WP/07/156

IMF Working Paper

Trade Openness and Growth: Pursuing Empirical *Glasnost*

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INTERNATIONAL MONETARY FUND

 WP/07/156

IMF Working Paper

Middle East and Central Asia Department

Trade Openness and Growth: Pursuing Empirical *Glasnost*

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Authorized for distribution by Klaus Enders

June 2007

Abstract

Studies of the impact of trade openness on growth are based either on cross-country analysis—which lacks transparency—or case studies—which lack statistical rigor. We apply transparent econometric methods drawn from the treatment evaluation literature to make the comparison between treated (i.e., open) and control (i.e., closed) countries explicit while remaining within a unified statistical framework. First, matching estimators highlight the rather far-fetched country comparisons underlying common cross-country results. When appropriately restricting the sample, we confirm a positive and significant effect of openness on growth. Second, we apply synthetic control methods—which account for endogeneity due to unobservable heterogeneity—to countries that liberalized their trade regime, and we show that trade liberalization has often had a positive effect on growth.

JEL Classification Numbers: C21, C23, F43, O57

Keywords: trade openness, growth, matching estimators, synthetic control

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I. Introduction

The relationship between trade openness or economic liberalization on the one hand, and income or growth on the other, is one of the main conundra in the economics profession, especially when it comes to combining theoretical and policy-related with empirical findings. The theoretical advantages of trade for growth are known at least since Ricardo: international trade enables a country to specialize using its comparative advantage and benefit both statically and dynamically from the international exchange of goods.[1] From a policy perspective, the continuing efforts to liberalize international trade on a multilateral basis—first under GATT and now WTO leadership—have contributed to better market access and rates of growth of international current account transactions much above worldwide economic growth. From an empirical point of view, however, the trade-growth link is still under discussion, both from a methodological angle and regarding the size and significance of the estimated effects.

Testing the empirical relevance of important theoretical predictions in macroeconomics, growth theory, and political economics builds on cross-country evidence. In the attempt to detect correlations or causal relationships between aggregate variables, usually within-country variation is not sufficiently large to estimate the parameters of interest in a significant way, or it is so peculiar to the countries under consideration that the estimates have no external validity. At the end of the day, one must use cross-country variation to make inference over macro variables.

There is, however, widespread skepticism regarding the possibility of making sound inference based on cross-country data. The empirical debate over the trade-growth nexus is a paradigmatic case. As Bhagwati and Srinivasan (2001) point out, both globalization supporters and foes rely on cross-country estimates, which dramatically suffer from specification problems, endogeneity, and measurement errors. According to them, cross-country regression estimates are completely unreliable. Robust evidence on the relationship between trade openness and growth "can come only from careful case studies of policy regimes of individual entries" (p. 19). Case studies, however, also suffer from apparent weaknesses as they lack statistical rigor and are exposed to arbitrary case selection. Instead of throwing out the baby (that is, cross-country statistical analysis) with the bath water (that is, growth regressions of any type), we propose to apply recent econometric techniques that perform data-driven comparative case studies.

In this paper, we evaluate the effect of a binary treatment—trade openness or economic liberalization—on the outcome, changes in per capita income. We first show the pitfalls stemming from applying estimators based on cross-sectional information—like Ordinary Least Squares (OLS)— to the trade-growth nexus. We use microeconometric matching estimators from the treatment evaluation literature that are based on the same identifying assumption as OLS (conditional independence; that is, the selection into treatment is fully determined by observable characteristics) to make the estimation procedure more transparent—to bring *glasnost* to muddied waters. In doing so, we are able to show that the country comparisons that lie behind simple cross-sectional estimates are often more than far-fetched. We argue that it is important to control for continent or macro-region dummies to make cross-country comparisons more sensible.

[1]Some theoretical models developed in the literature imply negative (or at least not necessarily positive) growth effects from trade; see the short discussion in Rodriguez and Rodrik (2001). By and large, however, macro theory has identified international exchange as a potential source of growth, theoretical exceptions being often associated with market failures that should be corrected by national policies different from protectionism; see, for example, the discussion in Bhagwati (2002). Rassekh (2004) and the December 2004 issue of the *Journal of International Trade and Economic Development* provide a recent review of the discussion.

We also show, however, that this remedy may not be enough if open and closed countries are not evenly distributed across regions—that is, they lack common support. For example, for a prominent openness indicator from the literature, developed countries should not be used to investigate the trade-growth link as all countries in this group are open and do not provide the necessary within-group variation to estimate the counterfactual outcome in the case of no treatment. Cleaning the sample for regions with no common support between treated and control units, we confirm a significant effect of openness on growth within selected areas and after 1970.

In the end, we wind up with the usual statistical trade-off between internal and external validity. While dropping the country groups that show little or no variation with regard to the treatment produces more sound statistical inference (internal validity), these results cannot be extrapolated to make general statements that go beyond the sample effectively used in the estimation, reducing external validity. In other words, it is unlikely that the effect of openness on growth can be robustly estimated for a world-wide sample of countries, casting doubt on much of the cross-sectional growth literature that strives to cover an ever-increasing set of countries.

While we find the matching results instructive for a number of reasons, they are not able to deal with two major endogeneity issues of OLS estimates in the trade-growth context—unobservable country heterogeneity and reverse causality. These limitations of cross-sectional estimators further strengthen the case for using panel set-ups to investigate the openness-growth nexus as argued in the recent literature. In fact, current panel methods can overcome some of the OLS weaknesses by using within-country (i.e., time-series) information to control for unobservable time-invariant country characteristics. However, as long as these estimators still use some cross-country variation—as does, for example, the difference-in-differences estimator—they still suffer from a lack of transparency. Therefore, we apply the synthetic control method, a recent econometric tool that is close in spirit to the matching estimator mentioned above and is able to account for time-varying country unobservables, in a panel set-up to infer the openness-growth link. In this context, the treatment takes a time dimension, i.e., it coincides with trade liberalization instead of openness. The advantage of this approach lies in the transparent construction of the counterfactual outcome of the treated, namely the synthetic control—a linear combination of untreated countries. These comparison countries are selected based on their similarity to the treated before the treatment, both with respect to relevant covariates and the past realizations of the outcome. We study all episodes of trade liberalization that took place after 1965 in countries that form part of the IMF's Middle East and Central Asia Department (MCD). We find that, for most of these countries, trade liberalization has had a positive effect on growth.

The remainder of this paper is structured as follows. In Section II, we briefly review the empirical literature related to our paper, mainly studies on trade and growth aiming at overcoming some of the OLS weaknesses and studies that apply matching estimators to cross-country data sets. In Section III, we briefly present the data sources and variables of interest. In Section IV, we introduce matching estimators and apply them to two data sets recently used in the literature. Going beyond a merely descriptive discussion of the association between openness and growth, the synthetic control method is employed in Section V to empirically explore the effect of trade liberalization on growth in MCD countries. Section VI concludes.

II. Literature Review

A. Empirical Studies on Trade Openness and Growth

Providing conclusive empirical evidence on the intuitively positive causal effect of trade on growth has been a challenging endeavor, complicated by a multiplicity of factors; see, for example, Winters (2004) for an overview. Most of the literature has used cross-country evidence that suffers from numerous shortcomings, related to both the measurement of openness and econometric modeling.

Following Barro's (1991) seminal paper on growth regressions, several prominent cross-country studies established a positive link between trade openness and growth; these studies include Dollar (1992), Sachs and Warner (1995), and Edwards (1992, 1998). Similarly, Vamvakidis (2002) finds, in a historical context, evidence that trade is associated with growth after 1970 but not before. In a stern review of the cross-sectional literature on trade and growth, Rodriguez and Rodrik (2001) criticize the choices of openness measure and weak econometric strategies. They find little evidence that open trade policies as measured in the aforementioned contributions are significantly associated with economic growth once they correct for the weaknesses they point out. Harrison (1996) shows that most of the explanatory power of the composite openness dummy assembled in Sachs and Warner (1995) comes from the non-trade components of this measure.

From a methodological perspective, deep skepticism has been brought to bear against cross-country evidence on the trade-growth issue. In addition to the citation above, Bhagwati and Srinivasan (2002, p.181) point out that "cross-country regressions are a poor way to approach this question" and that "the choice of period, of the sample, and of the proxies, will often imply many degrees of freedom where one might almost get what one wants if one only tries hard enough!" Pritchett (2000) also argues for detailed case studies of particular countries and growth events. Levine and Renelt (1992) and Temple (2000) apply extreme-bounds analysis to show that the results of cross-country growth regressions are not robust to even small changes in the conditioning information set (i.e., right-hand side variables).

Focusing on identification issues, cross-country studies suffer from two major weaknesses: reverse causality (that is, liberalized trade causes higher economic growth as opposed to more trade being the result of economic growth) and endogeneity (e.g., country-specific omitted characteristics affecting both openness and growth). Dealing with regressor endogeneity has triggered a substantial amount of interest in the use of instrumental variables (IV). This family of models suggests using regressors that have an impact on openness, but are uncorrelated with income. Using gravity models, Frankel and Romer (1999) and Irwin and Tervio (2002) find a positive effect running from trade to growth by isolating geographical components of openness that are assumed independent of economic growth, including population, land area, borders, and distances. But even these presumably exogenous instruments could have indirect effects on growth, thereby biasing the estimates.[2] Dollar and Kraay (2003) suggest estimating the regressions in differences and using lagged openness as instrument. However, the simultaneity bias in the trade-growth context could extend over time—trade today may depend on growth tomorrow via imports for investment purposes—and using lagged variables as instruments is unlikely to fully correct for the bias. As an alternative approach to classic IV, Lee, Ricci, and Rigobon (2004) use identification

[2]Moreover, they relate primarily to trade volumes, not trade policies, as discussed by Rodriguez and Rodrik (2001).

through heteroskedasticity in a panel framework, and find that openness has a small, positive, but not particularly robust effect on growth. They have to rely, however, on the non-testable assumption that the structural shocks in the system of simultaneous equations are uncorrelated.

Another strand in the trade and growth literature seeks to improve upon cross-country regressions by employing panel methods, geared at controlling for time-invariant unobservable country effects. An early example is Harrison (1996), who uses fixed-effect estimators and finds a stronger impact of various openness indicators in a panel set-up compared to standard cross-country regressions. Wacziarg and Welch (2003) further the discussion in the literature in three directions: they update, expand, and correct the trade openness indicator in Sachs and Warner (1995); they show that the Sachs and Warner (1995) results of a positive effect of trade on growth break down if extended to the 1990s in a cross-sectional set-up; and they provide evidence in a panel context that, even in the 1990s, there is a positive effect of trade on growth when the analysis is limited to within-country effects.[3] Slaughter (2001) uses a difference-in-differences approach to infer the effect of four very specific trade liberalization events on income growth dispersion, and finds no systematic link between trade liberalization and per capita income convergence. Giavazzi and Tabellini (2005) also apply a difference-in-differences approach to study the interactions between economic and political liberalizations. They find a positive and significant effect of economic liberalization on growth, but they claim that this effect cannot be entirely attributed to international trade, as liberalizations tend to be accompanied by other policy improvements.

B. Empirical Studies Applying Matching Estimators to Macro Data

A limited, but growing, strand of aggregate empirical literature—particularly in political economics—apply microeconometric estimators developed in the treatment evaluation literature to cross-country data, in order to overcome the weaknesses of OLS in cross-sectional set-ups. Persson and Tabellini (2005) use propensity-score matching methods to estimate the effects of political institutions (proportional against majoritarian electoral rule; presidential against parliamentary regime) on a set of relevant economic variables. Edwards and Magendzo (2003) apply matching estimators to analyze the macroeconomic record of dollarized economies. Atoyan and Conway (2006) use matching estimators to evaluate the impact of IMF programs.

All these studies point to the fact that non-parametric (or semi-parametric) matching estimators allow the OLS linearity assumption to be relaxed. This is not their only merit, however, since the linearity assumption can be also relaxed in the OLS set-up by specifying a fully saturated model. The major advantage of matching techniques is that they allow the researcher to carefully check for the existence of a common support in the distributions of treated and control units across covariates. This advantage can be even greater in a small sample of countries, since the "matched" treated and control units can be easily identified. The "transparency" attribute of matching estimators is described and exploited in Section IV with respect to the estimated effect of trade openness on growth.

[3] Wacziarg and Welch (2003) essentially conduct difference regressions in growth, or difference-in-differences regressions in log income.

III. Data and Variables of Interest

Under the trade and growth umbrella, a whole set of relationships have been analyzed in the literature. As the dependent variable, GDP levels, changes, GDP per capita, and relative incomes (or dispersion thereof) have been used as outcome measures, mainly to distinguish between level, growth, and convergence effects. We employ the difference of (log) per capita GDP, as we are interested in the dynamic impact of trade openness over time, not only in its one-off effects on the individual income level.

For trade and openness, two major groups of indicators have emerged in the literature, addressing somewhat different questions. On the one hand, some studies use simple measures of trade volumes that are particularly subject to endogeneity problems (especially if normalized by GDP), and have in fact been used especially within an IV framework (e.g., Frankel and Romer, 1999). On the other hand, there have been repeated efforts to identify the impact of trade policy and lower trade barriers on economic growth. To this end, a variety of indicators have been constructed, the most notable among them being the binary indicator by Sachs and Warner (1995), extended, updated, and revised by Wacziarg and Welch (2003); short SWWW.[4] According to this indicator, a country is considered closed to international trade in any given year if at least one of the following conditions is satisfied: (i) average tariffs exceed 40 percent; (ii) non-tariff barriers cover more than 40 percent of its imports; (iii) it has a socialist economic system; (iv) the black market premium on the exchange rate exceeds 20 percent; and (v) much of its exports are controlled by a state monopoly. A country is open if none of these conditions applies. As our binary indicator of openness—or economic liberalization in the language of Giavazzi and Tabellini (2005)—we use the SWWW trade openness policy dummy. In Section V, while applying synthetic control methods in a panel set-up, in line with Giavazzi and Tabellini (2005), we refer to the "treatment" as the event of becoming open, after being closed in the preceding year according to this indicator. Our treatment thus intends to capture policy changes that reduce the constraints on market operations below a critical threshold along these five dimensions.[5]

To anchor our results in the existing literature, we draw on two data sets used recently in a related context. Vamvakidis (2002) presents historical evidence of the connection between openness and growth over the period 1870-1990; we focus on the post-1950 part of his data set.[6] The data set consists of repeated country cross-sections for the periods 1950-70, 1970-90, and 1990-98. Besides the average GDP per capita growth and the openness dummy, the data set contains information on the initial GDP, investment share, population growth, secondary school enrollment, inflation, and black market premium.

The other data set we use has been analyzed in Giavazzi and Tabellini (2005) and Persson and Tabellini (2006). Of this very rich panel data set covering about 180 countries over the period 1960–2000, we use annual observations only for a few variables that are related to the question at hand: the updated SWWW, the log change in per capita GDP, and the same control variables mentioned above (with the only two exceptions that inflation is not reported, while a democracy dummy is present). In the context of synthetic control methods (see Section V), we extend the

[4]For a comparison of various indicators, see, e.g., Harrison (1996).

[5]The SWWW dummy captures, in fact, more than just openness to trade, e.g., also the socialist origin. Nevertheless, we base our analysis on the dummy, given the prominence it has achieved in the literature. Sachs and Warner (1995, p. 25) note that the socialism indicator serves as a proxy for central planning, which could be viewed as a substitute for overt trade policies such as tariffs.

[6]See Vamvakidis (2002) for a detailed description of the data sources.

outcome variable (GDP per capita) to 2005 where available, drawing on the International Monetary Fund's World Economic Outlook database. By doing so, we gain a few more observations to compare outcomes in treated and control countries, which is particularly important for countries that only liberalized recently, such as the transition economies.

IV. Cross-Country Analysis and Matching Estimators

A. Methodology

The common aim of most of the empirical studies reviewed in Section II is to assess whether a pro-openness trade policy has a causal effect on either the level or growth rate of GDP. This problem of inference involves "what if" statements and thus counterfactual outcomes. Hence, it can be translated into a treatment-control situation and analyzed within Rubin's (1974) potential-outcome framework for causal inference. The essential feature of this approach is to define the causal effect of interest as the comparison of the potential outcomes for the same unit measured at the same time: $Y(0) =$ (the value of GDP growth Y if the country is exposed to treatment $T = 0$, i.e., if it is closed to trade), and $Y(1) =$ (the value of GDP growth if the same country is exposed to treatment $T = 1$, i.e., it is open to trade). Only one of these two potential outcomes can be observed—specifically, the one corresponding to the treatment the country received—but the causal effect is defined by their comparison, i.e., $Y(1) - Y(0)$. This highlights that the estimation of the causal relationship between T and Y is hampered by a problem of missing data—i.e., the counterfactual outcomes $Y(0)$ for open countries and $Y(1)$ for closed countries.

In this setting, the aim of statistical analysis is usually that of estimating some features of the distribution of $Y(1) - Y(0)$, like

$$E[Y(1) - Y(0)], \tag{1}$$

which is called the *Average Treatment Effect* (ATE). Alternatively, one can be interested in the average treatment effect for the subpopulation of the treated observations:

$$E[Y(1) - Y(0)|T = 1], \tag{2}$$

which is called the *Average effect of Treatment on the Treated* (ATT). In the present context, the ATE corresponds to the counterfactual question: what would have been the growth rate of the countries in our sample, had they decided to switch their trade regime? On the contrary, the ATT focuses on the counterfactual question for treated units only: what would have been the growth rate of open countries, had they decided to close their economies?

Problems for the identification of these average treatment effects may arise from the existence of country-specific unobservables affecting both the two potential outcomes (or just one of them) and the treatment indicator. The fact that the treatment might be *endogenous* reflects the idea that the outcomes are jointly determined with the treatment, or that there are omitted confounders related to both the treatment and the outcomes. One of the assumptions that allows the identification of the ATE is the "unconfoundedness" condition, also referred to as "selection on observables" or "conditional independence assumption," which is the rationale behind common estimation strategies such as regression modeling and matching.[7] This assumption considers the conditioning set of all relevant pre-treatment variables X and assumes that:

$$Y(1), Y(0) \perp T|X \tag{3}$$

$$0 < Pr(T = 1|X) < 1. \tag{4}$$

That is, conditioning on observed covariates X, the treatment assignment is independent of potential outcomes.[8] Unconfoundedness says that treatment assignment is independent of potential outcomes after accounting for a set of observable characteristics X. In other words, exposure to treatment is random within cells defined by the variables X.

Under unconfoundedness, one can identify the ATE within subpopulations defined by X:

$$E[Y(1) - Y(0)|X] = E[Y(1)|T = 1, X] - E[Y(0)|T = 0, X], \tag{5}$$

and also the ATT as:

$$E[Y(1) - Y(0)|T = 1, X] = E[E[Y(1)|T = 1, X] - E[Y(0)|T = 0, X]|T = 1], \tag{6}$$

where the outer expectation is over the distribution of X in the subpopulation of treated units. In other words, thanks to unconfoundedness, one can use the observed outcome of treated (control) units, conditional on X, to estimate the counterfactual outcome of control (treated) units.

An implication of the above results is that, if we could divide the sample into cells determined by the exact values of the variables X, then we could just take the average of the within-cell estimates of the average treatment effects. Often the variables X are continuous, so that smoothing techniques are needed; under unconfoundedness several estimation strategies can serve this purpose. Regression modeling and matching are viable alternatives, which thus rely on the same identification condition. The main advantage of matching with respect to linear regression is that the latter obscures information on the distribution of covariates in the two treatment groups. In principle, one would like to compare countries that have the same values of all covariates; unless there is a substantial overlap on the two covariates distributions, with a regression model one relies heavily on model specification—i.e., on extrapolation—for the estimation of treatment effects. It is thus crucial to check how much the distributions of the treated and control units overlap across covariates, and which is the region of common support for the two distributions.

Differently from other studies that apply the propensity-score version of matching to macro data—see, e.g., Persson and Tabellini (2003)—we implement the above strategy by using the "nearest neighbor" algorithm for covariate matching.[9] Matching estimators impute the country's missing counterfactual outcome by using average outcomes for countries with "similar" values of the covariates. The nearest neighbor algorithm uses the following simple approach to estimate the pair of potential outcomes. The potential outcome associated to the treatment that country A received is simply equal to the observed outcome of A. The potential outcome associated to the treatment that country A did not receive is equal to the outcome of the nearest country that received the opposite treatment (country B), where "nearest" means that the vector of covariates of B shows the smallest distance from the vector of covariates of A according to some predetermined distance measure.

[7] See Imbens (2004) for a review of nonparametric estimation methods under this assumption.
[8] To identify the ATT, a weaker version of these conditions suffices: $Y(0) \perp T|X$ and $Pr(T = 1|X) < 1$.
[9] See Abadie and others (2004) for a description of this algorithm and the program that implements it in Stata.

Formally, define $||x||_V = (x'Vx)^{1/2}$ as the vector norm with positive definite weight matrix V, and let $||x - z||_V$ be the distance between vectors x and z.[10] Let $d(i)$ be the smallest distance from the covariates of country i, X_i, with respect to the covariates of all other countries with the opposite treatment. Allowing for the possibility of ties, define $J(i)$ as the set of indices for the countries that are at least as close to country i as its nearest neighbor:

$$J(i) = \{k = 1, ..., N | T_k = 1 - T_i, ||X_k - X_i||_V = d(i)\}. \tag{7}$$

The pair of potential outcomes for country i are estimated as:

$$\hat{Y}_i(l) = Y_i \qquad \texttt{if} \;\; T_i = l \tag{8}$$

$$\hat{Y}_i(l) = \frac{1}{\#J(i)} \sum_{k \in J(i)} Y_k \qquad \texttt{if} \;\; T_i = 1 - l, \tag{9}$$

where $\#J(i)$ is the numerosity of the set $J(i)$. The ATE and ATT are thus estimated as:

$$\tau_{ATE} = \frac{1}{I} \sum_{i=1}^{I} [\hat{Y}_i(1) - \hat{Y}_i(0)] \tag{10}$$

$$\tau_{ATT} = \frac{1}{I_T} \sum_{i:T_i=1} [Y_i - \hat{Y}_i(0)], \tag{11}$$

where I and I_T are the sample size and the number of treated countries, respectively. These nearest-neighbor matching estimators both allow for the identification of the ATE and ATT under unconfoundedness and are very transparent, as the list of country matches underlying the results can be displayed in small samples (see the next two subsections).

Summing up, the application of matching estimators to (small) cross-country samples comes with a disadvantage and an advantage. The disadvantage is that unconfoundedness is very unlikely to hold, since it is often implausible to assume that country-specific unobservable characteristics do not play a role in treatment assignment. The advantage is that it allows one to transparently check for the existence of common support. Consequently, matching estimators are not used in this section as a magic bullet able to produce more reliable estimates than regression, since both estimation strategies rest on the same identification condition and are therefore subject to the same specification problems. They are used to highlight the country comparisons that lie behind cross-sectional results, to assess their plausibility, and to check whether the treated and control countries share a common support. After these steps, the cross-sectional results are improved by restricting the estimates to the region of common support. Even though these refined results must also rely on the conditional independence assumption, its plausibility can be further assessed by a careful inspection of the new country matches produced by the nearest neighbor algorithm.

B. The Unbearable Lightness of Cross-Country Estimates

We now turn to the data sets introduced in Section III and apply matching estimators to shed light on the country comparisons underlying the coefficient estimates. In this subsection, we analyze the data sets in a cross-sectional fashion.

[10] Following Abadie and others (2004), we let V be the diagonal matrix with the inverses of the variances of the covariates on the main diagonal. All the estimates presented in this section are robust to the utilization of a different distance metric—the Mahalanobis distance suggested by Rubin (1980).

Table 1 presents results for the Vamvakidis (2002) data set. We confirm his results that openness—as represented by the Sachs-Warner (1995) dummy—has a significant effect on growth after 1970, but not before. The coefficients indicate that an open country grows, on average, by 1.5–2.0 percentage points per year faster than a closed economy. The results for both types of matching estimates, ATE and ATT, are qualitatively and quantitatively similar to the standard OLS results.[11] The estimates are robust to the introduction of regional dummies among control variables. Unfortunately, the data set comes with several drawbacks: (i) the data are pooled for 20-year intervals; (ii) the information stops in 1998, too early to meaningfully capture the countries of the former Soviet Union territory; and (iii) the sample size is very small—mainly OECD and Latin American countries—in the 1950s and 1960s.

In Table 2, we repeat the exercise switching to the Persson and Tabellini (PT) data set. This data set contains more countries over the whole sample and extends until 2000, using the Wacziarg and Welch (2003) update of the Sachs-Warner dummy. Although the data is in a panel format, we first produce a pooled estimate by decades for the whole data set. Again, the matching results are very similar to the OLS results, and we find a significant effect of trade on growth for the 1990s and 1970s. In these decades, open countries grew on average by 1.5–2.0 percentage points faster than closed countries, whether we control for regional dummies or not. The growth effect of openness is not significantly different from zero in the 1980s and 1960s.

So far, matching does not add anything to OLS results, since the estimates are very similar and based on the same identification assumption. We now turn to the transparency advantage of the nearest neighbor matching estimator to reveal the country comparisons underlying the estimates from the PT data set. Tables 3, 5, 7, and 9 display the full list of treated (i.e., open to trade for more than half of the decade) countries and their nearest neighbors in the ATE estimation for the 1990s, 1980s, 1970s, and 1960s, respectively. Tables 4, 6, 8, and 10 display the full list of control (i.e., closed) countries and their nearest neighbors in the ATE estimation for the 1990s, 1980s, 1970s, and 1960s, respectively. In all of these tables, the first column (*Country*) indicates the country under consideration; the second column (*Baseline*) shows the nearest neighbor used to estimate the counterfactual outcome of the country in the first column for the ATE estimation *without* area dummies; the third column (*Area*) shows the nearest neighbor used to estimate the counterfactual outcome of the country in the first column for the ATE estimation *with* area dummies. For example, the *Baseline* matches in Tables 3 and 4 for the 1990s are the country comparisons underlying the 1.505 coefficient in Table 2 (i.e., the effect of openness on growth without controlling for area dummies), while the *Area* matches in the same tables lie behind the 1.318 coefficient in Table 2 (i.e., the effect of openness after controlling for area dummies).

Browsing through the above tables, one can see that a few *Baseline* matches appear to work reasonably "well"—for example in the 1990s for Bulgaria and Egypt, which are matched with Ukraine and Algeria, respectively. Arguably, this intuitive appreciation is based on the implicit assumption that there are region-specific unobservable effects; for example a common language, colonization, level of development, geographic proximity, or legal origin. For others—e.g., Albania and Sri Lanka, which are matched with Central African Republic and Algeria, respectively—the matches are somewhat less meaningful. In particular, all (treated) developed countries give rise to very poor matches (e.g., Italy and UK with Russia, or US and Canada with China). In other words, most of the baseline matches do not appear robust to area-specific unobservables.

[11] For the Vamvakidis (2002) data set, tables of the country matches underlying the nearest-neighbor estimates are available from the authors but have been omitted from the present paper due to space constraints.

Therefore, we construct country groups that may capture some of these area-specific unobservables. We divide the world into six groups: Africa, Asia, Latin America, Middle East, OECD, and transition economies (where OECD participation takes precedence over geographic region).[12] The matches underlying the estimation with these area dummies are reported in the *Area* column. For Albania and Sri Lanka in the 1990s, this step appears to work reasonably well, as they are now matched with Belarus and Pakistan, which are certainly perceived as more similar than the baseline nearest neighbors. There are, however, certain surprising findings: for example, all OECD members are matched with Iceland! A similar result obtains for Latin America, where Venezuela is the only control that is picked to be a match. This is due to the fact that Iceland and Venezuela are, according to the SWWW classification, the only closed economies in the OECD group and Latin America in the 1990s.[13] In other words, there is no common support between treated and control countries in those two regions. Introducing area dummies is not enough to control for area-specific unobservables, unless there is a sufficient overlap of treated and untreated units in all areas.

Summing up, the matches listed in the *Baseline* and *Area* columns of Tables 3 through 10 show that country comparisons underlying cross-country analysis are often more than far-fetched. This unbearable lightness of cross-country analysis extends from matching to other cross-sectional estimators that rely on the unconfoundedness assumption, such as plain regression modeling. This is due to the fact that OLS estimates are based either on the same implicit but far-fetched country comparisons or—even worse—on parametric extrapolation beyond the region of common support. In fact, if treated and control countries are very different from each other with respect to covariates, the OLS estimate of the counterfactual outcome of the treated is constructed by linearly extrapolating the observed outcome of control units, and vice versa.

C. Refined Evidence in Selected Regions

The above discussion shows that—as long as we want to control for area-specific unobservable characteristics—we should restrict the analysis of the trade-growth nexus to regions with enough treatment variation. In other words, to improve the quality of the country matches underlying the results, we should drop regions with no common support between treated and control units.

In Table 11, we re-estimate the pooled specification eliminating countries that lack common support with respect to regional affiliation. As shown in the Appendix, this is true in the 1990s for the OECD and Latin America. The same holds for other regions and other decades, however: in the 1980s, almost all OECD countries are already open. In the 1970s and 1960s, almost all African economies (except Mauritius in the 1970s) are closed according to the SWWW dummy. The OECD is a borderline case in both decades, with 4 out of 25 countries in the sample closed in the 1970s and 5 out of 26 countries closed in the 1960s. For both decades, we show estimates with and without the OECD.

Dropping the above regions does not appear to systematically reduce the explanatory power of the models (as measured by the adjusted R-square). Table 11 reports matching estimates—of

[12] We count all countries that eventually joined the OECD into this group, even before they effectively became members to avoid a time-variant regional dummy. In addition, we add Cyprus and Israel for lack of better options. Finally, although Mexico ratified the OECD Convention already on May 18, 1994— implying OECD membership for more than half of the decade—we allocate the country with its Latin American peers.

[13] See the Appendix for a precise list of treated and untreated countries across regions.

both the ATE and ATT—restricted to countries that meet the common-support condition for geographic areas. Comparing these estimates to the previous ones for the unrestricted sample (Table 2), the coefficients appear to be slightly more significant and also larger in magnitude in the 1990s and 1970s. Moreover, we now find stronger evidence of a marginally significant positive effect of openness on growth in the 1980s, especially for the countries that were open to trade (ATT estimate). All the estimated effects lie in the 1.5–2.5 percentage points range. For the 1960s, again, the coefficients are never significantly different from zero. The *Refined* column in Tables 3 through 10 reports the country matches underlying these results. Counter-intuitive matches are now considerably reduced.

We conclude from this exercise that it is important to check for the existence of common support. In fact, in small samples of countries, the advantage of matching estimators lies in the guidance for appropriately restricting the analysis to specific subsamples. Unlike the estimates in Table 2—which replicate common results from growth regressions in the literature—the estimates presented in Table 11 fully control for area-specific unobservables and are based on more intuitive country comparisons. There is no free lunch, however, as the external validity of the estimates is now reduced. The results recommend refraining from commenting on the effect of trade openness on growth in developed countries, Africa in the 1960s and 1970s, and Latin America in the 1990s.

The estimates in Table 11 control for the existence of common support with respect to a set of covariates that we deem important to capture unobservable regional characteristics associated to geography, level of development, culture, or legal origins—that is, area dummies for Africa, Middle East, Asia, Latin America, transition economies, and OECD. The common support, however, should be checked also for other covariates. In principle, we would like to match countries that are very similar with respect to all covariates, but this is impossible if treated and control units are not evenly distributed across all the ranges of variation of covariates. Figures 1 through 8 show that, for example, this condition is not often met for investment share and secondary school enrollment. These figures report the kernel density of treated and control countries over the ranges of variation of these two variables. For instance, Figure 5 shows that the common support for investment share in the 1970s ranges from 0.11 to 0.39, with 27 (control) countries below this region and 1 (treated) country above. To meet the common-support condition, these 28 countries should be dropped from the estimation sample.

Table 12 reports matching estimates for samples restricted to the regions of common support identified in Figures 1 through 8. This evidence is consistent with the one described in Table 11. When carefully matching only countries that lie in the common support, cross-sectional estimates detect a positive and significant association between openness and growth in the 1970s, 1980s, and 1990s, but not in the 1960s.

Finally, we are aware that—even though our refined estimates improve the validity of cross-country results by checking for common support and controlling for unobservable area heterogeneity—our results still suffer from the fact that country-specific unobservables (that is, endogenous selection into treatment) might violate the conditional independence assumption. The major disadvantage of the pooled estimates presented so far is that they do not exploit the panel set-up of the PT data set and the corresponding time variation in the treatment. We therefore continue our strive for empirical *glasnost* by applying a longitudinal technique that uses both time and cross-sectional information in a transparent way.

V. Comparative Studies and Synthetic Control Methods

A. Methodology

Another estimation approach recently implemented for comparative case studies—the synthetic control methods (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie, Diamond, and Hainmueller (2007)—can be promisingly applied to the investigation of the trade-growth nexus. Under this approach, a weighted combination of potential control countries—namely, the synthetic control—is constructed to approximate the most relevant characteristics of the country affected by the intervention. After a regime change (e.g., trade liberalization) takes place in a specific country, SCM can be used to estimate the counterfactual situation of this country in the absence of the regime change by looking at the outcome trend of the synthetic control.

Also in this context, it is useful to reason in terms of potential outcomes, but the framework introduced in Section IV has to be extended to a panel set-up. Assume that we observe a panel of $I_C + 1$ countries over T periods. Only country i receives the treatment (i.e., liberalizes its trade regime) at time $T_0 < T$, while the remaining I_C potential control countries remain closed. The treatment effect for country i at time t can be defined as:

$$\tau_{it} = Y_{it}(1) - Y_{it}(0) = Y_{it} - Y_{it}(0) \tag{12}$$

where $Y_{it}(l)$ stands for the potential outcome associated with $T = l$. The estimand of interest is the vector $(\tau_{i,T_0+1}, ..., \tau_{i,T})$. For any period t, the estimation of the treatment effect is complicated by the missing counterfactual $Y_{it}(0)$.

Abadie, Diamond, and Hainmueller (2007) show how to identify the above treatment effects under the following general model for potential outcomes:

$$Y_{jt}(0) = \delta_t + \nu_{jt} \tag{13}$$

$$Y_{jt}(0) = \delta_t + \tau_{jt} + \nu_{jt} \tag{14}$$

$$\nu_{jt} = Z_j\theta_t + \lambda_t\mu_j + \epsilon_{jt}, \tag{15}$$

where Z_j is a vector of relevant observed covariates that are not affected by the intervention and can be either time-invariant or time-varying; θ_t is a vector of parameters; μ_j is a country-specific unobservable; λ_t is an unknown common factor; and ϵ_{jt} are transitory shocks with zero mean.

Define $W = (w_1 + ... + w_{I_C})'$ as a generic $(I_C \times 1)$ vector of weights such that $w_j \geq 0$ and $\sum w_j = 1$. Each value of W represents a potential synthetic control for country i. Further define $\bar{Y}_j^k = \sum_{s=1}^{T_0} k_s Y_{js}$ as a generic linear combination of pre-treatment outcomes. Abadie, Diamond, and Hainmueller (2007) show that, as long as we can choose W^* such that

$$\sum_{j=1}^{I_C} w_j^* \bar{Y}_j^k = \bar{Y}_i^k \qquad \text{and} \qquad \sum_{j=1}^{I_C} w_j^* Z_j = Z_i, \tag{16}$$

then

$$\hat{\tau}_{it} = Y_{it} - \sum_{j=1}^{I_C} w_j^* Y_{jt} \tag{17}$$

is an unbiased estimator of τ_{it}. Condition (16) can hold exactly only if $(\bar{Y}_i^k, Z_i)$ belongs to the convex hull of $[(\bar{Y}_1^k, Z_1), ..., (\bar{Y}_{I_C}^k, Z_{I_C})]$. Hence, in practice, the synthetic control W^* is selected so that condition (16) holds approximately. But the deviation from this condition imposed by the approximation process can be assessed and shown as a complementary output of the analysis.

The synthetic control algorithm estimates the missing counterfactual as a weighted average of the outcomes of potential controls (i.e., of the synthetic control of the treated country). The weights are chosen so that the pre-treatment outcome and the covariates of the synthetic control are, on average, very similar to those of the treated unit. Again, this approach comes with the evident advantages of transparency (as the weights W^* identify the countries that are used to estimate the counterfactual outcome of the country that liberalized trade) and flexibility (as the set of I_C potential controls can be appropriately restricted to make the underlying country comparisons more sensible). Moreover, SCM rest on identification assumptions that are weaker than those required, for example, by the difference-in-differences models applied in the trade and growth literature. While the difference-in-differences models only control for time-invariant unobserved confounders, the model in equation (15) allows the effects of unobserved confounders to vary with time. Hence, the synthetic control approach directly deals with the endogeneity problem caused by the presence of unobservable country heterogeneity.

B. Openness and Growth in MCD Countries

Taking on board Bhagwati and Srinivasan's (2001) suggestion to look at the trade-growth link through the mirrors of case studies, we use SCM to implement a set of comparative case studies and investigate the effect of economic liberalization on per-capita income paths in MCD countries. We present evidence for two symmetric "treatments." First, we consider countries that liberalize trade in a particular year (over the 1960–2000 period) and compare them with countries that remain closed for at least 10 more years. Second, we investigate a country (Uzbekistan) that has always been closed but in whose neighborhood a lot of countries liberalize trade in a very short span of time (1995–96), in order to see what effect the missed liberalization has had.

Of the economies in MCD, all but one (West Bank and Gaza) are contained in the PT data set. This is, however, not true for the trade openness dummy. In fact, of the 30 MCD economies in the data set, 8 liberalize during 1965-2000, 3 are always open (Jordan, Kyrgyz Republic, and Yemen), 9 are always closed, and 10 are missing. These missings include Afghanistan, Bahrain, Djibouti, Kuwait, Oman, Qatar, Saudi Arabia, Somalia, Sudan, and the United Arab Emirates. In the spirit of SWWW, we assume that they are all closed as, in many of them, natural resource exports play an important role and tend to be tightly controlled by governmental authorities (the export marketing board criterion in Sachs and Warner, 1995). This makes these countries eligible as controls but not as treated units. However, this assumption does not critically affect the results in the next subsection, as those countries are never picked by the synthetic control algorithm.

The set of countries that liberalize include Armenia (1996), Azerbaijan (1996), Egypt (1995), Georgia (1996), Morocco (1984), Mauritania (1995), Tajikistan (1996), and Tunisia (1989). For these countries, we represent graphically the outcome variable (real GDP per capita) for the treated unit and the synthetic control unit, where the match has occurred before the treatment based on the outcome variable and the other predictor variables described in Section III and already used as covariates in the matching estimation. Of course, the pre-treatment fit improves the longer the time span and the greater the explanatory power of predictor variables. Half of the treated economies (i.e., the former Soviet Union countries) only dispose of a few observations before the treatment. In the Middle Eastern economies, instead, the pre-treatment fit is much better as substantially more observations are taken into account. Table 12 provides the comparison by explanatory variable between each treated country and the constructed synthetic control. This table shows that, in general, the algorithm approximated well the observed characteristics of the synthetic control to those of the treated country (except for Tajikistan, which shows levels of pre-treatment GDP and investment far below those of its potential controls).

For each country, except Uzbekistan (reverse treatment) and Tajikistan (poor match), we show two figures. In the first figure, the set of I_C potential controls is only limited by the fact that we drop OECD and Latin American countries, since the former are open before the first MCD treatment considered (und could therefore not serve as controls), whereas the latter are very dissimilar in terms of both geography and culture. In the second figure, we exploit the flexibility property of SCM and further restrict the set of possible controls to nearby countries in the same region. For Uzbekistan, we show only one figure as the set of possible controls (that is, countries that open to trade in 1995–96) only consists of nearby economies.

The results are summarized in Figures 9 through 24. Most treated countries fare much better than their synthetic controls after trade liberalization.[14] In Central Asia and the Caucasus, this is visible using both a broad and a constrained set of potential controls for Georgia (Figures 9 and 10) and Armenia (Figures 11 and 12). For Azerbaijan, instead, trade liberalization has not had a positive effect (Figures 13 and 14). For Tajikistan (Figure 15), the matching procedure does not yield satisfactory results as the per capita GDP is too low to be successfully matched with a potential control even in the large sample. For the inverse example, Uzbekistan, the evidence points to a missed opportunity, as the counterfactual (a linear combination of liberalized Armenia, Kyrgyz Republic, and Tajikistan) performed substantially better in terms of per capita GDP (Figure 16).

In the Middle East and North Africa, the evidence is particularly striking for Morocco, which outperforms its synthetic controls (Figures 17 and 18). While with the broad set of possible controls the gap appears to close starting about six years after the treatment, the algorithm picks Lesotho as the largest part of the estimated counterfactual (Case A). Eliminating sub-Saharan Africa from the set of potential controls leads to an even larger discrepancy between Morocco and the synthetic control (Case B), with per capita GDP of the latter being about 40 percent below Morocco's 10 years after the treatment. This last result, however, could be partly confounded by the fact that 32 percent of the synthetic control is made up of Algeria, which experienced a costly civil war in the post-treatment period. The evidence for Tunisia is similar (Figures 19 and 20). For Mauritania, instead, there appears to be no clear effect associated with trade liberalization (Figures 21 and 22). Finally, the evidence on Egypt (Figures 23 and 24) warrants careful interpretation: while the pre-treatment match appears to be better in the larger set of potential controls, narrowing down the set leads to Egypt's per capita GDP being constantly below that of the control group, which consists mainly of Syria. Although Egypt's per capita GDP is comparatively lower than Syria's, the other covariates are more in line with Syrian data and the algorithm assigns a low weight to relatively poorer countries in nearby Africa such as Sudan.

[14] See the notes to Figures 9 through 24 for country-specific information on potential and actual controls.

VI. Conclusions and Extensions

In this paper, we take another look at the openness-growth nexus in international macroeconomics with the intention to add empirical *glasnost* to the results obtained in the literature. We explore this theme along two dimensions. First, we examine classic pooled cross-country regressions and show the pitfalls related to the underlying country comparisons. Second, we explore a statistical framework that control for unobservable heterogeneity and can be used for cross-country comparative studies and apply it to the trade-growth question.

Most of the contributions in the trade-growth field use estimators that include cross-sectional information and are based on empirically nontransparent methods, which mask the underlying country comparisons used to estimate the impact of trade on growth. Employing matching estimators from the treatment evaluation literature—which share their identification condition with OLS—we show that without appropriately restricting the sample, the country matches underlying the estimates are often far-fetched—the unbearable lightness of cross-country estimates. When restricting the sample to treated and control countries that share a common support, we broadly confirm the impact found in the literature, but our results indicate a somewhat more significant impact for some of the periods we investigate. We caution against extrapolating these results to regions where the effect is estimated without common support between treated and control countries, for example the OECD for much of the sample period.

Moving to a panel framework, we investigate episodes of trade liberalization in countries in the IMF's MCD region using synthetic control methods. For all treated countries that liberalized their trade regime, we construct a synthetic control unit that shows the growth performance for a weighted average of untreated countries that are similar to the treated country. The difference between the growth performance of the treated country and the synthetic control unit is assumed to reflect the impact of a liberalized trade regime. We find that trade liberalization, in most cases, has had a positive effect on per capita income growth in MCD countries.

We plan to extend some of the results presented in this paper in two directions. First, their robustness in MCD countries should be assessed by means of the inference procedure proposed by Abadie, Diamond, and Hainmueller (2007), that is, placebo tests through permutation methods. Second, the analysis should also cover other regions where relevant episodes of trade liberalization took place over the period 1960-2000, e.g., Latin America, East Asia, and Africa.

Tables and Figures

Table 1. Openness and Growth, Cross-Country Evidence (I), 1950-98

	1990-98	1970-90	1950-70
OLS without area dummies:			
- *Estimate*	1.630**	1.041**	0.041
- *Standard error*	(0.772)	(0.492)	(0.473)
- *Adjusted* R^2	0.36	0.52	0.41
OLS with area dummies:			
- *Estimate*	1.357**	1.883***	-0.175
- *Standard error*	(0.625)	(0.537)	(0.538)
- *Adjusted* R^2	0.52	0.57	0.42
Matching without area dummies:			
- *Estimate (ATE)*	1.731**	3.089***	-0.347
- *Standard error*	(0.808)	(0.749)	(0.490)
- *Estimate (ATT)*	1.974**	1.578***	-0.423
- *Standard error*	(0.968)	(0.616)	(0.724)
Matching with area dummies:			
- *Estimate (ATE)*	1.604**	3.180***	-0.414
- *Standard error*	(0.749)	(0.853)	(0.522)
- *Estimate (ATT)*	1.681*	1.421***	-0.726
- *Standard error*	(0.962)	(0.487)	(0.761)
Treated	73	24	22
Controls	36	64	24
Observations	109	88	46

Data: Vamvakidis (2002). Dependent variable: real GDP per capita growth. Treatment: trade openness dummy (Sachs and Warner, 1995). Control variables as understood in Vamvakidis (2002): initial GDP per capita, secondary school enrollment, population growth, investment share, black market premium, and inflation for 1990-98 and 1970-90; initial GDP per capita, illiteracy rate, population growth, and investment share for 1950-70. Area dummies refer to Africa, Asia, Latin America, Middle East, OECD, and transition economies. ATE and ATT stand for Average Treatment Effect and Average Treatment effect on the Treated, respectively. *** 1% significance level; ** 5% significance level; * 10% significance level.

Table 2. Openness and Growth, Cross-Country Evidence (II), 1961-2000

	1991-2000	1981-90	1971-80	1961-70
OLS without area dummies:				
- *Estimate*	1.576***	0.594	2.117***	-0.058
- *Standard error*	(0.508)	(0.520)	(0.686)	(0.607)
- *Adjusted* R^2	0.23	0.32	0.20	0.29
OLS with area dummies:				
- *Estimate*	1.514***	0.773	1.610**	-0.089
- *Standard error*	(0.489)	(0.502)	(0.725)	(0.613)
- *Adjusted* R^2	0.33	0.46	0.28	0.30
Matching without area dummies:				
- *Estimate (ATE)*	1.505**	0.056	2.094**	-0.254
- *Standard error*	(0.646)	(0.695)	(0.933)	(0.838)
- *Estimate (ATT)*	1.453**	-0.391	2.815***	0.138
- *Standard error*	(0.705)	(1.208)	(0.878)	(0.941)
Matching with area dummies:				
- *Estimate (ATE)*	1.318**	0.641	2.399***	-0.442
- *Standard error*	(0.672)	(0.462)	(0.825)	(0.829)
- *Estimate (ATT)*	1.130	0.826	1.895**	0.213
- *Standard error*	(0.742)	(0.579)	(0.831)	(0.943)
Treated	87	43	33	31
Controls	26	66	74	75
Observations	113	109	107	106

Data: Persson and Tabellini (2006). Dependent variable: real GDP per capita growth. Treatment: trade openness dummy (Sachs and Warner, 1995; Wacziarg and Welch, 2003). Control variables: initial GDP per capita, secondary school enrollment, population growth, and investment share. Area dummies refer to Africa, Asia, Latin America, Middle East, OECD, and transition economies. ATE and ATT stand for Average Treatment Effect and Average Treatment effect on the Treated, respectively. *** 1% significance level; ** 5% significance level; * 10% significance level.

Table 3. Cross-Country Matches, Treated Countries, 1991-2000

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Albania	C.A.R.	Belarus	Belarus	Latvia	Belarus	Belarus	Belarus
Argentina	Iran	Venezuela	*dropped*	Luxembourg	Iceland	Iceland	*dropped*
Australia	Belarus	Iceland	*dropped*	Madagascar	Chad	Chad	Senegal
Austria	Belarus	Iceland	*dropped*	Malaysia	Iran	China	China
Belgium	Belarus	Iceland	*dropped*	Mali	Chad	Chad	Chad
Benin	Malawi	Malawi	Malawi	Mauritania	Togo	Syria	Syria
Botswana	Belarus	Zimbabwe	Zimbabwe	Mauritius	Zimbabwe	Zimbabwe	Zimbabwe
Brazil	India	Venezuela	*dropped*	Mexico	Iran	Venezuela	*dropped*
Bulgaria	Ukraine	Ukraine	Ukraine	Morocco	Venezuela	Algeria	Algeria
Cameroon	Senegal	Senegal	Senegal	Mozambique	Rwanda	Rwanda	Rwanda
Canada	China	Iceland	*dropped*	Nepal	Zimbabwe	P.N.G.	P.N.G.
Cape Verde	Togo	Togo	Togo	Netherlands	Belarus	Iceland	*dropped*
Chile	Iran	Venezuela	*dropped*	New Zealand	Belarus	Iceland	*dropped*
Colombia	Algeria	Venezuela	*dropped*	Nicaragua	Gabon	Venezuela	*dropped*
Costa Rica	Zimbabwe	Venezuela	*dropped*	Niger	Chad	Chad	Chad
Cyprus	Iceland	Iceland	*dropped*	Norway	Belarus	Iceland	*dropped*
Czech R.	Belarus	Belarus	Belarus	Panama	Iceland	Venezuela	*dropped*
Denmark	Belarus	Iceland	*dropped*	Paraguay	Zimbabwe	Venezuela	*dropped*
Dominican R.	Zimbabwe	Venezuela	*dropped*	Peru	Iran	Venezuela	*dropped*
Ecuador	Zimbabwe	Venezuela	*dropped*	Philippines	Algeria	India	India
Egypt	Algeria	Algeria	Algeria	Poland	Belarus	Belarus	Belarus
El Salvador	P.N.G.	Venezuela	*dropped*	Portugal	Belarus	Iceland	*dropped*
Ethiopia	Senegal	Senegal	Senegal	Romania	Ukraine	Ukraine	Ukraine
Finland	Belarus	Iceland	*dropped*	Singapore	Lesotho	China	China
France	Russia	Iceland	*dropped*	Slovak R.	Belarus	Belarus	Belarus
Gambia	Togo	Togo	Togo	Slovenia	Belarus	Belarus	Belarus
Germany	Russia	Iceland	*dropped*	South Africa	Algeria	Zimbabwe	Zimbabwe
Ghana	Gabon	Gabon	Gabon	South Korea	China	China	China
Greece	Belarus	Iceland	*dropped*	Spain	Belarus	Iceland	*dropped*
Guatemala	Senegal	Venezuela	*dropped*	Sri Lanka	Algeria	Pakistan	Pakistan
Guinea	Senegal	Senegal	Senegal	Sweden	Belarus	Iceland	*dropped*
Honduras	Zimbabwe	Venezuela	*dropped*	Switzerland	Belarus	Iceland	*dropped*
Hong Kong	Iran	China	China	Tanzania	Burkina F.	Burkina F.	Burkina F.
Hungary	Belarus	Belarus	Belarus	Thailand	China	China	China
Indonesia	Iran	India	India	Trinidad T.	Zimbabwe	Venezuela	*dropped*
Ireland	Belarus	Iceland	*dropped*	Tunisia	Zimbabwe	Algeria	Algeria
Israel	Iran	Iceland	*dropped*	Turkey	Iran	Iceland	*dropped*
Italy	Russia	Iceland	*dropped*	Uganda	Chad	Chad	Chad
Ivory Coast	Senegal	Senegal	Senegal	U.K.	Russia	Iceland	*dropped*
Jamaica	Iceland	Venezuela	*dropped*	U.S.	China	Iceland	*dropped*
Japan	China	Iceland	*dropped*	Uruguay	Belarus	Venezuela	*dropped*
Jordan	Syria	Syria	Syria	Yemen	Angola	Syria	Syria
Kenya	Senegal	Senegal	Senegal	Zambia	Togo	Togo	Togo

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11). C.A.R. and P.N.G. stand for Central African Republic and Papua New Guinea, respectively.

Table 4. Cross-Country Matches, Control Countries, 1991-2000

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Algeria	Colombia	Morocco	Tunisia	Lesotho	Singapore	Cape Verde	Cape Verde
Angola	Tanzania	Tanzania	Guinea	Malawi	Mali	Mali	Mali
Belarus	Czech Republic	Czech Rep.	Czech Rep.	Nigeria	Guatemala	Kenya	Kenya
Burkina Faso	Tanzania	Tanzania	Tanzania	Pakistan	Morocco	Indonesia	Indonesia
Burundi	Mozambique	Mozambique	Mali	P.N.G.	Guinea	Nepal	Nepal
C.A.R.	Mozambique	Mozambique	Benin	Russia	Italy	Poland	Poland
Chad	Mali	Mali	Mali	Rwanda	Mozambique	Mozambique	Mozambique
China	Brazil	Indonesia	Indonesia	Senegal	Guinea	Guinea	Guinea
Congo	Nicaragua	Ghana	Ghana	Syria	Paraguay	Morocco	Morocco
Gabon	Ghana	Ghana	Ghana	Togo	Benin	Benin	Benin
Iceland	Cyprus	Cyprus	*dropped*	Ukraine	Poland	Poland	Poland
India	Brazil	Indonesia	Indonesia	Venezuela	Morocco	Colombia	*dropped*
Iran	Argentina	Tunisia	Tunisia	Zimbabwe	Ecuador	Zambia	Ghana

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11). C.A.R. and P.N.G. stand for Central African Republic and Papua New Guinea, respectively.

Table 5. Cross-Country Matches, Treated Countries, 1981-90

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Australia	Poland	Turkey	*dropped*	Israel	Peru	Iceland	*dropped*
Austria	Romania	Iceland	*dropped*	Italy	Poland	Turkey	*dropped*
Barbados	Guyana	Guyana	Guyana	Japan	Poland	Iceland	*dropped*
Belgium	Romania	Iceland	*dropped*	Jordan	Gabon	Syria	Syria
Bolivia	Guatemala	Guatemala	Guatemala	Luxembourg	Iceland	Iceland	*dropped*
Botswana	Cape Verde	Cape Verde	Cape Verde	Malaysia	Algeria	Philippines	Philippines
Canada	Poland	Turkey	*dropped*	Mauritius	Jamaica	Lesotho	Capo Verde
Chile	Sri Lanka	Peru	Peru	Mexico	Brazil	Brazil	Brazil
Colombia	Turkey	Venezuela	Venezuela	Morocco	Tunisia	Tunisia	Tunisia
Costa Rica	Nicaragua	Nicaragua	Nicaragua	Netherlands	Romania	Iceland	*dropped*
Cyprus	Iceland	Iceland	*dropped*	New Zealand	Romania	Iceland	*dropped*
Denmark	Romania	Iceland	*dropped*	Norway	Romania	Iceland	*dropped*
Finland	Romania	Iceland	*dropped*	Portugal	Hungary	Iceland	*dropped*
France	Poland	Turkey	*dropped*	Singapore	Iceland	Sri Lanka	Sri Lanka
Gambia	Benin	Benin	Benin	South Korea	Romania	China	Sri Lanka
Germany	Poland	Turkey	*dropped*	Spain	Romania	Iceland	*dropped*
Ghana	Honduras	Ivory Coast	Ivory Coast	Sweden	Romania	Iceland	*dropped*
Greece	Romania	Iceland	*dropped*	Switzerland	Romania	Iceland	*dropped*
Guinea	Angola	Angola	Angola	Thailand	Brazil	China	China
Hong Kong	Poland	Sri Lanka	Sri Lanka	U.K.	Poland	Turkey	*dropped*
Indonesia	Turkey	China	China	U.S.	Poland	Turkey	*dropped*
Ireland	Romania	Iceland	*dropped*				

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11).

Table 6. Cross-Country Matches, Control Countries, 1981-90

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Algeria	Malaysia	Morocco	Morocco	Madagascar	Ghana	Ghana	Guinea
Angola	Guinea	Guinea	Guinea	Malawi	Gambia	Gambia	Gambia
Argentina	Mexico	Mexico	Chile	Mali	Guinea	Guinea	Guinea
Bangladesh	Morocco	Indonesia	Indonesia	Mauritania	Guinea	Jordan	Morocco
Benin	Gambia	Gambia	Gambia	Mozambique	Bolivia	Guinea	Guinea
Brazil	Indonesia	Mexico	Mexico	Nepal	Costa Rica	Malaysia	Malaysia
Burkina F.	Guinea	Guinea	Guinea	Nicaragua	Costa Rica	Costa Rica	Costa Rica
Burundi	Guinea	Guinea	Guinea	Niger	Guinea	Guinea	Guinea
Cameroon	Guinea	Guinea	Guinea	Nigeria	Ghana	Ghana	Ghana
Cape Verde	Botswana	Botswana	Botswana	Pakistan	Morocco	Indonesia	Indonesia
C.A.R.	Guinea	Guinea	Guinea	Panama	Costa Rica	Costa Rica	Costa Rica
Chad	Guinea	Guinea	Guinea	P.N.G.	Guinea	Indonesia	Indonesia
China	Indonesia	Indonesia	Indonesia	Paraguay	Costa Rica	Costa Rica	Costa Rica
Congo	Jordan	Ghana	Ghana	Peru	Chile	Chile	Chile
Dominican R.	Costa Rica	Costa Rica	Costa Rica	Philippines	Mexico	Malaysia	Malaysia
Ecuador	Malaysia	Costa Rica	Costa Rica	Poland	Greece	Morocco	*dropped*
Egypt	Colombia	Morocco	Morocco	Romania	Greece	Morocco	*dropped*
El Salvador	Bolivia	Bolivia	Bolivia	Rwanda	Guinea	Guinea	Guinea
Ethiopia	Guinea	Guinea	Guinea	Senegal	Guinea	Guinea	Guinea
Gabon	Costa Rica	Ghana	Ghana	Sierra Leone	Bolivia	Guinea	Guinea
Guatemala	Guinea	Bolivia	Bolivia	South Africa	Colombia	Ghana	Ghana
Guinea-Bissau	Botswana	Botswana	Botswana	Sri Lanka	Chile	Hong Kong	Hong Kong
Guyana	Barbados	Barbados	Barbados	Syria	Ghana	Jordan	Jordan
Haiti	Bolivia	Bolivia	Bolivia	Tanzania	Costa Rica	Guinea	Guinea
Honduras	Ghana	Costa Rica	Costa Rica	Togo	Gambia	Gambia	Gambia
Hungary	Portugal	Morocco	*dropped*	Trinidad T.	Barbados	Barbados	Barbados
Iceland	Cyprus	Cyprus	*dropped*	Tunisia	Morocco	Morocco	Morocco
India	Mexico	Indonesia	Indonesia	Turkey	Colombia	Australia	*dropped*
Iran	Malaysia	Morocco	Morocco	Uganda	Guinea	Guinea	Guinea
Ivory Coast	Ghana	Ghana	Ghana	Uruguay	Mauritius	Barbados	Barbados
Jamaica	Mauritius	Barbados	Chile	Venezuela	Morocco	Colombia	Colombia
Kenya	Ghana	Ghana	Ghana	Zambia	Guinea	Guinea	Guinea
Lesotho	Costa Rica	Guinea	Guinea	Zimbabwe	Costa Rica	Ghana	Ghana

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11). C.A.R. and P.N.G. stand for Central African Republic and Papua New Guinea, respectively.

Table 7. Cross-Country Matches, Treated Countries, 1971-80

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Australia	Argentina	New Zealand	*dropped*	Japan	Poland	New Zealand	*dropped*
Austria	Romania	New Zealand	*dropped*	Jordan	Gabon	Syria	Syria
Barbados	Guyana	Guyana	Guyana	Luxembourg	Jamaica	Iceland	*dropped*
Belgium	Hungary	New Zealand	*dropped*	Malaysia	Peru	Philippines	Philippines
Bolivia	Dominican R.	Dominican R.	Dominican R.	Mauritius	Trinidad T.	Ghana	*dropped*
Canada	Argentina	New Zealand	*dropped*	Netherlands	New Zealand	New Zealand	*dropped*
Chile	Sri Lanka	Colombia	Colombia	Norway	Romania	New Zealand	*dropped*
Cyprus	Iceland	Iceland	*dropped*	Portugal	Hungary	New Zealand	*dropped*
Denmark	New Zealand	New Zealand	*dropped*	Singapore	Israel	Philippines	Philippines
Ecuador	Panama	Panama	Panama	South Korea	Argentina	Philippines	Philippines
Finland	Romania	New Zealand	*dropped*	Spain	Poland	New Zealand	*dropped*
France	Poland	New Zealand	*dropped*	Sweden	Hungary	New Zealand	*dropped*
Greece	Romania	New Zealand	*dropped*	Switzerland	Romania	New Zealand	*dropped*
Hong Kong	Panama	Philippines	Philippines	Thailand	Venezuela	Philippines	Philippines
Indonesia	Turkey	Bangladesh	Bangladesh	U.K.	Hungary	New Zealand	*dropped*
Ireland	New Zealand	New Zealand	*dropped*	U.S.	China	New Zealand	*dropped*
Italy	Poland	New Zealand	*dropped*				

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11).

Table 8. Cross-Country Matches, Control Countries, 1971-80

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Algeria	Thailand	Jordan	Jordan	Kenya	Bolivia	Mauritius	*dropped*
Angola	Bolivia	Mauritius	*dropped*	Lesotho	Mauritius	Mauritius	*dropped*
Argentina	Australia	Chile	Chile	Madagascar	Bolivia	Mauritius	*dropped*
Bangladesh	Indonesia	Indonesia	Indonesia	Malawi	Bolivia	Mauritius	*dropped*
Benin	Bolivia	Mauritius	*dropped*	Mali	Bolivia	Mauritius	*dropped*
Botswana	Mauritius	Mauritius	*dropped*	Mauritania	Bolivia	Jordan	Jordan
Brazil	Thailand	Ecuador	Ecuador	Mexico	Indonesia	Ecuador	Ecuador
Burkina F.	Bolivia	Mauritius	*dropped*	Morocco	Indonesia	Jordan	Jordan
Burundi	Bolivia	Mauritius	*dropped*	Mozambique	Bolivia	Mauritius	*dropped*
Cameroon	Bolivia	Mauritius	*dropped*	Nepal	Bolivia	Indonesia	Indonesia
Cape Verde	Mauritius	Mauritius	*dropped*	New Zealand	Ireland	Ireland	*dropped*
C.A.R.	Bolivia	Mauritius	*dropped*	Nicaragua	Bolivia	Bolivia	Bolivia
Chad	Bolivia	Mauritius	*dropped*	Niger	Bolivia	Mauritius	*dropped*
China	Indonesia	Indonesia	Indonesia	Nigeria	Indonesia	Mauritius	*dropped*
Colombia	Indonesia	Chile	Chile	Pakistan	Indonesia	Indonesia	Indonesia
Congo	Jordan	Mauritius	*dropped*	Panama	Ecuador	Ecuador	Ecuador
Costa Rica	Bolivia	Bolivia	Bolivia	P.N.G.	Bolivia	Indonesia	Indonesia
Dominican R.	Bolivia	Bolivia	Bolivia	Paraguay	Bolivia	Bolivia	Bolivia
Egypt	Chile	Jordan	Jordan	Peru	Malaysia	Ecuador	Ecuador
El Salvador	Bolivia	Bolivia	Bolivia	Philippines	Malaysia	Malaysia	Malaysia
Ethiopia	Bolivia	Mauritius	*dropped*	Poland	Greece	Mauritius	*dropped*
Gabon	Jordan	Mauritius	*dropped*	Romania	Greece	Mauritius	*dropped*
Gambia	Bolivia	Mauritius	*dropped*	Rwanda	Bolivia	Mauritius	*dropped*
Ghana	Bolivia	Mauritius	*dropped*	Senegal	Bolivia	Mauritius	*dropped*
Guatemala	Bolivia	Bolivia	Bolivia	Sierra Leone	Bolivia	Mauritius	*dropped*
Guinea	Bolivia	Mauritius	*dropped*	Sri Lanka	Chile	Malaysia	Malaysia
Guinea-Bissau	Jordan	Mauritius	*dropped*	Syria	Bolivia	Jordan	Jordan
Guyana	Barbados	Barbados	Barbados	Tanzania	Thailand	Mauritius	*dropped*
Haiti	Bolivia	Bolivia	Bolivia	Togo	Bolivia	Mauritius	*dropped*
Honduras	Bolivia	Bolivia	Bolivia	Trinidad T.	Mauritius	Barbados	Barbados
Hungary	Sweden	Mauritius	*dropped*	Tunisia	Bolivia	Jordan	Jordan
Iceland	Cyprus	Cyprus	*dropped*	Turkey	Indonesia	Portugal	*dropped*
India	Indonesia	Indonesia	Indonesia	Uganda	Bolivia	Mauritius	*dropped*
Iran	Malaysia	Jordan	Jordan	Uruguay	Luxembourg	Barbados	Barbados
Israel	Hong Kong	Australia	*dropped*	Venezuela	Thailand	Ecuador	Ecuador
Ivory Coast	Bolivia	Mauritius	*dropped*	Zambia	Bolivia	Mauritius	*dropped*
Jamaica	Luxembourg	Barbados	Barbados	Zimbabwe	Thailand	Mauritius	*dropped*

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11). C.A.R. and P.N.G. stand for Central African Republic and Papua New Guinea, respectively.

Table 9. Cross-Country Matches, Treated Countries, 1961-70

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Australia	New Zealand	New Zealand	*dropped*	Japan	New Zealand	New Zealand	*dropped*
Austria	Romania	Romania	*dropped*	Jordan	Costa Rica	Syria	Syria
Barbados	Trinidad T.	Trinidad T.	Uruguay	Luxembourg	Romania	Romania	*dropped*
Belgium	New Zealand	New Zealand	*dropped*	Malaysia	Colombia	South Korea	South Korea
Bolivia	Ghana	Paraguay	Paraguay	Netherlands	New Zealand	New Zealand	*dropped*
Canada	Argentina	New Zealand	*dropped*	Norway	Romania	Romania	*dropped*
Cyprus	Romania	Romania	*dropped*	Peru	Iran	Venezuela	Venezuela
Denmark	New Zealand	New Zealand	*dropped*	Portugal	Romania	Romania	*dropped*
Ecuador	Zimbabwe	Panama	Panama	Singapore	Israel	South Korea	South Korea
Finland	New Zealand	New Zealand	*dropped*	Spain	Argentina	Romania	*dropped*
France	Argentina	New Zealand	*dropped*	Sweden	New Zealand	New Zealand	*dropped*
Greece	Romania	Romania	*dropped*	Switzerland	Romania	Romania	*dropped*
Hong Kong	Tunisia	South Korea	South Korea	Thailand	Iran	Pakistan	Pakistan
Ireland	Uruguay	New Zealand	*dropped*	U.K.	Argentina	New Zealand	*dropped*
Italy	Romania	Romania	*dropped*	U.S.	Argentina	New Zealand	*dropped*
Jamaica	Romania	Guyana	Guyana				

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11).

Table 10. Cross-Country Matches, Control Countries, 1961-70

Country	*Baseline*	*Area*	*Refined*	*Country*	*Baseline*	*Area*	*Refined*
Algeria	Bolivia	Jordan	Jordan	Lesotho	Bolivia	Jordan	*dropped*
Angola	Bolivia	Jordan	*dropped*	Madagascar	Bolivia	Jordan	*dropped*
Argentina	Canada	Peru	Peru	Malawi	Bolivia	Jordan	*dropped*
Bangladesh	Malaysia	Malaysia	Malaysia	Mali	Bolivia	Jordan	*dropped*
Benin	Bolivia	Jordan	*dropped*	Mauritania	Bolivia	Jordan	Jordan
Botswana	Bolivia	Jordan	*dropped*	Mauritius	Bolivia	Jordan	*dropped*
Brazil	Thailand	Peru	Peru	Mexico	Thailand	Peru	Peru
Burkina F.	Bolivia	Jordan	*dropped*	Morocco	Bolivia	Jordan	Jordan
Burundi	Bolivia	Jordan	*dropped*	Mozambique	Bolivia	Jordan	*dropped*
Cameroon	Bolivia	Jordan	*dropped*	Nepal	Bolivia	Malaysia	Malaysia
Cape Verde	Bolivia	Jordan	*dropped*	New Zealand	Australia	Australia	*dropped*
C.A.R.	Bolivia	Jordan	*dropped*	Nicaragua	Bolivia	Bolivia	Bolivia
Chad	Bolivia	Jordan	*dropped*	Niger	Bolivia	Jordan	*dropped*
Chile	Malaysia	Bolivia	Bolivia	Nigeria	Bolivia	Jordan	*dropped*
China	Canada	Malaysia	Malaysia	Pakistan	Malaysia	Malaysia	Malaysia
Colombia	Malaysia	Bolivia	Bolivia	Panama	Ecuador	Ecuador	Ecuador
Congo	Ecuador	Jordan	*dropped*	P.N.G.	Bolivia	Malaysia	Malaysia
Costa Rica	Malaysia	Ecuador	Ecuador	Paraguay	Bolivia	Bolivia	Bolivia
Dominican R.	Bolivia	Bolivia	Bolivia	Philippines	Malaysia	Malaysia	Malaysia
Egypt	Malaysia	Jordan	Jordan	Romania	Greece	Greece	*dropped*
El Salvador	Malaysia	Bolivia	Bolivia	Rwanda	Bolivia	Jordan	*dropped*
Ethiopia	Bolivia	Jordan	*dropped*	Senegal	Bolivia	Jordan	*dropped*
Gabon	Barbados	Jordan	*dropped*	Sierra Leone	Bolivia	Jordan	*dropped*
Gambia	Bolivia	Jordan	*dropped*	South Africa	Malaysia	Jordan	*dropped*
Ghana	Bolivia	Jordan	*dropped*	South Korea	Malaysia	Malaysia	Malaysia
Guatemala	Bolivia	Bolivia	Bolivia	Sri Lanka	Bolivia	Malaysia	Malaysia
Guinea	Bolivia	Jordan	*dropped*	Syria	Malaysia	Jordan	Jordan
Guinea-Bissau	Barbados	Jordan	*dropped*	Tanzania	Ecuador	Jordan	*dropped*
Guyana	Jamaica	Jamaica	Jamaica	Togo	Bolivia	Jordan	*dropped*
Haiti	Bolivia	Bolivia	Bolivia	Trinidad T.	Bolivia	Bolivia	Bolivia
Honduras	Bolivia	Bolivia	Bolivia	Tunisia	Bolivia	Jordan	Jordan
Iceland	Jamaica	Cyprus	*dropped*	Turkey	Malaysia	Canada	*dropped*
India	Malaysia	Malaysia	Malaysia	Uganda	Bolivia	Jordan	*dropped*
Indonesia	Bolivia	Malaysia	Malaysia	Uruguay	Ireland	Barbados	Barbados
Iran	Thailand	Jordan	Jordan	Venezuela	Peru	Peru	Peru
Israel	Hong Kong	Australia	*dropped*	Zambia	Bolivia	Jordan	*dropped*
Ivory Coast	Malaysia	Jordan	*dropped*	Zimbabwe	Ecuador	Jordan	*dropped*
Kenya	Bolivia	Jordan	*dropped*				

Data: Persson and Tabellini (2006). *Baseline* and *Area* refer to the nearest-neighbor match without and with area dummies, respectively (see Table 2). *Refined* refers to nearest-neighbor match without Latin America and OECD (see Table 11). C.A.R. and P.N.G. stand for Central African Republic and Papua New Guinea, respectively.

Table 11. Openness and Growth, Refined Evidence (I), 1961-2000

	1991-2000	1981-90	1971-80		1961-70	
			(A)	(B)	(A)	(B)
Matching without area dummies:						
- *Estimate (ATE)*	1.921***	0.949	1.841***	1.940**	0.084	-0.287
- *Standard error*	(0.647)	(0.706)	(0.634)	(0.876)	(0.666)	(0.915)
- *Estimate (ATT)*	1.760***	2.358**	2.229***	2.475***	0.613	0.319
- *Standard error*	(0.633)	(0.959)	(0.625)	(0.757)	(0.915)	(1.260)
Matching with area dummies:						
- *Estimate (ATE)*	1.389**	0.809	2.240***	2.493***	-0.143	-0.491
- *Standard error*	(0.708)	(0.526)	(0.684)	(0.800)	(0.680)	(0.818)
- *Estimate (ATT)*	0.849	1.252**	1.986***	1.935**	0.544	0.081
- *Standard error*	(0.738)	(0.524)	(0.770)	(0.860)	(0.924)	(0.882)
Africa	yes (19-14)	yes (5-30)	no (1-33)	no (1-33)	no (0-35)	no (0-35)
Asia	yes (9-4)	yes (6-8)	yes (6-8)	yes (6-8)	yes (4-10)	yes (4-10)
Latin America	no (19-1)	yes (6-17)	yes (4-19)	yes (4-19)	yes (5-18)	yes (5-18)
Middle East	yes (6-3)	yes (2-6)	yes (1-7)	yes (1-7)	yes (1-7)	yes (1-7)
OECD	no (25-1)	no (24-2)	yes (21-4)	no (21-4)	yes (21-4)	no (21-4)
Transition economies	yes (9-3)	no (0-3)	no (0-3)	no (0-3)	no (0-1)	no (0-1)
Treated	43	19	32	11	31	10
Controls	24	61	38	34	39	35
Observations	67	80	70	45	70	45

Data: Persson and Tabellini (2006). Dependent variable: real GDP per capita growth. Treatment: trade openness dummy (Sachs and Warner, 1995; Wacziarg and Welch, 2003). Control variables: initial GDP per capita, secondary school enrollment, population growth, investment share, and area dummies (as indicated). The two numbers in parenthesis after each area refer to the number of treated and control countries, respectively. Samples restricted to certain areas to meet the common-support condition. ATE and ATT stand for Average Treatment Effect and Average Treatment effect on the Treated, respectively. *** 1% significance level; ** 5% significance level; * 10% significance level.

Table 12. Openness and Growth, Refined Evidence (II), 1961-2000

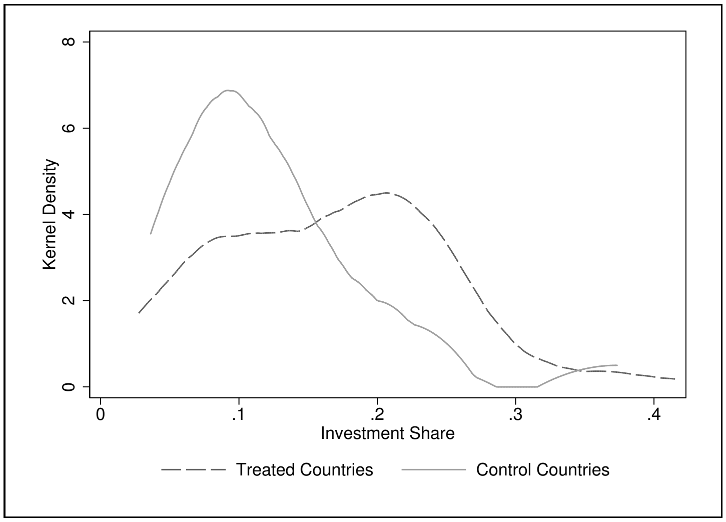
	1991-2000	1981-90	1971-80	1961-70
Matching with common support A:				
- *Estimate (ATE)*	1.318**	0.884*	1.916***	-0.273
- *Standard error*	(0.672)	(0.510)	(0.743)	(0.705)
- *Estimate (ATT)*	1.130	0.813	1.645**	0.513
- *Standard error*	(0.742)	(0.627)	(0.686)	(1.049)
Matching with common support B:				
- *Estimate (ATE)*	1.644**	1.139**	2.007***	-0.360
- *Standard error*	(0.731)	(0.524)	(0.717)	(0.641)
- *Estimate (ATT)*	1.407*	2.019***	1.936***	0.034
- *Standard error*	(0.819)	(0.747)	(0.690)	(0.833)
Matching with common support C:				
- *Estimate (ATE)*	1.644**	1.141**	1.774**	-0.203
- *Standard error*	(0.731)	(0.531)	(0.757)	(0.678)
- *Estimate (ATT)*	1.407*	1.691**	1.619**	0.304
- *Standard error*	(0.819)	(0.725)	(0.651)	(0.843)

Data: Persson and Tabellini (2006). Restricted samples to meet the common-support condition for investment share (A), secondary school enrollment (B), or both (C). See Figures 1 through 8 for the numbers of treated and control countries dropped because outside of common supports. Dependent variable: real GDP per capita growth. Treatment: trade openness dummy (Sachs and Warner, 1995; Wacziarg and Welch, 2003). Control variables: initial GDP per capita, secondary school enrollment, population growth, investment share, and area dummies. ATE and ATT stand for Average Treatment Effect and Average Treatment effect on the Treated, respectively. *** 1% significance level; ** 5% significance level; * 10% significance level.

Table 13. Economic Growth Predictor Means in the Pre-Treatment Period

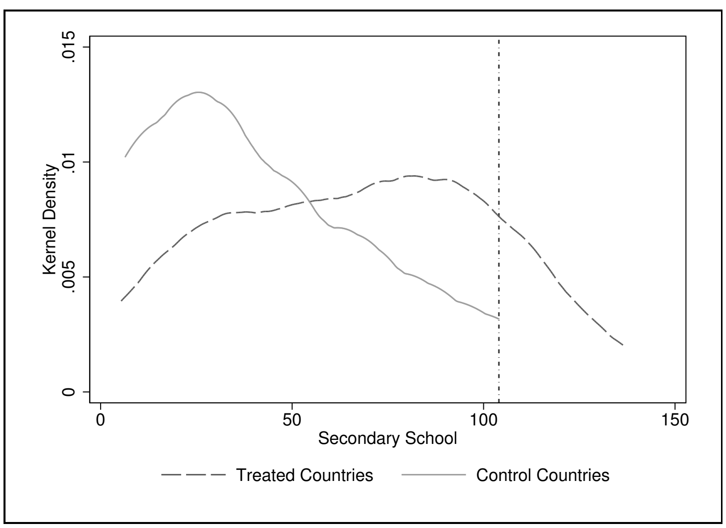
	Georgia	Synth. Control A	Synth. Control B
Secondary school	75.850	72.840	92.529
Population growth	-0.233	0.957	1.401
Investment share	0.040	0.124	0.115
Pre-treatment GDP	583.067	678.535	594.333
	Armenia	Synth. Control A	Synth. Control B
Secondary school	85.850	83.419	92.529
Population growth	0.663	1.531	1.401
Investment share	0.111	0.122	0.115
Pre-treatment GDP	441.634	511.739	594.333
	Azerbaijan	Synth. Control A	Synth. Control B
Secondary school	81.350	81.352	91.137
Population growth	1.341	1.337	0.713
Investment share	0.153	0.147	0.109
Pre-treatment GDP	833.773	821.143	840.535
	Tajikistan	Synth. Control A	Synth. Control B
Secondary school	80.925	77.957	-
Population growth	1.543	1.990	-
Investment share	0.090	0.122	-
Pre-treatment GDP	239.526	342.553	-
	Uzbekistan	Synth. Control A	Synth. Control B
Secondary school	93.750	85.530	-
Population growth	2.005	0.473	-
Investment share	0.121	0.095	-
Pre-treatment GDP	378.514	382.545	-
	Morocco	Synth. Control A	Synth. Control B
Secondary school	31.670	31.787	28.934
Population growth	2.494	2.499	2.838
Investment share	0.144	0.144	0.120
Pre-treatment GDP	814.392	818.135	847.556
	Tunisia	Synth. Control A	Synth. Control B
Secondary school	41.052	40.963	40.761
Population growth	2.287	2.281	2.723
Investment share	0.179	0.179	0.150
Pre-treatment GDP	1,089.462	1,070.248	1,088.473
	Mauritania	Synth. Control A	Synth. Control B
Secondary school	12.389	12.447	17.681
Population growth	2.474	2.479	2.628
Investment share	0.062	0.062	0.094
Pre-treatment GDP	398.482	421.044	433.074
	Egypt	Synth. Control A	Synth. Control B
Secondary school	62.071	57.181	48.433
Population growth	2.313	2.834	3.239
Investment share	0.070	0.159	0.113
Pre-treatment GDP	764.775	837.891	981.882

Figure 1. Common support for investment share, 1991-2000



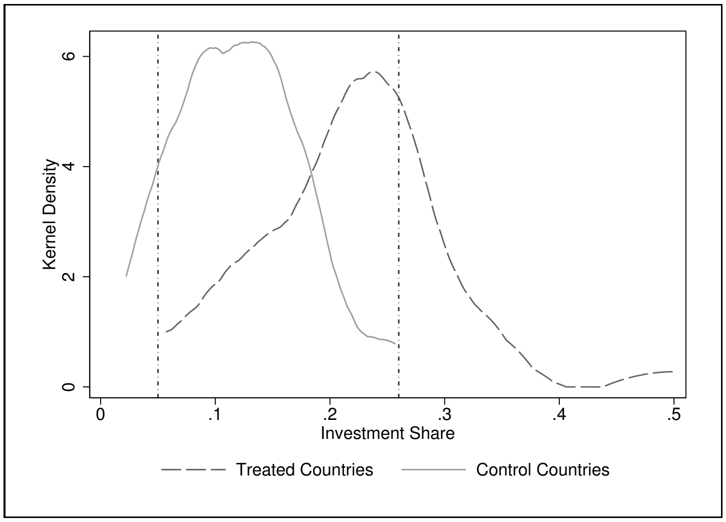
Data: Persson and Tabellini (2006). Treated countries: 87. Control countries: 26. All countries in common support.

Figure 2. Common support for secondary school enrollment, 1991-2000



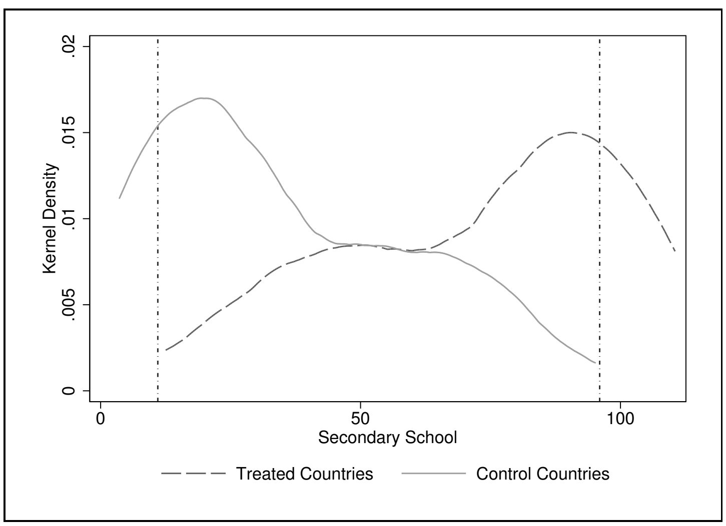
Data: Persson and Tabellini (2006). Treated countries: 87. Control countries: 26. Common support: (0, 104). Countries above common support: 14 treated.

Figure 3. Common support for investment share, 1981-90



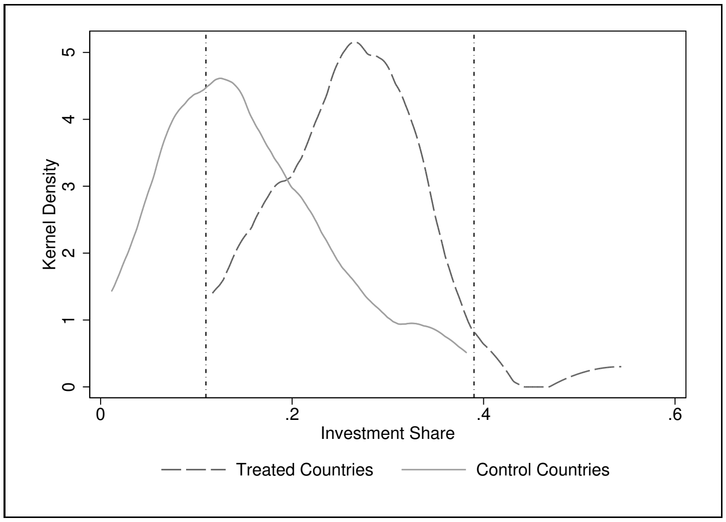
Data: Persson and Tabellini (2006). Treated countries: 43. Control countries: 66. Common support: (0.05, 0.26). Countries above common support: 10 treated. Countries below common support: 6 controls.

Figure 4. Common support for secondary school enrollment, 1981-90



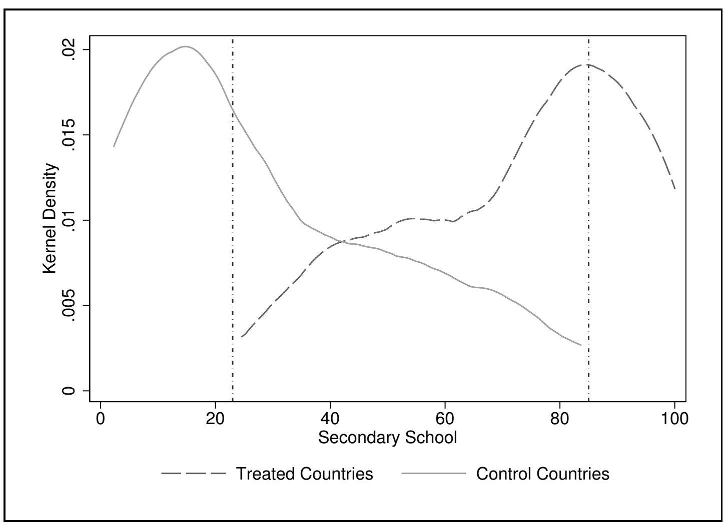
Data: Persson and Tabellini (2006). Treated countries: 43. Control countries: 66. Common support: (11, 96). Countries above common support: 11 treated. Countries below common support: 11 controls.

Figure 5. Common support for investment share, 1971-80



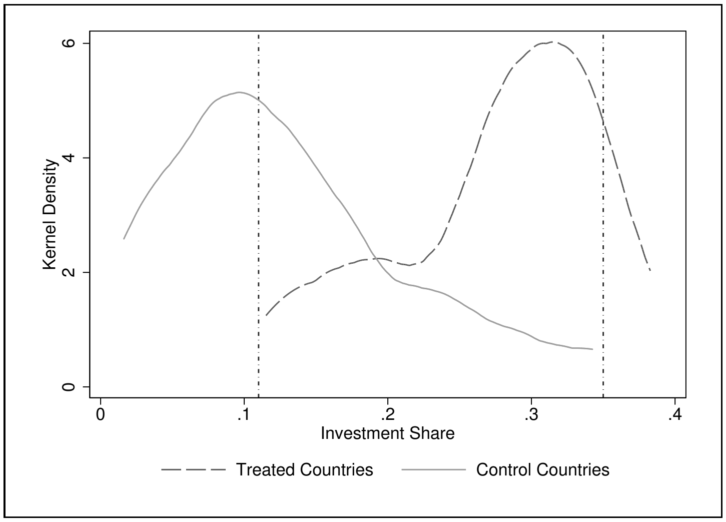
Data: Persson and Tabellini (2006). Treated countries: 33. Control countries: 74. Common support: (0.11, 0.39). Countries above common support: 1 treated. Countries below common support: 27 controls.

Figure 6. Common support for secondary school enrollment, 1971-80



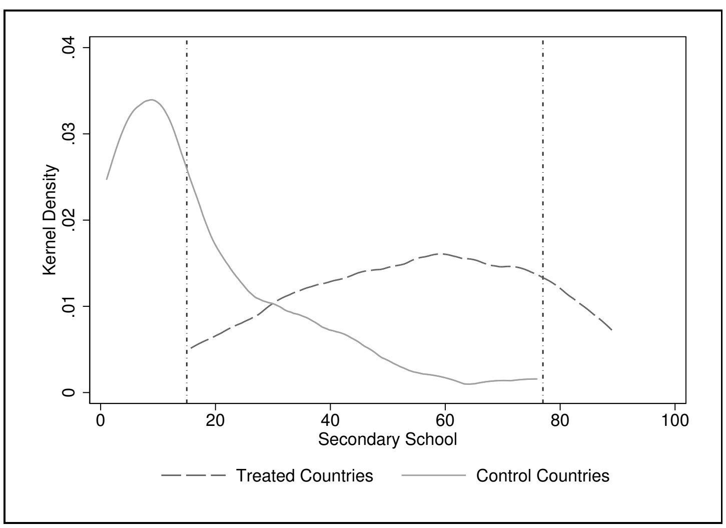
Data: Persson and Tabellini (2006). Treated countries: 33. Control countries: 74. Common support: (23, 85). Countries above common support: 11 treated. Countries below common support: 38 controls.

Figure 7. Common support for investment share, 1961-70



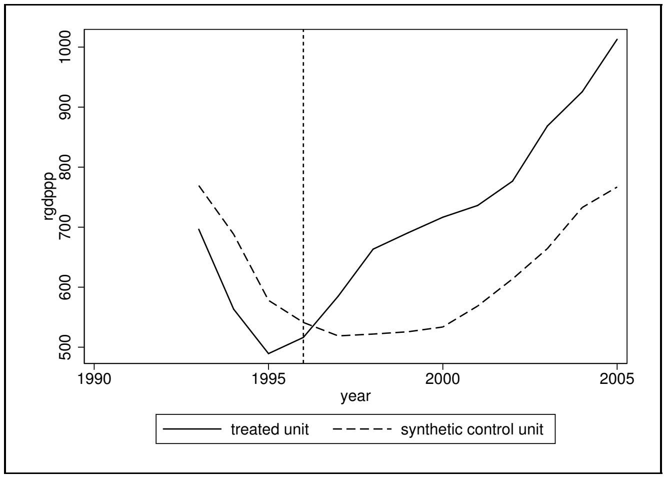
Data: Persson and Tabellini (2006). Treated countries: 31. Control countries: 75. Common support: (0.11, 0.35). Countries above common support: 4 treated. Countries below common support: 32 controls.

Figure 8. Common support for secondary school enrollment, 1961-70



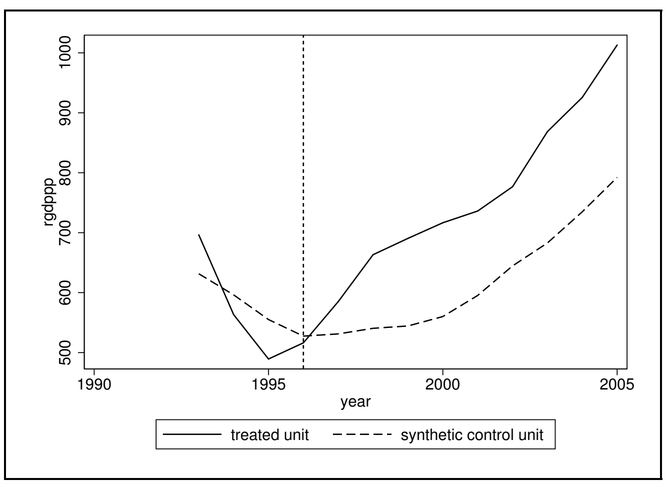
Data: Persson and Tabellini (2006). Treated countries: 31. Control countries: 75. Common support: (15, 77). Countries above common support: 5 treated. Countries below common support: 45 controls.

Figure 9. Trends in Real GDP Per Capita, Georgia vs. Synthetic Control - Case A



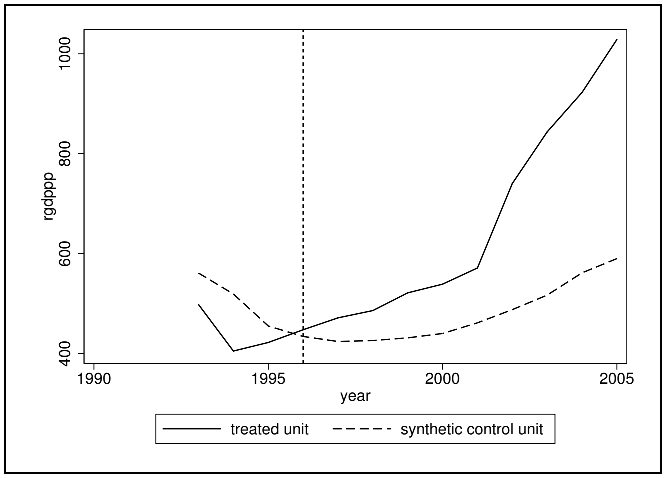
Potential controls: all closed countries except OECD and Latin America. Non-zero weights in synthetic control: Ukraine (0.41), Uzbekistan (0.17), Vietnam (0.42). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 10. Trends in Real GDP Per Capita, Georgia vs. Synthetic Control - Case B



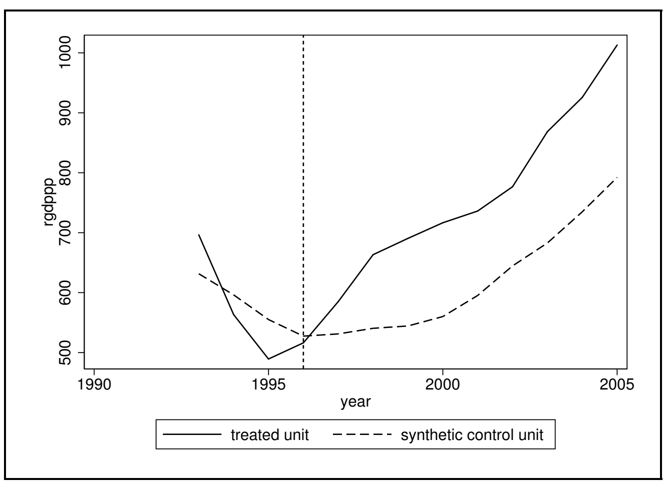
Potential controls: all closed countries in former Soviet Union. Non-zero weights in synthetic control: Kazakhstan (0.21), Uzbekistan (0.79). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 11. Trends in Real GDP Per Capita, Armenia vs. Synthetic Control - Case A



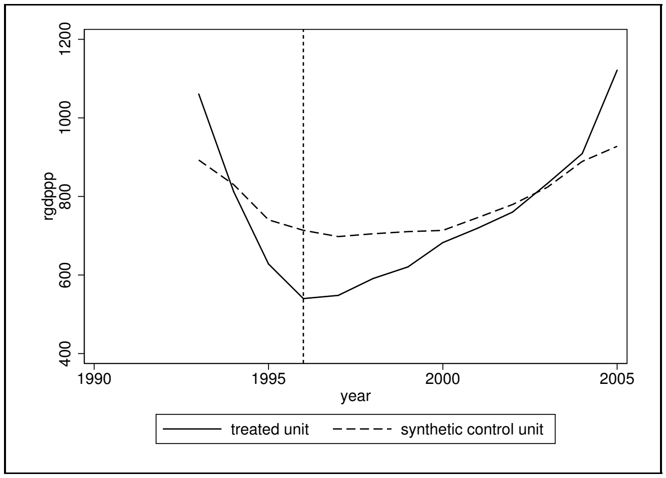
Potential controls: all closed countries except OECD and Latin America. Non-zero weights in synthetic control: Ukraine (0.19), Uzbekistan (0.61), Vietnam (0.20). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 12. Trends in Real GDP Per Capita, Armenia vs. Synthetic Control - Case B



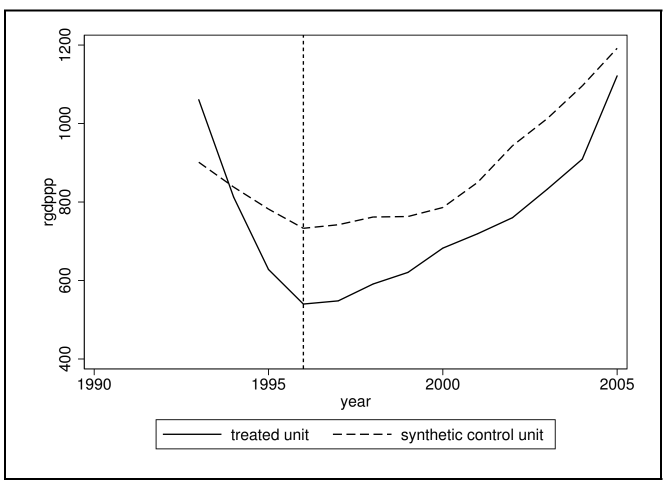
Potential controls: all closed countries in former Soviet Union. Non-zero weights in synthetic control: Kazakhstan (0.21), Uzbekistan (0.79). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 13. Trends in Real GDP Per Capita, Azerbaijan vs. Synthetic Control - Case A



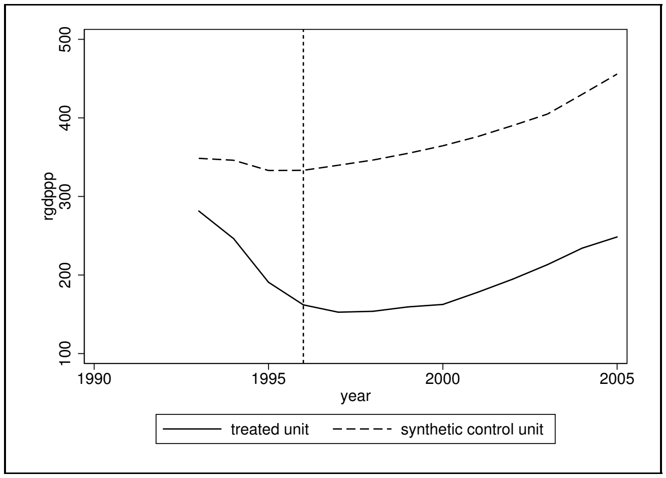
Potential controls: all closed countries except OECD and Latin America. Weights larger than 2% in synth. control: India (0.04), Ukraine (0.28), Uzbekistan (0.48), Vietnam (0.05). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 14. Trends in Real GDP Per Capita, Azerbaijan vs. Synthetic Control - Case B



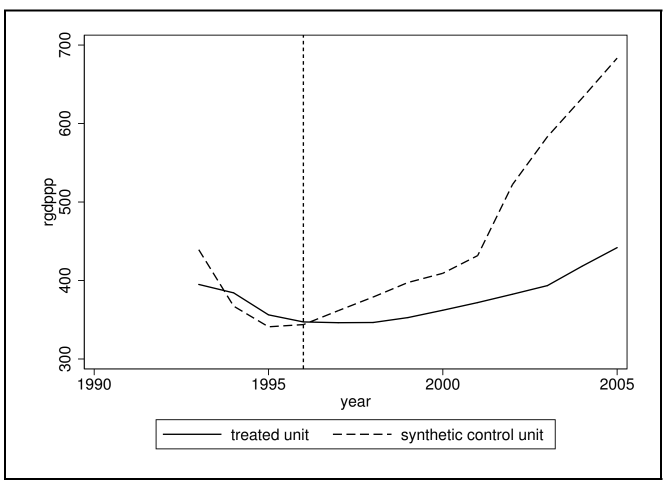
Potential controls: all closed countries in former Soviet Union. Non-zero weights in synthetic control: Kazakhstan (0.44), Uzbekistan (0.56). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 15. Trends in Real GDP Per Capita, Tajikistan vs. Synthetic Control - Case A



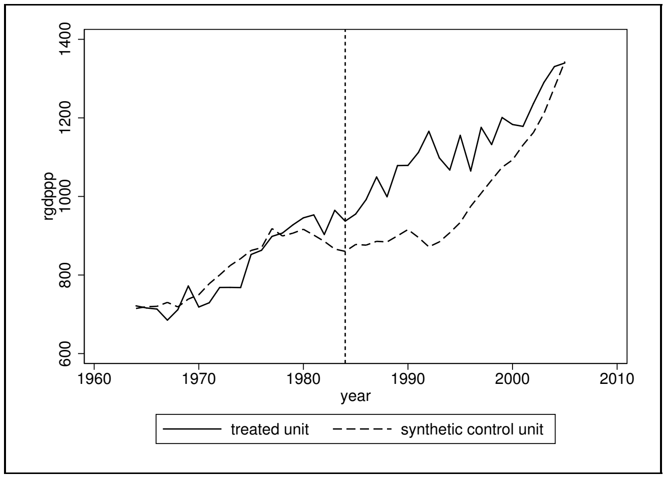
Potential controls: all closed countries except OECD and Latin America. Weights larger than 2% in synthetic control: Eritrea (0.04), Uzbekistan (0.68), Vietnam (0.28). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 16. Trends in Real GDP Per Capita, Uzbekistan vs. Synthetic Control - Case A



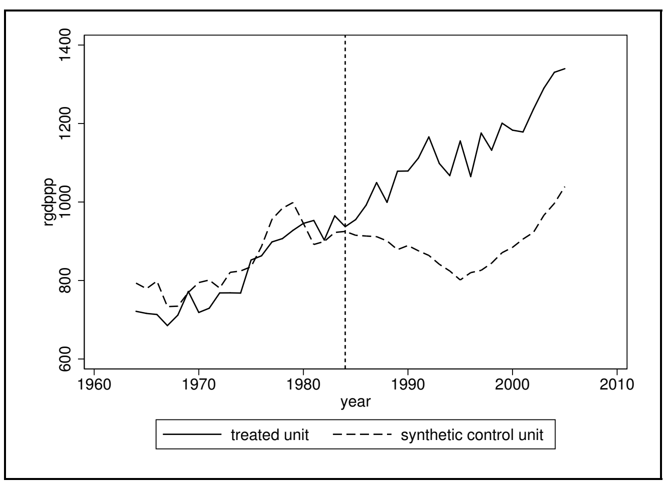
Potential controls: countries in former Soviet Union that opened trade in 1995-96. Non-zero weights in synthetic control: Armenia (0.49), Kyrgyz Republic (0.46), Tajikistan (0.05). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 17. Trends in Real GDP Per Capita, Morocco vs. Synthetic Control - Case A



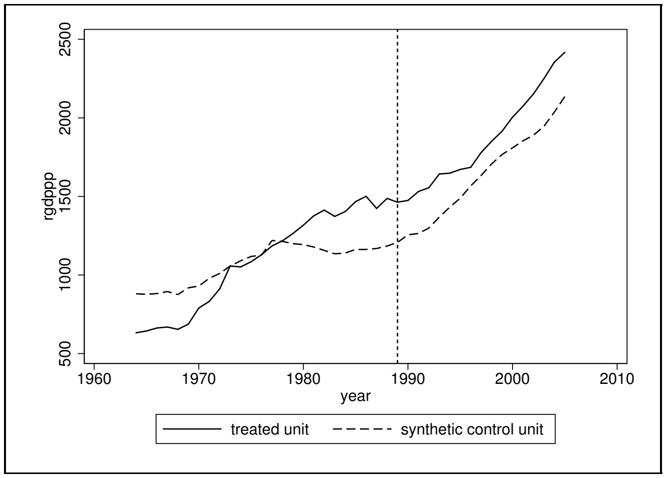
Potential controls: all closed countries except OECD and Latin America. Weights larger than 2% in synth. control: Egypt (0.19), Lesotho (0.46), Swaziland (0.02), Tanzania (0.05). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 18. Trends in Real GDP Per Capita, Morocco vs. Synthetic Control - Case B



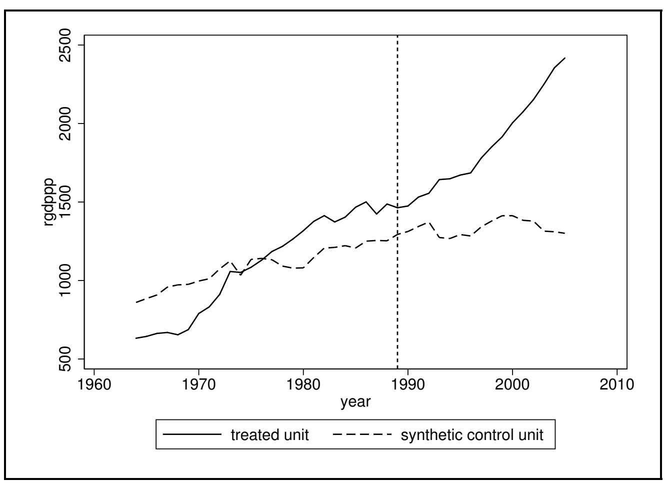
Potential controls: all closed countries in Middle East. Non-zero weights in synthetic control: Algeria (0.32), Egypt (0.03), Sudan (0.65). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 19. Trends in Real GDP Per Capita, Tunisia vs. Synthetic Control - Case A



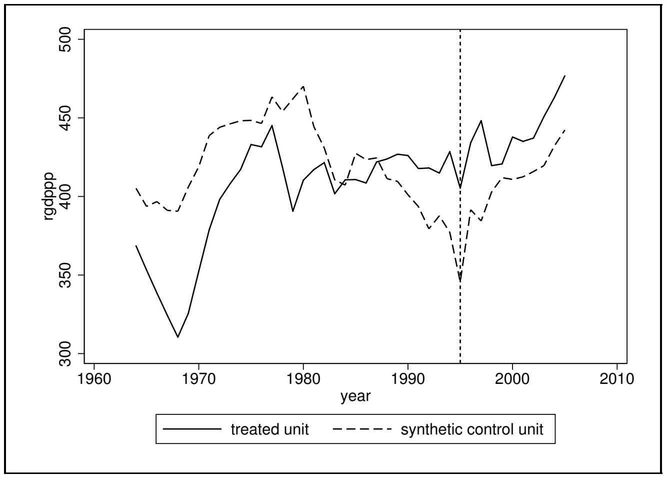
Potential controls: all closed countries except OECD and Latin America. Weights larger than 2% in synthetic control: China (0.53), Iran (0.07), Malta (0.06), Tanzania (0.16). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 20. Trends in Real GDP Per Capita, Tunisia vs. Synthetic Control - Case B



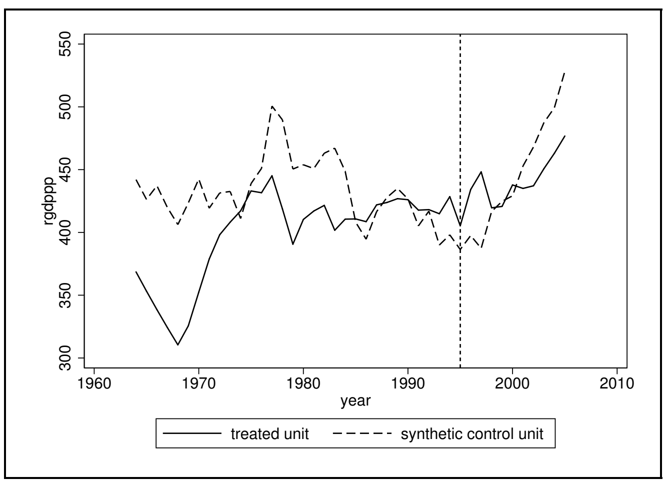
Potential controls: all closed countries in Middle East. Non-zero weights in synthetic control: Egypt (0.10), Iran (0.57), Sudan (0.33). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 21. Trends in Real GDP Per Capita, Mauritania vs. Synthetic Control - Case A



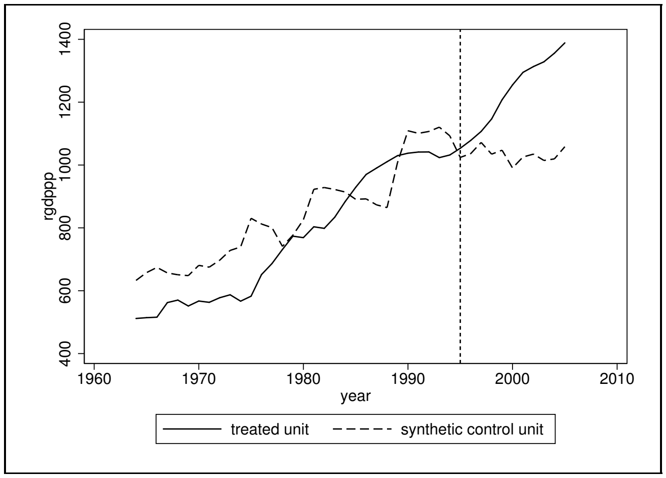
Potential controls: all closed countries except OECD and Latin America. Weights larger than 2% in synthetic control: India (0.07), Malawi (0.05), Rwanda (0.68), Togo (0.07). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 22. Trends in Real GDP Per Capita, Mauritania vs. Synthetic Control - Case B



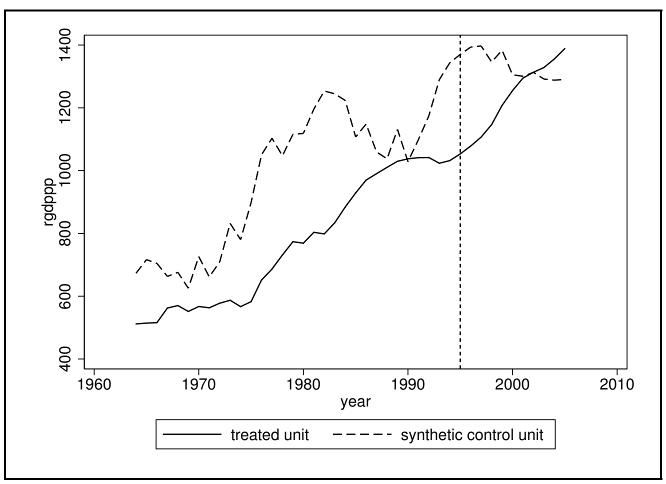
Potential controls: all closed countries in Middle East and nearby Africa. Non-zero weights in synthetic control: Djibouti (0.03), Sudan (0.97). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Figure 23. Trends in Real GDP Per Capita, Egypt vs. Synthetic Control - Case A



Potential controls: all closed countries except OECD and Latin America. Non-zero weights in synthetic control: Congo (0.85), Syria (0.15). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP, secondary school enrollment, population growth, investment share.

Figure 24. Trends in Real GDP Per Capita, Egypt vs. Synthetic Control - Case B



Potential controls: all closed countries in Middle East and nearby Africa. Non-zero weights in synthetic control: Sudan (0.01), Syria (0.99). Outcome variable: real GDP per capita. Predictors: pre-treatment GDP per capita, secondary school enrollment, population growth, investment share.

Appendix I. Treated and Control Countries by Region

A. Period 1991–2000

AFRICA

Treated: Benin, Botswana, Cameroon, Cape Verde, Ethiopia, Gambia, Ghana, Guinea, Ivory Coast, Kenya, Madagascar, Mali, Mauritius, Mozambique, Niger, South Africa, Tanzania, Uganda, Zambia.

Control: Angola, Burkina Faso, Burundi, Central African Republic, Chad, Congo, Gabon, Lesotho, Malawi, Nigeria, Rwanda, Senegal, Togo, Zimbabwe.

ASIA

Treated: Hong Kong, Indonesia, Malaysia, Nepal, Philippines, Singapore, South Korea, Sri Lanka, Thailand.

Control: China, India, Pakistan, Papua New Guinea.

LATIN AMERICA

Treated: Argentina, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Guyana, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Trinidad & Tobago, Uruguay.

Control: Venezuela.

MIDDLE EAST

Treated: Egypt, Jordan, Mauritania, Morocco, Tunisia, Yemen.

Control: Algeria, Iran, Syria.

OECD

Treated: Australia, Austria, Belgium, Canada, Cyprus, Denmark, Finland, France, Germany, Greece, Ireland, Israel, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States.

Control: Iceland.

TRANSITION ECONOMIES

Treated: Albania, Bulgaria, Czech Republic, Hungary, Latvia, Poland, Romania, Slovak Republic, Slovenia.

Control: Belarus, Russia, Ukraine.

B. Period 1981–90

AFRICA

Treated: Botswana, Gambia, Ghana, Guinea, Mauritius.

Control: Angola, Benin, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chad, Congo, Ethiopia, Gabon, Guinea-Bissau, Ivory Coast, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, South Africa, Tanzania, Togo, Uganda, Zambia, Zimbabwe.

ASIA

Treated: Hong Kong, Indonesia, Malaysia, Singapore, South Korea, Thailand.

Control: Bangladesh, China, India, Nepal, Pakistan, Papua New Guinea, Philippines, Sri Lanka.

LATIN AMERICA

Treated: Barbados, Bolivia, Chile, Colombia, Costa Rica, Mexico.

Control: Argentina, Brazil, Dominican Republic, Ecuador, El Salvador, Guatemala, Guyana, Haiti, Honduras, Jamaica, Nicaragua, Panama, Paraguay, Peru, Trinidad & Tobago, Uruguay, Venezuela.

MIDDLE EAST

Treated: Jordan, Morocco.

Control: Algeria, Egypt, Iran, Mauritania, Syria, Tunisia.

OECD

Treated: Australia, Austria, Belgium, Canada, Cyprus, Denmark, Finland, France, Germany, Greece, Ireland, Israel, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States.

Control: Iceland, Turkey.

TRANSITION ECONOMIES

Treated: None.

Control: Hungary, Poland, Romania.

C. Period 1971–80

AFRICA

Treated: Mauritius.

Control: Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chad, Congo, Ethiopia, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Ivory Coast, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Tanzania, Togo, Uganda, Zambia, Zimbabwe.

ASIA

Treated: Hong Kong, Indonesia, Malaysia, Singapore, South Korea, Thailand.

Control: Bangladesh, China, India, Nepal, Pakistan, Papua New Guinea, Philippines, Sri Lanka.

LATIN AMERICA

Treated: Barbados, Bolivia, Chile, Ecuador.

Control: Argentina, Brazil, Colombia, Costa Rica, Dominican Republic, El Salvador, Guatemala, Guyana, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Trinidad & Tobago, Uruguay, Venezuela.

MIDDLE EAST

Treated: Jordan.

Control: Algeria, Egypt, Iran, Mauritania, Morocco, Syria, Tunisia.

OECD

Treated: Australia, Austria, Belgium, Canada, Cyprus, Denmark, Finland, France, Greece, Ireland, Italy, Japan, Luxembourg, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States.

Control: Iceland, Israel, New Zealand, Turkey.

TRANSITION ECONOMIES

Treated: None.

Control: Hungary, Poland, Romania.

D. Period 1961–70

AFRICA

Treated: None.

Control: Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chad, Congo, Ethiopia, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Ivory Coast, Kenya, Lesotho, Madagascar, Malawi, Mali, Mauritius, Mozambique, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, South Africa, Tanzania, Togo, Uganda, Zambia, Zimbabwe.

ASIA

Treated: Hong Kong, Malaysia, Singapore, Thailand.

Control: Bangladesh, China, India, Indonesia, Nepal, Pakistan, Papua New Guinea, Philippines, South Korea, Sri Lanka.

LATIN AMERICA

Treated: Barbados, Bolivia, Ecuador, Jamaica, Peru.

Control: Argentina, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, El Salvador, Guatemala, Guyana, Haiti, Honduras, Mexico, Nicaragua, Panama, Paraguay, Trinidad & Tobago, Uruguay, Venezuela.

MIDDLE EAST

Treated: Jordan.

Control: Algeria, Egypt, Iran, Mauritania, Morocco, Syria, Tunisia.

OECD

Treated: Australia, Austria, Belgium, Canada, Cyprus, Denmark, Finland, France, Greece, Ireland, Italy, Japan, Luxembourg, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States.

Control: Iceland, Israel, New Zealand, Turkey.

TRANSITION ECONOMIES

Treated: None.

Control: Romania.

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