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Yes You Can, Can't You? A Statistical Comparison of Economic Liberalizations around the World*

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Abstract

We use a transparent statistical methodology for data-driven case studies—synthetic control methods—to investigate the impact of economic liberalization episodes on the pattern of real per capita GDP in a worldwide sample of countries. Economic liberalizations are measured by a widely used indicator that captures the scope of the market in the economy, mainly in terms of openness to international trade. The applied methodology compares the post-liberalization growth of treated (open) economies with the growth of a convex combination of similar but untreated (closed) economies, controlling for time-varying unobservables. We find that opening up the economy had a positive effect in most regions that we can analyze in our framework, but we note that more recent liberalizations (after 1989), mainly in Africa, had no significant impact on growth, indicating either an "early bird" gain from globalization or the lack of complementary policies in some countries.

JEL codes: C21, C23, F43, O57.
Keywords: economic liberalization, trade openness, growth, synthetic control methods.

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1 Introduction

Theoretical results in international economics and growth theory largely point to a positive relationship between trade liberalization and economic welfare, but confirming these results empirically has proven to be a Sisyphus job. A major complication in identifying the relationship between economic liberalization and growth lies in the well-known statistical trade-off between internal and external validity: Many empirical results in the literature draw on evidence from worldwide samples—sometimes of more than 180 countries—but lack internal validity, as the standard cross-country estimators are often biased for a variety of reasons. The alternative suggested by Bhagwati and Srinivasan (2001)—country-specific case studies—usually lacks statistical rigor and is exposed to discretionary case selection. Furthermore, conclusions ensuing from this type of exercise cannot be easily generalized to a broader set of countries; in other words, they fall short in terms of external validity.

Building on the—in our view correct—observation that internal validity needs to be the overarching goal of empirical research, we expand on Bhagwati and Srinivasan's suggestion and offer a set of empirical country case studies on the effect of trade liberalization on economic growth. At the same time, we provide a unified statistical framework to compare the growth performance of open and closed economies. We cover as large a sample as possible while respecting the constraint of internal validity as defined in the proposed framework. In particular, we apply a recent econometric technique, the synthetic control methods, to perform data-driven comparative case studies that can be seen as a "third way" between standard cross-country estimators and the hardly generalizable analysis of individual country episodes.

We use a worldwide panel of economies over the period 1963–2005 and evaluate the effect of a binary indicator of trade openness or economic liberalization—derived by Sachs and Warner (1995), extended, updated, and revised by Wacziarg and Welch (2003, 2008)—on the outcome, changes in per capita income.[1] We ask whether opening the economy in year T leads to higher growth performance in the years $T + i$ (with $i \in [1, 10]$) compared to similar countries that have not opened up. The advantage of our approach lies in the transparent estimation of the counter-

[1] As Giavazzi and Tabellini (2005) point out, this binary treatment arguably represents more than pure opening to international trade. Hence, we speak of economic liberalization in what follows.

factual outcome of the treated country, namely a linear combination of untreated countries—the synthetic control. The comparison economies that form the synthetic control unit are selected based on their similarity to the treated economy before the treatment, both with respect to relevant covariates and past realizations of the outcome.

We study all episodes of economic liberalization that took place in the world since 1963 as long as they qualify for our empirical framework. For technical reasons, 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 in the same region that did not liberalize before or soon thereafter. This feature distinguishes our study from the standard cross-sectional work in an important way, as we pay particular attention to the question whether there is enough variation in the treatment in a given region (that is, whether treated and comparison units share a *common support*). This transparent sample selection procedure indicates that for some regions we are skating on thin ice to identify the effect of openness on growth, because liberalization waves reduce the number of available comparison countries after a given year. Indeed, as shown by Billmeier and Nannicini (2009), the failure of standard cross-sectional estimators to control for the existence of such a common support leads to quite far-fetched country comparisons underlying common estimation results. Forcing us to focus on a smaller set of liberalization episodes is thus a first advantage—instead of a limitation—of the synthetic control methodology.

A second major advantage of the proposed statistical framework is that, unlike most of the standard estimators exploiting some degree of cross-sectional variation, it can deal with endogeneity issues by accounting for the presence of *time-varying* unobservable confounders.

Our empirical results show that, for many countries that we can analyze, economic liberalization had a positive effect on growth. However, we find a lot of heterogeneity in the results across regions and time. In particular, countries that liberalized their trade regime after 1989, many of which are located in Africa, did not benefit from these reforms in terms of higher GDP per capita compared to similar, but closed economies. We offer two explanations for this phenomenon: (a) a timing effect of trade openness with respect to subsequent liberalization waves—we call it the "early bird" gain from globalization—and (b) the (lack of) beneficial interaction with other

growth-enhancing fundamentals, such as institutional quality (Acemoglu *et al.*, 2001, 2003) or complementary physical and human capital investment (Rodrik, 1999), especially in Africa.

The remainder of this paper is structured as follows. In Section II, we briefly review the relevant literature. In Section III, we present the data sources and variables of interest. The synthetic control methodology is discussed in Section IV. Section V empirically explores the effect of economic liberalization on growth in a worldwide sample. Section VI concludes.

2 Related Literature

While a large body of broadly supportive theoretical literature exists, providing conclusive empirical evidence on the intuitively positive causal effect of trade on growth has been a rather challenging endeavor and hotly debated topic, complicated by a multiplicity of factors.[2] In an overview article of the empirical literature, Winters (2004) argues that notwithstanding methodological challenges and doubts about the strength of the evidence, the most plausible conclusion points to a positive effect of trade liberalization on growth, both directly and indirectly.[3]

On the plainly affirmative side of the argument, Dollar (1992) finds that countries with an inward-oriented trade regime—as reflected by relatively high price and protection levels, and real overvaluation of the currency—could increase their growth rates by 1.5–2 percentage points with a shift to more outward-oriented trade policies. Sachs and Warner (1995) provide evidence that, in an augmented Barro-type growth regression, being "open" is correlated with growth convergence among countries, and that "open economies grow, on average, by 2.45 percentage points more than closed economies, with a highly statistically significant effect" (p. 47). Edwards (1992, 1998) finds that for eight very different measures of openness, the impact on TFP growth is positive and significant in 13 out of 18 estimates, noting though that the effect of other covariates is often larger. Presenting historical evidence, Vamvakidis (2002) finds that a positive correlation between openness and growth is only present in the data starting from 1970.

[2]See Ventura (2005) for an overview article on theoretical growth models that differ on their assumptions about trade frictions. Eaton and Kortum (2002) parametrize a Ricardian trade model and show, among other things, that a reduction of trade barriers that corresponds to a doubling of world trade would lead to an unequivocal welfare gain in all 19 OECD countries contained in the sample.

[3]See also Kim and Kim (2000), who argue that another channel of transmission of gains from trade into higher income occurs via education, by allowing more efficient specialization in the most productive traded sector.

Rodriguez and Rodrik (2001), on the other hand, cast doubt on the robustness of these affirmative results. They point out that much of the literature during the 1990s documenting a positive effect suffers from various weaknesses, related especially to the openness measure and the econometric modeling approach, which they view as suffering from regressor endogeneity as it is often based on OLS estimates. More specifically, they argue that the commonly employed trade openness indicator, developed in Sachs and Warner (1995), is subject to a number of shortcomings—among the most notable ones that (i) the indicator captures much more than just openness to trade and should be interpreted accordingly, and that (ii) the positive correlations found in Dollar (1992), Sachs and Warner (1995), and Edwards (1998) are not robust.[4] DeJong and Ripoll (2006) follow up on one of the suggestions voiced in Rodriguez and Rodrik (2001) and develop an alternative measure of trade barriers—*ad valorem* tariff rates. In a sample of 60 countries, they find that the correlation between trade barriers and income is negative for rich countries but positive in poorer countries. Furthermore, 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.

Using an empirical strategy based on non-parametric matching estimators, Billmeier and Nannicini (2009) broadly confirm the overall positive effect of trade on growth, but show yet another potential pitfall of common cross-sectional estimators: if the treatment (openness to trade) is not evenly distributed over the sample—that is, almost all countries in a given covariate cell are either open or closed, lacking "common support"—cross-sectional estimators can lead to quite far-fetched (implicit) country comparisons.

Bhagwati and Srinivasan (2002) question cross-country evidence on the trade-growth nexus on a more fundamental level, and promote descriptive case studies as a way to avoid the pitfalls of standard cross-country evidence. They 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

[4]More recent contributions in the political economy literature—such as Giavazzi and Tabellini (2005) and Persson and Tabellini (2006), see further below—interpret the Sachs-Warner dummy more appropriately as a broader indicator of economic liberalization, no longer of trade openness alone. See Section 3 for a more detailed discussion of the components of the indicator.

tries hard enough!" (p. 181). On similar ground, Pritchett (2000) also argues for detailed case studies of particular countries and growth events.

This support for comparative case studies can be interpreted as a first attempt to sidestep the weaknesses of standard cross-country regressions, but of course the adoption of estimators explicitly devised to overcome some of these limitations has been the main alternative solution. Broadly speaking, two main econometric strategies have been used in this respect: Instrumental Variables (IV) and panel methods.

First, to control for the endogeneity issues in the early literature, a number of contributions have turned to IV as a remedy. Using a gravity model, Frankel and Romer (1999) instrument for trade shares in GDP with geographic characteristics and show that the positive effect of trade on income is underestimated when using OLS estimators. Quantitatively, they find that a one-percent increase in the (instrumented) trade share in GDP raises income per capita by between two and three percent depending on the sample, doubling and tripling the respective OLS coefficient. In another application of a gravity model, Irwin and Terviö (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. Their results confirm those of Frankel and Romer (1999)—that the 2SLS estimate significantly exceeds the OLS estimate—for the whole 20th century: in their results, by a factor of 2.6 on average. Another recent example for the IV approach is Romalis (2007), who instruments the openness measure for developing countries with tariff barriers by the United States. He finds that eliminating existing tariffs in the developed world would increase developing countries' annual GDP growth rates by 0.6 to 1.6 percentage points. All of these instruments, however, apply to quantitative trade volume measures, but not to measures of the policy stance and are therefore less relevant in the present context. Furthermore, geographical instruments might have direct effects on growth, thereby violating the exclusion restriction and biasing the IV estimates.

Second, the possibility to combine the analysis of time series with cross-sectional information has spurred another strand in the trade-and-growth literature that employs panel methods to control 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 compared to standard cross-country regressions. Wacziarg and Welch (2003, 2008) further the discussion in three directions: they update, expand, and correct the economic liberalization indicator by 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 setup; 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. According to their results, after liberalizing, countries on average grow faster by about 1.5 percentage points. Another typical panel approach—the difference-in-differences estimator—is used by Slaughter (2001) to infer the effect of four very specific trade liberalization events on income growth dispersion; he 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 note that this effect may not be entirely attributed to international trade, as liberalizations tend to be accompanied by other policy improvements. According to the evidence they present, economic liberalizations speed up growth by about one percentage point and raise the share of investment by almost two percentage points of GDP.

In this paper, we build a bridge between the case-study and the econometric responses to the weaknesses of standard cross-country estimators by employing synthetic control methods: A recent methodology that builds data-drive comparative case studies within a unified statistical framework and accounts for endogeneity from time-varying unobservable confounding factors.

3 The Data

To anchor our results in the existing literature, we draw on a dataset used recently by Giavazzi and Tabellini (2005) and Persson and Tabellini (2006). The data cover about 180 countries over the period 1963–2000. As a measure of economic liberalization, we use the binary indicator by Sachs and Warner (1995), extended, updated, and revised by Wacziarg and Welch (2003, 2008); short SWWW. 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. When applying synthetic control methods in a panel setup, in line with Giavazzi and Tabellini (2005), we refer to the "treatment" as the event of economic liberalization, after experiencing a closed economy in the preceding years 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.

We are interested in estimating the effect of economic liberalization on an outcome variable reflecting economic wellbeing. For the latter, we use the time series of real GDP per capita (measured in 2002 US$), because we focus on the dynamic impact of trade openness over time, not its one-off effects on the individual income level. The series comes from the IMF *World Economic Outlook* database, as this allows us to extend, in a consistent way, the post-treatment period for the outcome variable up to 2005 when necessary (i.e., when liberalizations take place very late in the original data set). Finally, from the original dataset, we use as control variables annual observations on: GDP per capita before the treatment; investment as a share of GDP; population growth; secondary school enrollment; the average inflation rate; and a democracy dummy. We use these variables as predictors only when they are available for at least one year in the pre-treatment period, which is not always the case for inflation and democracy.

4 Methodology: Synthetic Control Methods

An 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 the regime change (economic 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.

In this context, it is useful to reason in terms of potential outcomes in a panel setup. Assume that we observe a panel of I_C+1 countries over T periods. Only country i receives the treatment (that is, liberalizes its economy) 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:

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

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

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

$$Y_{jt}(0) = \delta_t + \tau_{jt} + \nu_{jt} \tag{3}$$

$$\nu_{jt} = Z_j\theta_t + \lambda_t\mu_j + \epsilon_{jt}, \tag{4}$$

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.

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

$$\sum_{j=1}^{I_C} w_j^* \bar{Y}_j^k = \bar{Y}_i^k \qquad \texttt{and} \qquad \sum_{j=1}^{I_C} w_j^* Z_j = Z_i, \tag{5}$$

then

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

is an unbiased estimator of τ_{it}. Condition (5) 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 (5) 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. 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 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, SCM rest on identification assumptions that are weaker than those required by estimators commonly applied in the trade-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 specified above 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 (time-varying) unobservable country heterogeneity.

The only limitation of SCM is that they do not allow to assess the significance of the results using standard (large-sample) inferential techniques, because the number of observations 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. Following their approach, we implement *cross-sectional* placebo tests; that is, we sequentially apply the synthetic control algorithm to every country in the pool of potential controls and compare these placebo results with the baseline estimates. This is meant to assess whether the estimated effect for the treated country is large relative to the effects for countries chosen at random.

5 Case Study Selection

Using SCM to implement a set of comparative case studies and investigate the effect of economic liberalization on per capita income paths in eligible economies around the world requires, as

a preliminary step, the identification of a pool of feasible experiments, that is, liberalization episodes that meet the following conditions: (i) the treated country liberalized at the earliest in 1965, as we require a few pre-liberalization observations to calibrate the synthetic control; and (ii) there exists a sufficient set of countries in the same region that remain closed for 10 years past the liberalization episode (or until the end of the sample in 2005) to effectively provide a pool of potential comparison economies that are "similar." To account for this similarity, which includes factors such as cultural proximity (but also stage of economic development in a broad sense), we group the countries by "geographic" region: OECD, Africa, Asia, Latin America, and the Middle East. Given the above requirements, we are not able to analyze the OECD countries, because the pool of potential comparison economies within the region is essentially empty and countries from other regions do not match up well in terms of GDP per capita.

Tables 1 through 4 provide the full picture of liberalization episodes in the remaining regions. As we can see for instance in Table 2, sweeping "waves of liberalization" are bad for our approach: We are quickly running out of potential control countries in Latin America as the trend toward greater economic liberalization essentially eliminates the control group by the end of the 1980s.

Based on the liberalization sequences shown in the above tables, we are able to perform 5 comparative case studies in Asia, 5 in Latin America, 4 in the Middle East and North Africa, and 16 in Africa, for a total of 30 experiments.[5] We choose the pool of potential comparison countries so as to perform two different types of synthetic control experiment. First, we allow the SCM algorithm to pick any eligible economy in the same region of the liberalizing country as a control (experiment A). Second, we increase the number of potential controls including eligible economies from the other developing regions (experiment B). Our final sample is therefore made up of 127 (treated and comparison) countries. For each of them, we observe the (non-missing) time series of per capita GDP from 1963 to 2005.

[5]Specifically, we end up with the following eligible treated countries by region (year of economic liberalization in parentheses). Asia: Singapore (1965), South Korea (1968), Indonesia (1970), Philippines (1988), Nepal (1991). Latin America: Barbados (1966), Chile (1976), Colombia (1976), Costa Rica (1986), Mexico (1986). Africa: Mauritius (1968), Botswana (1979), Gambia (1985), Ghana (1985), Guinea (1986), Guinea-Bissau (1987), Mali (1988), Uganda (1988), Benin (1990), South Africa (1991), Capo Verde (1991), Zambia (1993), Cameroon (1993), Kenya (1993), Ivory Coast (1994), Niger (1994). Middle East and North Africa: Morocco (1984), Tunisia (1989), Mauritania (1995), Egypt (1995).

6 Empirical Results: Economic Liberalizations Around the World

In this section, we present and discuss the implemented experiments. In a first step, we reflect on the results by region, highlighting specific countries of interest. We report the results both numerically (Tables 5 through 10) and graphically (Figures 1 through 6). The tables provide the numerical comparison by explanatory variable between each treated country and the constructed synthetic control. Synthetic control A refers to the estimated counterfactual composed of a pool of countries in the same region, synthetic control B to a worldwide donor pool. The overall pre-treatment fit is measured by the root mean square prediction error (RMSPE). Of course, the pre-treatment fit improves the longer the time span and the greater the explanatory power of predictor variables. The comparisons between the post-treatment outcome of the treated unit and the synthetic control after five (GDP at $T_0 + 5$) and ten years (GDP at $T_0 + 10$) provide estimates of the (dynamic) treatment effect.

The figures, instead, represent graphically the time series of the outcome variable (real GDP per capita) for the treated unit and the synthetic control unit, both in the pre-treatment period and for ten years after the treatment year (T_0). Note that, in the tables, we report the estimation results of all the experiments, but—to contain space—we only show the figure of one experiment per country. Our formal criterion to decide which experiment to present is as follows: If the RMSPE for experiment A is smaller than 40 or smaller than experiment B, we show the evidence under experiment A, otherwise we use the alternative control sample, that is, experiment B.[6]

In a second step, we further investigate several liberalization episodes by means of placebo tests to check the robustness of our baseline results. The placebo experiments are contained in Figures 7 and 8 and discussed in the context of the country-specific results. We have chosen to provide this robustness analysis for countries where the primary evidence points to a significant impact of economic liberalization. In some examples, the placebo tests in fact confirm and reinforce the evidence, whereas in other cases the placebo analysis reveals that the detected effect is rather coincidental.

[6]While the criterion is, of course, somewhat arbitrary, the figures for the alternative experiment in each country are available upon request from the authors. And they are broadly similar to the ones displayed in the next section, as it can be also seen from the (complete) estimation results reported in Tables 5 through 10.

We conclude with a discussion on the interpretation of the cross-country evidence provided by our worldwide synthetic control experiments. In particular, we ask why the positive growth effect associated with economic liberalization appears to dissipate toward the end of the 1980s.

6.1 Asia

The results for Asia are graphically represented in Figure 1 (SCM) and Figure 7 (placebo robustness check). Indonesia (treatment in 1970) is a prime example of economic liberalization gone well. The average income over the years before liberalization is literally identical to that of the synthetic control, which consists of Bangladesh (41 percent), India (23 percent), Nepal (23 percent), and Papua New Guinea (13 percent).[7] After the economic liberalization in 1970, however, Indonesian GDP per capita takes off and is 40 percent higher than the estimated counterfactual after only five years and 76 percent higher after ten years (see Table 5). The results for Indonesia are also strongly robust to placebo testing.

For the other four liberalization episodes in Asia, the intra-regional match is not good enough (see the RMSPE's in Table 5). Hence, we enlarge the pool of potential control economies to the worldwide sample of closed economies. This step helps to regain comparable GDP levels. Figure 5 shows that South Korea (1968) is a success story similar to Indonesia with income about twice as high as in the counterfactual case after 10 years. For Singapore (1965), we are tempted to argue that liberalization also worked, notwithstanding the fact that the counterfactual immediately after liberalization is performing better than Singapore. Note especially that GDP per capita in the synthetic control (a convex combination of Algeria and Mexico) continues to grow rather linearly, whereas in Singapore the path of GDP steepens drastically between 1965 and 1967. For both countries, the placebo test underscores the validity of the SCM results.

On the other hand, the later liberalization episodes—Philippines (1988) and Nepal (1991)—did either not lead to a significantly better trajectory than in the estimated counterfactual (Philippines), or it is not clear to what extent the 30-percent income difference after 10 years in favor of the liberalized economy (Nepal) is attributable to the economic liberalization as the

[7] See the Appendix for a complete list of the comparison countries included in each constructed synthetic control (with their relative weights).

steep income increase already starts a couple of years before the liberalization date according to the SWWW indicator. As a matter of fact, although the placebo test for Nepal is clearly less robust than those for Indonesia, Singapore, and South Korea, it should be noted that only 3 out of 20 placebo units showed a higher treatment effect than Nepal; it is only because of a scale effect (these three economies being much richer than Nepal) that this fact overshadows the other (robust) 17 placebo experiments in Figure 7.

6.2 Latin America

Graphical evidence on the liberalization episodes in Latin America are shown in Figure 2 (SCM results) and Figure 7 (placebo testing). In this region, economic liberalization episodes that can be analyzed in our framework took place rather early, that is, between 1966 (Barbados) and 1986 (Costa Rica). In all countries, the regional synthetic controls (experiment A) provide a fine match: On average, over the years before liberalization, the income of the synthetic control is less than two percent off that of the liberalizing country (see Table 6); the only exception is represented by Chile, where the pre-liberalization drop in GDP (coinciding with the Pinochet coup) makes it difficult for the SCM algorithm to find a suitable counterfactual.

Barbados is another excellent example of a clearly positive and robust impact of economic liberalization (see both Figure 2 and Table 6). Ten years after liberalization, GDP per capita in Barbados is about 57 percent higher than that of the synthetic control, which consists of Venezuela, Trinidad and Tobago, and Mexico.[8] Similarly, a positive impact from economic liberalization appears in Colombia, Costa Rica, and Mexico. The placebo tests confirm that the SCM results are largely robust for these four countries.

In Chile, the story appears to be somewhat more complicated, driven by the events around the Pinochet coup (again, see both Figure 2 and Table 6). Between September 1973—the time of the coup d'état that ended Allende's government and the democratic regime—and the introduction of liberal reforms in 1975–76, real income dropped substantially in an environment of high inflation.[9]

[8]Note that the weights of the countries forming the synthetic control reported in the Appendix do not always sum up to one, because we only report countries with a weight greater than 2%.

[9]The 12-month CPI inflation was above 100 percent between September 1972 and May 1977, peaking at 740 percent in April 1974.

Nevertheless, it could be argued that the economic policy measures taken in 1975–76, including economic liberalization, clearly turned income growth around and put it on the same track as in the estimated synthetic control (which mainly consists of Uruguay).

6.3 Africa

The 16 economic liberalization episodes that we can analyze in Africa under the SCM framework occured between 1968 (Mauritius) and 1994 (Ivory Coast and Niger). See Figures 3 through 5 for the SCM evidence, and Figure 8 for selected placebo tests. From the analysis of the African subsample, we draw three conclusions.

First, we note that there appears to be only very limited evidence of successful economic liberalization in Africa. Out of 16 episodes, only two economies—Mauritius and Botswana—are truly convincing success stories.[10] In particular, Botswana fared substantially better over the ten years after liberalization (1979) than the synthetic control in experiment B (see Figure 3).[11] As a matter of fact, the income per capita in Botswana is about five times as high as the one in the synthetic control ten years after liberalizing the economy. This preliminary conclusion is reinforced by the placebo test: The bold line is higher than any other permutations. In a number of other countries, economic liberalization has contributed to fast rebounds in income levels (e.g., Benin, Ghana, Ivory Coast, Mali, or Uganda) without marking the beginning of a lasting divergence from the estimated counterfactual. The placebo tests, however, indicate that these rebounds are not particularly robust.

Second, the positive evidence is concentrated in the early part of the sample before, say, 1990. In addition to Mauritius and Botswana, less striking examples of a positive liberalization impact are Ghana (1985) and Guinea (1986), where liberalization is associated with an at least temporary income boost. Gambia (1985) is somewhat difficult to match as GDP is extremely volatile during the period leading up to liberalization, but income is more than 20 percent higher than in the counterfactual situation ten years after liberalization.

[10] See Acemoglu *et al.* (2003) for a detailed assessment of Botswana.

[11] The largely positive impact of Botswana liberalization is also confirmed by experiment A (see Table 7).

Third, the effect of economic liberalization has little to no discernible positive effect after 1989. Except for some economic rebounds (see above), the liberalization stops a decline in income (e.g., Cameroon, Niger) while the counterfactual performs better during the post-liberalization period, or has no apparent impact on income levels, which remain stable (Kenya, South Africa). In Zambia, even after economic liberalization in 1993, income levels continued to decrease on a per capita basis. One somewhat positive example of a late liberalization is Capo Verde (1991), with GDP per capita about 30 percent higher than in the synthetic control after ten years. Note however that the divergence in favor of Capo Verde is very slow to emerge.

Summing up, also in sub-saharan Africa, it seems to broadly be the case that only early liberalizations had a positive impact on growth, while almost all of the late attempts did not benefit the liberalizing country much.

6.4 Middle East and North Africa

The results for the Middle East and North Africa (MENA) region are graphically summarized in Figure 6 (SCM results) and Figure 8 (placebo testing). In this region, the results are far from conclusive. In all countries, the difference in GDP per capita between the liberalizing economy and the synthetic control constrained to the region is quite small at the time of treatment. After ten years, only in Morocco (1984), liberalization has contributed to a somewhat higher income level than in the synthetic control. The positive effect is, however, not robust to placebo testing. In the three other countries where liberalization took place in 1989 or later (Tunisia, 1989; Mauritania, 1995; Egypt, 1995), the liberalizing country actually fares worse than the regional synthetic control both five and ten years after liberalization.

6.5 What Changed in the 1990s?

The analysis in the preceding sections indicates that liberalization is, in some countries, associated with a remarkable positive growth effect. However, we find a lot of heterogeneity in the results across regions and time. In particular, we note that countries that liberalized their economy after about 1990, many of which are located in Africa, did not benefit from these reforms in terms of higher GDP per capita compared to similar, but closed economies. Why is it?

One explanation for this phenomenon could reside in a timing effect of economic liberalization—we call it the "early bird" gain from openness. This is a different, but maybe related, timing effect to the one found by Giavazzi and Tabellini (2005), who document that countries that liberalize the economy before becoming democracies do substantially better than those that followed the opposite track. We argue that early economic liberalization in and of itself is better than late economic liberalization. In our view, the "early bird" effect could work as follows: If a developing economy liberalizes early on, it can still reap the gains of specialization (e.g., outsourcing from the developed world), while once "globalization" kicks in (and an increasing number of countries liberalize their economic system), there is much more competition for capital, but also more competition for the labor-intensive goods a developing economy can specialize in, e.g., agriculture or textiles.[12] Hence, the benefits from liberalization after globalization are smaller.

From a theoretical perspective, this effect is not visible in a simple two-country model where countries do not compete for capital and export demand from a third country. In a multilateral model, however, the timing of liberalization becomes crucial, especially if the specialization paths are not complementary (see Balassa, 1979). In our example above, when other (developing) countries that produce agricultural goods liberalize, a given economy needs to move up the specialization ladder to continue to benefit from its early integration in the world economy. The SCM evidence presented above is consistent with the history of economic liberalization in the developing world: Asian economies liberalized early on. For some time, that enabled them to benefit from their comparative advantage in labor-intensive goods. Once Latin American countries also started to liberalize, the Asian economies started shifting their comparative advantage to more capital-intensive production and higher-value exports (Weiss, 2005; see also Bhagwati, 2002). Finally, once the liberalization wave hit Africa, the benefits from joining the club of liberalized economies had become smaller, and continued to do so over time, as other countries that had liberalized somewhat earlier did not move up the ladder.

[12] We sidestep a full-blown discussion of the globalization phenomenon here as this goes beyond the scope of this paper. We note however, that a narrow measure of globalization, e.g., trade flows, would not capture more recent aspects of globalization that accelerated in the late 1980s–early 1990s, such as the increasing access to and use of telecommunications, and the internet. Almost by definition, there is no good measure for globalization over long periods of time as some of the very factors that shape globalization only become available over time. See Bhandari and Heshmati (2005) for an attempt to quantify globalization over the (short) period 1995–2001.

This "early bird" effect, however, is not the only possible interpretation of our empirical results. A second explanation—which again we cannot analyze in greater detail due to space constraints—is that economic liberalizations that occurred later lacked the beneficial interaction with other growth-enhancing fundamentals, such as institutional quality. While the political economy literature—see for example Acemoglu *et al.* (2001)—argues that good institutions will lead to good policies, which, in turn, will cause good outcomes, we show here that good policies do not always work. This constitutes, however, not necessarily a contradiction as some good policies (e.g., trade liberalization) could still lack the positive interaction with other favorable and complementary policies (such as investment in human capital, property rights protection, etc.) because of low-quality institutions.

Rodrik (2007) makes a similar point: The "new conventional wisdom" on globalization points to a range of institutional complements in developed and developing economies to deliver the benefits of globalization and remain sustainable by consolidating progress made so far and garnering further support in the public eye. In particular, this would entail reforms to the social safety nets (to ease adjustment and enable redistribution of globalization benefits) and, in developing economies, more basic institutional reforms including anti-corruption, labor, and financial markets. Furthermore, Sutton (2007) shows in a model drawn from the IO literature that gains from trade liberalization can be unevenly distributed across countries, with countries with intermediate income/capability levels suffering in the short run, but also benefiting over the medium run from the "moving window" by trading up in quality products as capabilities are transferred.

The policy conclusion from our assessment is not particularly upbeat: Yes, you still can liberalize your economy, but liberalization is by no means a sufficient condition for superior economic performance. In particular, timing and/or complementary ingredients seem important. Notwithstanding this broad conclusion, economic liberalization might still turn out beneficial if (i) the economy has a large potential of untapped (natural) resources (e.g., Ghana), or (ii) the policymakers are able to combine and complement economic liberalization with other growth-enhancing factors: healthy institutions *à la* Acemoglu *et al.* (e.g., Botswana), or investment in physical and human capital such as health care and education (see Rodrik, 1999, 2005).

7 Conclusions

In this paper, we explore the effect of economic liberalization on income by asking whether a policy geared at trade openness has a material impact on GDP per capita. We investigate this question in as large a set of countries as admissible given our econometric strategy. The basic question we ask is: Do economies that have experienced economic liberalization grow faster than those that have not? Our estimator, drawn from the treatment evaluation literature, establishes a middle ground between large-sample cross-country studies that potentially lack internal validity (e.g., due to a lack of common support) and descriptive case studies that lack external validity (i.e., cannot be generalized). This methodology (synthetic control methods) compares a treated (liberalized) country with an estimated (closed) counterfactual. The particularity of this method rests in the fact that the counterfactual is a linear combination of all potential comparison units, which are similar to the treated economy along covariates traditionally used in the literature.

Starting from a worldwide sample of countries, we devise a case study selection strategy that first focuses on liberalized economies with a sufficient pool of regional comparison countries that have not liberalized. For some countries, especially in Asia, we broaden the pool of eligible controls as the pre-treatment match within the region is not satisfactory. We find that economic liberalization (as represented by the updated Sachs-Warner indicator) tends to have, by and large, a positive—or at least nonnegative—impact on economic growth.

We also find, however, that the benefit of economic liberalization tends to be higher for countries that liberalized before the onset of the latest wave of globalization—the "early bird" effect. Especially in Africa, where a number of liberalization episodes took place in the late 1980s and 1990s, we show that the income differential between the treated country and the estimated counterfactual is either small or not robust to placebo tests for those economies that lagged behind. From a normative perspective, we conclude that economic liberalization can still be beneficial, but that it is more important than ever that it be complemented by other growth-enhancing institutions and policies.

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Appendix. Control Countries in each Synthetic Control

In this section, we provide a list of the countries that form each synthetic control with an individual weight greater than 0.02.

- ASIA

SINGAPORE 1965. Synth. Control A: Philippines (1.000). Synth. Control B: Algeria (0.164); Mexico (0.79).

SOUTH KOREA 1968. Synth. Control A: Philippines (1.000). Synth. Control B: Chile (0.236); Mexico (0.167); Zambia (0.597).

INDONESIA 1970. Synth. Control A: Bangladesh (0.41); India (0.23); Nepal (0.23); Papua New Guinea (0.13). Synth. Control B: Bangladesh (0.038); India (0.206); Kenya (0.051); Malawi (0.05); Mali (0.439); Mozambique (0.057); Nigeria (0.087).

PHILIPPINES 1988. Synth. Control A: Papua New Guinea (1.000). Synth. Control B: Algeria (0.029); Syria (0.471); Togo (0.446); Zimbabwe (0.046).

NEPAL 1991. Synth. Control A: India (1.000). Synth. Control B: Central African Republic (0.141); Malawi (0.859).

- LATIN AMERICA

BARBADOS 1966. Synth. Control A: Mexico (0.047); Trinidad & Tobago (0.034); Venezuela (0.743). Synth. Control B: Gabon (0.821); Trinidad & Tobago (0.155).

CHILE 1976. Synth. Control A: Honduras (0.2); Trinidad & Tobago (0.068); Uruguay (0.732). Synth. Control B: Honduras (0.156); Trinidad & Tobago (0.064); Uruguay (0.749); Zambia (0.031).

COLOMBIA 1976. Synth. Control A: Brasil (0.239); Honduras (0.686); Panama (0.075). Synth. Control B: Algeria (0.094); Nigeria (0.054); Panama (0.221); Rwanda (0.217); Syria (0.394).

COSTA RICA 1986. Synth. Control A: Brasil (0.458); Haiti (0.147); Trinidad & Tobago (0.061); Venezuela (0.334). Synth. Control B: Algeria (0.269); Gabon (0.116); Iran (0.029); Panama (0.585).

MEXICO 1986. Synth. Control A: Brasil (0.234); Panama (0.068); Trinidad & Tobago (0.698). Synth. Control B: Gabon (0.618); Panama (0.382).

- AFRICA

MAURITIUS 1968. Synth. Control A: Senegal (0.829); Zimbabwe (0.171); Synth. Control B: Algeria (0.253); Haiti (0.12); Senegal (0.627).

BOTSWANA 1979. Synth. Control A: Gabon (0.032); Nigeria (0.968). Synth. Control B: China (0.755); Iran (0.245).

GAMBIA 1985. Synth. Control A: Burundi (0.148); Gabon (0.004); Ivory Coast (0.007); Kenya (0.141); Malawi (0.697). Synth. Control B: Burundi (0.421); China (0.391); Egypt (0.033); Pakistan (0.075); Syria (0.066).

GHANA 1985. Synth. Control A: Burkina Faso (0.207); Ethiopia (0.042); Madagascar (0.457); Mozambique (0.234); Senegal (0.06). Synth. Control B: Burkina Faso (0.206); Madagascar (0.452); Mozambique (0.288); Senegal (0.054).

GUINEA 1986. Synth. Control A: Burkina Faso (0.206); Madagascar (0.452); Mozambique

(0.288); Senegal (0.054). Synth. Control B: Burkina Faso (0.164); Central African Republic (0.023); Chad (0.033); Ethiopia (0.172); Lesotho (0.08); Malawi (0.289); Pakistan (0.047); Senegal (0.033); Syria (0.028); Togo (0.085).
GUINEA BISSAU 1987. Synth. Control A: Burundi (0.789); Congo (0.056); Ethiopia (0.087); Niger (0.067). Synth. Control B: Burkina Faso (0.163); Burundi (0.408); China (0.158); Malawi (0.258).
MALI 1988. Synth. Control A: Angola (0.038); Burkina Faso (0.321); Congo (0.027); Ethiopia (0.248); Lesotho (0.069); Mozambique (0.254); Senegal (0.033). Synth. Control B: Angola (0.058); Burkina Faso (0.362); Burundi (0.462); Lesotho (0.042); Senegal (0.057.
UGANDA 1988. Synth. Control A: Burkina Faso (0.092); Ethiopia (0.609); Madagascar (0.299); Synth. Control B: Burkina Faso (0.669); Burundi (0.162); Chad (0.153).
BENIN 1990. Synth. Control A: Central African Republic (0.132); Congo (0.041); Ethiopia (0.309); Malawi (0.05); Senegal (0.217); Togo (0.229); Zimbabwe (0.022). Synth. Control B: Central African Republic (0.278); Malawi (0.208); Senegal (0.247); Togo (0.233).
SOUTH AFRICA 1991. Synth. Control A: Angola (0.126); Gabon (0.045); Malaysia (0.178); Zimbabwe (0.651). Synth. Control B: Algeria (0.266); Iran (0.03); Syria (0.026); Zimbabwe (0.67).
CAPO VERDE 1991. Synth. Control A: Burkina Faso (0.38); Congo (0.211); Malaysia (0.107); Senegal (0.248); Zimbabwe (0.055). Synth. Control B: China (0.521); Congo (0.163); Senegal (0.185); Zimbabwe (0.13).
ZAMBIA 1993. Synth. Control A: Angola (0.238); Madagascar (0.338); Togo (0.419). Synth. Control B: Angola (0.256); Haiti (0.037); Togo (0.707).
CAMEROON 1993. Synth. Control A: Central African Republic (0.098); Malaysia (0.316); Rwanda (0.586). Synth. Control B: Algeria (0.083); China (0.48); Syria (0.435).
KENYA 1993. Synth. Control A: Central African Republic (0.022); Congo (0.056); Ethiopia (0.188); Malaysia (0.076); Nigeria (0.057); Rwanda (0.442); Tanzania (0.145). Synth. Control B: China (0.145); Congo (0.109); Malawi (0.029); Nigeria (0.152); Rwanda (0.486); Syria (0.08).
IVORY COAST 1994. Synth. Control A: Central African Republic (0.485); Gabon (0.037); Malaysia (0.058); Sierra Leone (0.371); Zimbabwe (0.049). Synth. Control B: Algeria (0.124); Central African Republic (0.03); Haiti (0.067); Sierra Leone (0.579); Syria (0.199).
NIGER 1994. Synth. Control A: Malawi (0.745); Senegal (0.238). Synth. Control B: Malawi (0.779); Senegal (0.215).

- MIDDLE EAST

MOROCCO 1984. Synth. Control A: Algeria (0.233); Egypt (0.029); Sudan (0.563); Syria (0.175). Synth. Control B: Bahrain (0.03); Burundi (0.158); China (0.325); Senegal (0.052); Syria (0.146); Tanzania (0.172).
TUNISIA 1989. Synth. Control A: Egypt (0.515); Oman (0.049); Syria (0.353); Tanzania (0.069). Synth. Control B: Angola (0.153); China (0.192); Congo (0.094); Iran (0.048); Malta (0.162); Nigeria (0.025); Swaziland (0.119); Tanzania (0.204).
MAURITANIA 1995. Synth. Control A: Sudan (0.996). Synth. Control B: Nigeria (0.156); Rwanda (0.614); Senegal (0.057).
EGYPT 1995. Synth. Control A: Central African Republic (0.044); Congo (0.509); Malawi (0.186); Syria (0.243). Synth. Control B: Bahrain (0.027); India (0.864); Malta (0.061); Rwanda (0.043).

Tables and Figures

Table 1: Economic Liberalizations in Asia

Country	*Treatment Status*
Hong Kong, SAR	always open
Thailand	always open
Malaysia	open since 1963
Taiwan, Province of China	open since 1963
Singapore	open since 1965
Korea (Republic of)	open since 1968
Indonesia	open since 1970
Sri Lanka	open since 1977 (waves after)
Phillippines	open since 1988
Nepal	open since 1991
Bangladesh	open since 1996
China PR	always closed
India	always closed
Pakistan	always closed
Papua New Guinea	always closed
Afghanistan	not available
Bhutan	not available
Brunei	not available
Cambodia	not available
Fiji	not available
Korea (Democratic People's Republic of)	not available
Laos	not available
Mongolia	not available
Myanmar (Burma)	not available
Samoa (Western)	not available
Solomon Islands	not available
Tonga	not available
Vanuatu	not available
Vietnam	not available

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 2: Economic Liberalizations in Latin America

Country	*Treatment Status*
Bolivia	always open (except 1979-84)
Ecuador	always open (except 1982-90)
Barbados	open since 1966
Chile	open since 1976
Colombia	open since 1986
Costa Rica	open since 1986
Mexico	open since 1986
Guatemala	open since 1988
Guyana	open since 1988
El Salvador	open since 1989
Jamaica	open since 1989 (waves before)
Paraguay	open since 1989
Venezuela	open since 1989 (waves after)
Uruguay	open since 1990
Argentina	open since 1991
Brazil	open since 1991
Honduras	open since 1991
Nicaragua	open since 1991
Peru	open since 1991
Dominican Republic	open since 1992
Trinidad & Tobago	open since 1992
Panama	open since 1996
Haiti	always closed
Antigua	not available
Bahamas	not available
Belize	not available
Cuba	not available
Dominica	not available
Grenada	not available
St. Kitts & Nevis	not available
St. Lucia	not available
St. Vincent & Grenadines	not available
Suriname	not available

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 3: Economic Liberalizations in Africa

Country	*Treatment Status*
Mauritius	open since 1968
Botswana	open since 1979
Gambia	open since 1985
Ghana	open since 1985
Guinea	open since 1986
Guinea Bissau	open since 1987
Mali	open since 1988
Uganda	open since 1988
Benin	open since 1990
Cape Verde	open since 1991
South Africa	open since 1991
Cameroon	open since 1993
Kenya	open since 1993
Zambia	open since 1993
Ivory Coast	open since 1994
Niger	open since 1994
Mozambique	open since 1995
Tanzania	open since 1995
Ethiopia	open since 1996
Madagascar	open since 1996
Burkina Faso	open since 1998
Burundi	open since 1999
Angola	always closed
Central African Republic	always closed
Chad	always closed
Congo	always closed
Gabon	always closed
Lesotho	always closed
Malawi	always closed
Nigeria	always closed
Rwanda	always closed
Senegal	always closed
Sierra Leone	always closed
Togo	always closed
Zimbabwe	always closed
Comoros	not available
Djibouti	not available
Equatorial Guinea	not available
Eritrea	not available
Liberia	not available
Namibia	not available
Sao Tome & Principe	not available
Seychelles	not available
Somalia	not available
Sudan	not available
Swaziland	not available
Zaire	not available

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 4: Economic Liberalizations in the Middle East

Country	*Treatment Status*
Yemen	always open
Jordan	open since 1965
Morocco	open since 1984
Tunisia	open since 1989
Egypt	open since 1995
Mauritania	open since 1995
Algeria	always closed
Iran	always closed
Iraq	always closed
Syria	always closed
Bahrain	missing
Kuwait	missing
Lebanon	missing
Libya	missing
Oman	missing
Qatar	missing
Saudi Arabia	missing
United Arab Emirates	missing

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 5: Predictors and Outcome Means — Asia

	Singapore 1965	Synth. Control A	Synth. Control B
Population growth	2.87	2.94	2.88
Pre-treatment GDP	2,569.49	612.54	2,573.46
GDP at $T_0 + 5$	3,901.08	693.39	3,270.89
GDP at $T_0 + 10$	6,143.02	791.80	3,801.23
RMSPE		1,958.21	0.00
	South Korea 1968	Synth. Control A	Synth. Control B
Secondary school	31.00	33.50	11.87
Population growth	2.62	3.05	2.76
Investment share	0.17	0.15	0.17
Pre-treatment GDP	1,290.43	631.62	1,290.46
GDP at $T_0 + 5$	2,045.33	731.28	1,612.73
GDP at $T_0 + 10$	3,008.49	890.37	1,572.70
RMSPE		663.59	12.82
	Indonesia 1970	Synth. Control A	Synth. Control B
Secondary school	9.00	11.30	7.81
Population growth	2.20	2.32	2.28
Investment share	0.07	0.10	0.09
Pre-treatment GDP	247.90	247.96	258.15
GDP at $T_0 + 5$	361.11	258.19	308.00
GDP at $T_0 + 10$	465.99	264.62	303.05
RMSPE		5.20	0.01
	Philippines 1988	Synth. Control A	Synth. Control B
Secondary school	57.80	9.88	32.84
Population growth	2.74	2.31	2.99
Investment share	0.16	0.13	0.11
Inflation	11.34	7.84	8.89
Democracy	0.50	1.00	0.04
Pre-treatment GDP	794.88	507.82	802.20
GDP at $T_0 + 5$	848.96	581.68	945.83
GDP at $T_0 + 10$	949.28	582.53	977.31
RMSPE		303.00	35.74
	Nepal 1991	Synth. Control A	Synth. Control B
Secondary school	21.11	33.61	5.13
Population growth	2.34	2.22	2.79
Investment share	0.10	0.12	0.14
Inflation	8.47	7.81	14.36
Democracy	0.03	1.00	0.00
Pre-treatment GDP	159.81	225.23	162.65
GDP at $T_0 + 5$	192.78	383.81	167.28
GDP at $T_0 + 10$	234.59	460.82	179.75
RMSPE		78.28	17.94

Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the outcome and predictor variables. Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment 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 pool of potential controls including only Asian countries; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Table 6: Predictors and Outcome Means — Latin America

	Barbados 1966	Synth. Control A	Synth. Control B
Secondary school	45.50	23.54	14.00
Population growth	0.37	3.40	0.59
Investment share	0.15	0.29	0.16
Pre-treatment GDP	3,377.93	3,381.60	3,376.01
GDP at $T_0 + 5$	4,604.42	3,877.56	4,574.42
GDP at $T_0 + 10$	6,345.67	4,053.99	5,920.59
RMSPE		0.00	0.00
	Chile 1976	Synth. Control A	Synth. Control B
Secondary school	35.70	41.61	42.07
Population growth	2.06	1.27	1.23
Investment share	0.17	0.12	0.12
Democracy	0.81	0.62	0.64
Pre-treatment GDP	2,069.70	2,067.31	2,068.03
GDP at $T_0 + 5$	2,331.43	2,814.71	2,810.66
GDP at $T_0 + 10$	2,061.61	2,431.42	2,426.75
RMSPE		84.51	84.49
	Colombia 1976	Synth. Control A	Synth. Control B
Secondary school	33.50	27.60	34.18
Population growth	2.54	2.97	3.03
Investment share	0.13	0.16	0.14
Inflation	17.60	40.24	7.44
Democracy	1.00	0.23	0.09
Pre-treatment GDP	1,262.72	1,282.51	1,260.06
GDP at $T_0 + 5$	1,718.01	1,475.12	1,354.10
GDP at $T_0 + 10$	1,947.22	1,581.01	1,569.98
RMSPE		45.35	35.21
	Costa Rica 1986	Synth. Control A	Synth. Control B
Secondary school	38.19	27.24	42.88
Population growth	3.29	2.58	2.61
Investment share	0.15	0.23	0.21
Inflation	12.87	72.83	5.91
Democracy	1.00	0.48	0.18
Pre-treatment GDP	2,767.40	2,758.52	2,744.17
GDP at $T_0 + 5$	3,232.27	2,793.82	2,754.89
GDP at $T_0 + 10$	3,708.23	2,945.76	3,037.53
RMSPE		115.58	187.98
	Mexico 1986	Synth. Control A	Synth. Control B
Secondary school	40.95	52.31	38.88
Population growth	2.86	1.74	2.24
Investment share	0.20	0.16	0.18
Inflation	20.30	41.87	6.81
Democracy	0.00	0.76	0.12
Pre-treatment GDP	4,331.43	4,444.00	4,013.55
GDP at $T_0 + 5$	5,461.90	4,258.11	3,793.08
GDP at $T_0 + 10$	5,380.47	4,452.21	4,047.28
RMSPE		254.40	947.43

Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the outcome and predictor variables. Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment 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 pool of potential controls including only Latin American countries; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Table 7: Predictors and Outcome Means — Africa Before 1987

	Mauritius 1968	Synth. Control A	Synth. Control B
Secondary school	25.00	5.17	5.57
Population growth	2.51	2.70	2.39
Investment share	0.15	0.10	0.09
Pre-treatment GDP	917.09	914.67	917.12
GDP at $T_0 + 5$	898.80	945.07	873.38
GDP at $T_0 + 10$	1,322.95	867.21	973.66
RMSPE		45.89	25.45
	Botswana 1979	Synth. Control A	Synth. Control B
Secondary school	6.73	5.97	28.05
Population growth	1.26	2.69	2.22
Investment share	0.17	0.08	0.16
Democracy	1.00	0.31	0.00
Pre-treatment GDP	465.76	486.04	488.41
GDP at $T_0 + 5$	1,539.36	439.19	597.72
GDP at $T_0 + 10$	2,182.08	429.51	596.12
RMSPE		106.74	92.54
	Gambia 1985	Synth. Control A	Synth. Control B
Secondary school	10.73	5.50	19.75
Population growth	2.95	2.76	2.05
Investment share	0.03	0.14	0.10
Democracy	1.00	0.02	0.04
Pre-treatment GDP	208.25	198.21	210.66
GDP at $T_0 + 5$	242.98	203.98	356.05
GDP at $T_0 + 10$	244.43	193.50	471.19
RMSPE		13.08	10.22
	Ghana 1985	Synth. Control A	Synth. Control B
Population growth	2.47	2.43	2.42
Investment share	0.10	0.04	0.04
Democracy	0.16	0.02	0.02
Secondary school	30.31	9.76	9.51
Pre-treatment GDP	298.92	300.63	299.63
GDP at $T_0 + 5$	256.76	253.83	252.77
GDP at $T_0 + 10$	283.32	230.53	230.65
RMSPE		14.91	14.81
	Guinea 1986	Synth. Control A	Synth. Control B
Secondary school	12.44	10.65	10.14
Population growth	1.86	2.42	2.56
Investment share	0.11	0.09	0.10
Democracy	0.00	0.02	0.06
Pre-treatment GDP	316.63	318.88	316.64
GDP at $T_0 + 5$	342.61	334.71	321.87
GDP at $T_0 + 10$	345.17	307.98	335.91
RMSPE		2.80	2.60

Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the outcome and predictor variables. Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment 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 pool of potential controls including only African countries; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Table 8: Predictors and Outcome Means — Africa Between 1987 and 1991

	Guinea-Bissau 1987	Synth. Control A	Synth. Control B
Secondary school	7.25	6.21	8.79
Population growth	1.90	2.11	2.19
Investment share	0.13	0.06	0.10
Democracy	0.00	0.00	
Pre-treatment GDP	151.38	150.47	151.03
GDP at $T_0 + 5$	189.74	188.29	213.20
GDP at $T_0 + 10$	199.29	152.89	255.38
RMSPE		9.96	9.19
	Mali 1988	Synth. Control A	Synth. Control B
Secondary school	6.01	8.37	4.46
Population growth	2.15	2.33	2.12
Investment share	0.08	0.06	0.07
Democracy	0.00	0.05	0.05
Pre-treatment GDP	231.89	234.36	233.40
GDP at $T_0 + 5$	241.47	224.33	227.85
GDP at $T_0 + 10$	259.16	249.36	223.60
RMSPE		11.98	13.17
	Uganda 1988	Synth. Control A	Synth. Control B
Secondary school	7.31	11.18	3.35
Population growth	3.03	2.54	2.09
Investment share	0.02	0.04	0.08
Democracy	0.36	0.01	0.07
Pre-treatment GDP	198.23	200.92	200.97
GDP at $T_0 + 5$	162.68	152.82	219.99
GDP at $T_0 + 10$	205.59	165.06	219.99
RMSPE		12.48	17.25
	Benin 1990	Synth. Control A	Synth. Control B
Secondary school	13.66	15.11	13.50
Population growth	2.76	2.66	2.62
Investment share	0.06	0.08	0.09
Democracy	0.10	0.02	0.00
Pre-treatment GDP	372.66	374.91	371.83
GDP at $T_0 + 5$	355.08	333.35	315.38
GDP at $T_0 + 10$	395.34	348.76	333.64
RMSPE		13.47	12.39
	South Africa 1991	Synth. Control A	Synth. Control B
Secondary school	51.96	29.96	32.21
Population growth	2.35	2.89	3.03
Investment share	0.16	0.22	0.24
Democracy	1.00	0.71	0.54
Pre-treatment GDP	2,424.17	2,431.30	2,443.50
GDP at $T_0 + 5$	2,273.23	2,721.67	2,416.28
GDP at $T_0 + 10$	2,402.46	2,752.29	2,417.09
RMSPE		110.09	90.21
	Capo Verde 1991	Synth. Control A	Synth. Control B
Population growth	1.89	2.53	2.28
Investment share	0.17	0.13	0.16
Secondary school	12.87	23.03	35.20
Pre-treatment GDP	691.79	688.25	683.25
GDP at $T_0 + 5$	986.36	930.14	980.44
GDP at $T_0 + 10$	1,315.49	998.44	1,149.68
RMSPE		57.98	55.68

Source: authors' calculations based on data in Persson and Tabellini (2006). See the notes to Table 7.

Table 9: Predictors and Outcome Means — Africa After 1991

	Zambia 1993	Synth. Control A	Synth. Control B
Secondary school	17.84	17.52	18.33
Population growth	3.01	2.58	2.61
Investment share	0.14	0.06	0.07
Democracy	0.21	0.02	0.00
Pre-treatment GDP	576.04	577.92	578.98
GDP at $T_0 + 5$	345.84	439.88	442.80
GDP at $T_0 + 10$	358.90	427.91	409.70
RMSPE		45.23	45.13
	Cameroon 1993	Synth. Control A	Synth. Control B
Secondary school	20.04	19.67	43.68
Population growth	2.57	2.86	2.48
Investment share	0.08	0.09	0.15
Democracy	0.00	0.32	0.00
Pre-treatment GDP	681.22	700.96	681.48
GDP at $T_0 + 5$	620.22	1,410.83	1,146.56
GDP at $T_0 + 10$	670.92	1,416.98	1,321.51
RMSPE		94.00	91.93
	Kenya 1993	Synth. Control A	Synth. Control B
Secondary school	18.95	13.49	22.19
Population growth	3.42	2.92	2.81
Investment share	0.13	0.10	0.09
Democracy	0.10	0.10	0.05
Pre-treatment GDP	360.64	359.74	363.86
GDP at $T_0 + 5$	411.45	533.44	494.63
GDP at $T_0 + 10$	421.51	541.75	550.62
RMSPE		11.20	13.47
	Ivory Coast 1994	Synth. Control A	Synth. Control B
Secondary school	17.47	16.62	24.78
Population growth	3.67	2.17	2.30
Investment share	0.08	0.07	0.07
Democracy	0.00	0.21	0.16
Pre-treatment GDP	745.56	742.04	745.80
GDP at $T_0 + 5$	729.60	726.35	634.04
GDP at $T_0 + 10$	643.90	694.39	656.04
RMSPE		60.99	37.78
	Niger 1994	Synth. Control A	Synth. Control B
Secondary school	4.99	7.70	7.43
Population growth	3.12	2.85	2.85
Investment share	0.09	0.13	0.13
Democracy	0.09	0.00	0.00
Pre-treatment GDP	236.28	245.42	235.33
GDP at $T_0 + 5$	198.30	249.99	236.36
GDP at $T_0 + 10$	185.47	262.91	246.54
RMSPE		26.34	25.05

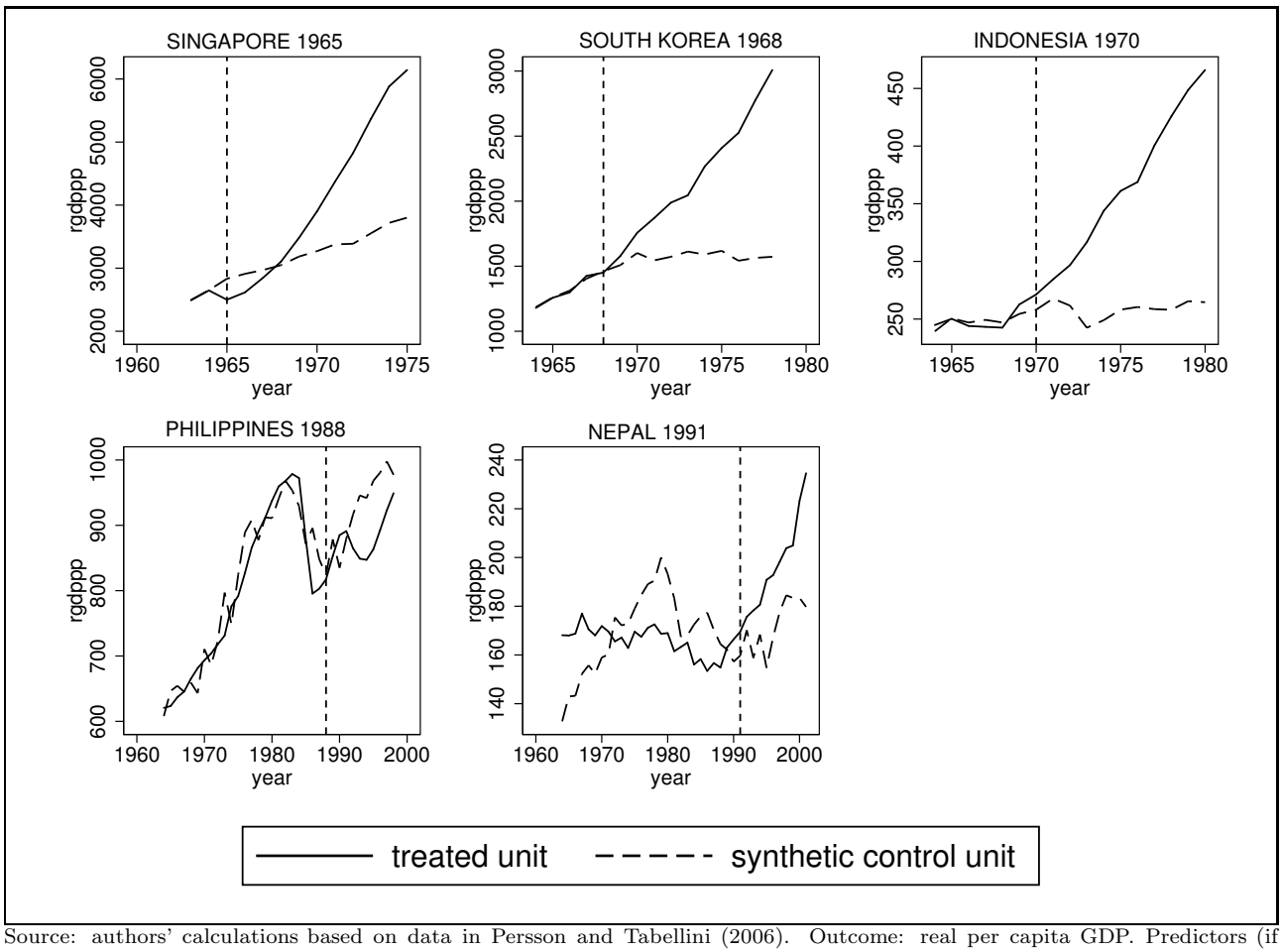
Source: authors' calculations based on data in Persson and Tabellini (2006). See the notes to Table 7.

Table 10: Predictors and Outcome Means — Middle East

	Morocco 1984	Synth. Control A	Synth. Control B
Secondary school	31.67	31.50	31.69
Population growth	2.49	2.91	2.49
Investment share	0.14	0.12	0.14
Pre-treatment GDP	825.47	823.18	811.31
GDP at $T_0 + 5$	1,078.58	881.46	961.98
GDP at $T_0 + 10$	1,098.18	879.01	1,040.32
RMSPE		30.87	25.71
	Tunisia 1989	Synth. Control A	Synth. Control B
Secondary school	41.05	51.90	37.95
Population growth	2.29	2.82	2.21
Investment share	0.18	0.10	0.18
Pre-treatment GDP	1,117.94	1,118.54	1,116.01
GDP at $T_0 + 5$	1,647.92	1,507.70	1,896.77
GDP at $T_0 + 10$	1,914.99	1,659.60	2,289.96
RMSPE		97.30	42.07
	Mauritania 1995	Synth. Control A	Synth. Control B
Secondary school	12.39	17.92	12.49
Population growth	2.47	2.55	2.48
Investment share	0.06	0.10	0.06
Pre-treatment GDP	400.23	406.47	398.32
GDP at $T_0 + 5$	437.83	445.63	451.69
GDP at $T_0 + 10$	476.77	552.70	491.75
RMSPE		29.05	13.91
	Egypt 1995	Synth. Control A	Synth. Control B
Secondary school	62.07	44.22	41.10
Population growth	2.31	2.91	2.12
Investment share	0.07	0.14	0.12
Pre-treatment GDP	764.58	787.82	768.19
GDP at $T_0 + 5$	1,254.94	964.21	1,366.50
GDP at $T_0 + 10$	1,388.62	1,026.88	1,518.83
RMSPE		85.45	48.33

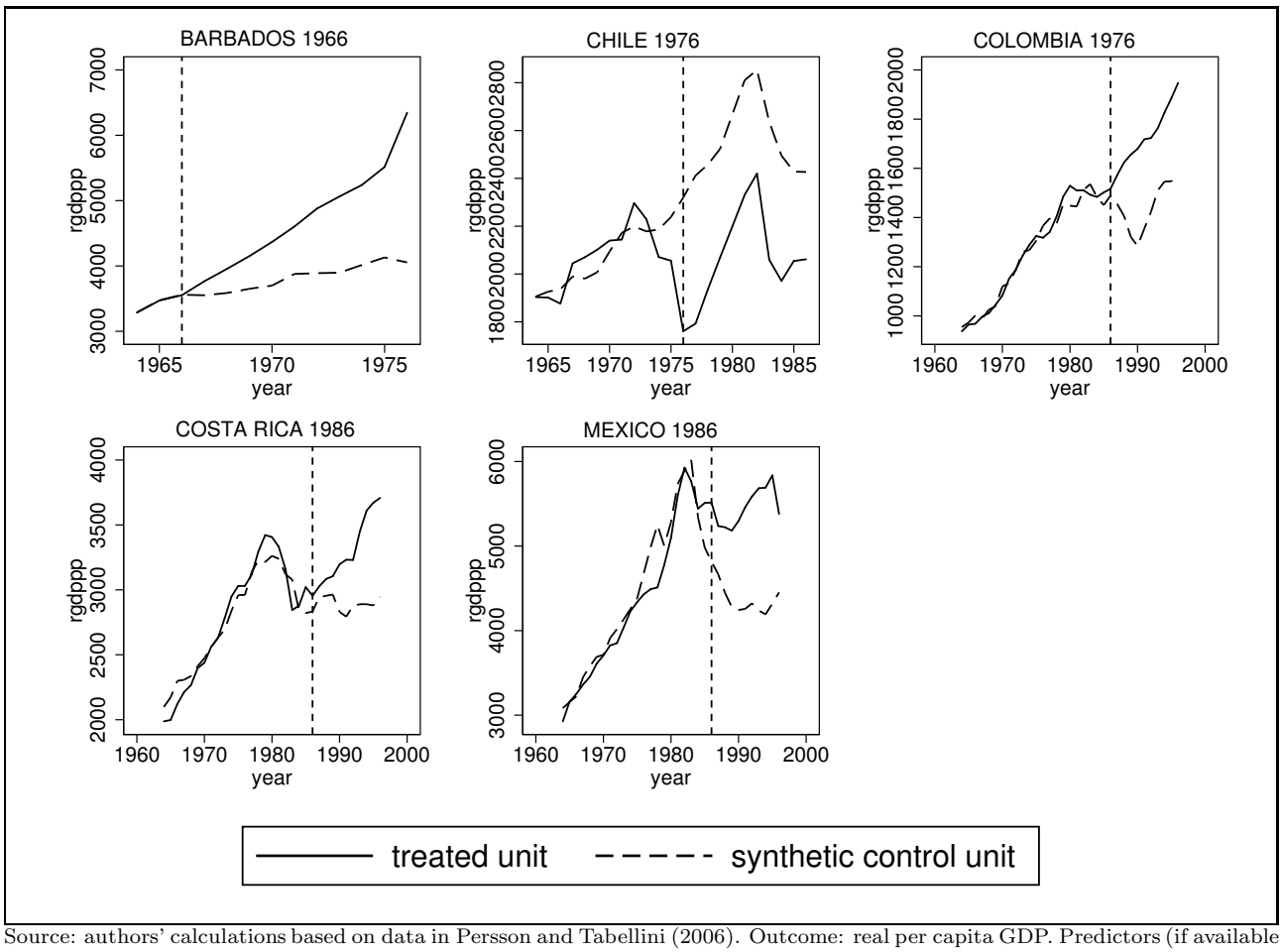
Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the outcome and predictor variables. Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment 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 pool of potential controls including only countries in the Middle East; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 1: GDP Trends, Treated Country vs. Synthetic Control — Asia



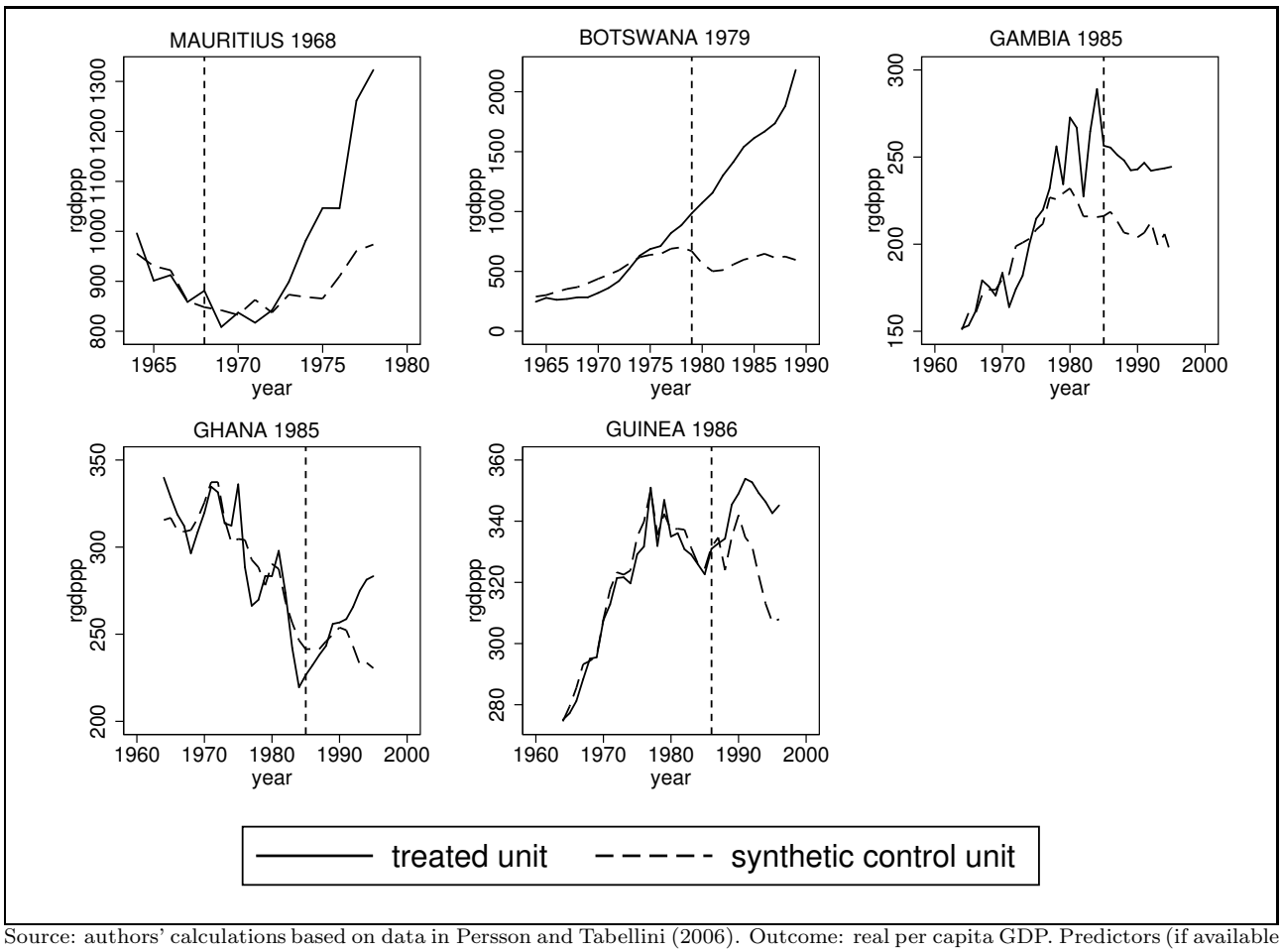
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Indonesia; synthetic control B for Singapore, South Korea, Philippines, and Nepal. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 2: GDP Trends, Treated Country vs. Synthetic Control — Latin America



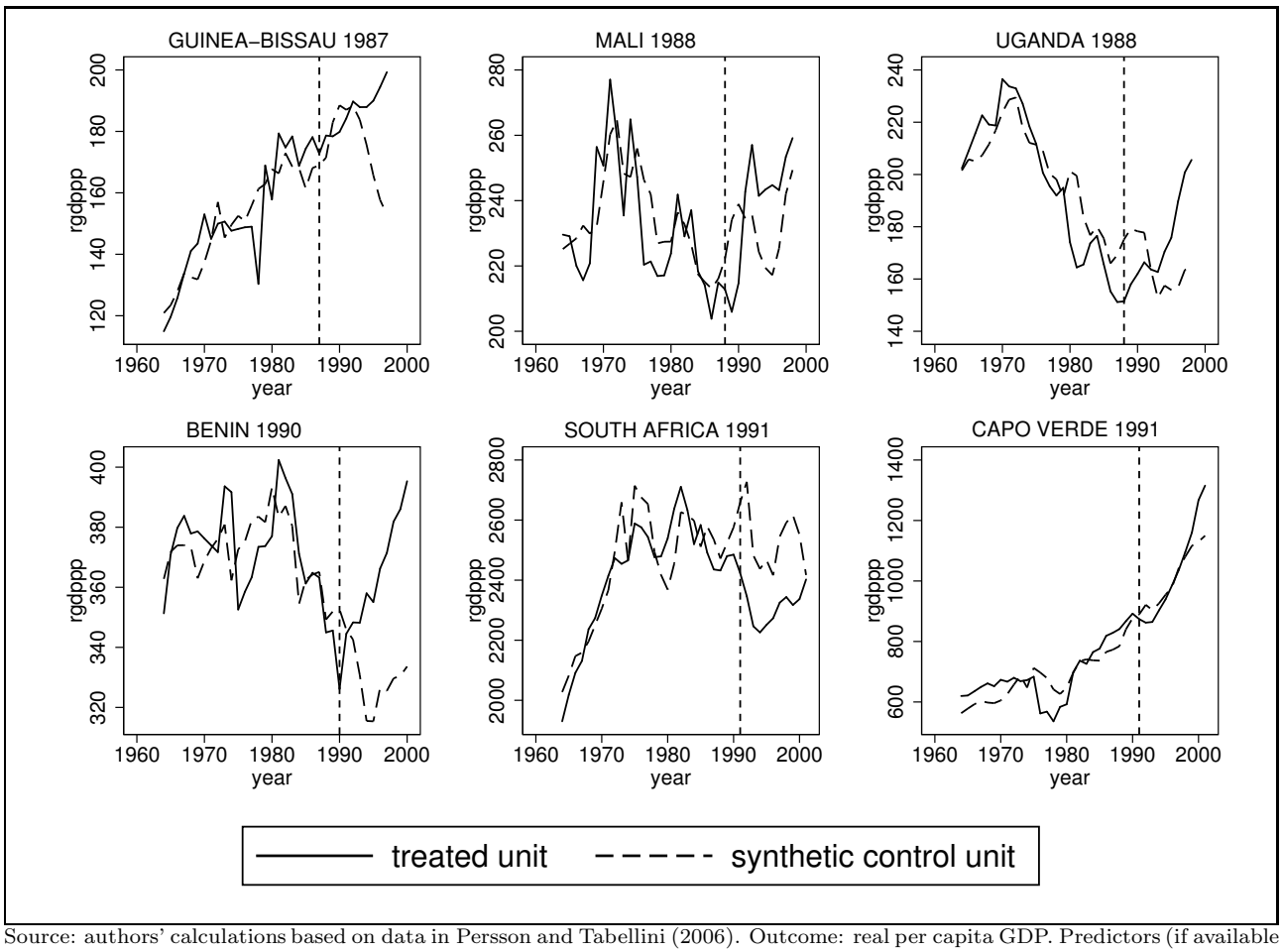
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Barbados, Costa Rica, and Mexico; synthetic control B for Chile and Colombia. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 3: GDP Trends, Treated Country vs. Synthetic Control — Africa Before 1987



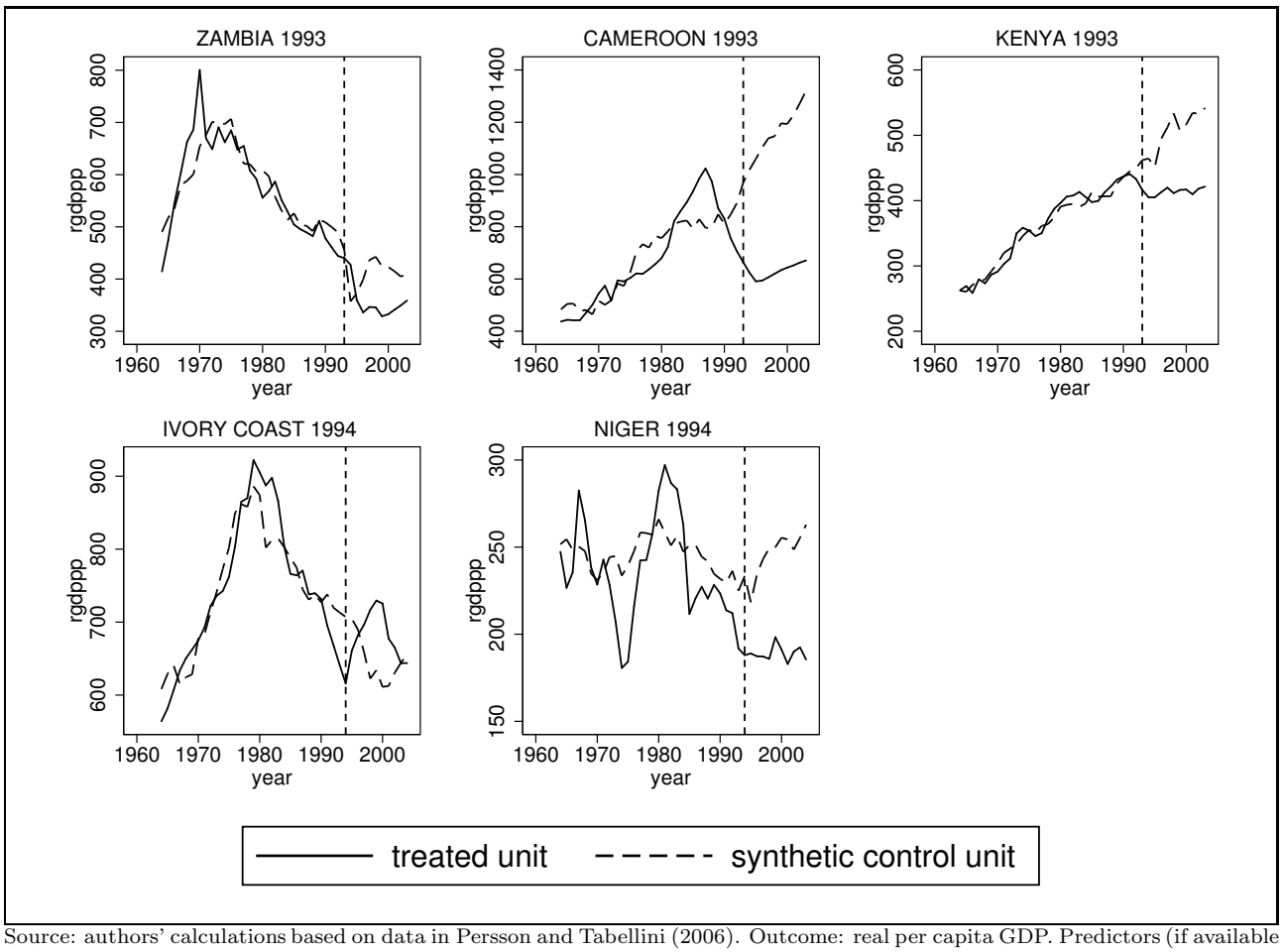
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Gambia, Ghana, and Guinea; synthetic control B for Mauritius and Botswana. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 4: GDP Trends, Treated Country vs. Synthetic Control — Africa Between 1987 and 1991



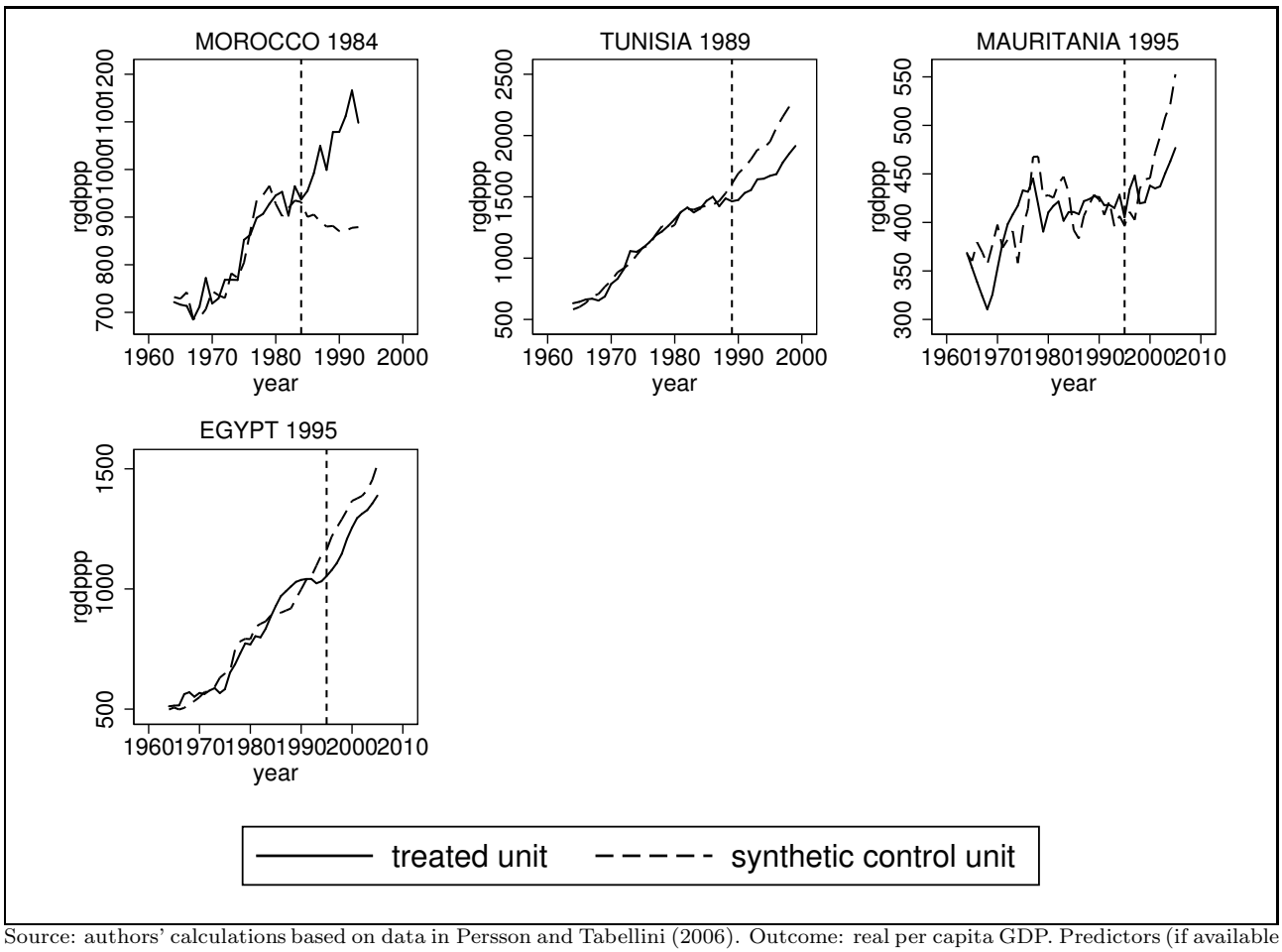
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Guinea-Bissau, Mali, Uganda, and Benin; synthetic control B for South Africa and Capo Verde. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 5: GDP Trends, Treated Country vs. Synthetic Control — Africa After 1991



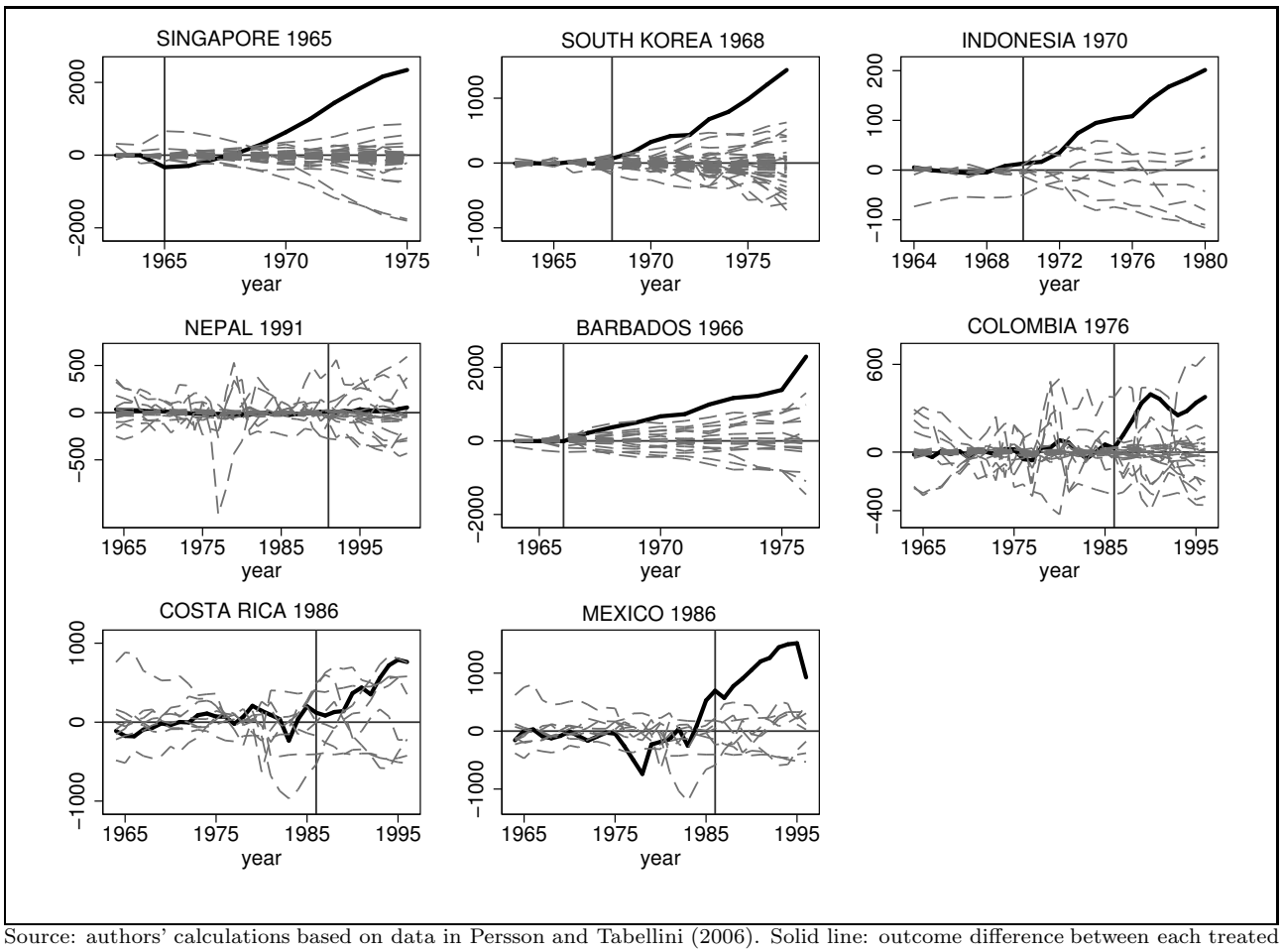
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Kenya and Niger; synthetic control B for Zambia, Cameroon, and Ivory Coast. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 6: GDP Trends, Treated Country vs. Synthetic Control — Middle East



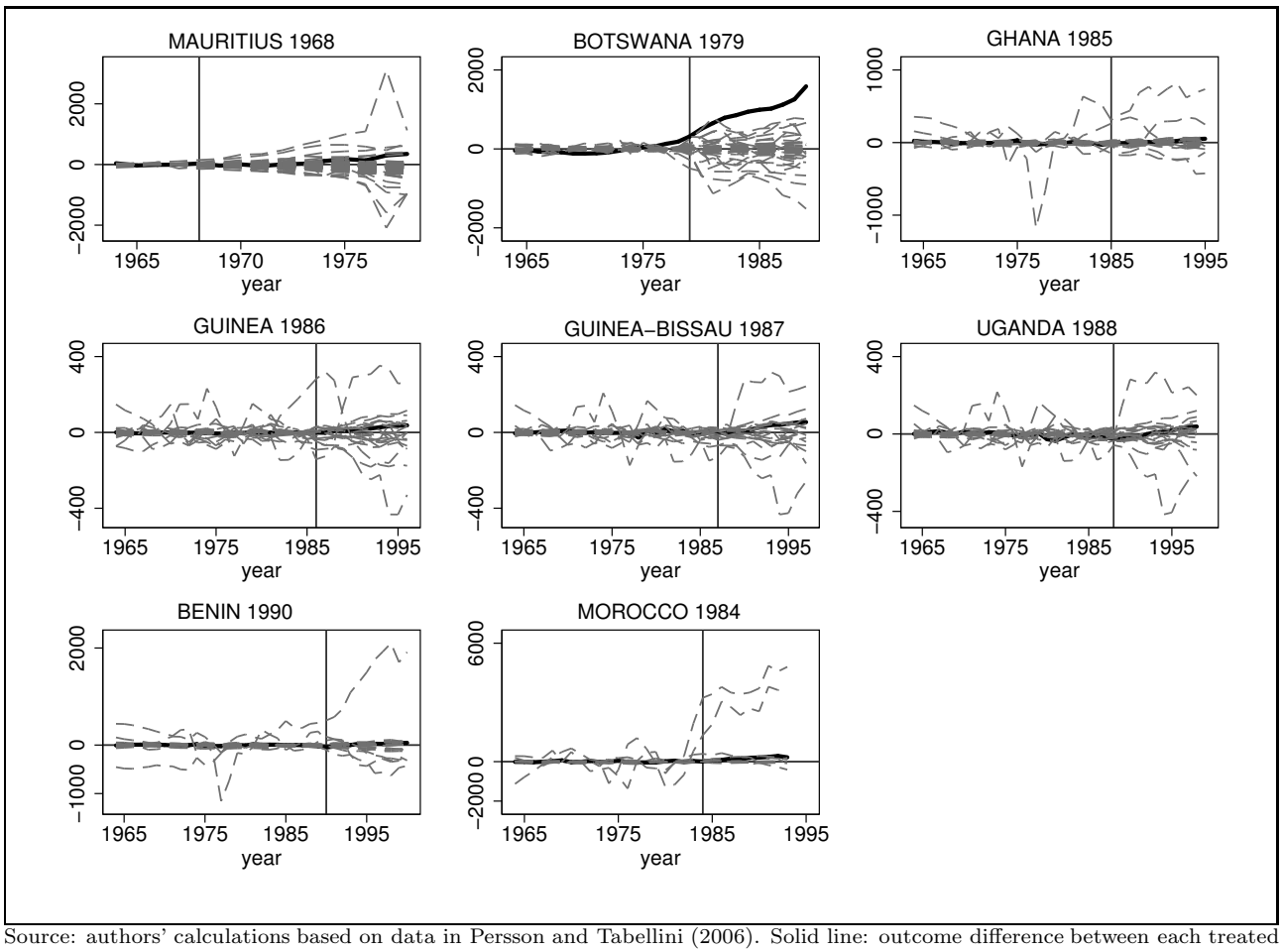
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Morocco and Mauritania; synthetic control B for Tunisia and Egypt. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 7: Placebo Experiments — Asia & Latin America



Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between each treated country and its synthetic control. Dashed lines: outcome difference between each of the treated country's potential controls and their synthetic control in placebo experiments. Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Indonesia, Barbados, Costa Rica, and Mexico; synthetic control B for Singapore, South Korea, Nepal, and Colombia.

Figure 8: Placebo Experiments — Africa & Middle East



Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between each treated country and its synthetic control. Dashed lines: outcome difference between each of the treated country's potential controls and their synthetic control in placebo experiments. Outcome: real per capita GDP. Predictors (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Ghana, Guinea, Guinea-Bissau, Uganda, Benin, and Morocco; synthetic control B for Mauritius and Botswana.