,-0.00]	-0.01 [-0.01,0.00]	-0.08 [-0.09,-0.08]
Initial Domestic Transportation Infrastructure	0.13 [0.12,0.14]	0.15 [0.15,0.16]	0.23 [0.22,0.24]	0.42 [0.40,0.44]
Regime Change	-0.03 [-0.04,-0.03]	-0.06 [-0.07,-0.06]	-0.11 [-0.12,-0.10]	-0.02 [-0.03,-0.01]
Log Initial Population	-0.00 [-0.00,0.00]	-0.00 [-0.00,0.00]	0.00 [-0.00,0.00]	0.02 [0.01,0.02]
Log Initial GDP Per Capita				-0.32 [-0.33,-0.31]
Log Initial Schooling				0.49 [0.47,0.51]
Log Initial Institutions				0.39 [0.37,0.41]
Initial Tariff				0.20 [0.16,0.23]
R-Squared	0.81	0.83	0.82	0.70
Sample Size	3691	3691	3691	3691

Notes: All regressions include a constant. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A6 – First-Stage Results of Alternative TSLS Estimation Using The Second Instrument

	Dependent Variable			
Instruments and Exogenous Variables	**Tariff Change in Capital Goods**	**Tariff Change in Intermediate Inputs**	**Tariff Change in Consumption Goods**	**Per Capita GDP Growth**
Initial Tariff × Average Deviation of 1930-35 GDP level from 1929 level	-3.36 [-3.57,-3.14]	-3.50 [-3.68,-3.32]	-2.20 [-2.35,-2.05]	
Capital City Dummy	0.01 [0.00,0.01]	-0.00 [-0.01,0.00]	-0.01 [-0.02,-0.01]	-0.01 [-0.02,0.00]
Port Dummy	-0.03 [-0.04,-0.03]	-0.01 [-0.02,-0.01]	-0.03 [-0.04,-0.03]	-0.11 [-0.12,-0.10]
Initial Domestic Transportation Infrastructure	0.23 [0.22,0.24]	0.24 [0.23,0.25]	0.31 [0.30,0.32]	0.47 [0.45,0.49]
Regime Change	-0.05 [-0.06,-0.04]	-0.05 [-0.06,-0.04]	-0.05 [-0.06,-0.04]	0.03 [0.02,0.05]
Log Initial Population	-0.00 [-0.01,-0.00]	-0.01 [-0.01,-0.01]	-0.01 [-0.01,-0.01]	0.02 [0.02,0.02]
Log Initial GDP Per Capita				-0.35 [-0.36,-0.35]
Log Initial Schooling				0.57 [0.56,0.59]
Log Initial Institutions				0.25 [0.22,0.28]
Initial Tariff				0.00 [-0.03,0.04]
R-Squared	0.67	0.70	0.75	0.78
Sample Size	2878	2878	2878	2878

Notes: All regressions include a constant. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A7 – Estimation Results with All Cities - Benchmark Analysis

Estimation Methodology	Tariff Change in Capital Goods	Tariff Change in Intermediate Inputs	Tariff Change in Consumption Goods	Log Initial Population	R-Squared	Sample Size
OLS	-0.42* (0.00) [-0.49,-0.35]			-0.05* (0.00) [-0.06,-0.05]	0.05	5522
		-0.35* (0.00) [-0.41,-0.30]		-0.05* (0.00) [-0.06,-0.05]	0.05	5522
			-0.20* (0.00) [-0.24,-0.16]	-0.05* (0.00) [-0.05,-0.04]	0.04	5522
TSLS using First Instrument	-0.18* (0.01) [-0.27,-0.08]			-0.05* (0.00) [-0.05,-0.04]	0.03	5522
		-0.09* (0.02) [-0.16,-0.02]		-0.04* (0.00) [-0.05,-0.04]	0.03	5522
			0.15* (0.00) [0.09,0.20]	-0.04* (0.00) [-0.04,-0.03]	0.03	5522
TSLS using Second Instrument	-0.60* (0.01) [-0.80,-0.41]			-0.06* (0.00) [-0.07,-0.06]	0.05	4400
		-0.62* (0.01) [-0.78,-0.47]		-0.07* (0.00) [-0.07,-0.06]	0.06	4400
			-0.75* (0.00) [-0.93,-0.57]	-0.07* (0.00) [-0.08,-0.06]	0.06	4400

Notes: All regressions include a constant. * represents significance at the 10% level. The p-values (for the null hypothesis of no effect) associated with the wild cluster bootstrap-t method developed by Cameron et al. (2008) are given in parenthesis to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A8 – Estimation Results with All Cities - Alternative (Robustness) Analysis

Estimation Methodology	Tariff Change in Capital Goods	Tariff Change in Intermediate Inputs	Tariff Change in Consumption Goods	Per Capita GDP Growth	Per Capita GDP Growth Squared	Capital City Dummy	Port Dummy	Initial Domestic Transportation Infrastructure	Regime Change	Log Initial Population	R-Sqd	Sample Size
OLS	-1.32* (0.00) [-1.42,-1.22]			-1.05* (0.00) [-1.17,-0.93]	1.49* (0.00) [1.35,1.63]	-0.19* (0.00) [-0.21,-0.17]	-0.09* (0.00) [-0.11,-0.07]	0.51* (0.00) [0.47,0.55]	-0.04* (0.00) [-0.06,-0.01]	-0.08* (0.00) [-0.09,-0.08]	0.27	3691
OLS		-0.71* (0.00) [-0.79,-0.64]		-0.81* (0.00) [-0.93,-0.69]	1.20* (0.00) [1.06,1.34]	-0.20* (0.00) [-0.22,-0.18]	-0.06* (0.00) [-0.08,-0.04]	0.41* (0.00) [0.37,0.45]	-0.05* (0.00) [-0.07,-0.02]	-0.08* (0.00) [-0.09,-0.07]	0.23	3691
OLS			-0.33* (0.00) [-0.39,-0.26]	-0.76* (0.00) [-0.88,-0.64]	1.25* (0.00) [1.10,1.40]	-0.20* (0.00) [-0.22,-0.17]	-0.05* (0.00) [-0.07,-0.03]	0.33* (0.00) [0.29,0.37]	-0.04* (0.01) [-0.06,-0.01]	-0.07* (0.00) [-0.08,-0.07]	0.20	3691
TSLS using First Instrument	-0.91* (0.00) [-1.04,-0.77]			-1.20* (0.00) [-1.39,-1.01]	1.87* (0.00) [1.61,2.13]	-0.17* (0.00) [-0.19,-0.14]	-0.10* (0.00) [-0.12,-0.08]	0.42* (0.00) [0.38,0.47]	0.00 (0.45) [-0.02,0.03]	-0.07* (0.00) [-0.08,-0.07]	0.19	3691
TSLS using First Instrument		-0.48* (0.00) [-0.58,-0.39]		-1.12* (0.00) [-1.30,-0.95]	1.90* (0.00) [1.66,2.15]	-0.17* (0.00) [-0.19,-0.14]	-0.07* (0.00) [-0.09,-0.05]	0.35* (0.00) [0.31,0.40]	0.00 (0.43) [-0.02,0.03]	-0.07* (0.00) [-0.08,-0.06]	0.19	3691
TSLS using First Instrument			-0.10* (0.02) [-0.18,-0.02]	-0.94* (0.00) [-1.14,-0.75]	1.47* (0.00) [1.20,1.75]	-0.18* (0.00) [-0.20,-0.16]	-0.06* (0.00) [-0.08,-0.04]	0.27* (0.00) [0.22,0.32]	0.01 (0.19) [-0.01,0.04]	-0.06* (0.00) [-0.07,-0.05]	0.15	3691
TSLS using Second Instrument	-1.57* (0.00) [-1.86,-1.29]			-2.36* (0.00) [-2.68,-2.05]	2.75* (0.00) [2.38,3.11]	-0.25* (0.00) [-0.27,-0.22]	-0.10* (0.00) [-0.13,-0.08]	0.62* (0.00) [0.54,0.70]	0.17* (0.00) [0.12,0.22]	-0.09* (0.00) [-0.10,-0.09]	0.23	2878
TSLS using Second Instrument		-1.20* (0.00) [-1.44,-0.97]		-2.46* (0.00) [-2.76,-2.15]	2.96* (0.00) [2.60,3.32]	-0.24* (0.00) [-0.27,-0.21]	-0.05* (0.00) [-0.07,0.03]	0.56* (0.00) [0.49,0.63]	0.19* (0.00) [0.14,0.24]	-0.10* (0.00) [-0.11,-0.09]	0.24	2878
TSLS using Second Instrument			-2.24* (0.00) [-2.52,-1.94]	-2.23* (0.00) [-2.54,-1.91]	2.52* (0.00) [2.15,2.88]	-0.31* (0.00) [-0.34,-0.28]	-0.15* (0.00) [-0.18,-0.13]	0.97* (0.00) [0.87,1.08]	0.14* (0.00) [0.09,0.19]	-0.10* (0.00) [-0.11,-0.10]	0.25	2878

Notes: All regressions include a constant. * represents significance at the 10% level. The p-values (for the null hypothesis of no effect) associated with the wild cluster bootstrap-t method developed by Cameron et al. (2008) are given in parenthesis to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A9 – Weighted Least Squares Estimation Results with the Same Sample across Regressions - Benchmark Analysis

Estimation Methodology	Tariff Change in Capital Goods	Tariff Change in Intermediate Inputs	Tariff Change in Consumption Goods	Log Initial Population	R-Squared	Sample Size
OLS	-1.03* (0.00) [-1.16,-0.90]			-0.20* (0.00) [-0.21,-0.19]	0.23	2878
		-1.34* (0.01) [-1.45,-1.24]		-0.21* (0.00) [-0.22,-0.20]	0.27	2878
			-0.56* (0.00) [-0.66,-0.46]	-0.19* (0.00) [-0.21,-0.18]	0.18	2878
TSLS using First Instrument	-1.34* (0.01) [-1. 50,-1.19]			-0.21* (0.00) [-0.22,-0.20]	0.24	2878
		-1.71* (0.02) [-1.86,-1.55]		-0.23* (0.00) [-0.25,-0.22]	0.25	2878
			-0.74* (0.00) [-0.90,-0.59]	-0.21* (0.00) [-0.22,-0.20]	0.18	2878
TSLS using Second Instrument	-1.73* (0.01) [-1.94,-1.53]			-0.26* (0.00) [-0.28,-0.25]	0.20	2878
		-1.42* (0.01) [-1.60,-1.24]		-0.25* (0.00) [-0.27,-0.24]	0.19	2878
			-1.83* (0.00) [-2.03,-1.62]	-0.28* (0.00) [-0.29,-0.26]	0.22	2878

Notes: Estimations are by weighted least squares where weights have been determined by "1/no. of cities from each country." All regressions include rank fixed effects and a constant. * represents significance at the 10% level. The p-values (for the null hypothesis of no effect) associated with the wild cluster bootstrap-t method developed by Cameron et al. (2008) are given in parenthesis to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Table A10 – Weighted Least Squares Estimation Results with the Same Sample across Regressions - Alternative (Robustness) Analysis

Estimation Methodology	Tariff Change in Capital Goods	Tariff Change in Intermediate Inputs	Tariff Change in Consumption Goods	Per Capita GDP Growth	Per Capita GDP Growth Squared	Capital City Dummy	Port Dummy	Initial Domestic Transportation Infrastructure	Regime Change	Log Initial Population	R-Sqd	Sample Size
OLS	-1.61* (0.00) [-1.80,-1.42]			-2.05* (0.00) [-2.32,-1.79]	2.15* (0.00) [1.89,2.42]	-0.32* (0.00) [-0.35,-0.29]	-0.20* (0.00) [-0.24,0.16]	0.67* (0.00) [0.61,0.72]	-0.07* (0.00) [-0.11,-0.02]	-0.17* (0.00) [-0.18,-0.16]	0.38	2878
		-1.97* (0.00) [-2.15,-1.79]		-0.52* (0.00) [-0.85,-0.20]	0.47* (0.00) [0.13,0.81]	-0.34* (0.00) [-0.37,-0.31]	-0.25* (0.00) [-0.28,-0.21]	0.61* (0.00) [0.56,0.66]	-0.07* (0.00) [-0.11,-0.03]	-0.20* (0.00) [-0.21,-0.18]	0.37	2878
			-1.18* (0.00) [-1.36,-1.01]	-2.05* (0.00) [-2.33,-1.76]	2.31* (0.00) [2.03,2.59]	-0.33* (0.00) [-0.36,-0.30]	-0.17* (0.00) [0.21,0.13]	0.67* (0.00) [0.61,0.73]	-0.03 (0.15) [-0.07,0.02]	-0.18* (0.00) [-0.20,-0.16]	0.34	2878
TSLS using First Instrument	-2.22* (0.00) [-2.41,-2.03]			-1.79* (0.00) [-1.98,-1.60]	1.66* (0.00) [1.40,1.91]	-0.34* (0.00) [-0.37,-0.31]	-0.37* (0.00) [-0.40,-0.33]	0.97* (0.00) [0.90,1.04]	-0.11* (0.00) [-0.16,-0.07]	-0.18* (0.00) [-0.19,-0.17]	0.27	2878
		-2.26* (0.00) [-2.44,-2.07]		-1.77* (0.00) [-1.95,-1.58]	1.57* (0.00) [1.32,1.83]	-0.36* (0.00) [-0.39,-0.32]	-0.30* (0.00) [-0.34,-0.27]	1.00* (0.00) [0.93,1.07]	-0.08* (0.00) [-0.13,-0.04]	-0.20* (0.00) [-0.21,-0.19]	0.27	2878
			-1.22* (0.02) [-1.44,-1.00]	-1.68* (0.00) [-1.89,-1.46]	1.65* (0.00) [1.36,1.94]	-0.31* (0.00) [-0.34,-0.27]	-0.34* (0.00) [-0.38,-0.30]	0.76* (0.00) [0.67,0.86]	-0.05* (0.00) [-0.10,-0.01]	-0.17* (0.00) [-0.19,-0.16]	0.21	2878
TSLS using Second Instrument	-1.44* (0.00) [-1.72,-1.16]			-1.62* (0.00) [-1.85,-1.40]	1.96* (0.00) [1.67,2.24]	-0.17* (0.00) [-0.21,-0.14]	-0.30* (0.00) [-0.33,-0.26]	0.47* (0.00) [0.41,0.53]	0.10* (0.00) [0.06,0.14]	-0.19* (0.00) [-0.20,-0.18]	0.22	2878
		-0.86* (0.00) [-1.09,-0.63]		-1.85* (0.00) [-2.07,-1.63]	2.30* (0.00) [2.01,2.58]	-0.18* (0.00) [-0.22,-0.15]	-0.24* (0.00) [-0.28,-0.21]	0.38* (0.00) [0.32,0.43]	0.13* (0.00) [0.09,0.17]	-0.19* (0.00) [-0.20,-0.17]	0.22	2878
			-2.09* (0.00) [-2.39,-1.78]	-1.49* (0.00) [-1.71,-1.28]	1.67* (0.00) [1.39,1.95]	-0.21* (0.00) [-0.25,-0.18]	-0.34* (0.00) [-0.37,-0.30]	0.80* (0.00) [0.71,0.89]	0.07* (0.00) [0.03,0.11]	-0.20* (0.00) [-0.21,-0.19]	0.24	2878

Notes: Estimations are by weighted least squares where weights have been determined by "1/no. of cities from each country." All regressions include rank fixed effects and a constant. * represents significance at the 10% level. The p-values (for the null hypothesis of no effect) associated with the wild cluster bootstrap-t method developed by Cameron et al. (2008) are given in parenthesis to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.