# ASSESSING ECONOMIC LIBERALIZATION EPISODES:
# A SYNTHETIC CONTROL APPROACH

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*Abstract*—We use a transparent statistical methodology for data-driven case studies—the synthetic control method—to investigate the impact of economic liberalization on real GDP per capita in a worldwide sample of countries. Economic liberalization is measured by a widely used indicator that captures the scope of the market in the economy. The methodology compares the postliberalization GDP trajectory of treated economies with the trajectory of a combination of similar but untreated economies. We find that liberalizing the economy had a positive effect in most regions, but more recent liberalizations, in the 1990s and mainly in Africa, had no significant impact.

## I.    Introduction

THEORETICAL results in international economics and growth theory largely point to a positive relationship between economic liberalization and economic welfare, but confirming this prediction empirically has proven to be a Sisyphean job. A major complication in identifying this relationship lies in the well-known limitations of cross-country econometric studies due to endogeneity or measurement issues (see Rodriguez & Rodrik, 2001). But the alternative suggested by some authors (see Bhagwati & Srinivasan, 2001), country-specific case studies, also faces limitations, as these studies usually lack statistical rigor and are exposed to discretionary case selection.

In this article, we expand on Bhagwati and Srinivasan's (2001) suggestion and offer a set of empirical country studies on the effect of economic liberalization on the pattern of income per capita. At the same time, we provide a unified statistical framework to compare the income performance of open versus closed economies to minimize the impact of discretion in the analysis—or at least to make it transparent. In particular, we apply a recent econometric technique, the synthetic control method, to perform data-driven comparative case studies, which we view as a third way between standard cross-country estimators that are prone to suffer from multiple endogeneity issues and the hardly generalizable analysis of individual country episodes. We cover as large a sample as possible within the data constraints imposed by the proposed framework.

We use a worldwide panel of economies over the period 1963 to 2005 and evaluate the effect of a binary indicator of economic liberalization—derived by Sachs and Warner (1995) and extended, updated, and revised by Wacziarg and Welch (2003, 2008)—on the outcome, namely, the posttreatment trajectory of real GDP per capita. This binary indicator has been widely used in cross-country studies and therefore allows us to anchor our findings in the existing literature on the nexus between openness and income. Following Giavazzi and Tabellini (2005, p. 1298), we interpret it as a broad measure of "comprehensive reforms that extend the scope of the market, and in particular of international markets."

Within the synthetic control framework, we ask whether liberalizing 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 this approach lies in the transparent estimation of the counterfactual 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 by an algorithm based on their similarity to the treated economy before the treatment with respect to both relevant covariates and past realizations of the outcome variable (real GPD per capita).

We study all episodes of economic liberalization in the world from 1963 to 2005 as long as they qualify for our empirical framework. In the sample selection procedure, we require that for each country that liberalized its economy in a certain year, we required a sufficient number of comparison countries in the same region that did not liberalize before or soon after. This feature distinguishes our study from the standard cross-sectional work in an important way: we pay particular attention to whether there is enough variation in the treatment within a given geographical 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 very thin ice when we try to identify the effect of economic liberalization on income because reform waves reduce the number of available comparison countries after a given year. Indeed, as Billmeier and Nannicini (2009) showed, 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 standard estimation results. Forcing us to select a more appropriate set of comparison units is thus a first advantage of the synthetic control method. Moreover, the transparent construction of the estimated counterfactual safeguards against the risk of drawing inference from (hidden) parametric extrapolation.

A second advantage of the proposed statistical framework is that unlike most of the estimators used in the literature,

it can deal with endogeneity from omitted variable bias by accounting for the presence of time-varying unobservable confounders. This feature improves on panel models such as fixed effects or difference-in-differences, which can account for only time-invariant unobservable confounders. A remaining limitation, however, is that economic reforms might be triggered by the anticipation of future growth prospects, thus leading to endogeneity from reverse causation. As long as growth expectations are not captured by the unobservable heterogeneity included in the model, this would bias the findings of the synthetic control approach.

As an additional caveat, we note the difficulty of comparing economic reforms across countries because liberalization efforts might take very different forms. Moreover, economic liberalization cannot be decoupled from the political background (for example, see the diverse economic reform agendas that came with political transformation in the Comecon countries). When interpreting the empirical results on each economic liberalization episode, we therefore try to account for the fact that our treatment may encompass heterogeneous reforms at the country level.

In selecting the potential comparison countries, we exploit the flexibility of the method, and for each treated country, we implement two types of experiments. In type A experiments, we restrict the control group to eligible countries in the same macroregion of those treated (Asia, Latin America, Africa, and Middle East); in type B experiments, we incorporate all eligible countries (available across all regions) into the control group. As a result, type A experiments ensure the existence of a common support between treated and comparison countries according to factors related to geographical location and exclude the rather far-fetched country comparisons discussed by Billmeier and Nannicini (2009), while type B experiments increase the sample size and the power of our test.

Our empirical findings show that for many countries we can analyze, economic liberalization had a positive effect on income per capita. However, we find a lot of heterogeneity in the results across regions and time. In particular, countries that liberalized their economy after 1990, many of them in Africa, did not benefit from these reforms in terms of higher GDP compared to similar but closed economies.

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 approach is discussed in section IV. Section V discusses our case studies selection, while section VI presents the empirical results on the effect of economic liberalization on income patterns. Section VII concludes.

## II.    Related Literature

While a large body of broadly supportive theoretical literature exists, providing conclusive and robust empirical evidence on the intuitively positive effect of market liberalization on income has been a challenging endeavor and hotly debated topic, complicated by a number of factors. Regarding the connection between trade openness and growth, 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 to 2 percentage points with a shift to more outward-oriented trade policies. Sachs and Warner (1995, p. 47) 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." Edwards (1992, 1998) finds that for eight measures of openness, the impact on TFP growth is positive and significant in thirteen out of eighteen 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 can be detected in the data only starting from 1970.

Rodriguez and Rodrik (2001) cast doubt on the robustness of these affirmative results, however. They point out that much of the literature produced during the 1990s documenting a positive effect suffers from various weaknesses, related especially to the measure of liberalization and openness 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 (a) the indicator captures much more than just openness to trade and should be interpreted accordingly and that (b) the positive correlations found in Dollar (1992), Sachs and Warner (1995), and Edwards (1998) are not robust.[1] 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 sixty 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 regressions are not robust to even small changes in the conditioning information set.

Bhagwati and Srinivasan (2002) question cross-country evidence on the openness-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

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[1] More recent contributions in the political economy literature, such as Giavazzi and Tabellini (2005) and Persson and Tabellini (2006), interpret the Sachs-Warner dummy more appropriately as a broader indicator of economic liberalization, no longer of trade openness alone. See section III for a more detailed discussion on this point.

of freedom where one might almost get what one wants if one only tries hard enough!" (p. 181). On similar grounds, Pritchett (2000) argues for detailed case studies of individual countries.

This support for comparative case studies can be interpreted as one 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 1% increase in the (instrumented) trade share in GDP raises income per capita by between 2% and 3% 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 twentieth 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 our context. Furthermore, geographical instruments might have direct effects on growth, thereby violating the exclusion restrictions.

Second, the possibility of combining 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, countries on average grow faster by about 1.5 percentage points after liberalizing. Another typical panel approach, the difference-in-differences estimator, is used by Slaughter (2001) to infer the effect of four very specific liberalization events on income growth dispersion; he finds no systematic link between 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 1 percentage point and raise the share of investment by almost 2 percentage points of GDP.

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

## III. Data

To anchor our results in the existing literature, we draw on a data set used recently by Giavazzi and Tabellini (2005) and Persson and Tabellini (2006). The data cover about 180 countries over the period 1963 to 2000. As a measure of economic liberalization, we use the binary indicator by Sachs and Warner (1995) as extended, updated, and revised by Wacziarg and Welch (2003, 2008)—for short, SWWW. According to this indicator, a country is considered closed in any given year if at least one of the following conditions is satisfied: (a) average tariffs exceed 40%, (b) nontariff barriers cover more than 40% of its imports, (c) it has a socialist economic system, (d) the black market premium on the exchange rate exceeds 20%, and (e) many of its exports are controlled by a state monopoly. When applying the synthetic control method in a panel setup, in line with Giavazzi and Tabellini (2005, p. 1298), we refer to the treatment as the event of economic liberalization ("comprehensive reforms that extend the scope of the market, and in particular of international markets") after experiencing a closed economy 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. It is true that the SWWW indicator is mainly defined in terms of trade policies, but in many countries contained in our sample, trade openness was accompanied by other market-oriented policy measures, and this indicator should therefore be interpreted more broadly (see the discussion in Rodriguez & Rodrik, 2001).

We are interested in estimating the effect of economic liberalization on an outcome variable reflecting economic well-being. Based on the theory, we expect economic liberalization to have a step effect that could build up over time given the lags of the economy; we do not expect divergent long-run growth paths between a liberalizing economy and its counterfactual that did not liberalize. Therefore, for the variable reflecting well-being, we use the time series of real GDP per capita (measured in 2002 US$), so as to focus on the dynamic impact of liberalization 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 posttreatment period for the outcome variable up to 2005 when necessary (that is, when liberalizations take place toward the end of the original data set).

Finally, from the original data set, we draw a set of covariates used in the literature on cross-country growth regressions (see Barro, 1991): the 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 covariates only when they are available for at least one year in the pretreatment period, which is not always the case for inflation and democracy.

## IV. Methodology: The Synthetic Control Approach

An estimation approach recently implemented for comparative case studies, the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie, Diamond, and Hainmueller (2010), can be promisingly applied to the investigation of the impact of economic liberalization. Under this approach, a weighted combination of potential control countries, 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, the 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.

Formally, 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 $T_0 < T$; the remaining $I_C$ potential control countries remain closed. The treatment effect for country $i$ at time $T_0$ can be defined as

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

where $Y_{it}(1)$, $Y_{it}(0)$ stand for the potential outcome with and without treatment, respectively. The estimand of interest is

the vector $(\tau_{i,T_0+1}, \ldots, \tau_{i,T})$. For any period $t$, the estimation of the treatment effect is complicated by the missing counterfactual $Y_{it}(0)$.

Abadie et al. (2010) show how to identify the above treatment effects under the following general model for the potential outcomes of all units:

$$Y_{jt}(0) = \delta_t + \nu_{jt}, \tag{2}$$
$$Y_{jt}(1) = \tau_{jt} + \delta_t + \nu_{jt}, \tag{3}$$

with $j = 1, \ldots, I_C + 1$. Given the treatment assignment mechanism described above, $\tau_{jt}$ is different from 0 only when $j = i$ and $t > T_0$. Besides the (dynamic) treatment effects $\tau_{jt}$, potential outcomes depend on a common factor $\delta_t$ and an error $\nu_{jt}$. Assume that $\nu_{jt}$ can be expressed by the following factor model,

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

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, $\theta_t$ is a vector of time-specific parameters, $\mu_j$ is a country-specific unobservable, $\lambda_t$ is an unknown common factor, and $\epsilon_{jt}$ are zero-mean transitory shocks. The $j$ subscript to the $Z$ vector does not impose any restriction on the covariates included in the model, which may vary with time or not and may be pre- or posttreatment, as long as they do not depend on the intervention.

In our context, because all the elements in $Z_j$ (GDP values in every pretreatment year, average population growth, secondary school enrollment, investment share, inflation, and democracy) refer to the preliberalization period, the assumption that they are not affected by the treatment means that we have to rule out anticipation effects—that these variables change in response to the anticipation of the future reform, before the liberalization actually takes place. Interestingly, the model allows the impact of unobservable country heterogeneity to vary with time, while standard difference-in-differences or fixed-effect specifications impose $\lambda_t$ to be constant over time.

Define $W = (w_1, \ldots, w_{I_C})'$ as a generic ($I_C \times 1$) vector of weights such that $w_j \geq 0$ and $\sum w_j = 1$. Each possible choice of $W$ corresponds to 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 pretreatment outcomes. Abadie et al. (2010) show that as long as we can choose $w^*$ such that

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

then

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

is an unbiased estimator of $\tau_{it}$. Condition (5) can hold exactly only if $(\bar{Y}_i^k, Z_i)$ belongs to the convex hull of $[(\bar{Y}_1^k, Z_1), \ldots, (\bar{Y}_{I_C}^k, Z_{IC})]$. Hence, in practice, the synthetic control weights $w^*$ are estimated in a completely nonparametric fashion and are selected so that condition (5) holds approximately: the distance (or pseudo-distance) between the vector of pretreatment characteristics of the treated country and the vector of the pretreatment characteristics of the potential synthetic control is minimized with respect to $w^*$ and according to a specified metric.[2] Specifically, let $X_1$ be the vector of pretreatment characteristics for the treated country and $X_0$ the matrix collecting the vectors of pretreatment characteristics of the untreated countries. The vector $w^*$ is then chosen to minimize the distance $||X_1 - X_0w||_V = \sqrt{(X_1 - X_0w)'V(X_1 - X_0w)}$, where $V$ is a $(k \times k)$ symmetric and positive semidefinite matrix. In assigning larger weights to pretreatment variables that have larger predictive power on the outcome, one possibility is to choose $V$ so that the mean squared prediction error of the outcome variable is minimized in the pretreatment period (see Abadie & Gardeazabal, 2003).

In particular, one can implement an iterative optimization procedure that searches among all (diagonal) positive semidefinite $V$-matrices and sets of $w^*$-weights for the best-fitting convex combination of the control units, where "best fitting" refers to the fit between the outcome of the treated unit and of its synthetic control before the treatment takes place (see Abadie et al., 2010).[3] Furthermore, the deviation from condition (5) imposed by this implementation process can be easily assessed and can be shown as a complementary output of the analysis.

In other words, 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 pretreatment 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 the economy) and flexibility (as the set $I_C$ of potential controls can be appropriately restricted

to make the underlying country comparisons more sensible). Furthermore, the SCM rests on identification assumptions that are weaker than those required by estimators commonly applied in the trade-growth literature. For example, while panel models control only 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. Although the SCM can handle endogeneity due to (time-varying) omitted bias, it would still suffer from reverse causation if the timing of economic liberalization were decided by expectations on future growth prospects.

A limitation of the SCM is that it does not allow assessing 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 Abadie et al. (2010) suggested, however, placebo experiments based on permutation techniques can be implemented to make inferences. 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 the placebo with the baseline results. In other words, for each of the $I_C$ potential controls, we estimate the dynamic treatment effects $\tau_{it}$, including the true treated economy in the donor pool and using the SCM as explained above. We then compare these effects with those estimated for the true treated economy. This is meant to assess whether the baseline estimates for the treated country are large relative to the effects for countries chosen at random.

## V.    Case Study Selection

Using the 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: (a) the treated country liberalized at the earliest in 1965, as we require a few preliberalization observations to calibrate the synthetic control, and (b) there exists a sufficient set of countries in the same region that remain closed for ten years past the liberalization episode (or until the end of the sample) 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: sub-Saharan Africa, Asia, Latin America, and the Middle East and North Africa. Given these requirements, we are not able to analyze OECD countries because the pool of potential comparison economies within this group

[2] Note that this distance minimization problem resembles that of covariate matching estimators in the microeconometric treatment evaluation literature, where the Mahalanobis or normalized Euclidean distances are commonly used as metrics.

[3] In principle, one could choose V without assigning any particular weight to the different covariates (say, using the Euclidean or Mahalanobis distance). However, in order to assign larger weights to the pretreatment variables that have larger predictive power on the outcome, we follow Abadie and Gardeazabal (2003) and Abadie et al. (2010) by choosing V so that the mean squared prediction error of the outcome variable (Y) is minimized in the pretreatment period. In other words, the weights $w^*$ are chosen so as to minimize the distance in the covariates space, but the distance metric is chosen so as to minimize the prediction error with respect to the outcome before the treatment. In a sense, the pretreatment period ends up being a validation period because the choice of the distance metric V is conditional on the minimization of the prediction error of the outcome over this exact period.

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) |
| Philippines | 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 | NA |
| Bhutan | NA |
| Brunei | NA |
| Cambodia | NA |
| Fiji | NA |
| Korea (Democratic People's Republic of) | NA |
| Laos | NA |
| Mongolia | NA |
| Myanmar (Burma) | NA |
| Samoa (Western) | NA |
| Solomon Islands | NA |
| Tonga | NA |
| Vanuatu | NA |
| Vietnam | NA |

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

TABLE 2.—ECONOMIC LIBERALIZATIONS IN LATIN AMERICA

| Country | Treatment Status |
| --- | --- |
| Bolivia | Always open (except 1979–1984) |
| Ecuador | Always open (except 1982–1990) |
| 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 and Tobago | Open since 1992 |
| Panama | Open since 1996 |
| Haiti | Always closed |
| Antigua | NA |
| Bahamas | NA |
| Belize | NA |
| Cuba | NA |
| Dominica | NA |
| Grenada | NA |
| St. Kitts and Nevis | NA |
| St. Lucia | NA |
| St. Vincent and Grenadines | NA |
| Suriname | NA |

Sources: 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 | NA |
| Djibouti | NA |
| Equatorial Guinea | NA |
| Eritrea | NA |
| Liberia | NA |
| Namibia | NA |
| São Tome and Principe | NA |
| Seychelles | NA |
| Somalia | NA |
| Sudan | NA |
| Swaziland | NA |
| Zaire | NA |

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

is essentially empty and countries from other regions do not match up well in terms of GDP per capita.[4]

Tables 1 through 4 provide the full picture of liberalization episodes in the regions covered by our sample. 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 comparison countries in Latin America as the trend toward greater economic liberalization essentially eliminates the intraregional control group by the end of the 1980s.

[4] We also do not consider former socialist countries, because we have analyzed the specific problems raised by transitions away from a socialist economic system in Billmeier and Nannicini (2011).

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 |

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

Based on the liberalization sequences shown in these tables, we are able to perform five comparative case studies in Asia, five in Latin America, sixteen in Africa, and four in the Middle East and North Africa, for a total of thirty experiments.[5]

Finally, we choose the pool of potential comparison countries so as to perform two different types of SCM experiment. In what we call a type A experiment, we allow the synthetic control algorithm to pick any eligible economy in the same region of the liberalizing country as a control. In type B experiments, we increase the number of potential comparison units to include eligible economies from the other (developing) regions covered by our sample. Our final sample is therefore made up of 127 (treated and comparison) countries; for each of them, we observe the (nonmissing) time series of per capita GDP from 1963 to 2005. In the online appendix, for transparency, we list all potential comparison countries included in each experiment. The trade-off between the two types of experiment is clear. Type A experiments aim at excluding rather far-fetched country comparisons by ensuring a common support between treated and comparison countries with respect to factors related to geography and possibly cultural proximity, while type B experiments increase sample size and the power of the test. We believe that exploiting the flexibility of the SCM and providing both types of results is useful to assess the robustness of our conclusions.

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[5] 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), and Nepal (1991). Latin America: Barbados (1966), Chile (1976), Colombia (1986), Costa Rica (1986), and Mexico (1986). Sub-Saharan Africa: Mauritius (1968), Botswana (1979), Gambia (1985), Ghana (1985), Guinea (1986), Guinea-Bissau (1987), Mali (1988), Uganda (1988), Benin (1990), South Africa (1991), Cape Verde (1991), Zambia (1993), Cameroon (1993), Kenya (1993), Ivory Coast (1994), and Niger (1994). Middle East and North Africa: Morocco (1984), Tunisia (1989), and Mauritania (1995), Egypt (1995).

TABLE 5.—COVARIATES AND OUTCOME MEANS—ASIA

| | Singapore 1965 | Synthetic Control A | Synthetic Control B |
|---|---|---|---|
| Population growth | 2.87 | 2.94 | 2.88 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |

Authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. The value of each predictor is averaged over the pretreatment 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: 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 macroregion; see the online appendix for the list (and relative weights) of the countries included in each synthetic control.

## VI. Economic Liberalization Episodes

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. As discussed in section V, "synthetic control A" refers to the estimated

TABLE 6.—COVARIATES AND OUTCOME MEANS—LATIN AMERICA

| | Barbados 1966 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 1986 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |

Authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. The value of each predictor is averaged over the pretreatment 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: 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

TABLE 7.—COVARIATES AND OUTCOME MEANS—AFRICA BEFORE 1987

| | Mauritius 1968 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |

Authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. The value of each predictor is averaged over the pretreatment 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: 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

counterfactual composed of a pool of countries in the same region and "synthetic control B" to a worldwide donor pool. The overall pretreatment fit is measured by the root mean square prediction error (RMSPE) of real GDP per capita. The selected covariates are included as averages in the algorithm so as to maximize the sample size, because some of them have missing values in many years. Following Abadie et al.

(2010), we use each annual observation of the pretreatment outcome variable (real GDP per capita) instead as a separate control variable in order to improve the pretreatment fit.[6] The comparisons between the posttreatment outcome of the treated unit and the synthetic control after five (GDP at $T_0 + 5$)

[6] In the tables, "pretreatment GDP" is averaged over the entire pretreatment period to provide a simple reference, but the algorithm minimizes the distance between each yearly value of GDP for the treated country and its synthetic control.

TABLE 8.—COVARIATES AND OUTCOME MEANS—AFRICA BETWEEN 1987 AND 1991

| | Guinea-Bissau 1987 | Synthetic Control A | Synthetic 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 | |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |
| | Cape Verde 1991 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |

Authors' calculations based on data in Persson and Tabellini (2006). See the notes to table 7.

TABLE 9.—COVARIATES AND OUTCOME MEANS—AFRICA AFTER 1991

| | Zambia 1993 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |

Authors' calculations based on data in Persson and Tabellini (2006). See the notes to table 7.

and ten years (GDP at $T_0 + 10$) provide the baseline estimates of two of the dynamic treatment effects.[7]

The figures represent graphically the time series of the outcome variable, real GDP per capita, for the treated unit (solid line) and the synthetic control unit (dashed line), in both the entire pretreatment period and for ten years after the treatment year ($T_0$). The comparison between the solid and dashed line before $T_0$ captures the quality of the pretreatment fit reached by the SCM algorithm; the same comparison after $T_0$ provides the (ten) dynamic treatment effects. Note that in the tables, we report the estimation results of all the experiments (type A

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[7] We acknowledge that economic reforms do not always happen overnight; sometimes they are the marginal result of a gradual shift toward more market-friendly policies. However, this measurement error, inherent in the use of the SWWW indicator, would introduce a simple attenuation bias in our results, as the effect detected by the SCM would be lower if reforms were diluted across multiple years. Moreover, because the SCM

can be implemented only with binary treatments, we believe that the advantages of this method, discussed in section IV, are worth the price in terms of measurement error.

TABLE 10.—COVARIATES AND OUTCOME MEANS—MIDDLE EAST

| | Morocco 1984 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 | Synthetic Control A | Synthetic 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 |
| Pretreatment 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 |

Authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. The value of each predictor is averaged over the pretreatment 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: 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

and type B), but, to contain space, we show the figure of only one experiment (A or B) per country. Our formal criterion to decide which experiment to present graphically 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.[8]

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

[8] While the criterion is, of course, somewhat arbitrary, the figures for the alternative experiment in each country are available on request. They are broadly consistent with the ones reported in this section, as can be also seen from the (complete) estimation results reported in tables 5 through 10.

some examples, the placebo exercises in fact confirm and reinforce the evidence, whereas in other cases, the placebo analysis reveals that the effect found in the initial assessment is rather coincidental.

Regarding the potential of reform heterogeneity and to provide some contextual background, we discuss the broader reform agenda in one country per region in somewhat more depth; the analysis is mainly based on IMF (various). We conclude with a discussion on the interpretation of the evidence provided by our synthetic control experiments.

### A. Asia

The results for Asia are presented graphically in figure 1 (SCM results) and figure 7 (placebo tests). 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%), India (23%), Nepal (23%), and Papua New Guinea (13%).[9] After the economic liberalization in 1970, however, Indonesian GDP per capita takes off and is 40% higher than the estimated counterfactual after only five years and 76% higher after ten years (see table 5). The results for Indonesia are also strongly robust to placebo testing, as none of the "fake" experiments for the eight (regional) potential comparison countries show treatment effects larger than the baseline estimates.

The economic reform backdrop in Indonesia during 1970 (and 1971) is characterized by a strong (and successful) effort to reduce inflation. While the government already ran a balanced budget, it put emphasis on improving tax assessment and collection to expand tax revenues. Notwithstanding a substantial decrease in rubber prices, a staple export, the Indonesian current account improved on the back of export promotion measures and wood-related exports. From a liberalization perspective, the main event was the exchange reform introduced in April 1970. However, Indonesia continued to maintain a 10% export tax, albeit with numerous exceptions.

For the other four liberalization episodes in Asia, the intraregional match is not particularly good (see the RMSPEs in table 5). This shows why it is useful to enlarge the pool of potential control economies to the worldwide sample of closed economies (type B experiment), as this step helps to regain comparable GDP levels in the comparison countries. Figure 5 shows that South Korea (1968) has a success story similar to that of Indonesia, with income about twice as high as in the counterfactual case after ten 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

[9] See the online appendix for the complete list of potential comparison countries, as well as for the control units included by the algorithm in each synthetic control (with their relative weights).

FIGURE 1.—GDP TRENDS, TREATED COUNTRY VERSUS SYNTHETIC CONTROL—ASIA
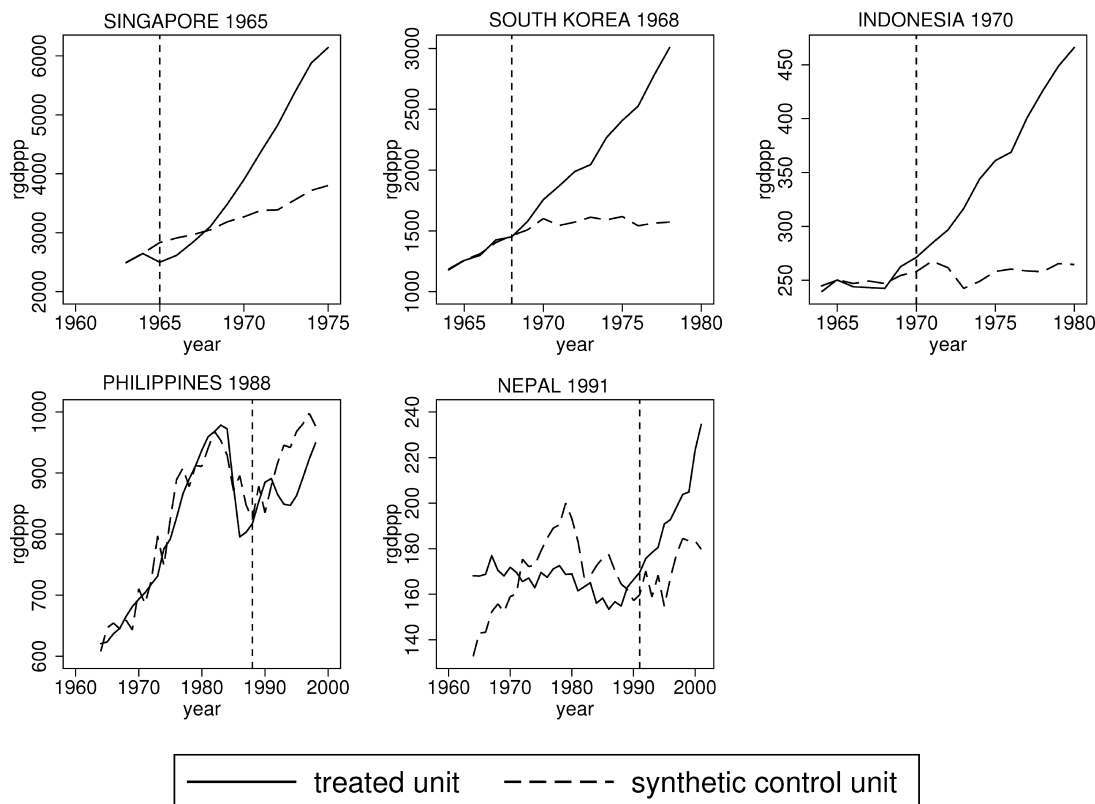


Authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. Synthetic 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

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 dramatically between 1965 and 1967. For both countries, the placebo test confirms the validity of the SCM results, as none of the 42 fake experiments shows stronger results than the baseline.[10]

The later liberalization episodes—Philippines (1988) and Nepal (1991)— either did not lead to a significantly better trajectory than in the estimated counterfactual (in the case of the Philippines), or it is not clear to what extent the 30% income difference after ten years in favor of the liberalized economy (Nepal) is attributable to the economic liberalization as the steep income increase starts a couple of years before the liberalization year according to the SWWW indicator, and the placebo test for Nepal reported in figure 7 is not particularly robust.[11]

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[10] The number of potential comparison countries, and therefore of placebo exercises, is the same for South Korea and Singapore because they both liberalized in 1965.

[11] In fact, although the placebo test for Nepal is clearly less robust than those for Indonesia, Singapore, and South Korea, only three of twenty placebo units (15%) showed a higher treatment effect than Nepal; it is only because of a scale effect (the above three economies being much richer

### B. Latin America

Evidence on the liberalization episodes in Latin America is shown in figure 2 (SCM results) and figure 7 (placebo tests). In this region, economic liberalization episodes that can be analyzed in our framework took place rather early— between 1966 (Barbados) and 1986 (Colombia, Costa Rica, and Mexico). In all countries, the regional synthetic controls (type A experiment) provide a fine match: On average, over the years before liberalization, the income of the synthetic control diverges by less than 2% from the liberalizing country's income (see table 6); the only exception is Chile (1976), where the preliberalization drop in GDP (coinciding with the Pinochet coup) makes it difficult for the SCM algorithm to find a suitable counterfactual and reduces the inferential value of this experiment because of the poor pretreatment fit.

In Chile, the implementation of the SCM is hampered by the exceptional events around the Pinochet coup in September 1973. Yet, some lessons can be learned from this country episode. During the Unidad Popular government (1970–1973), the state assumed a dominating role in the economy by controlling prices, interest rates, credit, and capital

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than Nepal) that this fact overshadows the other (robust) seventeen placebo experiments in figure 7.

FIGURE 2.—GDP TRENDS, TREATED COUNTRY VERSUS SYNTHETIC CONTROL—LATIN AMERICA
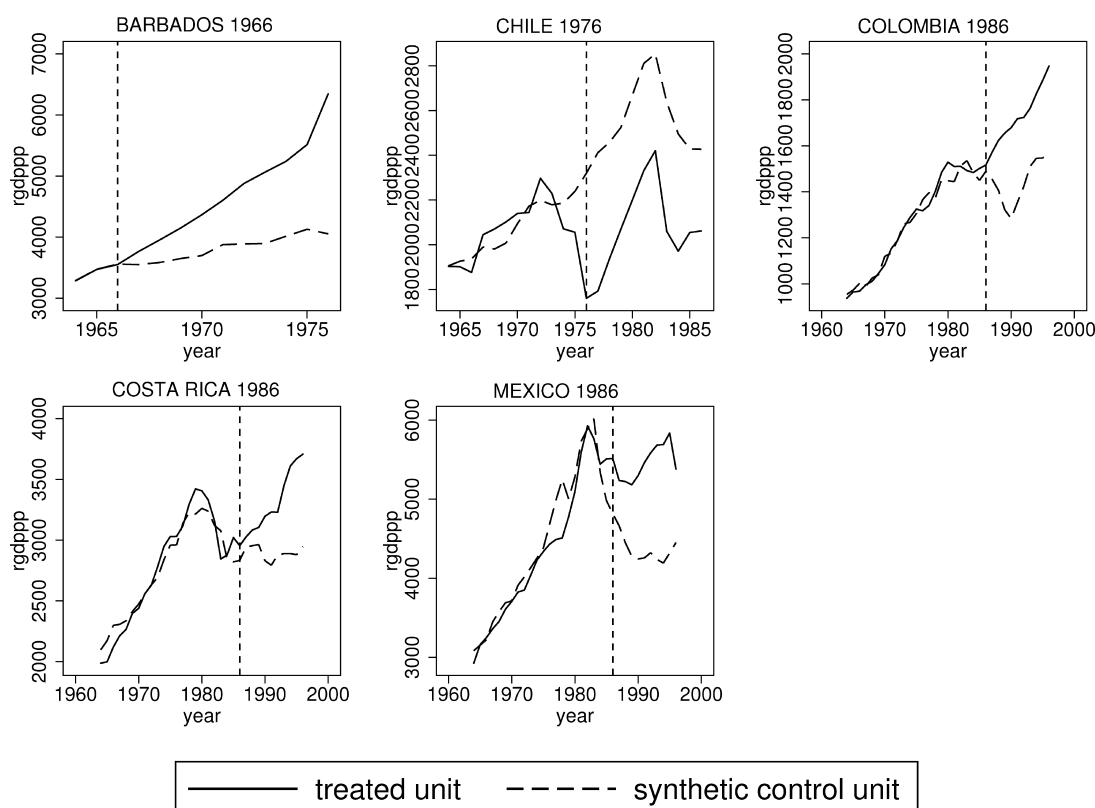


Authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. Synthetic 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

movements. Expansionary fiscal policies, including due to a strongly rising public payroll, led to a hefty increase in the budget deficit (on the order of 20% of GDP in 1973). After the 1973 coup, the widespread removal of price controls provoked a surge to triple-digit inflation rates and, reinforced by the wage adjustment formula, a wage-price spiral during the following three years. Chile's position was also weakened by the sharp downturn in world copper prices in 1974 and 1975. Over the 1973–1975 period, Chile's output fell dramatically, and unemployment rose from about 3% to around 18% as public sector employment suffered from significant demand retrenchment. In 1976, Chile started to rebound, helped by rising copper prices, and embarked on second-round economic liberalization measures geared toward the external sector, which included rationalizing the import duty system and lowering marginal rates while also reducing nontariff barriers and removing the multiple exchange rate system. Looking at figure 2, it could be argued that the economic policy measures taken in 1975–1976, including economic liberalization, turned income growth around and put it on a parallel track to the estimated synthetic control (which mainly consists of Uruguay and Honduras).

Barbados, Colombia, Costa Rica, and Mexico are excellent examples of a positive and robust impact of economic liberalization (see both figure 2 and table 6). Ten years after liberalization, GDP per capita is about 57% higher than that of the regional synthetic control in Barbados, 23% in Colombia, 26% in Costa Rica, and 21% in Mexico. The placebo tests confirm that the SCM results are largely robust for these countries (see figure 7). For Barbados and Mexico, none of the fake experiments in the potential controls is above the effect in the treated country, while this happens for only 1 case (out of 24) in Colombia and 1 case (out of 9) in Costa Rica.

Summing up, the feasible country experiments in Latin America, which took place mainly in the 1980s, lead to the conclusion that economic liberalization had a positive impact on income per capita.

## C. Africa

The sixteen economic liberalization episodes that we can analyze in Africa under the SCM framework occurred 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 two conclusions.

First, the empirical evidence seems to indicate four groups: (a) treated countries outperforming the estimated counterfactual with strong support from the placebo tests, (b) outperformance with some support from the placebo tests,

FIGURE 3.—GDP TRENDS, TREATED COUNTRY VERSUS SYNTHETIC CONTROL—AFRICA BEFORE 1987
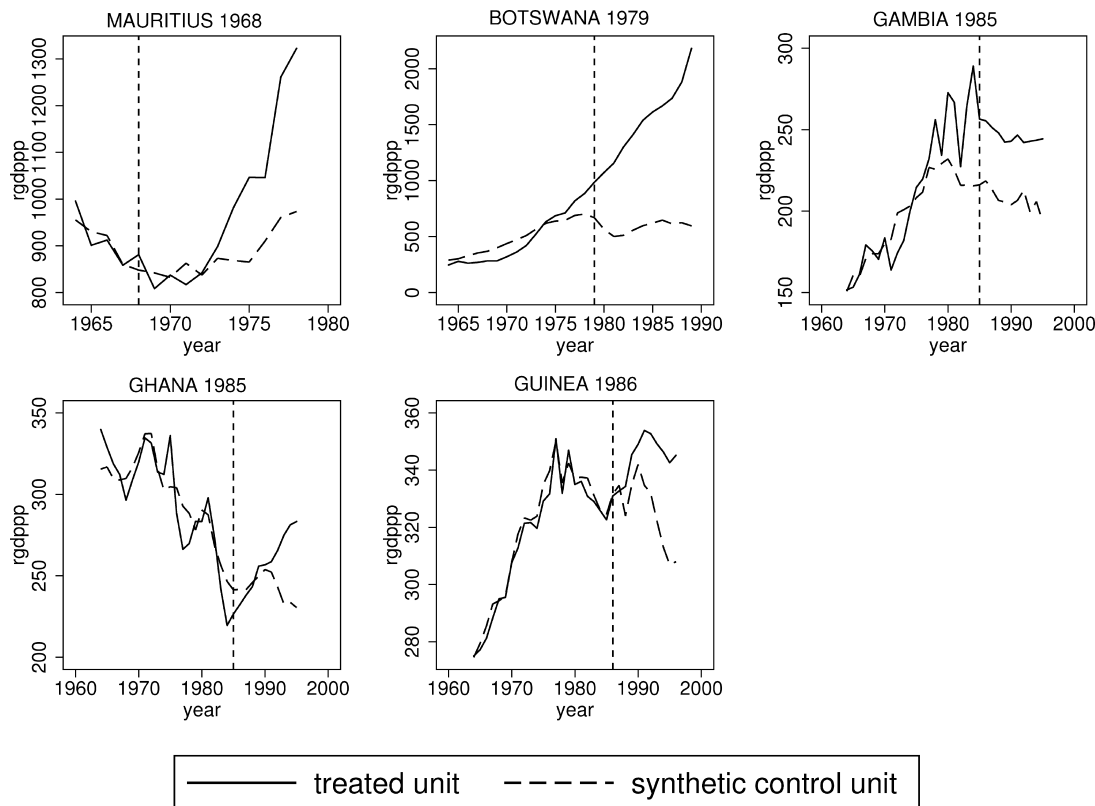


Authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. Synthetic 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

(c) strong economic rebound but without significant outperformance, and (d) limited effect. Botswana is the only country clearly in the first group, offering a truly convincing success story.[12] In particular, Botswana fared substantially better over the ten years after the liberalization (1979) than the synthetic control in the type B experiment (see figure 3), and this is also confirmed by the type A experiment (see table 7). In fact, the income per capita in Botswana is about five times as high as the one of the synthetic control ten years after liberalizing the economy. This preliminary conclusion is strongly reinforced by the placebo test (see figure 8): the bold line is higher than any other of the 47 permutations, indicating that the baseline results are not driven by random chance and capture the true treatment effect of economic liberalization in this country.

Countries in the second group have outperformed the respective counterfactual, and the placebo tests (see figure 8) lend good support to this conclusion as only very few placebo permutations are above the treated country ten years after

liberalization. This group includes Benin (3 out of 19 permutations above the treated economy), Ghana (3/24), Guinea (5/24), Guinea-Bissau (4/21), Mauritius (5/62), and Uganda (5/21).[13] In the late 1980s, Benin began a process of general economic liberalization after many years of political oppression, which had left the country in ruins: a collapsed banking system, public enterprises under distress, large internal and external financing gaps, and arrears on both accounts. This situation led to political unrest and strikes, triggering a national convention in February 1990, which led to free elections in early 1991. The economic program adopted by the national convention aimed at encouraging private sector activity, reducing internal and external imbalances, containing cost pressures by strong demand management and a reduction of administrative measures and the role of the state in the economy more broadly (including via privatization of state-owned enterprises). Good progress in initiating structural reforms was initially hampered by the severe financial crisis, but the economy turned around in 1990 and recorded

[12] We refrain from commenting further on Botswana as its success has been well documented in the literature. See Acemoglu, Johnson, and Robinson (2003), who point to the interaction between solid institutions and sound economic policy as key ingredients of the country's economic success.

[13] Note that the key information content of the placebo exercises resides in the number of dashed lines above the bold one, not in the magnitude or difference between the true and the fake effects. In other words, a result is more significant in the placebo test sense if only very few dashed lines are above the treated country, even if by a lot, as opposed to many dashed lines by a small amount.

FIGURE 4.—GDP TRENDS, TREATED COUNTRY VERSUS SYNTHETIC CONTROL—AFRICA BETWEEN 1987 AND 1991
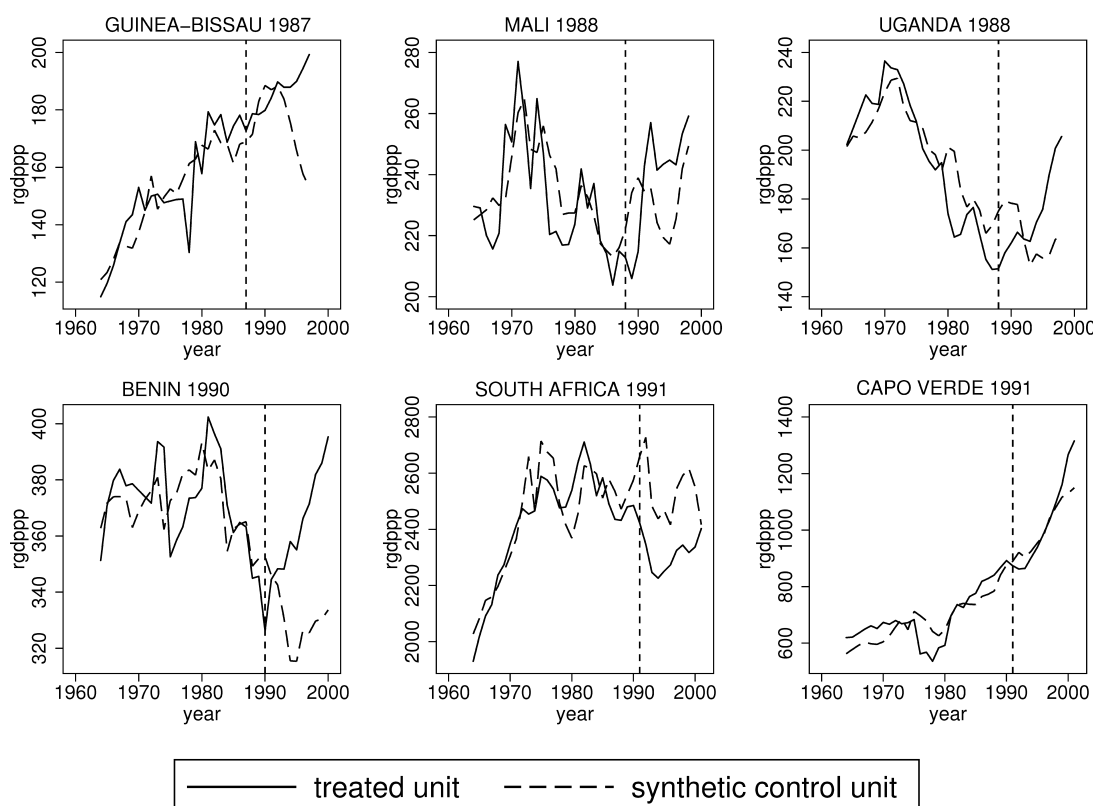


Authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. Synthetic control A for Guinea-Bissau, Mali, Uganda, and Benin; synthetic control B for South Africa and Cape Verde. See tables 1 through 4 for the list of potential controls in each macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

positive growth. The main measures from a trade liberalization perspective included eliminating the import license requirements for goods from certain countries, as well as the renewal of trading licenses for commercial importers. Moreover, a program for tariff reform was introduced.

The third group, characterized by strong rebounds (or expansions) in income per capita around the liberalization year but without outperformance of the counterfactual, contains Mali, Cape Verde and possibly Ivory Coast. Given the mixed empirical message and to contain space, we refrain from showing the corresponding placebo tests. Finally, the fourth group contains Cameroon, Gambia, Kenya, Niger, South Africa, and Zambia, as well as Ivory Coast to the extent that this country seems to be a candidate for both groups 3 and 4. None of these countries seems to have benefited from economic liberalization: real income per capita either stagnated or even declined over the ten years after economic liberalization.

The second conclusion we draw, restating the previous point regarding group 4 differently, is that the positive evidence seems to be concentrated in the first part of the sample, while the effect of economic liberalization in Africa has little to no discernible positive effect after around 1990. Ever since, liberalization either stops a decline in income whereas the counterfactual grows steadily during the postliberalization

period (Cameroon, Niger), or has no apparent impact on income levels (Kenya, South Africa).[14] In Zambia, even after economic liberalization in 1993, the income level continued to decline on a per capita basis. One somewhat positive example of a rather late liberalization is Cape Verde (1991), with income per capita about 15% higher than that of the synthetic control after ten years. That said, the divergence from the counterfactual is very slow to emerge, weakening the causal link to economic liberalization.[15]

Summing up, in sub-Saharan Africa, it seems to broadly be the case that only early liberalizations had a positive impact on income per capita, while almost all of the late attempts

[14] An earlier reformer, Gambia, also displays this pattern. We consider Gambia a special case, however, because the volatility of per capita income right before liberalization makes the construction of the counterfactual particularly difficult, therefore reducing the inferential value of this experiment.

[15] Note that the type B experiment for Cape Verde provides a particularly far-fetched county match, as the estimated counterfactual is mainly composed of China. Results should thus be interpreted with care. We believe, however, that this exemplifies one of the advantages of the SCM identified above. By being able to identify the countries actually chosen by the algorithm, we can discuss the meaningfulness and robustness of each empirical exercise using additional, case study–type considerations. Other estimators commonly used in the literature might well be based on the same country comparisons or, worse, on parametric extrapolation, but often do not make these comparisons explicit, which therefore cannot be assessed (see Billmeier & Nannicini, 2009).

FIGURE 5.—GDP TRENDS, TREATED COUNTRY VERSUS SYNTHETIC CONTROL—AFRICA AFTER 1991
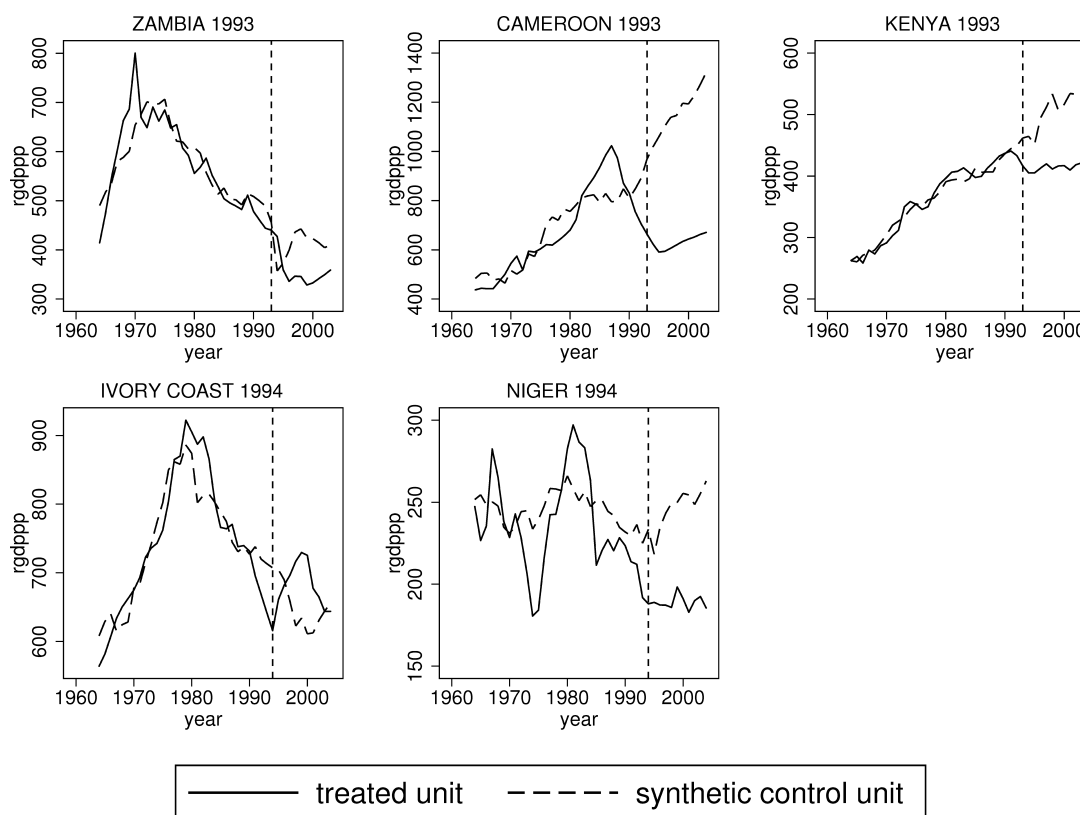


Authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. Synthetic 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

did not benefit the liberalizing country much. An alternative explanation of the zero findings in the 1990s, however, might be that late liberalizers in Africa adopted gradual reform strategies, leading to attenuation bias in the results.[16]

### D. 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 tests). 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. But after ten years, only in Morocco (1984) has liberalization contributed to a somewhat higher income level than in the synthetic control (see table 10). This positive effect, however, is not particularly robust to placebo testing, as four out of eleven permutations are either above or essentially identical to the baseline effect in the treated country. Since the late 1970s, Morocco had built up large internal and external imbalances due to expansionary policies in the context of deteriorating terms of trade, bad harvests, and structural weaknesses in the economy. Under the auspices of

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[16] See the discussion in note 7.

an IMF program, the country made good progress in reducing the balance-of-payments gap sharply curtailing government spending and implementing tight incomes and credit policies in 1984. Moreover, the country also pursued a substantial trade and price liberalization. Apart from the reduction in import tax and tariffs, the import restrictions that had been severely tightened in the previous year were eased in January and July 1984. As a result, the share of unrestricted imports in total imports rose to almost 50%. In addition, economic liberalization included eliminating the import deposit scheme and the state export monopoly for processed foods, as well as the export licensing requirement for industrial and most agricultural products.

In the three other countries where liberalization took place rather late (Tunisia 1989, Mauritania 1995, Egypt 1995), however, the liberalizing country actually fares worse than the regional synthetic control both five and ten years after liberalization.

### E. What Changed in the 1990s?

The analysis in the preceding sections indicates that economic liberalization in some countries is associated with a remarkable positive effect on real income. However, we find

FIGURE 6.—GDP TRENDS, TREATED COUNTRY VERSUS SYNTHETIC CONTROL—MIDDLE EAST
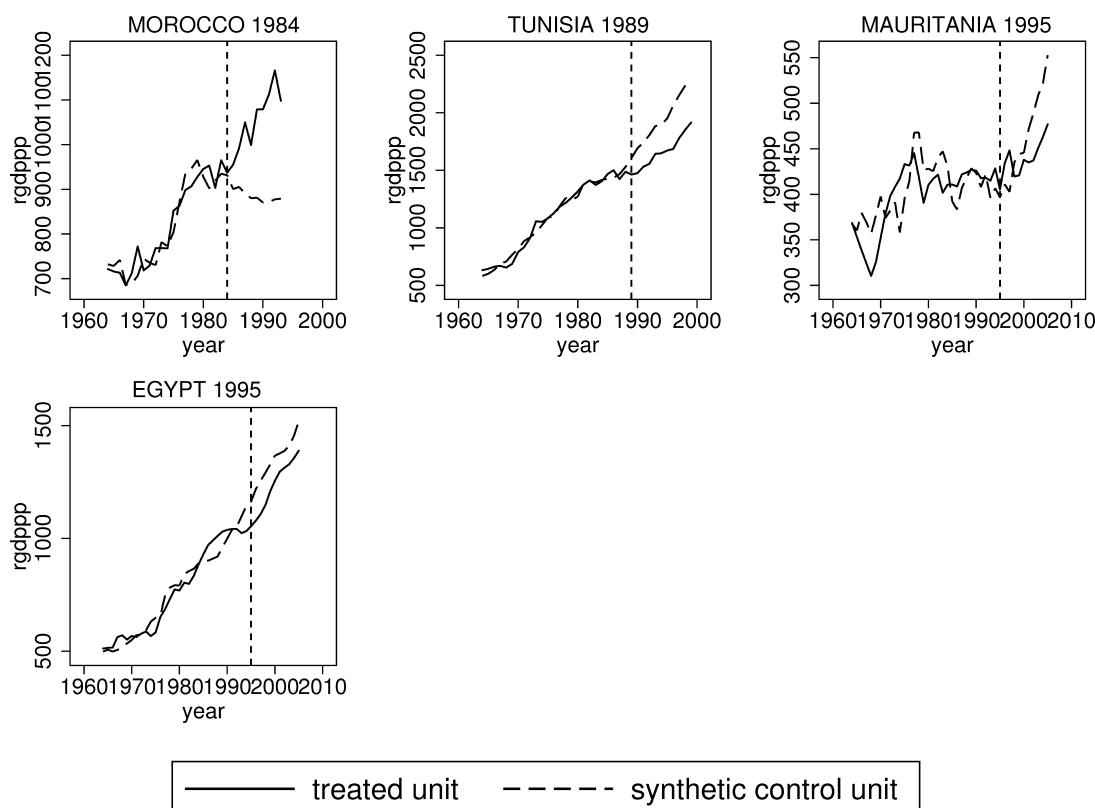


Authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pretreatment real GDP per capita. Synthetic 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 macroregion. See the online appendix for the list (and relative weights) of the countries included in each synthetic control.

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 them located in Africa, did not benefit from these reforms in terms of higher GDP per capita compared to similar, but closed economies. Why is this?

We speculate that one explanation could reside in a timing effect of economic liberalization. In our view, this timing effect is slightly different, but possibly related to the ones found in the literature by Giavazzi and Tabellini (2005), who document that countries that liberalize the economy before becoming democracies do substantially better than those that follow the opposite track, and by Wood (1997), who shows a time effect in the interaction between trade openness and wage inequality.[17] In our view, the timing argument could also work as follows. If a developing economy liberalizes early on, especially in terms of trade openness, it can still reap the gains of specialization; once "globalization" kicks

in (and an increasing number of countries liberalize their economy), there is much more competition for capital, but also for the labor-intensive goods a developing economy can specialize in, such as agriculture or textiles.[18] Hence, the benefits from liberalization after globalization are smaller.[19] The SCM evidence presented above is consistent with the history of economic liberalization in the developing world: Asian economies liberalized early on, and this allowed 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). Finally, once the liberalization

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[17] Focusing on the skilled versus unskilled labor content of trade, Wood (1997) shows that wage inequality shrinks in East Asia in the 1960s and 1970s with increasing international trade—consistent with the conventional wisdom according to Heckscher-Ohlin—while inequality rises with trade in Latin America in the 1980s and early 1990s. He ascribes the differences mainly to temporal factors (for example, the entry of China in the global labor market, technological progress biased against unskilled workers) rather than geographical differences (for example, resource endowments, different trade policies).

[18] 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, such as trade flows, would not capture more recent aspects of globalization that accelerated in the late 1980s and 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 it become available only over time. See Bhandari and Heshmati (2005) for an attempt to quantify globalization over the (short) period 1995 to 2001.

[19] 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).

FIGURE 7.—PLACEBO EXPERIMENTS—ASIA AND LATIN AMERICA
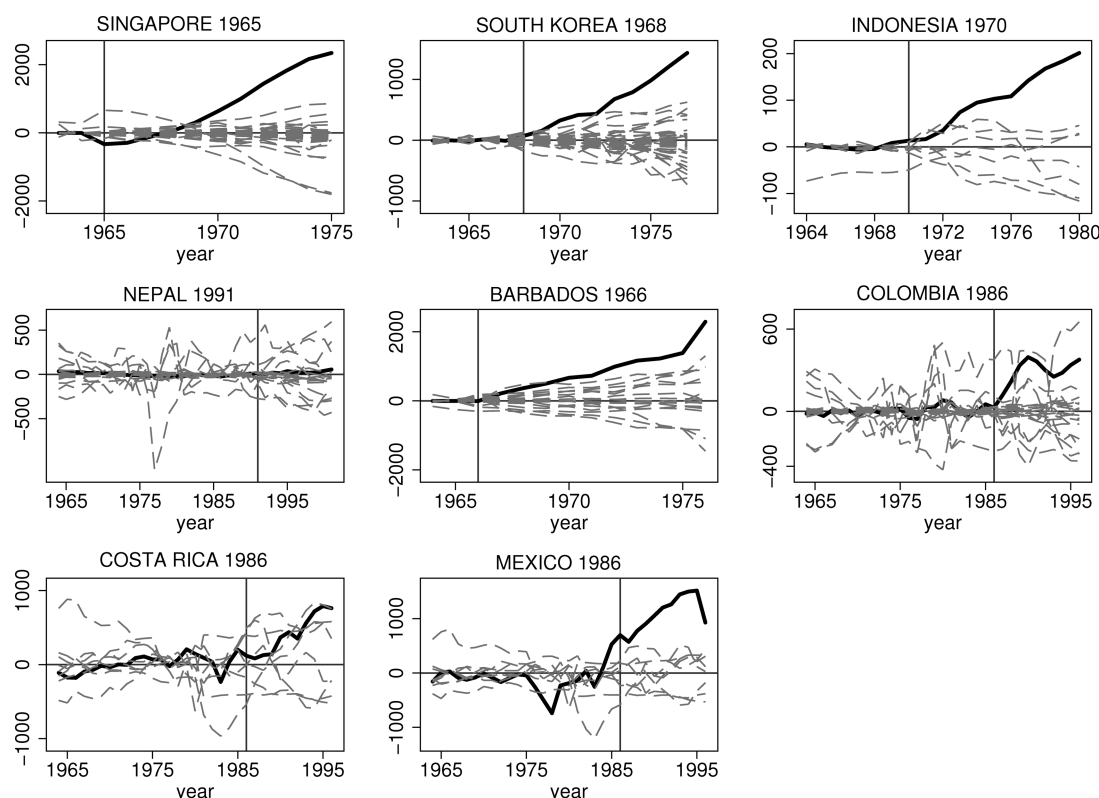


Authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between each treated country and its synthetic control. Dashed lines: outcome difference between each of the treated country's potential controls and their synthetic control in placebo experiments. Outcome: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

wave hit Africa, the benefits from joining the club of liberalized economies had become smaller, as other countries that had liberalized somewhat earlier did not move up the specialization ladder.

The above timing argument, however, is not the only possible interpretation of our empirical results. A second explanation, which is again speculative but broadly consistent with the SCM evidence, 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, Johnson, and Robinson (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, such as economic liberalization, could still lack the positive interaction with other favorable and complementary policies, such as investment in physical and human capital or property rights protection (see Rodrik, 1999, 2005), because of low-quality institutions.[20]

[20] 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

## VII. Conclusion

In this paper, we explore the effect of economic liberalization on income 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 and descriptive case studies. This methodology, the synthetic control method, compares a treated (liberalized) country with an estimated (closed) counterfactual. The peculiarity of this method rests in the fact that the counterfactual is a linear combination of comparison units that are similar to the treated economy along covariates traditionally used in the literature and pretreatment realizations of the outcome variable.

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. We then broaden the pool of eligible controls to improve the pretreatment fit between the treated country and the estimated counterfactual.

benefits) and, in developing economies, more basic institutional reforms including anticorruption, labor, and financial markets.

FIGURE 8.—PLACEBO EXPERIMENTS—AFRICA AND MIDDLE EAST
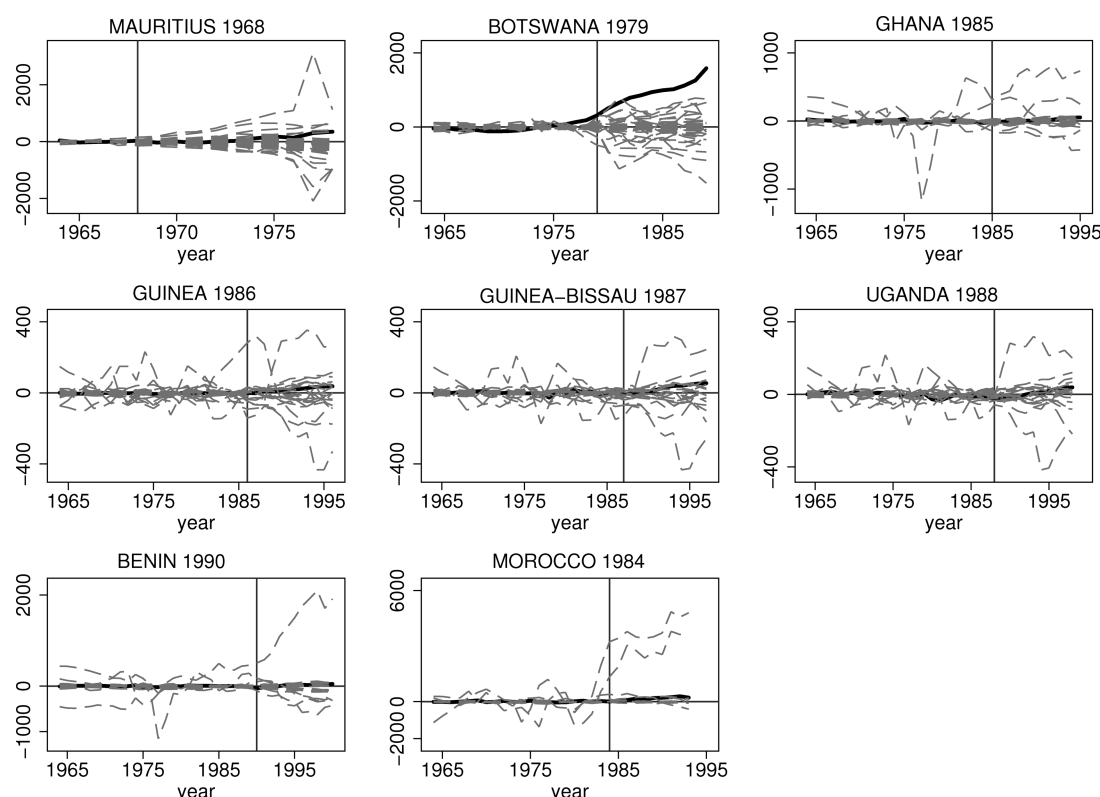


Authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between each treated country and its synthetic control. Dashed lines: outcome difference between each of the treated country's potential controls and their synthetic control in placebo experiments. Outcome: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

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 the trajectory of real income per capita.

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. Especially in sub-Saharan Africa and in the MENA region, where a number of liberalization episodes took place in the 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.

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