Trade and Cities

Cem Karayalcin and Hakan Yilmazkuday

Many developing countries display remarkably high degrees of urban concentration that are 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 of 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 of 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 been weak so far; in the existing literature, trade policies are poorly measured (or are 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 the largest cities that have ports and, thus, have better access to external markets) liberalizing trade leads to a reduction in urban concentration.
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How does trade liberalization affect urban concentration? This is an important question because many developing countries display a remarkable degree of urban concentration. 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 examples using two measures of urban concentration, the percentage of urban population in the largest city and the 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 approximately 12%, 16% of their urban population lived in 14 large cities that had populations of more than 500,000. Similar levels of

Cem Karayalcin (corresponding author) is a Professor of Economics at Florida International University, Miami, FL, USA; his email address is karayalc@fiu.edu. Hakan Yilmazkuday is an Assistant Professor of Economics at Florida International University, Miami, FL, USA; his e-mail address is hakan.yilmazkuday@fiu.edu. The authors thank Andrew Foster and two anonymous referees for their feedback. The usual disclaimer applies. A supplemental appendix to this article is available at http://wber.oxfordjournals.org/.

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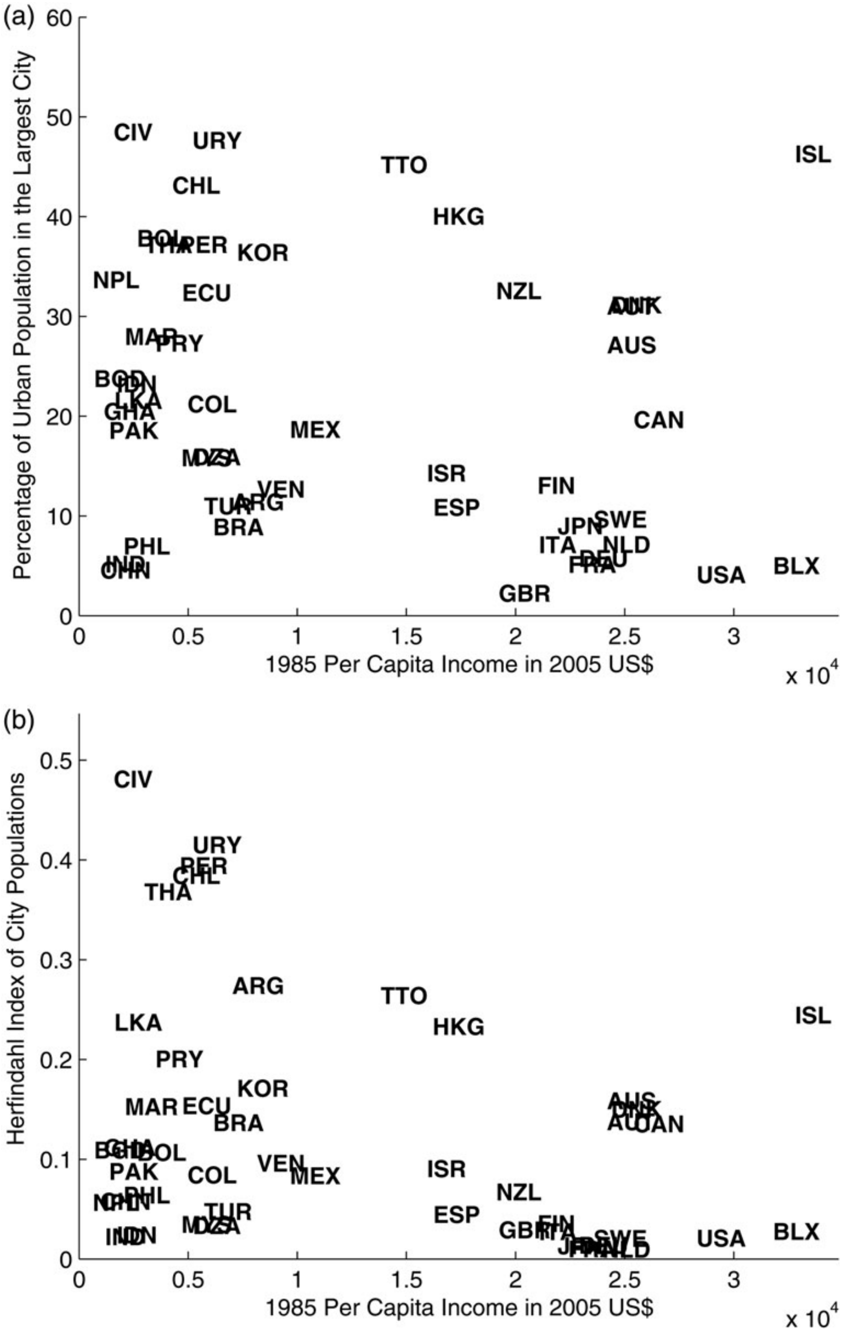
FIGURE 1. (a) Percentage of Urban Population in the Largest City
(b) Herfindahl Index of City Populations



Source: World Development Indicators.

urban concentration in the developed world had been attained in 1880, when the average level of urbanization was much higher, at 23%. The number of large cities in the developing world as well as their share of the total urban population increased radically between 1930 and 1980, when they had 43% of the urban population, a number that paralleled that of developed countries. However, the level of urbanization in the latter stood at 65%, whereas developing market economies had an urbanization level that was half that size.[1] As a recent survey puts it, "Since 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, 64).

Policymakers and international agencies are concerned about the cost of over-population, congestion, crime, and "unbalanced urban hierarchies" in these megacities.[2] The literature in urban and development economics points out that although a high degree of urban concentration might be useful in the early stages of development by conserving the 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] Once a certain level of urban concentration is attained, economies of scale become exhausted, and mega-cities transform into sites that are excessively congested with high infrastructure costs. The consequences of this misallocation are both static and dynamic. For instance, Henderson (2003) provides evidence that supports the notion that excessive urban concentration has significant 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 body of literature that argues that the 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). 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, and the disproportionate provision of local public services.[4]

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).
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 led to low developmental outcome measures.

Another argument proffered along these lines, and the one that we empirically test in this paper, is that mega-cities may arise from the adoption of restrictive trade policies. 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 it 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 increases 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 to think of the agglomeration forces that come 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 as possible to the large number of consumers (backward linkages) found in a metropolis. Firms also prefer a metropolis because it offers 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 abroad and would 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 can go. According to 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) introduce two additional forces of dispersion. One force arises from the assumed immobility of some workers ("farmers'") across regions that induce firms and mobile industrial workers to spread out close to 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 force arises from the assumption that markups fall with the intensity of local competition.[5] Thus, firms prefer to spread out spatially to avoid reduced profits caused by lower markups in cities

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

with high firm concentrations. Studies such as those by Monfort and Nicolini (2000) and Paluzie (2001) that predict that trade liberalization that exceeds a certain threshold induces higher levels of urban concentration rely on the intensity of the dispersion forces falling faster than that of the agglomeration forces. Studies such as those by Krugman and Livas (1996) and Behrens et al. (2007), which reach the opposite conclusion, have built into their structure the reverse configuration of the two opposing forces.

With locations that differ in some dimension from others, additional considerations arise. Working in a perfectly competitive setup, Rauch (1991) 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 larger than 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 because the cost of access to foreign markets 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 has 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) argues 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, which 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-Shakhs 1972; Rosen and Resnick 1980; Wheaton and Shishido 1981; Mutlu 1989; Ades and Glaeser 1995; Junius 1999; Davis and Henderson 2003; Moomaw and Alwosabi 2004).

The question of whether trade liberalization intensifies the forces of urban agglomeration or dispersion becomes an empirical one. The empirical literature on

6. There is now also a small body of 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.

the subject may be divided into two groups.[7] The first group relies on cross-country regressions, whereas the second group studies the 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 14 papers surveyed by 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 second group by Redding and Sturm (2008), which examines the effects of the loss of trading partners triggered by the division of Germany on urban concentration, finds that trade reduces urban concentration.

To understand these results, it is useful to start with the second group, which 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). Regarding the measure of trade openness, it must be noted that half of the papers in this group use Mexican data and 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 US. Because these regions were relatively more industrialized and richer than the rest of Mexico prior to liberalization, it is not surprising 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 1985–94 period tended to have employment that grew faster in regions that are not usually associated with 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. 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 by 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). 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 issue is recognized by Ades and Glaeser (1995), where trade openness

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

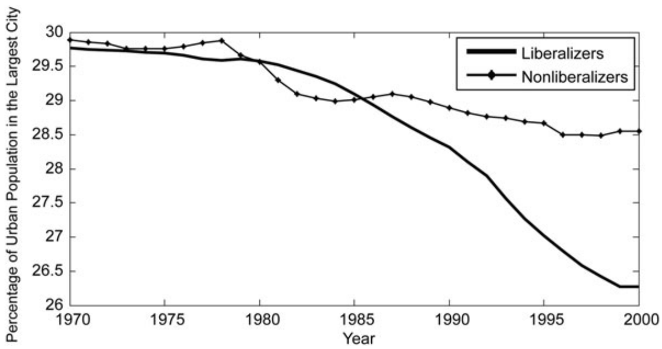
(as measured by trade-to-GDP ratio) loses its significance in IV regressions, thereby calling into question its causal effect on urban concentration.

In this paper, we take the question of causality seriously and differ from the existing literature by avoiding the use of endogenous "outcome" measures (such as 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 attempt to answer the correct 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 as well as archival sources for the 1980s that have not been used previously). Based on an empirical identification strategy in which 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 obtained results are robust to many alternative estimation methodologies and considerations 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 barely any discernible difference. If our argument is valid, we should see a significant divergence after the treatment. This is exactly what we observe in figure 2. With the Uruguay round of liberalization, a dramatic divergence begins 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. 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

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

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 from 1970–1985 for comparison. 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.

Source: World Development Indicators.

that standard instruments are not useful in this context. In the final main section, we discuss our estimation results. A concluding section ends the paper.

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 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, such as 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.

Openness as a Continuous Treatment

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

We follow an alternate strategy that takes post-1990 trade liberalization as a treatment. We consider the following question: do the rates of growth of the populations of cities in a given country accelerate relative to that of the largest city in liberalizing countries (the treatment group) compared with 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 the 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 must be a group of countries that were subject to treatment. ET cogently argue that the Uruguay round of 1986–94 provided exactly this type of treatment. Prior to the Uruguay round, very few developing countries had undergone any serious trade liberalization, whereas the 1986–94 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 kept 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:

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

where the dependent variable is the change in a city-specific urban concentration measure, which is 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

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

9. See ET for a detailed discussion and list.

where p_m^i is the population of the i^{th} largest (i.e., most populated) city in country j in year m. To capture the convergence effects, we include the log initial population of the i^{th} largest city, $\ln p_{1985}^{i,j}$, 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

$$\Delta \ln (1 + t^j) = \ln (1 + t_{2000}^j) - \ln (1 + t_{1985}^j),$$

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 $\Delta \ln p^{i,j} > 0$ 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 $\ln p_{1985}^{i,j}$ has been included in the regression to capture the convergence effects among small cities because a small city may grow faster than a larger city (where a larger city is not necessarily the largest city). Hence, the coefficient in front of the log initial population β also has an expected negative sign.[10]

We employ two alternative estimation methods: Ordinary Least Squares (OLS) and Two Stage Least Squares (TSLS). Although 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 are 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 causally affect urban concentration. Although it is difficult 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 (Acemoglu et al. 2001, 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 that trade policy is a choice variable and therefore endogenous, there remains a need for a source of exogenous variation in the trade policies of the 1980s and 1990s. Here, we again follow ET in taking the view that the largest 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

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

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 also in wide use by the end of World War II. The creation of GATT in 1947 and, much later, the 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 with tariffs the levels that dated back to the policy shift of the 1930s.

To see how this history helps us address the possible endogeneity of our treatment variables, we argue, following ET, that an exogenous component can be constructed in the following manner. We first observe that the interwar shocks led all countries toward more protectionist policies. The degree and the duration of protectionism each country adopted, however, depended on the size of the exogenous shock to which it was subjected by the Great Depression. Thus, those countries that suffered less from the Great Depression had relatively lower tariffs and less persistent protection later. Furthermore, for a country to be able to see a large cut in tariffs later, not only did it have to be willing to cut them, but 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 to reduce tariffs under the Uruguay round. The first of these instruments is defined as

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

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 it had to enter the Uruguay round with a willingness to actually cut 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 toward 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 that predicted the speed of trade liberalization roughly five decades later, we construct our second instrument as the interaction of the intensity of the Great Depression (as measured by the average deviation of the 1930–5 GDP level from the 1929 level) with, again, the pre-Uruguay tariff level:

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

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

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

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

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 occur 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 as the capital city and/or a port city, country-specific log initial domestic transportation infrastructure, and country-specific regime change) that we define below. The regression equation is revised as follows:

$$\Delta \ln p^{i,j} = \alpha \Delta \ln(1 + t^j) + \mu_y \Delta \ln(y^j) + \mu_{y^2}(\Delta \ln(y^j))^2 + \mu_x X_p^{i,j} + \beta \ln p_{1985}^{i,j} + c,$$

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 include economic growth squared in addition to economic growth to capture any nonlinear relation between the change in urban concentration and economic growth.[11] Henderson (2000) shows that urban concentration increases with per capita income up to a certain level and then declines.

11. We also considered only the rate of growth itself; however, it was econometrically insignificant. These results are available upon request.

Within these additional explanatory variables, the only concern is the possible endogeneity of the country-specific economic growth. Therefore, in the TSLS estimation of the robustness analysis, in addition to instrumenting the tariff change according to the first stage regression of

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

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

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

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, to obtain a healthy comparison across the regression results, we use information from all cities in our sample. The number of cities differ across countries, and some countries are ignored due to the availability of the data for instruments. We also consider a robustness analysis in which we use all of 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 country. Because 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 numbers of cities (up to 80) from each country. We also consider another robustness analysis in which we weight the information 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). These authors 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).

Data

Because we want 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, the previous literature has typically used the population in the largest city (and its share of the 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 US, in our benchmark case, we consider 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 differences in the rates of population growth 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 the 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 agglomerations/metropolitan areas that include a central city and neighboring towns (suburbs) forming a connected region of a dense, predominately urban population that is economically and culturally linked to the central city (e.g., by commuters).[16] The data can be downloaded from http://www.populstat.info/,

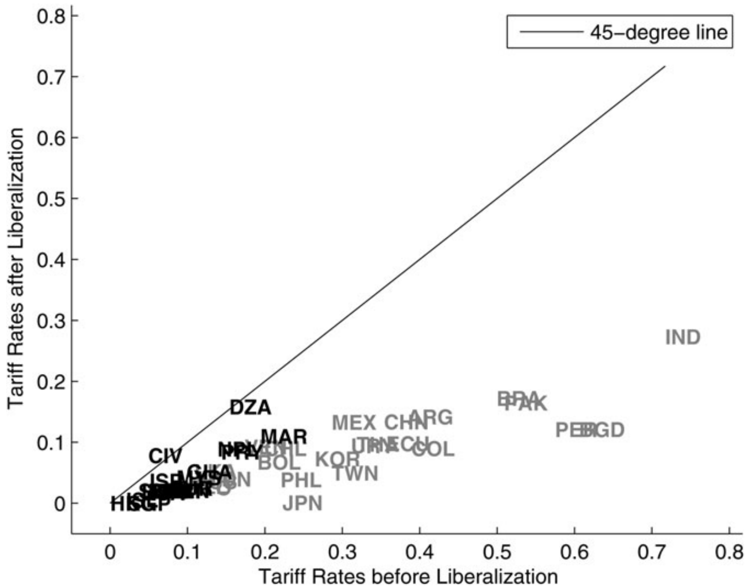
13. Redding and Sturm (2008) find that loss of trade leads to more urban concentration and a slower rate of population growth for border cities. This finding is similar to ours in that we find that the creation of trade leads to less urban concentration.

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 started to decline prior to the signing of the agreement and that the decline has accelerated with it. See Figure 3 in ET.

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.

FIGURE 3. Tariffs on Capital Goods - After versus Before



Notes: The country codes in gray represent liberalizers. See figure 2 for the exact list of countries.

Source: Estevadeordal and Taylor (2013).

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. 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.

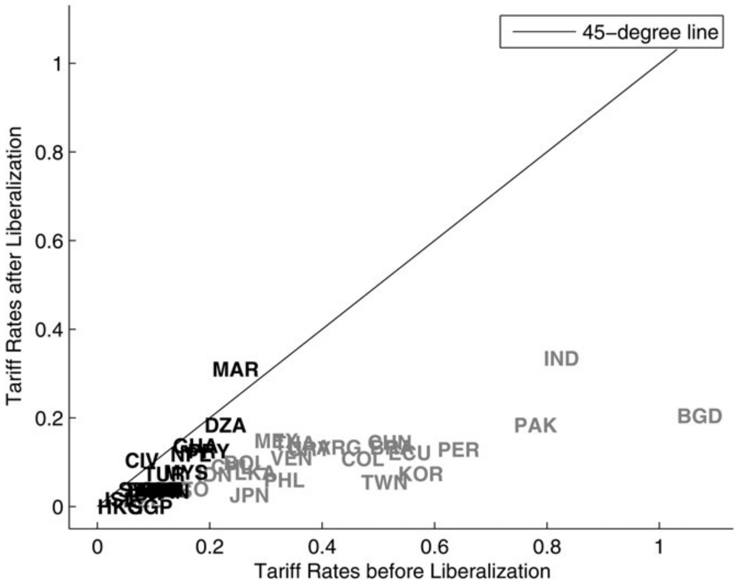
GDP PER CAPITA. The country-specific GDP per capita data are obtained from PWT (rgdpch) for the years 1985 and 2000.

17. Country-specific details of the data set are given in the supplemental appendix (available at http://wber.oxfordjournals.org/), where we present the exact dates and sources of data for tariff rates and city-level populations for each country in our sample for the pre-liberalization and post-liberalization periods. In the supplemental appendix, we also include a table showing the representativeness of our country sample.

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

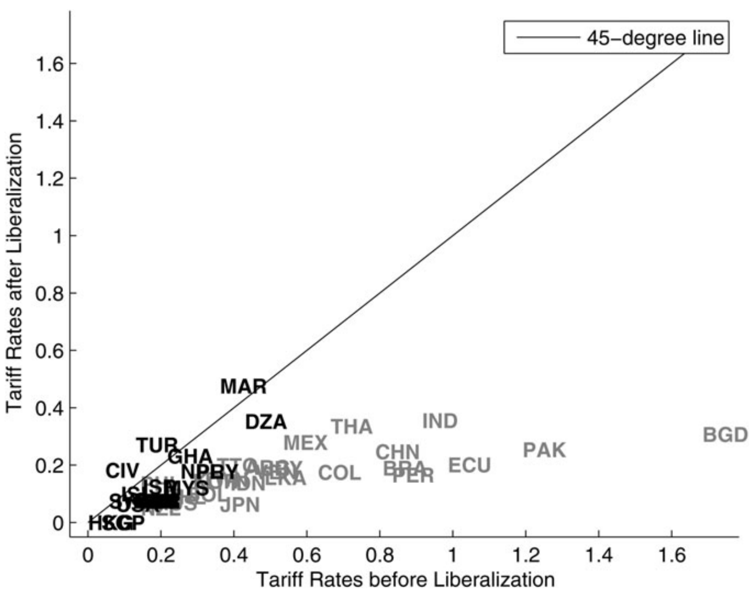
19. Historical GDP data from Angus Maddison has been obtained from http://www.ggdc.net/MADDISON/.

FIGURE 4. Tariffs on Intermediate Inputs - After versus Before



Notes: The country codes in gray represent liberalizers. See figure 2 for the exact list of countries.
Source: Estevadeordal and Taylor (2013).

FIGURE 5. Tariffs on Consumption Goods - After versus Before



Notes: The country codes in gray represent liberalizers. See figure 2 for the exact list of countries.
Source: Estevadeordal and Taylor (2013).

Schooling. The country-specific measure of human capital is proxied by the total years of schooling obtained from Barro and Lee (2013). 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 the studies of 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 because it benefits from increased access to foreign markets as a port city. Consequently, we would expect that the 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 a democracy when their Gastil index decreases from above 3 (in 1970–4) to below 3 (in 1980–4). We use the Gastil index as documented by Barro and Lee (1994). This variable is important for our purposes because the literature (e.g., Ades and Glaeser 1995) has documented a significant and robust positive relationship between the urban concentration levels and dictatorships.

Estimation Results

The regression results for our benchmark case are given in table 1, where the sample is the same across different regressions. The 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

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)			−0.09* (0.00)	0.12	2878
	[−0.82, −0.61]			[−0.06, −0.05]		
		−0.97* (0.00)		−0.10* (0.00)	0.16	2878
		[−1.07, −0.87]		[−0.11, −0.10]		
			−0.33* (0.00)	−0.09* (0.00)	0.10	2878
			[−0.42, −0.23]	[−0.10, −0.08]		
TSLS using	−1.12* (0.01)			−0.10* (0.00)	0.14	2878
first instrument	[−1.25, −0.98]			[−0.11, −0.10]		
		−1.27* (0.02)		−0.11* (0.00)	0.15	2878
		[−1.41, −1.13]		[−0.12, −0.10]		
			−0.50* (0.00)	−0.09* (0.00)	0.10	2878
			[−0.64, −0.36]	[−0.10, −0.08]		
TSLS using	−1.08* (0.01)			−0.10* (0.00)	0.10	2878
second instrument	[−1.32, −0.83]			[−0.11, −0.09]		
		−1.03* (0.01)		−0.11* (0.00)	0.11	2878
		[−1.24, −0.81]		[−0.12, −0.10]		
			−1.26* (0.00)	−0.12* (0.00)	0.12	2878
			[−1.46, −1.05]	[−0.13, −0.11]		

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 parentheses to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Source: Authors' analysis based on data sources discussed in the text.

is employed).[20] For instance, when the tariff change in capital goods is considered, the significantly estimated α by OLS is approximately −0.71 , suggesting that when tariffs are reduced by 1%, on average, the cities that are smaller than the largest city grow 0.71% faster than the largest city in the same country over the 15-year period between 1985 and 2000. Because the average tariff change in capital goods is about 12%, on average, smaller cities grew about 8.4% faster than did the largest 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 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 the initial population is also negative and significant, as expected, in table 1. The explanatory power of the regressions measured by R-squared is low, primarily because we ignore other channels that might affect city population growth. We obtain higher values in the following tables that report the 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 S2-S4 (in the supplemental appendix, available at http://wber.oxfordjournals.org/). 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 strong instruments.

The regression results for our first robustness analysis are given in table 2. We include per capita GDP growth, per capita GDP growth squared, a capital city dummy, a port dummy, initial domestic transportation infrastructure, and a regime change dummy in our regressions. As in the benchmark case, the 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 1capita GDP growth enters the regressions significantly with a negative sign, whereas per capita GDP growth squared enters significantly with a positive sign. Therefore, there is evidence of a nonlinear relation between the change in urban concentration and economic growth: in countries that have grown faster, the largest city has grown faster than other smaller cities (i.e., the urban concentration has increased).[21] The results for the first-stage

20. Changes in the tariffs for consumption goods would generally 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).

21. We also considered only the economic growth itself; however, it was econometrically insignificant. These results are available upon request.

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

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 parentheses to the right of the corresponding estimates. The 90% confidence intervals are given in brackets underneath the corresponding estimates.

Source: Authors' analysis based on data sources discussed in the text.

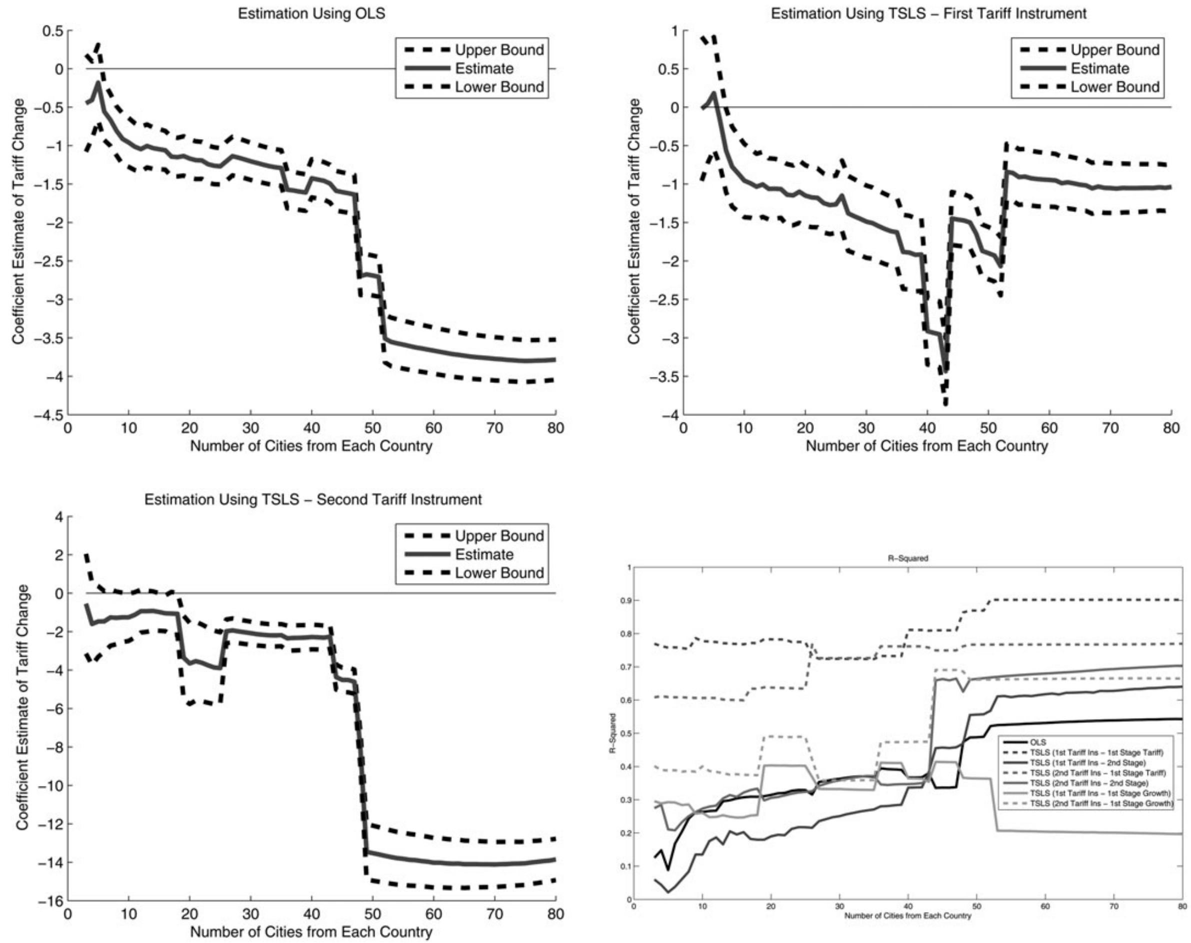
regressions to instrument both the tariff change and the economic growth are given in appendix tables S5-S6 (in the supplemental appendix, available at http://wber.oxfordjournals.org/). As is evident, all considered instruments enter the first-stage regressions significantly, and the R-squared values are relatively high, showing the strength of our instruments. Returning 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 converge less to (or diverged from) that 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 estimate β for the log initial population is again negative and significant, as expected. The explanatory power of regressions has increased compared with the benchmark case.[22]

The regression results for our second robustness analysis are given in figures 6–8, where we treat countries symmetrically by considering equal numbers of cities from each country. Because 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 grow 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 it becomes higher as we increase the (equal) number of cities from each country (although the number of countries decreases in such a case).[23]

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 S7-S8 (in the supplemental appendix, available at http://wber.oxfordjournals.org/). 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 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 S9-S10 (in the supplemental appendix, available at http://wber.oxfordjournals.org/), where the estimates of α are negative and significant in all cases.

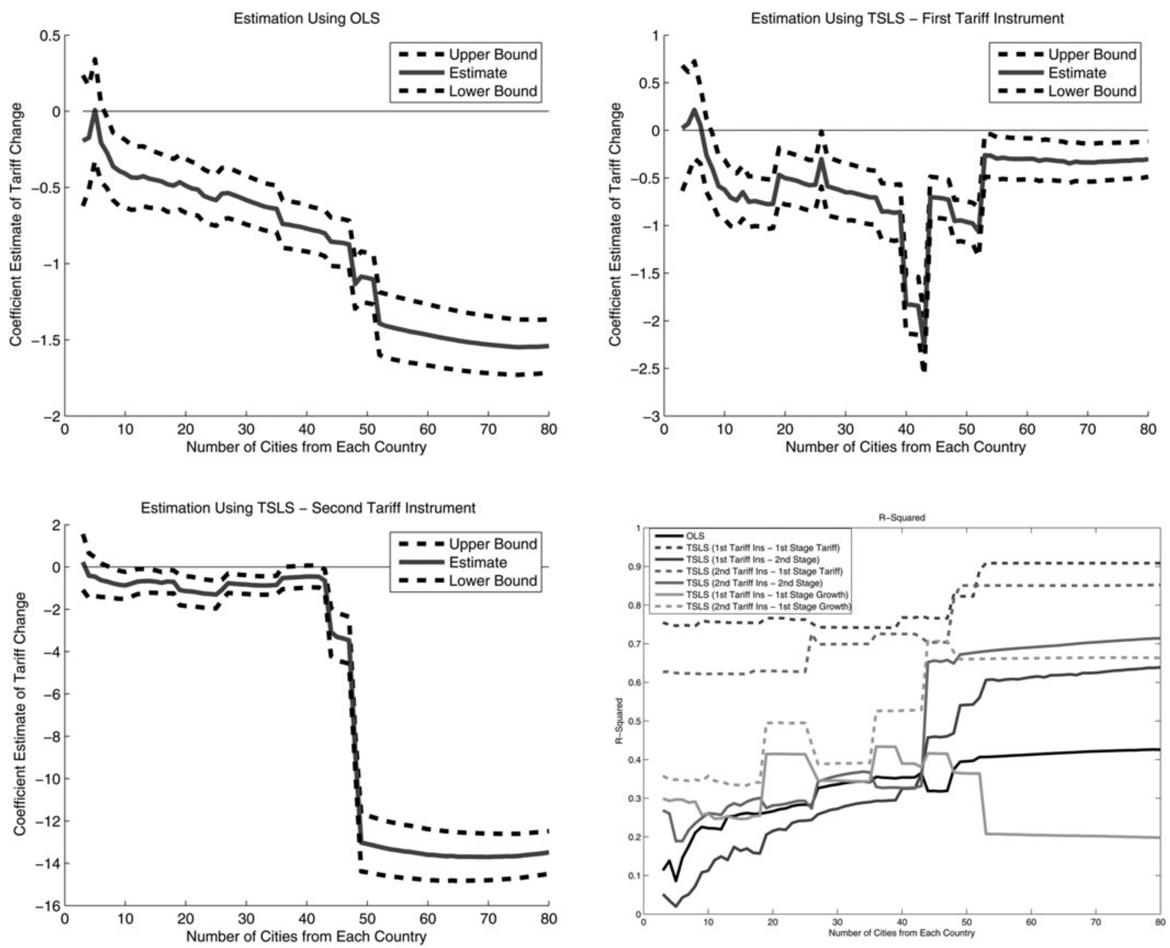
FIGURE 6. Results with Equal Number of Cities from Each Country - Tariff Change in Capital Goods



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 90% confidence intervals.

Source: Authors' analysis based on data sources discussed in the text.

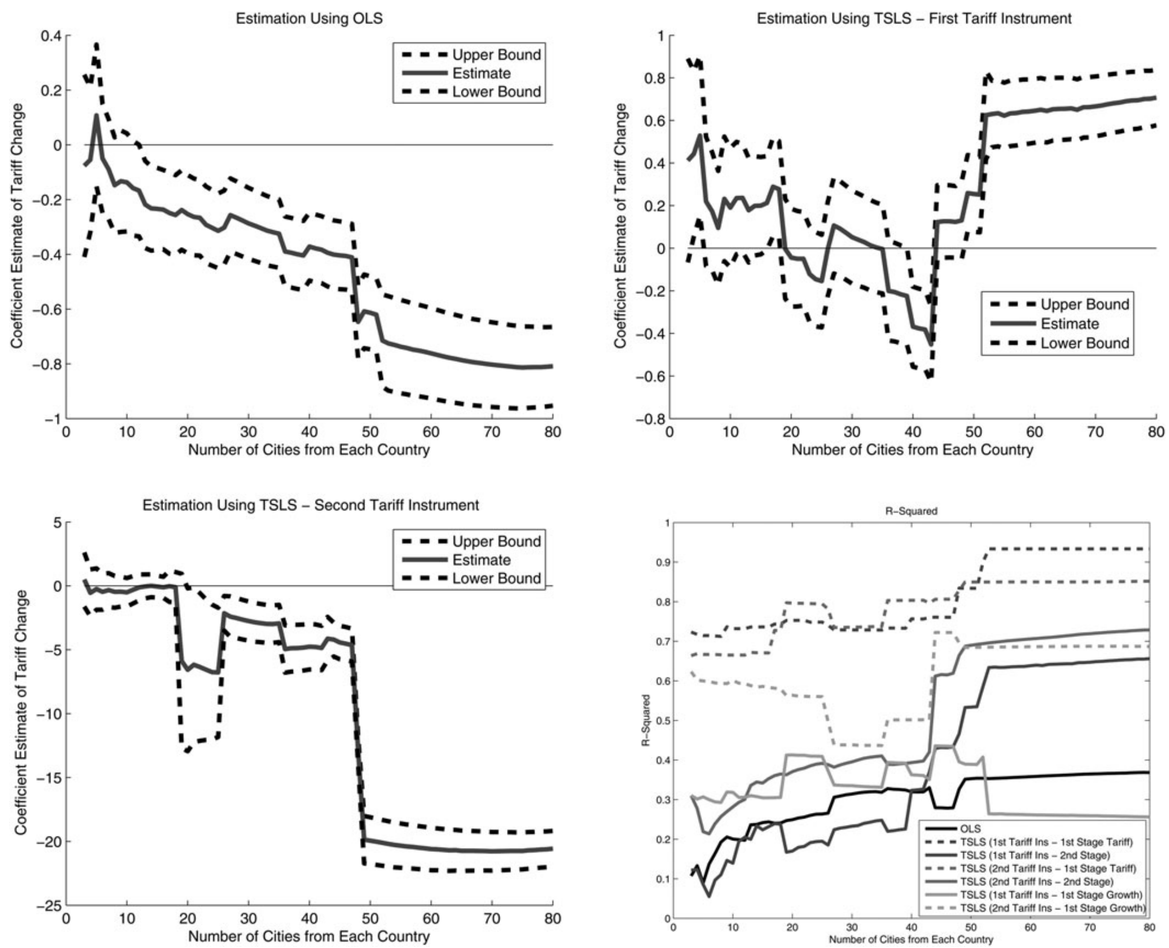
Figure 7. Results with Equal Number of Cities from Each Country - Tariff Change in Intermediate Inputs



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 90% confidence intervals.

Source: Authors' analysis based on data sources discussed in the text.

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 90% confidence intervals.

Source: Authors' analysis based on data sources discussed in the text.

Conclusion

In this paper, we explore the effects of trade liberalization on the change in urban concentration. The theoretical literature on the subject identifies two relevant and opposing mechanisms. The first mechanism suggests that trade liberalization may diminish the effect of the agglomeration forces leading to the creation of megacities and thus may lead to reduced urban concentration. The second mechanism postulates that trade liberalization may lead to the expansion of those megacities that have better access to world markets, thereby increasing urban concentration. The 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 after controlling for the largest cities that have ports and, thus, 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 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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