

Do Open Transition Economies Grow Faster?*

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Abstract

We investigate the effect of trade openness on economic growth in transition countries using a transparent statistical methodology that leads to data-driven case studies. In particular, we employ synthetic control methods in a panel of transition economies and compare GDP growth in treated (that is, open) countries with growth in a convex combination of similar but untreated (that is, closed) countries. We find that trade liberalization tends to have a positive effect on the pattern of real GDP per capita. One of our most robust results shows that making the transition without opening up to trade considerably hampers growth.

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I. Introduction

A heated debate exists in the economics profession regarding the question whether openness to trade is beneficial for growth. Theoretical frameworks are often based on the concept of comparative advantage and assign an at least transitory positive growth effect to economic liberalization. Empirical evaluations, however, have been complicated by a number of factors.

Standard empirical techniques employing cross-country estimators in the spirit of Barro (1991) to assess this question are fraught with problems, including treatment endogeneity. For this reason, it has been suggested to rely on case studies to investigate the effect of trade openness on growth (Bhagwati and Srinivasan, 2001). Case studies, however, also suffer from certain deficiencies as they lack statistical rigor and are exposed to arbitrary case selection.

As an intermediate way between standard cross-country analysis and case studies, in this paper we propose to apply a recent econometric technique, the synthetic control methods proposed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2007), to perform data-driven comparative case studies. We evaluate the effect of a binary treatment—trade liberalization—on the outcome—real per capita income—in a panel framework. This methodology accounts for time-varying country unobservables, and therefore overcomes a major drawback of more standard estimation techniques.

In other words, we ask whether opening the economy in year T_0 will lead to higher growth performance in the years T_0+i compared to similar countries that have not opened up. The advantage of our approach lies in the transparent construction of the counterfactual outcome of the treated, namely a linear combination of untreated countries—the synthetic control. The comparison countries that form the synthetic control unit are selected based on their similarity to the treated country before the treatment takes place, both with respect to relevant covariates and past realizations of the outcome.

In this paper, we focus on a group of countries, transition economies, for which this discussion is particularly relevant for at least three reasons. First, the question why some transition economies have done better than others is *a priori* not settled—an example is the discussion about initial conditions versus liberalization policies as the main driver for results. In this context, some authors underline that initial conditions have a stronger effect, especially in the long run, on economic development than good policies; others take the opposite view, maintaining that the impact of initial conditions on economic performance diminishes rapidly over time.¹ Second, most transition economies only emerged in the early 1990s, and only slowly enough data become available to evaluate empirically this question and others that are of a more long-run nature, especially given the widespread data weaknesses in the early 1990s. Third, transition economies represent a relatively homogeneous set of countries that started from an environment of socialist central planning and were all subject to a broadly similar external shock (dissolution of the Soviet Union) but somewhat different economic policies, rendering them particularly interesting for comparing alternative growth experiences.²

We investigate whether the choice of the reform program—and in particular implementing a policy geared at trade openness—has had a material impact on economic growth and wellbeing. The basic question we ask is: do open transition economies grow faster? In this context, we are more interested in trade reform efforts as opposed to trade reform outcomes. We study all episodes of trade liberalization that took place in transition economies—as covered by the European Bank for Reconstruction and Development

¹On the former position, see, e.g., Lee and Jeong (2006); Berg et al. (1999) take the latter view. For a more accentuated view, see de Melo et al. (1997), who find that initial conditions dominate in explaining inflation, whereas economic liberalization emerges in their research as more important in determining differences in growth.

²Whether transition should follow a “big bang” approach—implementing many drastic policy reforms as early as possible—or “gradualism” was a prominent discussion for much of the 1990s. Wyplosz (2000) portrays the discussion in terms of feasibility—big bang highly desirable but impracticable, gradualism unavoidable but to be compressed to the extent possible—and reviews the extensive literature on the subject.

(EBRD)—to the extent that they can be analyzed in our empirical framework. In our sample selection procedure, we require that for each country that liberalized trade in a certain year, there should be a sufficient number of comparison countries that did not liberalize before or immediately after.

We find that, for most of the countries that we can study, trade liberalization has had a positive effect on growth. However, the case selection procedure indicates that for transition economies, we are skating on thin ice to identify the effect of trade openness, because liberalization waves considerably reduce the number of available comparison countries. As a result of our strive for sound country comparisons, we can only analyze five transition economies using this framework, and although the results on the positive effect of openness on growth are robust to a number of additional tests as evidenced below, we have to caution against generalizing our results beyond the pool of transition countries analyzed. We claim, however, that this is indeed an advantage of the synthetic control approach: a restrictive sample selection that improves internal validity and avoids the unsatisfactory comparisons hidden behind standard econometric techniques based on some degree of cross-sectional variation (see also Billmeier and Nannicini, *forthcoming*).

The remainder of the paper is structured as follows. In Section II, we briefly review the empirical literature related to our paper, including studies on trade and growth aiming at overcoming some of the OLS weaknesses, and related research on transition economies. In Section III, we briefly present the data sources and variables of interest. Synthetic control methods are applied in Section IV to empirically explore the effect of trade liberalization on growth in transition economies. Section V concludes.

II. Related Literature

Empirical Studies on Trade Openness and Growth

Notwithstanding a large body of supportive literature, providing conclusive empirical evidence on the intuitively positive causal effect of trade on growth has been a challenging endeavor, complicated by various factors; see Winters (2004) for an overview, and Dollar (1992), Sachs and Warner (1995), and Edwards (1992, 1998) as examples of evidence in favor of a positive effect; Vamvakidis (2002) provides historical evidence. Rodriguez and Rodrik (2001), however, point out that much of this literature has used cross-country evidence that suffers from numerous shortcomings, related to both the measurement of openness and econometric modeling.

To measure trade and openness, two sets of indicators have emerged in the literature, addressing somewhat different questions. Some studies have used simple measures of trade volumes—somewhat more of a reform outcome in terms of the discussion in the introduction—that are particularly subject to endogeneity issues, especially if normalized by GDP.³ To overcome the endogeneity issues in this literature, a number of contributions have turned to instrumental variables (IV) as a remedy; see, for example, Frankel and Romer (1999). This strategy has gained more prominence, however, for trade *volume* measures, less for measures of the *policy* stance. To identify the impact of trade policies, a measure of the stance is needed that is reasonably independent of possible regressands.

In an influential paper, Sachs and Warner (1995) undertook the ambitious effort to analyze data for a large amount of countries, thereby deriving a binary indicator for the trade policy stance (open/closed) based on data for: tariff and non-tariff barriers; the black market premium associated with possible parallel markets for the exchange rate; and the economic and political background (socialist country, state export monopoly).

³For example, higher imports of investment goods today might be driven by higher growth tomorrow.

Condensing these diverse indicators into a single variable follows the logic that these are the dimensions along which an economic policymaker can close an economy to international trade. While the effect of the Sachs-Warner dummy on growth has often been found to be high and apparently robust,⁴ Rodriguez and Rodrik (2001) have shown that it is heavily biased toward two components: the black-market premium, and the state export monopoly. A measure constructed only with these two indicators would yield very similar results, whereas the more direct measures of trade openness—tariff and non-tariff barriers—contribute surprisingly little.

Another strand in the trade and growth literature seeks to improve upon basic 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 (*forthcoming*) 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 variation.

Slaughter (2001) uses a difference-in-differences approach to infer on 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.⁵ Giavazzi and Tabellini (2005) also apply a difference-in-differences approach to study the interactions

⁴In the original benchmark specification, a country grows by approximately 2.4 percentage points faster if it can be considered open in line with the definition.

⁵Only in the cases of the European Economic Community and the Kennedy Round of the General Agreement on Tariffs and Trade, the annual rate of change of income dispersion ('sigma'-convergence) decreases insignificantly by about 0.3 percentage points.

between economic and political liberalizations. They find a positive and significant effect of economic liberalization on per capita income growth of: 0.9 percent if a country only opened to trade; 2.2 percent if a country opened to trade first and then experienced also political liberalization. Furthermore, they show that the sequencing matters in that it is advantageous, from a growth perspective, to first liberalize the trade regime and only later the political environment.

Bhagwati and Srinivasan (2002) question cross-country evidence on the trade-growth nexus from a methodological perspective, and promote case studies as a way to avoid the pitfalls of standard cross-country evidence. They point out (p. 181) 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 events. Using an empirical strategy based on non-parametric matching estimators, Billmeier and Nannicini (*forthcoming*) show yet another potential pitfall of common cross-country estimates of the effect of trade on growth: if the treatment (openness to trade) is not evenly distributed over the covariate space—almost all countries with certain characteristics are either open or closed—the countries lack “common support”. In this situation, estimators using cross-sectional variation lose internal validity.

In this paper, we take on board three of the main recommendations of the above studies on the weaknesses of cross-country estimation procedures in the growth literature. First, to focus the analysis on a subsample of countries with a similar—geographic or otherwise—background, we study only transition economies as they have a common institutional starting position (economy characterized by socialist central planning), while the discussion between *initial conditions* and *policies* is limited to the economic situation and does not imply a comparison of countries that have experienced very different his-

torical developments. Second, to compare apples with apples and check for the existence of common support between treated and untreated countries, we carefully discuss our case selection procedure and the country comparisons lying behind our results. Third, to control for treatment endogeneity, we apply synthetic control methods, which account for time-varying unobservable confounding factors.

Determinants of Growth in Transition Economies

The breakdown of the Soviet Union triggered a whole economic research agenda on the fate of transition economies. While one of the major concerns of the literature in this context relates to explaining the growth experience of these countries, little has been done to connect the question of trade openness explicitly to the growth outcome.

Some key papers in this literature do not consider explicitly the causal role of trade in promoting growth. Svejnar (2002) distinguishes “big bang” from gradual reformers and assesses performance and challenges of transition economies in a broad sense, covering also issues related to life expectancy, fertility, and unemployment. Campos and Coricelli (2002) provide a wide-ranging overview of the issues at hand and present some purely descriptive evidence on trade volumes, patterns, and the degree of openness between 1990 and 1998. Fischer, Sahay, and Vegh (1996a) characterize the growth process during the transition experience as a linear combination of a temporary transformation process—which hinges on low inflation, a solid fiscal position, official assistance, and progress with privatization—and more standard long-run growth determinants (initial conditions, population growth, secondary school enrollment, investment share). Similarly to Campos and Coricelli, they also do not relate trade openness to growth.

Several other papers instead allow for the possibility that trade openness has affected growth in transition countries without focusing explicitly on the openness-growth nexus. A major discussion in this literature responds to the question whether growth in tran-

sition economies is due to initial conditions or transition policies. Although measuring trade or liberalization policies in transition countries is intrinsically difficult, trade openness is usually subsumed under the policy variables, but often in a rather implicit way, bundling trade policy with other indicators; one prominent example is the index assembled by de Melo, Denizer, and Gelb (1996). Based on data drawn from the EBRD's *Transition Reports*, these authors construct an indicator of economic liberalization, which has been used repeatedly in the literature. Liberalization of external markets including the foreign trade regime corresponds to about one-third of the overall index. They find a quadratic relationship between output growth and the cumulative liberalization index, with liberalization being extremely beneficial at the initial stages of transition.

Other examples of contributions that use this index and find that policies and structural reforms—as opposed to initial conditions—have a strong impact on economic performance include Hernandez-Cata (1997) and Berg *et al.* (1999). On the other hand, Heybey and Murrell (1999)—based on de Melo, Denizer and Gelb (1996)—and Falcetti, Raiser, and Sanfey (2002)—based on an index that is also derived from the EBRD's *Transition Report*—find that initial conditions are more important than policies during transition, at least at the beginning. Radulescu and Barlow (2002) present an extreme bounds analysis of growth determinants in transition countries and do not find evidence that overall liberalization policies or specifically trade liberalization enhance growth.⁶ Fischer and Sahay (2001) reconcile both competing views with the data.

Fischer, Sahay, and Vegh (1996b) treat trade openness (exports to other countries in the Council of Mutual Economic Assistance, CMEA, as a share of total GDP) as initial condition and introduce trade liberalization as a policy variable. By introducing the notion of “stabilization time” as opposed to chronological time—that is, the authors shift countries contained in their analysis such that all start their transformation in period

⁶See also the comment by Rzonca and Cizkowicz (2003), who dispute the conclusion drawn by Radulescu and Barlow (2002).

T_0 instead of various calendar years—they find that the cumulative liberalization index taken from de Melo, Denizer, and Gelb (1996) is highly significant in explaining the growth performance in transition economies. Beck and Laeven (2006) follow a similar strategy—treat CMEA trade as a share of GDP as an initial condition and control for trade policy using the de Melo et al. indicator—but focus mainly on the role of institutions in promoting growth in transition economies.

To the best of our knowledge, Barlow (2006) appears to be the contribution that focuses most explicitly on the link between trade policy liberalization by itself (as well as other separate aspects of liberalization) and growth in transition economies. Using the EBRD’s transition indices, he finds that the level of trade liberalization raises the growth level especially in transition countries that are closer to the European Union of 12/15. Moreover, the effect appears stronger in the first half of his sample (1993–1996). The estimates are somewhat hard to interpret numerically, however, as they refer to the EBRD’s trade liberalization index: an improvement in the index by 1 unit improves the growth rate of GDP by about 3 percentage points (baseline estimates, whole sample, no interaction terms).

III. Variables of Interest and Data

We are interested in estimating the effect of trade liberalization in transition economies in the 1990s on an outcome measure reflecting economic wellbeing. For the latter, we chose the time series of real per capita GDP, because we are interested in the dynamic impact of trade openness over time, not its one-off effects on the individual income level. The series comes from the IMF’s *World Economic Outlook* database. The outcome variable (GDP per capita) extends to 2005 where available, to gain a few more post-treatment years when comparing outcomes in treated and control countries, which is particularly

important for transition economies as many of them liberalized only recently. The real GDP per capita is measured in 2002 US\$.

As indicator of trade liberalization, we use a binary indicator of trade policy familiar from the literature, by Sachs and Warner (1995), extended, updated, and revised by Wacziarg and Welch (*forthcoming*); short SWWW. We discard the measure by de Melo, Denizer, and Gelb (1996) as we focus on trade policy and we also include non-transition economies—for which the measure does not exist—as potential controls in some specifications of our estimation procedure. According to the SWWW 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.⁷

When applying synthetic control methods in a panel set-up, we refer to the “treatment” as the event of becoming open, after being closed in the preceding years according to the SWWW indicator. Our treatment thus intends to capture policy changes that reduce the constraints on market operations below a critical threshold along the above five dimensions. As the variation in dimension (iii) is common to all transition economies, in our sample the SWWW indicator mainly captures trade policy (with the exception of the black market premium dimension).

For our control variables, we draw on a data set used recently by Persson and Tabellini (2006); short PT. The data set includes data for about 180 advanced and developing countries but stops in 2000. The set of control variables used in this paper includes those usually employed in the growth literature (initial GDP, investment as a share of GDP, population growth, and secondary school enrollment).

⁷See Billmeier and Nannicini (*forthcoming*) for an overview of the various approaches to measure trade openness and associated problems. See also Harrison (1996) for a comparison of various indicators.

As we are interested in periods of trade liberalization that took place in the 1990s, we include in our sample—in addition to all transition economies—all countries for which the SWWW indicator is available (113) and that were not completely open in the 1990s according to the SWWW dummy. We therefore drop OECD and Latin American economies, ending up with 67 countries, of which 43 are open and 24 are closed for the majority of the 1990s.⁸

IV. Comparative Studies Using Synthetic Control

Methods

Methodology

An estimation approach recently implemented for comparative case studies—the synthetic control methods (SCMs) 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 comparison countries—namely, the synthetic control—is constructed to approximate the most relevant characteristics of the country affected by the intervention. After a regime change (for instance, trade liberalization) takes place in a given country, SCMs 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.

In our empirical application, we present evidence for two symmetric treatments. First, we consider countries that liberalize trade in a particular year (over the 1990–2000 period) and compare them with countries that remain closed for at least 10 more years (or until the end of the sample in 2005). Second, we investigate a country (Uzbekistan, see below)

⁸See Appendix I for a worldwide overview of available control countries in the 1990s. See Appendix II for more details on the set of potential controls.

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. For the sake of simple illustration, in the following formal discussion of the methodology we just refer to the first type of treatment, that is, trade liberalization.

In the present context, it is useful to reason in terms of potential outcomes in 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:

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

where $Y_{it}(l)$ stands for the potential outcome associated with $T = l$. The estimand of interest is the vector $(\tau_{i,T_0+1}, \dots, \tau_{i,T})$. For any period $t > T_0$, 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:

$$Y_{jt}(0) = \delta_t + \nu_{jt} \quad (2)$$

$$Y_{jt}(1) = \delta_t + \tau_{jt} + \nu_{jt} \quad (3)$$

$$\nu_{jt} = Z_j \theta_t + \lambda_t \mu_j + \epsilon_{jt}, \quad (4)$$

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. In the present context, as all of the variables in Z_j (initial GDP, population growth, secondary school enrollment, and investment share) refer to the pre-

treatment period, the assumption that they are not affected by the treatment means that we have to rule out “anticipation” effects, i.e., that those variables immediately change in response to the anticipation of the future reform.

Define $W = (w_1 + \dots + w_{I_C})'$ 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

$$\sum_{j=1}^{I_C} w_j^* \bar{Y}_j^k = \bar{Y}_i^k \quad \text{and} \quad \sum_{j=1}^{I_C} w_j^* Z_j = Z_i, \quad (5)$$

then

$$\hat{\tau}_{it} = Y_{it} - \sum_{j=1}^{I_C} w_j^* Y_{jt} \quad (6)$$

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 $[(\bar{Y}_1^k, Z_1), \dots, (\bar{Y}_{I_C}^k, Z_{I_C})]$. Hence, in practice, the synthetic control W^* is selected so that condition (16) holds approximately: the distance (or pseudo-distance) between the vector of pre-treatment characteristics of the treated country and the vector of the pre-treatment characteristics of the potential synthetic control is minimized with respect to W^* and according to a specified metric.⁹ The deviation from condition (16) imposed by this implementation process, however, 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., 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 country. This

⁹This distance minimization problem resembles that of covariate matching estimators in the microeconomic treatment evaluation literature, where Mahalanobis or normalized Euclidean distance are commonly used as a metric. See Abadie, Diamond, and Hainmueller (2007) for technical details.

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). Furthermore, SCMs rest on identification assumptions that are weaker than those required by estimators commonly applied in the trade and growth literature. For example, while panel models only control for confounding factors that are time invariant (fixed effect) or share a common trend (difference-in-differences), the model in equation (15) allows the effect of unobservable confounding factors to vary with time. In other words, the synthetic control approach successfully deals with the endogeneity problem caused by the presence of unobservable country heterogeneity.

Case Study Selection

We use SCMs to implement a set of comparative case studies and investigate the effect of trade liberalization on per capita income paths in transition economies. Our choice of case studies is based on data for the 29 transition economies covered by the EBRD, see Table 1.¹⁰ At the outset, we have to drop three countries in former Yugoslavia (Bosnia and Herzegovina, Montenegro, and Serbia) that are not contained in the PT data set because they were founded only recently. Furthermore, we drop Mongolia because it is not covered by the SWWW indicator.

INSERT TABLE 1 HERE

According to the SWWW indicator, of the 25 countries that remain in the sample of eligible case studies, eight never opened, which makes these countries in the baseline

¹⁰These are, in alphabetical order: Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyz Republic, Latvia, Lithuania, FYR Macedonia, Moldova, Mongolia, Montenegro, Poland, Romania, Russia, Serbia, Slovak Republic, Slovenia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan.

exercise eligible as controls but not as treated units.¹¹ Furthermore, this framework cannot be used to assess countries that opened up very early after their independence because the data to match them to their potential controls before the treatment is insufficient.¹² Finally, for long-established countries in Eastern Europe that were among the first to start the transition process, some countries (including in Central Asia) that liberalized later could in principle be used as controls. The latter group of countries did not exist in the late 1980s, however, such that they cannot be matched to the Eastern European economies before the treatment. As a result, we are not able to investigate the early transition countries in Eastern Europe, again because of the lack of available controls.¹³ This last step is dictated by the fact that, although in some specifications we use non-transition developing countries that have never opened as potential controls, we prefer to avoid relying on control pools with non-transition economies only.

Based on these considerations, we are able to perform four comparative case studies to investigate the effect of trade liberalization (Armenia 1995, Azerbaijan 1995, Georgia 1996, and Tajikistan 1996), and one counterexample to measure the impact of a “missed liberalization” (Uzbekistan). In principle, all remaining countries in the PT sample can be used to estimate the counterfactual outcomes. To be more specific, we implement two exercises for the four countries that opened up their trade regime: in a first step, the algorithm can choose any closed country contained in the PT and SWWW data sets as a control (except for countries and geographical regions that are open in the 1990s, see below).¹⁴ In the second step, we restrict the pool of potential control countries to be

¹¹Belarus, Croatia, Estonia, Kazakhstan, Russia, Turkmenistan, Ukraine, and Uzbekistan. Note that we use the carefully researched Wacziarg-Welch version of the Sachs-Warner dummy and, although tempting in several cases (Estonia), we refrain from discussing individual country classifications because this is beyond the scope of the paper.

¹²Czech Republic, Kyrgyz Republic, Latvia, Lithuania, FYR Macedonia, Slovak Republic, and Slovenia.

¹³Albania, Bulgaria, Hungary, Poland, and Romania.

¹⁴In addition to transition countries that do not liberalize, the pool of potential controls includes the following countries: *Africa*: Angola, Central African Republic, Chad, Congo, Gabon, Lesotho, Malawi, Nigeria, Rwanda, Senegal, Togo, and Zimbabwe; *Asia*: China, India, Pakistan, and Papua New Guinea; *Middle East*: Algeria, Iran, and Syria.

the set of 25 EBRD transition countries left after cleaning the sample from already open economies.

The Growth Benefits of Openness

For the countries that liberalize, we represent graphically the outcome variable (real GDP per capita) for the treated unit and its synthetic control, where the match has occurred before the treatment based on the outcome variable and the other predictor variables (Z_j) described in Section III.. Of course, the pre-treatment fit improves the longer the time span and the greater the explanatory power of predictor variables. Moreover, as shown by Abadie, Diamond, and Hainmueller (2007), the bias of the SCM estimator may be positively affected by the small number of pre-treatment periods. This is indeed a limitation of restricting the attention to transition countries. We believe that the advantages discussed above and the interest of the question more than compensate for this shortcoming. And, most of all, in the majority of the following exercises the resulting fit turns out to be extremely good, although transition economies only dispose of a few observation years before the treatment.

To evaluate the quality of the match between each treated country and the constructed synthetic controls, Table 2 compares them by explanatory variable, as well as by the average GDP in the pre-treatment period; the root mean squared prediction error is also reported.¹⁵ The pre-treatment fit can also be graphically evaluated in Figures 1 through 8 by looking at the proximity of the outcome trends of each treated country and its synthetic control before T_0 (i.e., on the left of the vertical dashed line).

¹⁵As predictors, population growth, investment share, and secondary school enrollment are included as averages to maximize the sample size, while each annual observation of the pre-treatment GDP is used as a separate predictor to improve the fit.

INSERT TABLE 2 HERE

Table 2 shows that the algorithm approximates well the characteristics of the synthetic control to those of the treated country in the cases of Georgia and Armenia, as the averages of the predictor variables—reported in the first four rows of every panel of the table—are fairly similar between each of the two countries and its synthetic control. The mean squared error is instead higher in the cases of Azerbaijan and, especially, Tajikistan, where the level of pre-treatment GDP is far below that of any potential control. As a result, the Tajikistan experiment ends up being uninformative because of a lack of pre-treatment fit.

In general, the mean squared error is obviously lower when we use the unrestricted pool of potential comparison countries (synthetic control A) than when we include only transition economies in the donor pool (synthetic control B). The unrestricted pool includes all countries that are contained in the SWWW sample and that are closed both at the time of liberalization in the country under investigation and in the 5 following years.¹⁶ In the restricted exercise, however, we make the underlying country comparisons more sensible along observable dimensions such as investment and schooling, as well as along unobservable dimensions such as cultural origins, by retaining only closed transition economies in the pool of potential controls I_C . In fact, the algorithm faces strong difficulties in selecting a synthetic control A with the same investment share and secondary school enrollment as the treated transition countries, because, worldwide, these countries have higher investment and schooling than economies at the same level of development.

INSERT FIGURES 1 THROUGH 8 HERE

It is thus reassuring that the two exercises—A and B—deliver the same results in terms of the growth pattern: results that are summarized both by the difference in the

¹⁶This implies dropping countries in two macro groups—OECD and Latin America—as in both groups, all countries are open (with the exception of Iceland). See Appendix I for details.

real GDP per capita between the treated country and the synthetic control after 5 or 10 years (see Table 2, last two rows of every panel), and by their different post-treatment GDP trends (on the right of the vertical dashed line) in Figures 1 through 8. For each country, except Uzbekistan (reverse treatment, see next section), we show two figures: the first for the synthetic control A, and the second for the synthetic control B. Three out of four treated countries fare much better than their synthetic controls in response to trade liberalization and we will describe the evidence for each country in turn.¹⁷

The positive effect of trade liberalization is clearly visible using both a broad and a constrained set of potential controls for Armenia (Figures 1 and 2; first panel of Table 2) and Georgia (Figures 3 and 4; second panel of Table 2). The per capita GDP one year before trade liberalization in the treated country (about US\$422 in Armenia and US\$489 in Georgia) is very similar to that in the synthetic control, but after 10 years it is between 44 percent (Georgia) and almost 100 percent (Armenia) higher than the one in the synthetic control economy. In both countries, the pre-treatment fit is almost perfect.

For Armenia, Togo (Uzbekistan) is the major control chosen from the unconstrained (constrained) sample of potential controls (see notes to Figures 1 and 2). Within 10 years after liberalization, Armenia succeeded in expanding per capita GDP to about double the amount prevailing in the synthetic control. Trade liberalization may have contributed to a generally more (foreign) investor-friendly business environment—which enabled to country to raise the investment-to-GDP ratio from about 19 percent during 1996–98 to 30 percent in 2007 and achieve an average growth rate of 12 percent between 2000 and 2007.¹⁸

In Georgia, trade liberalization has also contributed to a rapid increase in per capita GDP. Once economic stabilization was achieved in 1995 in the aftermath of protracted

¹⁷See Appendix II and the notes to Figures 1 through 8 for country-specific information on potential and actual controls, respectively.

¹⁸See Gelbard *et al.* (2005) for a comprehensive account of reforms in Armenia.

civil conflicts and institutions (such as the currency and the central bank; see Bakradze and Billmeier, 2007) were set up or substantially modified, the opening of the trade regime contributed to the integration of Georgia in the international community. This effect is especially visible in the late 1990s, when Georgia pulled ahead of its counterfactuals both in the unconstrained and in the constrained sample—consisting mainly of Angola/Malawi and Uzbekistan, respectively. In 2005, per capita GDP in Georgia was 44 (64) percent higher than in the synthetic control A (B).

Trade liberalization enabled Azerbaijan to catch up with and overtake the counterfactual outcome—reflecting chiefly Ukraine in both samples—where trade liberalization is absent (Figures 5 and 6; third panel of Table 2). The somewhat poorer fit in the pre-treatment period (mainly due to the rapid fall of GDP in the treated country) reflected by the high root mean square prediction error (RMSPE) in Table 2, however, cautions against drawing conclusions on the trade-growth nexus from the Azerbaijan case. The steep decrease of income in the early 1990s is partly related to an international conflict with Armenia, combined with a reduction of natural resource extraction by about 30 percent. To some extent, ceasing hostilities are responsible for the rebound in the second half of the 1990s. In our empirical framework, we are not able to distinguish this rebound effect from the trade liberalization effect. Nonetheless, the liberalized trade regime also benefited a gradual recovery of Azerbaijan’s major export, hydrocarbon resources, as extraction capacity was added and new fields in the Caspian Sea were developed slowly. The large jump in oil production, however, only took place in 2005–2006, consistent with the step GDP increase at the end of our sample period.

For Tajikistan (Figures 7 and 8; fourth panel of Table 2), the matching procedure yields unsatisfactory results, because this country experiences a too sharp drop in GDP immediately before the treatment (unrestricted exercise) or it is too poor with respect to

the comparison transition economies (restricted exercise).¹⁹ In other words, it is impossible to draw sound inference from the Tajikistan liberalization, because its pre-treatment characteristics fall outside the convex hull of the characteristics of the potential comparison countries.

The Growth Cost of Remaining Closed

In all of the constrained exercises presented above, Uzbekistan has been picked as part of the estimated counterfactual. The fact that this country is such a good match along the covariate space begs the following question: what could have happened in Uzbekistan had it liberalized together with the transition countries around it? As there are quite a few transition economies in close proximity that liberalize in 1995–96, we can use the inverse SCM procedure to assess this question. In other words, we construct a synthetic control and match it to Uzbekistan before 1996. The set of eligible countries consists of Armenia, Azerbaijan, Georgia, Kyrgyz Republic, and Tajikistan. Since the set of possible controls contains only economies that are geographically close, we only show one figure to summarize the results (Figure 9). The predictor means, the pre-treatment outcome, and the post-treatment outcomes of both Uzbekistan and its synthetic control are summarized in Table 3, together with the root mean squared prediction error.

INSERT TABLE 3 AND FIGURE 9 HERE

What do transition countries lose from not liberalizing their trade regime? For our reverse exercise, Uzbekistan, the evidence points to a missed opportunity. The estimated

¹⁹Note, however, that the classification of Tajikistan as an open economy since 1996 is based on the Wacziarg and Welch (*forthcoming*) update of the Sachs-Warner dummy (which was, originally, not available for this country). In the working paper version, Wacziarg and Welch (2003) elaborate in Appendices 1 and 3 that the classification of Tajikistan as open is a borderline call, essentially based on the fact that according to the IMF, the cotton export monopoly was abolished in 1996 while data for most of the other SWWW categories are missing.

counterfactual (a linear combination of liberalized Armenia, Kyrgyz Republic, and Tajikistan) performed substantially better than Uzbekistan in terms of per capita real GDP. One year before the hypothetical missed liberalization, per capita real GDP was equal to about US\$356 in Uzbekistan and US\$347 in its synthetic control; 10 years later, it was considerably lower in Uzbekistan (US\$442) than in the synthetic control (US\$775). In other words, according to the counterfactual evidence produced by the SCM, Uzbekistan wellbeing in 2005 would have been 75 percent higher had it decided to liberalize trade along with its neighbors. Notwithstanding the recent slight increase in per capita GDP, resource-rich Uzbekistan (cotton, energy, gold) has had limited success in converting the commodity boom to a sustained increase in income, including due to a lack of international trade liberalization.²⁰ A caveat is in order: Uzbekistan's weak economic record is, of course, not entirely due to the fact that the country lacks an open trade regime. However, controlling for the covariates allows us to capture at least some of the other policy failures to the extent they have had an impact on the covariates.

Placebo Tests

On the basis of the above results, the magnitude of the growth effect of trade liberalization (both realized and missed) in transition countries appears sizeable. SCMs, however, do not allow to assess the significance of the results using standard (large-sample) inferential techniques, as the number of units in the control pool and the number of periods covered by the sample are usually quite small in comparative case studies like ours. As suggested by Abadie, Diamond, and Hainmueller (2007), however, placebo experiments can be implemented to make inference. A first type of *cross-sectional* placebo tests consists in applying the SCM to every country in the pool of potential controls; this is meant to assess whether the estimated effect for the treated country is large relative to the effect for

²⁰See IMF (2007).

a country chosen at random. A second type of *in-time* placebo tests consists in applying the SCM to treatment years set at random. Because of our limited sample period, we can only perform the first type of placebo tests. This means repeating the SCM exercise for each potential control country.

INSERT FIGURES 10 THROUGH 12 HERE

Figures 10, 11, and 12 summarize the placebo results for Armenia, Georgia, and Uzbekistan, respectively: that is, for the three exercises pointing to a strong impact of trade liberalization on the pattern of per capita GDP. In each figure, the solid line is the difference between the outcome of the treated country and the outcome of its synthetic control (i.e., the counterpart of the results shown in Figure 2 for Armenia, Figure 4 for Georgia, and Figure 9 for Uzbekistan); the dashed lines capture the same difference for each of the fake treated countries in placebo experiments. In Figure 10 (11), the outcome difference for Armenia (Georgia) appears as the upper boundary of the other exercises (excluding an uninformative case where the pre-treatment fit is poor). Symmetrically, in Figure 12, the outcome difference for Uzbekistan appears as the lower boundary of the other exercises (excluding, again, a case with poor pre-treatment fit). This rules out the possibility that the baseline results for Armenia, Georgia, and Uzbekistan be entirely driven by chance.

V. Conclusions

In this paper, we explore the effect of trade liberalization on growth in a set of countries—transition economies—that are all “special” and “similar” at the same time: they all started from an environment of socialist economic planning and shared a dominant unifying culture, making them a rather homogeneous group of countries and an ideal environ-

ment to analyze and compare the impact of different economic policies. We investigate whether the choice of the reform program—and in particular implementing a policy geared at trade openness—has had a material impact on economic growth and wellbeing. The basic question we ask is: do open transition economies grow faster?

After showing that the impact of trade openness on growth is generally well researched, we note that large samples are not necessarily beneficial to answer this question. Based on a substantial amount of criticism in the growth literature, we move away from standard cross-country evidence, and approach the question in the context of comparative case studies. We employ an empirical strategy drawn from the treatment evaluation literature, the synthetic control approach, that compares the income pattern of a treated country with an estimated counterfactual. The particularity of this method rests in the fact that the counterfactual is a linear combination of control units that are similar to the treated economy along covariate dimensions traditionally used in the growth literature.

Given the set of EBRD transition economies, so as to enable a sufficient pre-treatment match, we need to restrict the set of comparative studies to four transition economies that liberalized their trade regime in the 1990s (Armenia, Azerbaijan, Georgia, Tajikistan) and to one counterexample that did not liberalize while many countries in the neighborhood did (Uzbekistan).

We find that trade liberalization (as represented by the updated Sachs-Warner indicator) has mostly a positive impact on economic growth in the transition economies we analyze. For Uzbekistan, we also show the opposite: missing the opportunity to liberalize can come at a substantial cost in the medium-to-long run. Our results are both quantitatively relevant (indicating that open economies have expanded their per capita GDP by as much as 44–100 percent more after 10 years compared to their synthetic controls that remained closed) and statistically robust to a set of placebo experiments and further robustness checks.

Although we believe that our findings on the positive effect of trade liberalization on growth in transition economies are very robust for the countries analyzed, we have to caution against generalizing them due to the small number of transition economies that can be analyzed using synthetic control methods. However, we take comfort in the fact that our results are very similar whether we use a broader set of potential control countries (including non-transition economies) or a smaller set that consists only of rather similar transition economies.

While we are aware that trade liberalization is likely not to be the only factor in determining the differential economic growth in transition countries, we stress that these results hold conditional on standard growth covariates—that is, the open and the closed economies are similar in many dimensions except for trade openness. We then conclude that trade liberalization appears as a key ingredient of a successful transition strategy when evaluated in terms of growth performance.

Tables and Figures

Table 1: Transition Economies and Trade Openness

<i>Country</i>	<i>Treatment Status</i>
Hungary	open since 1990
Poland	open since 1990
Bulgaria	open since 1991
Czech Republic	open since 1991
Slovak Republic	open since 1991
Slovenia	open since 1991
Albania	open since 1992
Romania	open since 1992
Latvia	open since 1993
Lithuania	open since 1993
Kyrgyz Republic	open since 1994
FYR Macedonia	open since 1994
Moldova	open since 1994
Armenia	open since 1995
Azerbaijan	open since 1995
Georgia	open since 1996
Tajikistan	open since 1996
Belarus	always closed
Croatia	always closed
Estonia	always closed
Kazakhstan	always closed
Russia	always closed
Turkmenistan	always closed
Ukraine	always closed
Uzbekistan	always closed
Bosnia & Herzegovina	not available
Mongolia	not available
Montenegro	not available
Serbia	not available

Source: Wacziarg and Welch (2003).

Table 2: Predictor and Outcome Means for the Openness Treatment

	Armenia	Synth. Control A	Synth. Control B
Secondary school	85.85	24.64	93.54
Population growth	0.66	3.02	1.87
Investment share	0.11	0.10	0.12
Pre-treatment GDP	441.63	447.06	442.51
GDP at $T_0 + 5$	538.78	480.90	411.12
GDP at $T_0 + 10$	1,028.62	534.77	514.37
RMSPE		0.01	30.50
	Georgia	Synth. Control A	Synth. Control B
Secondary school	75.85	30.04	93.10
Population growth	-0.23	2.28	1.40
Investment share	0.040	0.08	0.12
Pre-treatment GDP	583.07	583.82	587.49
GDP at $T_0 + 5$	716.57	536.07	458.29
GDP at $T_0 + 10$	1,013.02	702.39	618.34
RMSPE		5.89	31.25
	Azerbaijan	Synth. Control A	Synth. Control B
Secondary school	81.35	51.64	92.31
Population growth	1.34	1.37	0.64
Investment share	0.15	0.10	0.12
Pre-treatment GDP	833.77	843.10	846.03
GDP at $T_0 + 5$	682.54	654.40	577.27
GDP at $T_0 + 10$	1,121.43	918.59	836.62
RMSPE		50.66	64.52
	Tajikistan	Synth. Control A	Synth. Control B
Secondary school	80.93	20.41	93.75
Population growth	1.54	2.46	2.01
Investment share	0.09	0.06	0.12
Pre-treatment GDP	239.53	242.73	378.51
GDP at $T_0 + 5$	162.55	227.06	362.12
GDP at $T_0 + 10$	248.47	257.54	441.91
RMSPE		18.06	140.60

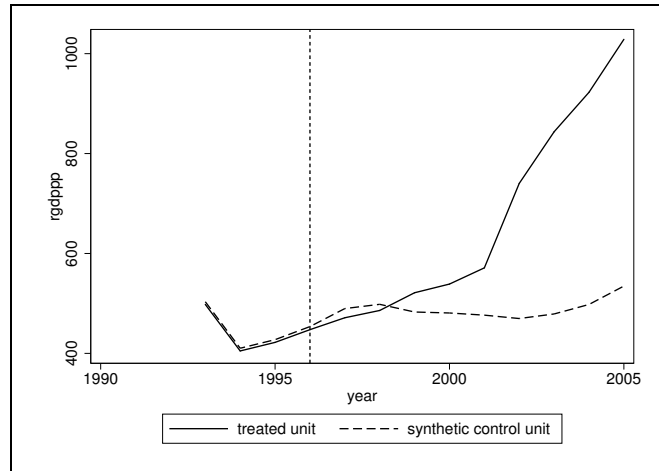
Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of predictors and outcomes. Predictors: pre-treatment real GDP per capita, secondary school enrollment, population growth, investment share. Outcome: real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years ($T_0 + 5$) and ten years ($T_0 + 10$) after the treatment year T_0 . RMSPE stands for Root Mean Squared Prediction Error. Synthetic control A is constructed from a worldwide pool of potential controls; synthetic control B is constructed from a pool of potential controls including transition economies only. See Appendix II for the list of countries in the donor pools; see the notes to Figures 1 through 8 to know the control countries included in each synthetic control.

Table 3: Predictor and Outcome Means for the Reverse Treatment

	Uzbekistan	Synthetic Control
Secondary school	93.75	84.25
Population growth	2.01	0.95
Investment share	0.12	0.10
Pre-treatment GDP	378.51	376.04
GDP at $T_0 + 5$	362.12	416.64
GDP at $T_0 + 10$	441.91	775.18
RMSPE		26.52

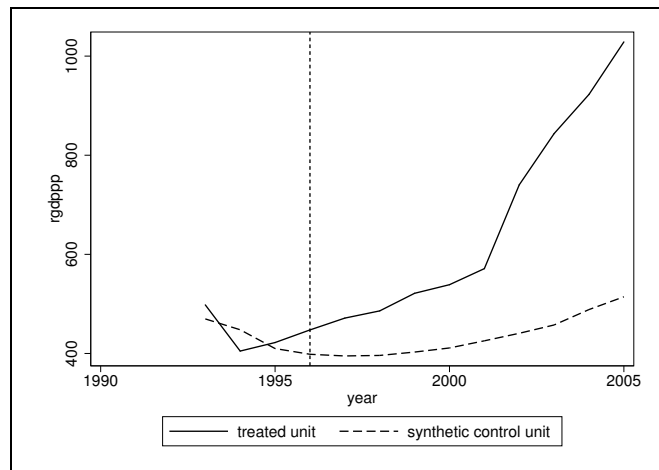
Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of predictors and outcomes. Predictors: pre-treatment real GDP per capita, secondary school enrollment, population growth, investment share. Outcome: real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years ($T_0 + 5$) and ten years ($T_0 + 10$) after the treatment year T_0 . RMSPE stands for Root Mean Squared Prediction Error. The synthetic control is constructed from a pool of transition economies that liberalized their trade regime around T_0 . See Appendix II for the list of countries in the donor pool; see the note to Figure 9 to know the control countries included in the synthetic control.

Figure 1: Trends in Real GDP Per Capita, Armenia vs. Synthetic Control - Case A



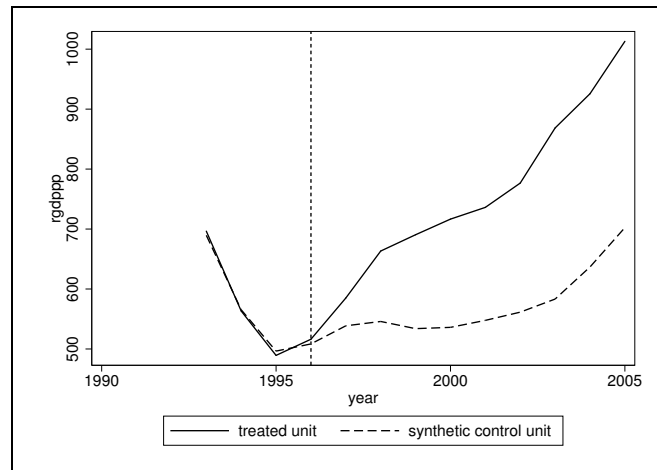
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed countries except OECD and Latin America. Larger-than-1% weights in the synthetic control: Angola (0.23), Togo (0.70). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 2: Trends in Real GDP Per Capita, Armenia vs. Synthetic Control - Case B



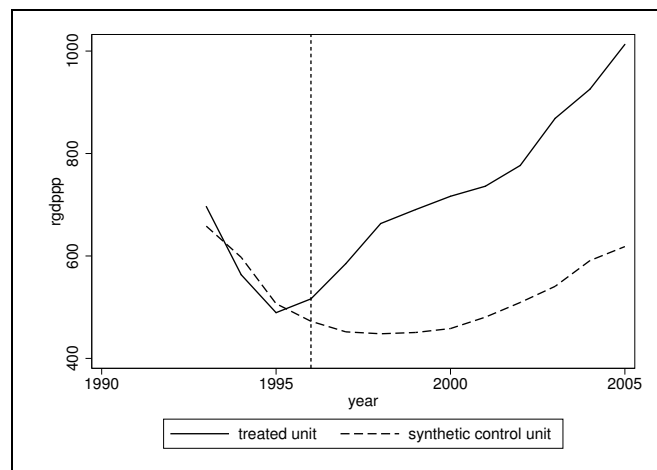
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed transition economies. Non-zero weights in the synthetic control: Croatia (0.01), Ukraine (0.05), Uzbekistan (0.94). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 3: Trends in Real GDP Per Capita, Georgia vs. Synthetic Control - Case A



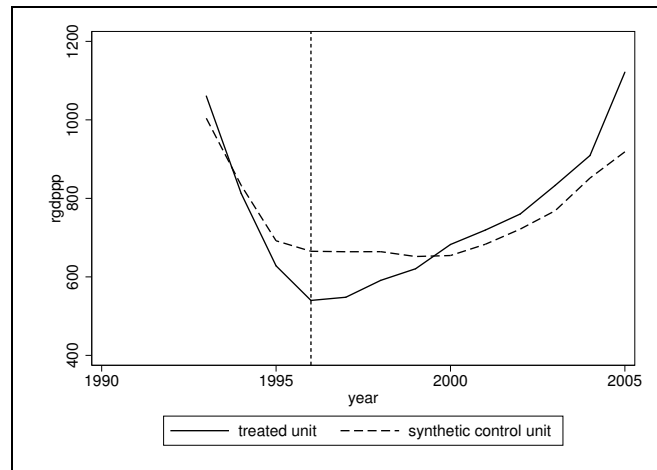
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed countries except OECD and Latin America. Non-zero weights in the synthetic control: Angola (0.38), Malawi (0.41), Ukraine (0.21). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 4: Trends in Real GDP Per Capita, Georgia vs. Synthetic Control - Case B



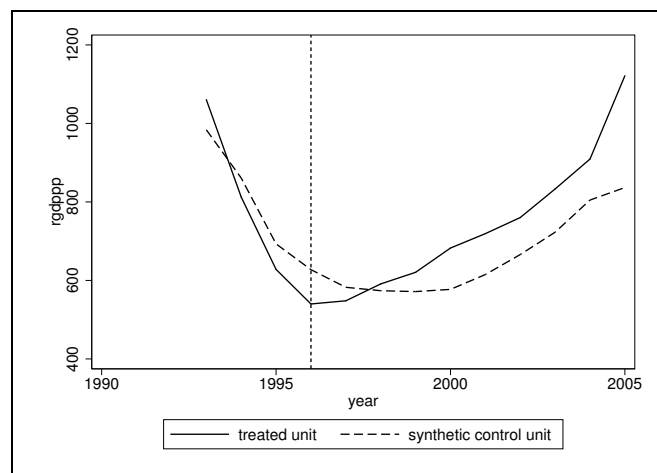
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed transition economies. Non-zero weights in the synthetic control: Ukraine (0.25), Uzbekistan (0.75). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 5: Trends in Real GDP Per Capita, Azerbaijan vs. Synthetic Control - Case A



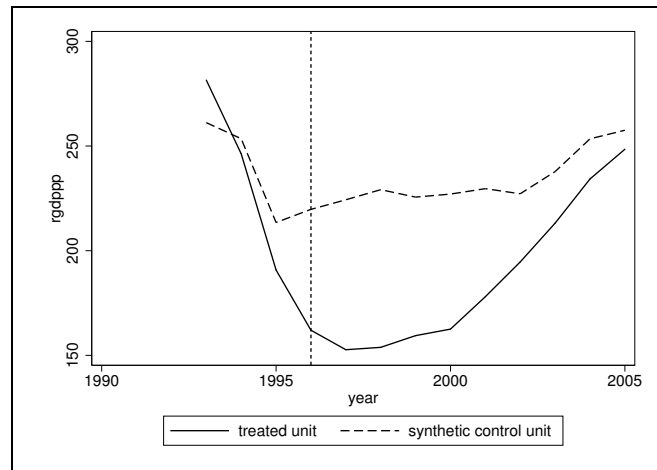
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed countries except OECD and Latin America. Non-zero weights in the synthetic control: Angola (0.32), Malawi (0.19), Ukraine (0.49). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school, population growth, investment share.

Figure 6: Trends in Real GDP Per Capita, Azerbaijan vs. Synthetic Control - Case B



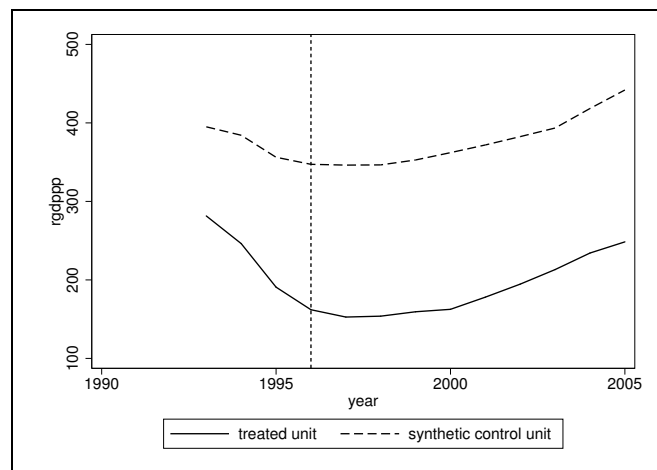
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed transition economies. Non-zero weights in the synthetic control: Ukraine (0.57), Uzbekistan (0.43). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 7: Trends in Real GDP Per Capita, Tajikistan vs. Synthetic Control - Case A



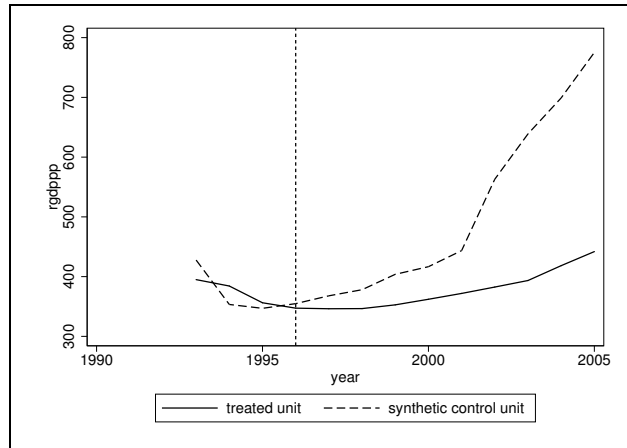
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed countries except OECD and Latin America. Non-zero weights in the synthetic control: Angola (0.01), Malawi (0.90), Ukraine (0.09). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 8: Trends in Real GDP Per Capita, Tajikistan vs. Synthetic Control - Case B



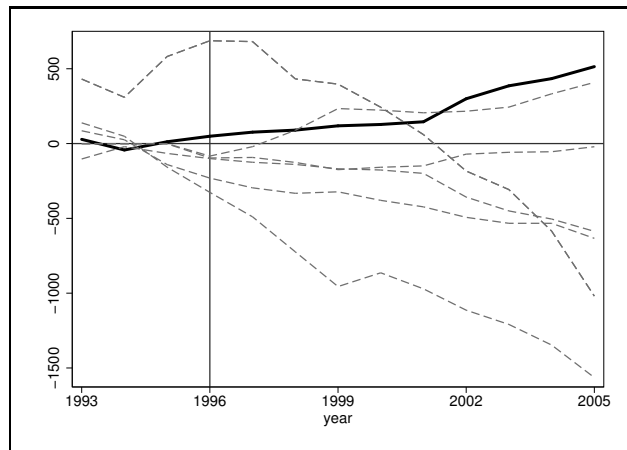
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: all closed transition economies. Non-zero weights in the synthetic control: Uzbekistan (1.00). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 9: Trends in Real GDP Per Capita, Uzbekistan vs. Synthetic Control



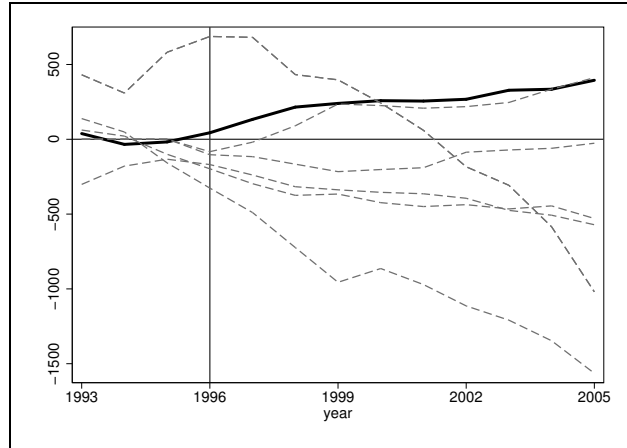
Source: authors' calculations based on data in Persson and Tabellini (2006). Potential controls: transition economies that opened trade in 1995–96, see Table 1. Non-zero weights in the synthetic control: Armenia (0.67), Kyrgyz Republic (0.01), Tajikistan (0.32). Outcome: real GDP per capita. Predictors: pre-treatment real GDP per capita (year by year), secondary school enrollment, population growth, investment share.

Figure 10: Placebo Tests for Armenia (Case B)



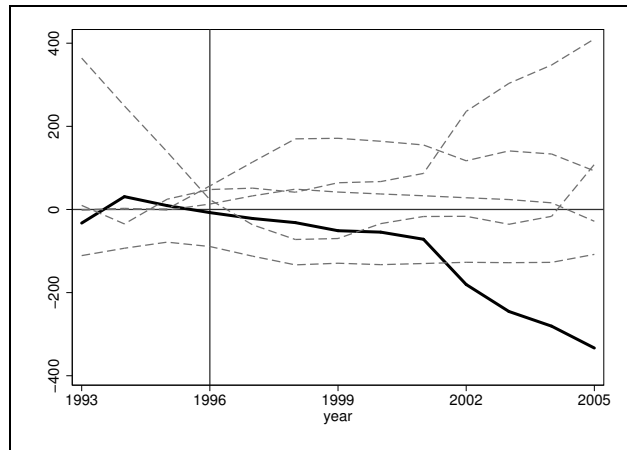
Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between Armenia and its synthetic control B. Dashed lines: outcome difference between each of the Armenia's potential control units and their synthetic control in placebo experiments. Outcome: real GDP per capita. Predictors: pre-treatment GDP (year by year), secondary school enrollment, population growth, investment share.

Figure 11: Placebo Tests for Georgia (Case B)



Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between Georgia and its synthetic control B. Dashed lines: outcome difference between each of the Georgia's control units and their synthetic control in placebo experiments. Outcome: real GDP per capita. Predictors: pre-treatment GDP (year by year), secondary school enrollment, population growth, investment share.

Figure 12: Placebo Tests for Uzbekistan (Reverse Treatment)



Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between Uzbekistan and its synthetic control. Dashed lines: outcome difference between each of the Uzbekistan's control units and their synthetic control in placebo experiments. Outcome: real GDP per capita. Predictors: pre-treatment GDP (year by year), secondary school enrollment, population growth, investment share.

Appendix I. Treated and Control Countries by Region (Year of Trade Liberalization)

AFRICA

Treated: Benin (1990), Botswana (1979), Burkina Faso (1998), Burundi (1999), Cameroon (1993), Cape Verde (1991), Ethiopia (1996), Gambia (1985), Ghana (1985), Guinea (1986), Ivory Coast (1994), Kenya (1993), Madagascar (1996), Mali (1988), Mauritius (1968), Mozambique (1995), Niger (1994), South Africa (1991), Tanzania (1995), Uganda (1988), Zambia (1993).

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

ASIA

Treated: Hong Kong (always), Indonesia (1970), Malaysia (1963), Nepal (1991), Philippines (1988), Singapore (1965), South Korea (1968), Sri Lanka (1991), Thailand (always).

Control: China, India, Pakistan (2001), Papua New Guinea.

LATIN AMERICA

Treated: Argentina (1991), Brazil (1991), Chile (1976), Colombia (1986), Costa Rica (1986), Dominican Republic (1992), Ecuador (1991), El Salvador (1989), Guatemala (1988), Guyana (1988), Honduras (1991), Jamaica (1989), Mexico (1986), Nicaragua (1991), Panama (1996), Paraguay (1989), Peru (1991), Trinidad & Tobago (1992), Uruguay (1990), Venezuela (1996).

Control: None.

MIDDLE EAST

Treated: Egypt (1995), Jordan (1965), Mauritania (1995), Morocco (1984), Tunisia (1989), Yemen (always).

Control: Algeria, Iran, Syria.

OECD

Treated: Australia (1964), Austria (1960), Belgium (1959), Canada (1952), Cyprus (1960), Denmark (1959), Finland (1960), France (1959), Germany (1959), Greece, (1959) Ireland (1966), Israel (1985), Italy (1959), Japan (1964), Luxembourg (1959), Netherlands (1959), New Zealand (1986), Norway (always), Portugal (always), Spain (1959), Sweden (1960), Switzerland (always), Turkey (1989), United Kingdom (always), United States (always).
Control: Iceland.

Appendix II. Treated Countries and Respective Donor Pools for Synthetic Controls

Georgia (1996)

Donor Pool Case A – Unrestricted: Algeria, Angola, Belarus, Central African Republic, Chad, Congo, Croatia, Estonia, Gabon, India, Iran, Kazakhstan, Lesotho, Malawi, Malta, Nigeria, Pakistan, Russia, Senegal, Swaziland, Syria, Togo, Ukraine, Uzbekistan.

Donor Pool Case B – Restricted: Belarus, Croatia, Estonia, Kazakhstan, Russia, Ukraine, Uzbekistan.

Armenia (1996)

Donor Pool Case A – Unrestricted: Algeria, Angola, Belarus, Central African Republic, Chad, Congo, Croatia, Estonia, Gabon, India, Iran, Kazakhstan, Lesotho, Malawi, Malta, Nigeria, Pakistan, Russia, Senegal, Swaziland, Syria, Togo, Ukraine, Uzbekistan.

Donor Pool Case B – Restricted: Belarus, Croatia, Estonia, Kazakhstan, Russia, Ukraine, Uzbekistan.

Azerbaijan (1996)

Donor Pool Case A – Unrestricted: Algeria, Angola, Belarus, Central African Republic, Chad, Congo, Croatia, Estonia, Gabon, India, Iran, Kazakhstan, Lesotho, Malawi, Malta, Nigeria, Pakistan, Russia, Senegal, Swaziland, Syria, Togo, Ukraine, Uzbekistan.

Donor Pool Case B – Restricted: Belarus, Croatia, Estonia, Kazakhstan, Russia, Ukraine, Uzbekistan.

Tajikistan (1996)

Donor Pool Case A – Unrestricted: Algeria, Angola, Belarus, Central African Republic, Chad, Congo, Croatia, Estonia, Gabon, India, Iran, Kazakhstan, Lesotho, Malawi, Malta, Nigeria, Pakistan, Russia, Senegal, Swaziland, Syria, Togo, Ukraine, Uzbekistan.

Donor Pool Case B – Restricted: Belarus, Croatia, Estonia, Kazakhstan, Russia, Ukraine, Uzbekistan.

Uzbekistan (Reverse Treatment – Always closed)

Donor Pool: Armenia, Azerbaijan, Georgia, Kyrgyz Republic, Tajikistan.

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