Trade Liberalization and Child Mortality: A Synthetic Control Method*

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Abstract. This paper investigates the causal effect of trade liberalization on children mortality by exploiting 40 policy reform experiments spanning the 1960-2010 period. We use a new approach – the Synthetic Control Method – for comparative case studies. Using this method we assess at the country level the trajectory of post-reform health outcomes of treated countries, which experienced a trade liberalization, with the trajectory of a combination of similar untreated countries. Contrary to previous findings, we showed that the effect of trade liberalization on health outcomes display a huge heterogeneity both in the direction and the magnitude of the effect. Among the 40 investigated case studies, 20 displayed a reduction in children mortality after a trade liberalization, and the majority of these are statistically significant. In 18 country case-studies we did not find any relevant effect, while in two cases – South Africa and Mauritania – we found a strong statistically significant worsening in child mortality after trade liberalization. Yet, the underline reasons of these negative effects are driven by very different situations.

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1. Introduction

The impact of globalization and trade liberalization on welfare and poverty remains controversial (Harrison, 2006; Ravallion, 2009). While several economic studies show that open trade enhances growth (e.g. Dollar, 1992; Sachs and Warner, 1995; Giavazzi and Tabellini, 2008; Wacziarg and Welch, 2008; Bilmeier and Nannicini, 2013), the impact on poverty and inequality is much less clear (e.g. Topolova, 2010; Anukriti and Kumler, 2014). In an elaborate review of the evidence, Winters et al. (2004) conclude that "there can be no simple general conclusions about the relationship between trade liberalization and poverty". In a recent update Winters and Martuscelli (2014) argue that this conclusion still holds.[1]

In this paper we study the impact of trade liberalization on health, and more specifically child mortality. While children's health is an important indicator of welfare and development, it is also an important end in its own right (Sen, 1999). Moreover health is also itself important for economic growth (Levine and Rothman, 2006).

Despite the importance of the issue (and the extensive literature discussing the issue and the mechanisms through which trade may affect health)[2] there are only two studies that quantitatively assess the impact of trade on health. Levine and Rothman (2006) use a cross-country analysis to measure the (long-run) effect of trade on life expectancy and child mortality. Because trade can be endogenous to income and health, they follow Frankel and Romer's (1999) approach of exploiting the exogenous component of trade predicted from a gravity model. They find that trade significantly improves health outcomes, although the effect tends to be weaker and often insignificant when they control for countries' income levels. They conclude that the main channel through which trade openness improves health is through enhanced incomes.

The second study, by Owen and Wu (2007), uses panel data econometrics. Controlling for income and other observed and unobserved determinants of health through fixed effects, they find that trade openness improves life expectancy and child mortality in a panel of more than 200 developed and developing countries. They also find evidence suggesting that some of the positive correlation between trade and health can be attributed

[1] See also Goldberg et al. (2007) for an extensive review on the distributional effects of globalization in developing countries.

[2] Deaton (2004), Levine and Rothman (2006), Owen and Wu (2007), Cornia et al. (2008) and Blouin et al. (2009) among others, identify several mechanisms through which trade liberalization may affect health. These include through its impact on economic growth, poverty and inequality, public expenditures, environmental quality, urbanization, the spread of diseases, cultural influences, dietary changes, food price level, knowledge spillovers, fertility rate, etc.

to knowledge spillovers – an hypothesis previously advanced by Deaton (2004). However, also in their analysis the impact is not always robust. For example, when the authors work with a sub-sample of only developing countries, the trade on health effect is weaker, and not significant when child mortality is considered.

Given the fact that trade can affect health and in particular child mortality through different channels and the conclusion of Winters et al. (2004) that the impact of trade liberalization can be different under different economic and institutional conditions, the average effects as measured by the previous studies may hide important heterogeneity among countries and regions.

To analyze this we use a different empirical methodology, the Synthetic Control Method (SCM) recently developed by Abadie and Gardeazabal (2003) and by Abadie et al. (2010). Billmeier and Nannicini (2013) applied the SCM to study the relation between trade liberalization and growth. Our approach follows their application of the SCM. The SCM allows choosing the *best* comparison units in comparative case studies. Using this approach, we compare the post-reform child mortality of countries that experienced a trade liberalization – *treated* countries – with child mortality of a combination of similar, but *untreated* countries. Using this method, we assess separately (i.e. at the country level) the health impacts of 40 trade liberalization cases which occurred during the 1960-2010 period.

This approach thus allows to explicitly identify the heterogeneity of the effects. The SCM methodology adds flexibility and transparency in the selection of the counterfactual, and thus improves the comparability between treated and untreated units. Importantly, SCM also accounts for endogeneity bias due to omitted variables by accounting for the presence of *time-varying* unobservable confounders. Another advantage is that it allows separating short-run versus long-run effects, an issue not formally addressed by previous studies but of particular relevance when the focus of the analysis is the effect of trade reforms (Billmeier and Nannicini, 2013).

As an indicator of health we use child mortality because the data on child mortality are more precise and, importantly, they are available with a yearly dimension. Indeed, other standard health indicators available worldwide, like the life expectancy at bird, other than derived from infant and child mortality (see Lopez et al. 2000), are estimated only every five years, and as such cannot be used as output variable in a SCM experiment.[3]

We find that in the 40 investigated case studies, 20 (50%) showed a short- and long-run reduction in child mortality rate after the trade liberalization, with an average effect of about 17 percentage points (in comparison to the non reforming control situation). For 18 cases (45%) we do not find a significant effect. In 2 cases (5%) we find a significant increase in child mortality after trade liberalization.

The remainder of the paper is organized as follows. In the next Section the synthetic control method will be presented and discussed. Section 3 presents the data on trade policy reforms, child mortality and other covariates used in the empirical exercise. In Section 4 the main results will be presented and discussed. Section 5 presents some robustness checks. Section 6 concludes.

2. The Synthetic Control Method

Consider a panel of $I_C + 1$ countries over T periods, where country i changes its trade policy at time $T_0 < T$, and all the other countries of I_C remain closed to international trade, thus representing a sample of potential control. The treatment effect for country i at time t can be defined as follows:

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

where $Y_{it}(T)$ represents the potential outcome associated with $T \in \{0,1\}$, that in our application refers to the level of under five mortality rate in an economy closed (0) or open (1) to international trade, respectively. The statistic of interest is the vector of dynamic treatment effects $(\tau_{i,T0+1}, \dots, \tau_{i,T})$. As is well known from the literature, in any period $t > T_0$ the estimation of the treatment effect is complicated by the lack of the counterfactual outcome, $Y_{it}(0)$. To circumvent this problem, the SCM identifies the above treatment effects under the following general model for potential outcomes (Abadie et al. 2010):

(2) $$Y_{jt}(0) = \delta_t + X_j\theta_t + \lambda_t\mu_j + \varepsilon_{jt}$$

where δ_t is an unknown common factor with constant factor loadings across units; X_j is a vector of relevant observed covariates (not affected by the intervention) and θ_t the related

[3] For a more general discussion about the weakness of life expectancy as health indicator, see Deaton (2006).

vector of parameters; μ_j is a country specific unobservable, with λ_t representing the unknown common factor;[4] finally, ε_{jt} are transitory shocks with zero mean. As explained further, the variables that we include in the vector X_j (e.g. real per capita GDP, population growth, fraction of rural population, frequency of wars and conflicts, female primary education) refer to the pre-treatment period. Thus the assumption that they are not affected by the treatment (trade liberalization) means that we have ruled out "anticipation" effects (see Abadie, 2013).[5]

Next, define $W = (w_1, \dots, w_{I_C})'$ as a generic ($I_C \times 1$) vector of weights such that $w_j \geq 0$ and $\sum w_j = 1$. Each value of W represents a potential synthetic control for country *i*. Moreover, define $\bar{Y}_j^k = \sum_{s=1}^{T_0} k_s Y_{js}$ as a generic linear combination of pre-treatment outcomes. Abadie et al. (2010) showed that, as long as one can choose W^*such that

(3) $$\sum_{J=1}^{I_C} w_j^* \bar{Y}_j^k = \bar{Y}_i^k \quad \text{and} \quad \sum_{J=1}^{I_C} w_j^* X_j = X_i,$$

then

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

is an unbiased estimator of the average treatment effect, τ_{it}.

Note that condition (3) can hold exactly only if $(\bar{Y}_j^k, X_j)$ belongs to the convex hull of $[(\bar{Y}_1^k, X_j), \dots, (\bar{Y}_{I_C}^k, X_{I_C})]$. However, in practice, the synthetic control W^* is selected so that condition (3) holds approximately. This is obtained by minimizing the distance between the vector of pre-treatment characteristics of the treated country and the vector of the pre-treatment characteristics of the potential synthetic control, with respect to W^*, according to a specific metric.[6] Then, any deviation from condition (3) imposed by this procedure can be evaluated in the data, and represents a part of the SCM output.

Hence, the general idea behind the SCM is to select a weighted combination of (untreated) countries, called the synthetic control, with the aim of minimizing the differences between the treated and the untreated countries according to some salient characteristics (the variables included in the vector X_j). The construction of the synthetic

[4] Note that standard difference-in-differences approach set λ_t to be constant across time. Differently, the SCM allows the impact of unobservable country heterogeneity to vary over time.

[5] Namely that those covariates immediately change in response to the anticipation of the future reform.

[6] Abadie et al. (2010) choose W^* as the value of W that minimizes: $\sum_{m=1}^{k} v_m (X_{1m} - X_{0m}W)^2$, where v_m is a weight that reflects the relative importance that we assign to the *m*-th variable when we measure the discrepancy between X_1 and X_0W. Typically, these weights are selected in accordance to the covariates' predictive power on the outcome.

control is done by considering the pre-treatment period, namely the years *before* the trade reform. Then, by comparing the trend in the outcome variable (under-five mortality rate) between the synthetic control and the treated country in the years *after* the reform, we can establish the extent to which the treated country behaves differently from its (synthetic) counterfactual.

The SCM has three key advantages in comparison to other parametric and semi-parametric estimators. First, it is transparent, as the weights W^* identify the countries that are used to estimate the counterfactual outcome of the country liberalizing trade. Second, it is flexible, because the set of I_C potential controls, the so-called *donor pool* in the language of Abadie et al. (2010), can be restricted to make underlying country comparisons more appropriate. Finally, it is based on identification assumptions that are weaker than those normally used by standard estimators – i.e. the difference-in-difference – because it allows for the effect of unobservable confounding factors to be time variant.

However, there are also drawbacks. First, as in the program evaluation literature, our estimator does not distinguish between direct and indirect causal effects of trade liberalization on health outcomes. A second issue is due to the small number of observations involved in such comparative case studies, which translates to the impossibility to use standard inferential techniques to assess the significance of the results. To overcome this limitation, Abadie et al. (2010) suggested the use of placebo tests, which consist of comparing the magnitude of the estimated effect on the treated country, with the size of those obtained by assigning the treatment randomly to any (untreated) country of the donor pool. In our analysis, we applied both the standard placebo tests and a generalization proposed by Cavallo et al. (2013) and summarized below.

2.1 Average Effect and Inference with SCM

In the previous SCM applications the analysis of the results and the respective placebo tests, have been largely conducted at the level of (each) single country case-study. However, when the analysis covers many countries, it is interesting to measure average treatment effects across a specific group of countries. Such measure is developed by Cavallo et al. (2013). Denote by $\left(\hat{\tau}_{1,T0+1}, \ldots, \hat{\tau}_{1,T}\right)$ a specific estimate of the trade liberalization effects on child mortality of the country of interest 1 and consider the average trade liberalization effects across G countries of interest. The estimated average effect for these G trade reforms can then be computed as:

(5) $$\bar{\tau} = (\bar{\tau}_{T0+1}, \dots, \bar{\tau}_T) = G^{-1} \sum_{g=1}^{G} (\hat{\tau}_{g,T0+1}, \dots, \hat{\tau}_{g,T}).$$

Note also that, because the size of the country specific effect will depend on the level of the child mortality rate, one needs to normalize the estimates before aggregating the individual country effects.[7]

To estimate the extent to which this (dynamic) average treatment effect is statistically significant, we follow Cavallo et al. (2013). Like Abadie et al. (2010), these authors used inference techniques similar to permutation tests that allow consistent inference measurement regardless of the number of available controls or pre-treatment periods, although the precision of inference clearly increases with the number of controls. The logic is to first apply the SCM algorithm to every potential control in the donor pool. Then one evaluates whether the estimated effect of the treated country outperforms the ones of these fake experiments.

For example, if one wants to measure inference for the trade liberalization effect on child mortality for each of the ten post-reform years, it is possible to compute the year-specific significant level, namely the *p*–value, for the estimated trade reform effect as follows:

(6) $$p\text{−value}_t = \Pr\left(\hat{\tau}_{1,t}^{PL} < \hat{\tau}_{1,t}\right) = \frac{\sum_{j=2}^{J+1} I\left(\hat{\tau}_{1,t}^{PL_j} < \hat{\tau}_{1,t}\right)}{\#\text{ of controls}}.$$

Where $\hat{\tau}_{1,t}^{PL_j}$ is the year specific effect of trade reform when control country *j* is assigned a placebo reform at the same time as the treated country 1, and is calculated using the same algorithm outlined for $\hat{\tau}_{1,t}$. The operation is performed for every country *j* of the donor pool to build the distribution of the fake experiments so as to evaluate how the estimate $\hat{\tau}_{1,t}$ is positioned in that distribution. Finally, because we are interested in valid inferences on $\bar{\tau}$, we can compute the year *t* specific *p*-value for the average effect as

(7) $$p\text{−value}_t = \Pr\left(G^{-1} \sum_{g=1}^{G} \hat{\tau}_{g,t}^{PL} < \bar{\tau}_{1,t}\right) = \Pr\ (\bar{\tau}_t^{PL} < \bar{\tau}_t).$$ [8]

3. Data, Measures and Sample Selection

The first issue is related to how we measure trade liberalization episodes. Following the cross-country growth literature, we use the binary indicator of Sachs and Warner (1995) as recently revisited, corrected and extended by Wacziarg and Welch (2008).

[7] This is done by setting the child mortality of the treated country equal to 1 in the year of trade reform, T_0.
[8] Further details on this computation approach can be find in Cavallo et al. (2013).

Using this index, a country is classified *closed* to international trade in any given year where at least one of the following five conditions is satisfied (otherwise, it will be considered *open*): (1) overall average tariffs exceed 40 percent; (2) non-tariff barriers cover more than 40 percent of its imports; (3) it has a socialist economic system; (4) the black market premium on the exchange rate exceeds 20 percent; (5) much of its exports are controlled by a state monopoly. Following Giavazzi and Tabellini (2005) we define a trade liberalization episode or a "treatment" as the first year when a country can be considered *open* to international trade according to the criteria above, after a preceding period where the economy was closed to international trade.[9] Finally, as discussed in Billmeier and Nannicini (2013), trade reforms may not occur suddenly, but may be a gradual shift toward more liberal trade policies. If so, this means that our treated variable based on a binary indicator is measured with error. This problem will introduce an attenuation bias in our results, meaning that our results are underestimating the actual impact.

To measure health outcomes (Y_{it}), we use the under-5 mortality rate (per 1,000 live births), hereafter U5MR for brevity, from the United Nation Inter-agency Group for Child Mortality.[10] The choice of this indicator of health is based on several reasons. The U5MR has the key advantage of being available on a yearly basis from 1960 for almost all the countries in the world. This represents a key property for our identification procedure, because the SCM works with yearly data, and the dataset covers a period when many trade reforms happened, i.e. before or during the 1980s. Moreover, from a conceptual point of view, the quality of the U5MR estimation is better than other indicators because it represents a key index of the United Nations Millennium Development Goals (see Alkema and New 2013).

The vector of covariates X_j used to identify the synthetic controls has been selected on the basis of previous (cross-country) studies of health (see, e.g., Charmarbagwala et al., 2004; Owen and Wu, 2007; Hanmer et al., 2015). More specifically, the synthetic controls are identified using the following covariates: real per capita GDP (source: Penn World Table); population growth (Penn World Table) and the fraction of rural population into

[9] Note that, by focusing on a sample constituted of developing country, the agricultural and food sectors represents the bulk of these economies. Hence, trade liberalization involves primarily this sector (see Nicita et al. 2012). This is strongly supported by Olper et al. (2014) who showed that there exists a strong cross-country and within country correlation between the Wacziarg and Welch (2008) opens index and the protection level in agriculture.

[10] See: http://www.childmortality.org.

total population (source: FAO); years of wars and conflicts based on Kudamatsu (2012) (source: *Armed Conflict database*, Gleditsch et al. 2009); female primary education (source: Barro and Lee, 2010); the average U5MR in the pre-treatment period (source: United Nations); Polity2 index from the Polity IV data set (see Marshall and Jaggers, 2013), to classify countries as autocracy or democracy.

Concerning the country sample, we started from a dataset of about 130 developing countries. For about 33 of them, information related to the trade policy reform index is lacking (see Wacziarg and Welch, 2003, 2008 for details). A further selection was based on following criteria. First, the treated countries were liberalized at the earliest in 1970, to have at least 10 years of pre-treatment observations to match with the synthetic control.[11] Second, that there exists a sufficient number of countries with similar characteristics that remain closed to international trade (untreated) for at least 10 years before and after each trade reform, so as to provide a sufficient donor pool of potential controls to build the synthetic unit. Moreover, as suggested by Abadie (2013), we eliminated from the donor pool countries that have suffered large idiosyncratic shocks to the outcome of interest during the studied period.[12]

Using these criteria, we ended up with a usable data set of about 80 developing countries, of which 40 experienced a trade liberalization episode (see Tables A1-A4, in the Appendix A).[13] The overall time span runs from 1960 to 2010. However, the time span will be different for each country case-study based on the year of liberalization. For every experiment, we use the years from $T_0 - 10$ to T_0 as the pre-treatment period to select the synthetic control, and the years from T_0 to $T_0 + 5$ and $T_0 + 10$ as the post-treatment periods, on which to evaluate the outcome, where T_0 is the year of trade liberalization.

4. Results

This section summarizes the results obtained from our 40 SCM experiments. We first present the effects aggregated by regions and then detailed results at the country level.

4.1 Average Effects by Region

[11] Abadie at al. (2010) show that, the bias of the synthetic control estimator is clearly related to the number of pre-intervention periods. Therefore, in designing a synthetic control study it is of crucial importance to collect sufficient information on the affected unit and the donor pool for a large pre-treatment window.

[12] The countries excluded from the donor pool due to anomalous spikes in child mortality are: the Republic of Congo, Lesotho, Rwanda and Zimbabwe.

[13] More precisely, using these criteria we end up with 43 usable treated countries. However, for three countries it has been impossible to find a good counterfactual, due to their extremely high level of child mortality in comparison to the donor pool. These countries are: Mali, Niger, and Sierra Leone.

Figure 1 reports the dynamic treatment effect by regions computed using equation (5), namely by aggregating each country-year treatment effect at the regional level. Before the year of the treatment T_0, the lines are close to zero, meaning that the treated countries and their synthetic control behave quite similarly. This means that, on average, the SCM algorithm worked well in selecting the counterfactual. After the year of the treatment T_0, each regional line becomes negative, and more so moving away from T_0. This suggests that in every region, the countries that experienced a trade liberalization, child mortality reduced more (or increased less) than in the synthetic control. In none of regions trade liberalization seems to hurt, on average, child mortality. Instead it had a health improvement effect everywhere (on average).

Figure 1 suggests differential effects across regions. In particular, the average effect of trade liberalization on child mortality seems to be stronger for the sample of Asian countries and Middle East and North African (MENA) countries. These countries in the long run (T_0+10) experienced an average reduction of child mortality that is about 9% to 13% lower in comparison with the synthetic control. The average effects are smaller for the other regions: around 5% to 6% in Latin American and SSA countries. Interestingly, the gap between the regions grows over time. While the effect increases over the 10 year period for the Asia and MENA region, most of the impact is reached after 6 years in the other regions (as the treatment effect line flattens out).

Overall, these findings suggest that trade liberalization improved child mortality and more so in MENA and in Asia where the size of the effect is higher and tend to be statistically significant. However, the regional averages also mask substantial heterogeneity at country level.

4.2 County Effects

Table 1 reports the numerical comparison of the outcome variable between the treated and the respective synthetic control for each country case study. The overall pre-treatment fit, measured by the root mean square prediction error (RMSPE), is reported for each experiment. The RMSPE values indicate that the pre-treatment fit is quite good in most of the cases (19 have RMSPE < 1 and 31 have RMSPE < 3).

In Appendix A and B we present more details on the covariates and the synthetic controls for each of the countries and a series of placebo tests for the country case studies.

In Table 1 the significance tests have been summarized by the *p*-value (last column) based on equation (6).[14]

The comparison between the post-treatment outcome of the treated unit with its synthetic control after five (U5MR T_0+5) and ten years (U5MR T_0+10) from the reforms, represent two estimates of the treatment effect. The countries are ranked based on the magnitude of ten year treatment effect (T_0+10).

What is obviously clear from Table 1 is the strong heterogeneity of the effects. The 10-year impacts range from +45% to -80%.[15] The country case studies where the *p*-value is lower than 0.15 are on the top and the bottom of the table. With a *p*-value < 0.15 cut-off, 20 cases had a significant effect and 20 not. From the 20 significant effects, 18 are positive (i.e. trade liberalization reduced child mortality) and 2 had a negative effect (i.e. it worsened child mortality). Hence these detailed country results show that in 45% of the cases trade liberalization had a significant positive effect, 50% no significant effect and in only 5% of the cases a significant negative effect.

Figure 2 presents the results, as well as the placebo test, of the two country case studies where we detected a negative impact of trade reforms on child mortality. Understanding the reasons behind such a strong negative impact in Mauritania (-42%) and in particular South Africa (-80%) is of particular interest. Our hypothesis is that these negative effects are due to different reasons.

The South African deterioration in child health is most likely due to the spread of HIV/AIDS in the mid 1990s, which may have been reinforced by trade liberalization. National antenatal clinic data show a rise in seroprevalence from 1 percent in 1990 to 25 percent in 2000 – the decade after trade liberalization (see Karim and Karim, 1999; South Africa Department of Health, 2005). Oster (2012) argues that trade liberalization and, especially, trade flows stimulated the HIV/AIDS spread. [16]

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[14] As critical value for the significance level we used 15% (*p*-value < 0.15), instead the more standard 10% or 5% level, simply because the placebo test for several case studies suffer for a quite low usable fake experiments.

[15] In this section the magnitude of the impact of trade liberalization on the U5MR is measured as the % deviation of the treated country in comparison to the (counterfactual) synthetic control.

[16] Although the Synthetic South Africa is built also with countries that suffer from HIV/AIDS diffusion, in particular the Central African Republic, because the HIV/AIDS shock happened in the post-treatment period, this translated to a very low weight attributed to this country, in comparison to the higher weight attributed by the algorithm to countries that did not experience a similar shock. From this perspective, it is important to note that if we exclude from the donor pool the non-African countries, then the trade reform effect on South Africa child mortality shrink substantially and becomes insignificant to the placebo test, exactly because the algorithm, when building this "new" synthetic South Africa, is forced to use other African countries that suffer from the HIV/AIDS shock. These additional results are available from the author upon request.

The Mauritania case appears different and seems to be an example of trade liberalization that spurs overall economic growth, without improving poverty and inequality. According to the World Bank (2003), trade liberalization mainly resulted in an improvement of exports conditions in the extractive and fishery sectors, with only a small fraction of total employment, but not in agriculture and livestock where productivity stagnated. Agriculture and livestock sectors have the greatest potential to contribute to poverty alleviation, as these sectors employ most of the population and poverty is most deeply rooted there.

However, child health in several SSA countries also benefited from trade liberalization: Gambia, Tanzania and Ghana are all in the top 10.

The case study Ghana is worth noting to illustrate some of the possible mechanisms at work. The reform analysis by Thomas (2006) indicates that Ghana simultaneously reformed its overall macroeconomic policies (fiscal and monetary policy), its trade policy (especially by changing exchange rate policy), and its agricultural trade policy (see Thomas 2006, Table 4 p. 10). Ghana gradually reduced export taxation on key agricultural commodities (i.e. cocoa) and introduced institutional changes at the level of the marketing board. These reforms induced a significant reduction of the overall agricultural taxation[17] and contributed to a significant reduction in poverty and inequality in rural areas (see Coulombe and Wodon, 2007).

Hence the Ghana and Mauritania case studies, taken together, seem to suggest that an important channel through which trade reform may affect child health is their contribution to agricultural growth and the related poverty reduction, *ceteris paribus.*

In four of six Asian SCM experiments we find that trade liberalizing countries had a reduction in U5MR that outperforms the one of the respective synthetic control. This happens in Indonesia (reform in 1970), Thailand (1970), Sri Lanka (1977) and Philippines (1988). The effect is the strongest in Sri Lanka, where the U5MR is 12% lower than the estimated counterfactual after five years, and 37% lower after ten years. In Sri Lanka trade liberalization reduced the taxation of agricultural export crops, especially tea, coconuts and rubber. The taxation of traditional export products was over 40 percent in the 1960s and 1970s, and fell to about 20 percent in the 1980s (Karunagoda et al. 2011, 245). The trade liberalization contributed to productivity growth in agriculture, structural

[17] The agriculture nominal rate of assistance increased from an average level of -23% in the decade before the trade reform to -2.8% in the decade after. This trend is due to both a strong reduction in commodities export taxation (especially cocoa), and a switch from taxation to subsidization of import-competing commodities, such as rice and maize (see Anderson and Valenzuela, 2008).

transformation of the economy and poverty reduction (De Silva et al. 2013). This again suggests that the sectors affected by the trade liberalization, and the extent to which the poor are involved, play a major role in the health effects.

Moving to Latin America trade liberalization episodes, in more than half of the experiments (seven out of twelve) we find that the treated countries tend to outperform the U5MR reduction of the synthetic control. In the other experiments the trade liberalization effect is not significant, but never negative. The strongest improvements following trade reforms were in Chile (1976) and Guatemala (1988). Ten years after the trade reform, the U5MR was about 45% lower than that of synthetic control in Chile and 28% in Guatemala.

Also for Latin American trade reforms there appear interesting relationships between trade liberalizations, agricultural policy, and the situation of the poor. For example, the Chile trade reform of 1976 has been followed by a strong shift from agricultural taxation to agricultural subsidization, which has been accompanied by a shift from subsistence crops to high value added export productions.[18] However, Thomas (2006) argues that the main impact of economic reforms on poverty, more than through agricultural prices, was the growth of off-farm income generation opportunities derived from growth in both the agricultural sector and the broader economy. Also in Mexico (1986) and Brazil (1991), where child mortality was around 20% lower than the synthetic control, trade liberalization implied a shift from agricultural taxation to agricultural subsidization (Anderson and Nelgen, 2013).

In the four reform experiments that occurred in the Middle East and North Africa (MENA) countries (Morocco (1984), Tunisia (1989), Turkey (1989) and Egypt (1995)), the U5MR dynamic of the treated country outperforms that of the respective synthetic control, with a magnitude ranging from 14% for Morocco to 37% for Tunisia.

Interestingly, these findings contrast with those of Billmeier and Nannicini (2013) who found no significant GDP growth effects after trade liberalization in these countries. This suggests that, while GDP growth induced by trade liberalization may represent one of the factors responsible for the improvement in the U5MR, in these specific situations other factors are at work. This conclusion is consistent with the findings of Chemingui and Thabet (2003), who, by simulating trade liberalization in Tunisia using a CGE model

[18] The nominal rate of assistance in agriculture shift form an overall level of taxation equal to -10%, in the ten years before the start of trade reform (1976), to a level of protection of 15% in the ten years later (see Anderson and Nelgen, 2013). For an indebt discussion about agricultural policy reforms in Chile, see Anderson and Valdés (2008).

combined with household survey data, showed that trade liberalization has only modest effects on the level of GDP, but it has a substantial effect in reducing poverty especially among rural households.[19]

Also for the MENA countries (at least for countries where data are available (Turkey and Egypt)) the impact (or joint reform of) agricultural trade policies seems important. In these countries around the reform years there was a significant switch from taxation to (slight) subsidization (see Anderson and Nelgen, 2013), supporting the idea that, when trade reform improves the conditions in agriculture, a sector where many poor are employed, the effect on child mortality appears to be more positive.

5. Robustness check

So far, we have ruled out the possibility that the above results are driven by other relevant shocks occurred around the trade reform. This may represents a potential shortcoming of the SCM. The most important confounding factor is the occurrence of political reforms. This is because, on the one hand there is evidence that democratic reforms could improve health outcomes (see Besley and Kudamatsu, 2006; Kudamatsu, 2012; Pieters et al., 2014). On the other hand, both theoretical (e.g. Galiani and Torrens, 2014) and empirical evidence (Giavazzi and Tabellini, 2005) show that trade and political reforms are strongly interrelated in developing countries.

With the aim to study whether or not political reforms interact with trade reforms in affecting health outcomes, we divided the twenty countries which displayed an improvement in child mortality after trade reforms in three not overlapping groups using the Polity 2 index of democracy.[20] In the first group (G_1) we consider the five countries where the political reform occurred simultaneously with the trade reform or within the ten years of the post-reform period;[21] the second group (G_2) instead considers four countries that were already democracy at the time of the trade reform; finally the last group (G_3)

[19] The reduction in rural poverty happened because the terms-of-trade loss due to higher food prices, is more than offset by the reduction of distortions due to the switch from (inefficient) protected agricultural commodities to activities involving export commodities (i.e. olive oil, dates and citrus).

[20] The Polity2 index assigns a value ranging from -10 to +10 to each country and year, with higher values associated with better democracies. We code a country as democratic (= 1, 0 otherwise) in each year that the Polity2 index is strictly positive. A political reform into democracy occurs in a country-year when the democracy indicator switches from 0 to 1. See Giavazzi and Tabellini (2005) and Olper et al. (2014) for details.

[21] Because the year of trade and political reforms can be measured with error, we consider all countries where the political reform occurred from two years before the trade liberalization (T_0–2) until two years later (T_0+2). However, only one country, Guatemala switches to democracy two years before trade liberalization, while other countries or switch one year before (Philippines and Nicaragua), simultaneously or later on (Perù and Mexico). Small changes in these rules do not affect our conclusions.

considers seven autocratic countries, where the democratic transition never took place in the ten years after the trade reform.[22]

Figures from 3 to 5 present the results for each country group. The left panel of these figures display a graphical representation of U5MR for the treated unit (solid line) and the synthetic control (dashed line) considering the *average* effect obtained by aggregating across the G_i groups using equation (5). Instead, the right panel reported the corresponding *p*-values based on equation (7).

Starting from Figure 3, we can observe that when we restrict the analysis to the subset of countries where political reforms came near or followed the trade liberalization, both the short- and long-run child mortality rate outperforms the one of the synthetic control, and is significant at conventional statistical level (p-value < 0.075). Moreover, the magnitude of the average effect is of about 15 percent points at T_0+10.[23] However, where trade reforms occur in consolidated democracies (see Figure 4), we observe a larger effect. The short- and long-run effects are statistically significant, and the magnitude at the year T_0+10 reacts about 26 percent points. Finally, in Figure 5 considering trade reforms that occurred in autocratic regimes, we find that the average effect at T_0+10 is equal to 16 percent points, and is also barely significant in both the short- and the long-run.

Taken together the above findings are of some interest. Firstly because they suggest that political reforms, *per se*, are not driving the effect of trade liberalization on child mortality, *ceteris paribus*. Second, because there is evidence that when health outcomes are considered, liberalizing trade after that a country have reached a certain level of political rights, seems to perform better. Interestingly, the last result is in sharp contrast with the Giavazzi and Tabellini's (2005) findings who, instead, found that when an economic liberalization preceded the political reform, the country perform better in term of GDP growth.

5. Concluding remarks

[22] The composition of these three groups is as follow: G_1 (Philippines, Mexico, Guatemala, Nicaragua, and Perù); G_2 (Brazil, Turkey, Sri Lanka, and the Gambia); G_3 (Indonesia, Morocco, Tunisia, Egypt, Chile, Guinea, and Tanzania). Countries like Ghana, Guinea Bissau and Thailand are excluded from these samples because or took the political reform at the end of the post-treatment period (Ghana), or they took more than a reform in the period under interest (Guinea Bissau and Thailand).

[23] The value of child mortality is normalized by setting child mortality of the treated country to be equal to 1 in the year of trade reform (T_0). Thus, the difference in the outcome variable between the treated and synthetic counterfactual in the post-reform period, represents an estimate of the average effect.

In this paper we analyzed the effect of trade liberalization on health outcomes, exploiting 40 reform episodes during the last half-century. We used a new econometric approach for case studies analysis, the synthetic control method. The SCM allows to take into account a time-varying impact of country heterogeneity, and thus to overcome a major drawback of the most standard econometric estimators.

The main results show that the effect of trade liberalization on child health is, first of all, largely heterogeneous, both in terms of the direction and magnitude of the estimated effects. Yet, and interesting, in half of our SCM experiments the reforms have had a positive impact on the reduction of children mortality, and thus improving children health conditions. In the other half of the investigated case studies, the trade reform effect has been always non-negative, with only two case-studies showing a strong deterioration of child mortality after trade liberalization, South Africa and Mauritania. However, we argued and documented that the underline reasons of this negative effect seems to be driven by totally different conditions: a classical example of trade liberalization that spurs overall economic growth, without improving poverty and inequality in the Mauritania case study, and the concomitance of HIV/AIDS epidemic spread in South Africa.

We also showed that these results are fairly robust, and are not driven by the simultaneous occurrence of political reforms, and also that trade reforms that happened in well established democracies seem to work better both in terms of the magnitude of the estimated effect and their significant level. Finally, although this paper does not investigate explicitly the possible channels through which trade reform may impacts child mortality, we documented several stylized facts suggesting that the positive health outcomes of trade liberalization were associated with an overall switch from taxation to slight subsidization in agricultural trade policy. This findings support the idea that when trade reform improves the conditions of agriculture, a sector where many poor are employed, the effect of trade reform on child mortality appears to be more positive.

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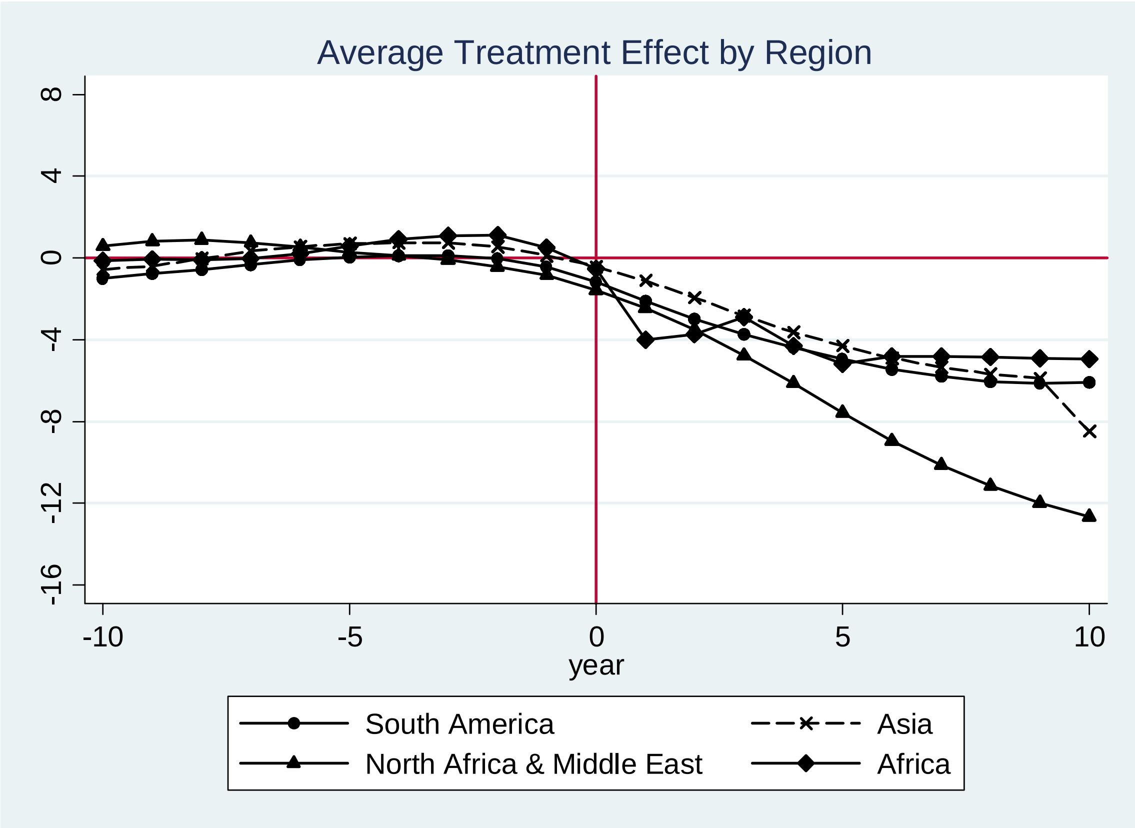
Table 1. Summary of the SCM results at country level

#	Country	*Region*	Year of Reform (T_0)	*U5MR level*			*Average Treatment Effect*		*RMSPE*	Significance level *P < 0.15*
				T_0	*T+5*	*T+10*	*T+5 (%)*	*T+10 (%)*		
1	Chile	Latin America	1976	57.10	30.00	22.10	40.03%	44.85%	3.62	YES
2	Turkey	MENA	1989	78.10	58.00	40.59	18.21%	37.09%	0.98	YES
3	Sri Lanka	Asia	1977	59.30	42.10	24.40	12.47%	36.98%	0.50	YES
4	Gambia	Sub-Saharan Africa	1985	203.30	169.70	141.00	10.08%	34.13%	3.06	YES
5	Tanzania	Sub-Saharan Africa	1995	159.60	131.50	90.10	11.78%	28.48%	0.61	YES
6	Guatemala	Latin America	1988	88.40	69.50	55.09	12.61%	27.81%	0.67	YES
7	Tunisia	MENA	1989	53.90	41.40	31.50	9.78%	22.47%	0.39	NO
8	Ghana	Sub-Saharan Africa	1985	154.70	128.10	113.30	13.77%	22.43%	1.78	YES
9	Brazil	Latin America	1991	59.20	44.20	30.79	9.94%	21.83%	0.30	YES
10	Egypt	MENA	1995	64.20	45.09	31.20	14.38%	20.53%	2.41	YES
11	Perù	Asia	1991	74.90	53.59	37.00	11.42%	19.69%	1.63	YES
12	Mexico	Latin America	1986	56.20	43.80	32.70	8.55%	18.87%	0.66	YES
13	Philippines	Sub-Saharan Africa	1988	65.60	49.90	42.09	16.44%	18.24%	3.18	YES
14	Guinea	Sub-Saharan Africa	1986	259.60	235.30	201.50	7.27%	17.47%	3.32	YES
15	Nicaragua	Latin America	1991	63.30	49.70	38.10	7.06%	14.80%	0.25	YES
16	Morocco	MENA	1984	108.40	83.80	66.40	5.91%	14.21%	0.20	NO
17	Indonesia	Asia	1970	165.20	139.89	120.00	5.67%	11.13%	0.23	YES
18	Thailand	Asia	1970	99.40	77.90	61.79	4.83%	10.20%	0.08	YES
19	Zambia	Sub-Saharan Africa	1993	192.40	179.10	143.30	-5.64%	9.01%	12.42	NO
20	Guinea-Bissau	Sub-Saharan Africa	1987	211.70	201.60	185.00	1.57%	6.86%	1.94	NO
21	Cote d'Ivory	Sub-Saharan Africa	1994	152.30	147.40	134.50	-1.48%	3.17%	1.68	NO
22	Colombia	Latin America	1970	40.40	34.09	28.90	5.04%	2.99%	5.56	NO
23	Paraguay	Latin America	1989	47.20	39.59	33.79	2.36%	2.79%	1.38	NO
24	Guyana	Latin America	1988	63.10	55.29	48.79	6.55%	2.63%	3.32	NO
25	Cape Verde	Sub-Saharan Africa	1991	59.10	47.50	35.50	-0.13%	1.96%	0.15	NO
26	Uganda	Sub-Saharan Africa	1988	180.40	169.60	157.39	1.13%	1.59%	5.14	NO
27	Cameroon	Sub-Saharan Africa	1993	143.50	155.10	134.60	-15.54%	-2.42%	3.00	NO
28	Burkina Faso	Sub-Saharan Africa	1998	191.40	174.00	131.60	-12.34%	-2.44%	6.44	NO
29	Benin	Sub-Saharan Africa	1990	180.70	158.20	147.39	4.64%	-2.60%	1.46	NO
30	Madagascar	Sub-Saharan Africa	1996	131.80	102.60	76.09	1.75%	-3.09%	0.62	NO
31	Panama	Latin America	1996	108.10	83.59	63.40	-1.62%	-5.19%	0.36	NO
32	Pakistan	Asia	2001	109.60	98.40	90.00	-1.26%	-5.41%	0.27	NO
33	Bangladesh	Asia	1996	108.10	83.59	63.40	-0.77%	-6.27%	1.28	NO
34	Honduras	Latin America	1991	56.20	45.09	36.29	-1.62%	-6.64%	0.22	NO
35	Dominican Republic	Sub-Saharan Africa	1992	55.00	44.70	37.29	0.93%	-7.65%	0.44	NO
36	Ethiopia	Sub-Saharan Africa	1996	167.70	139.70	101.90	-4.84%	-7.98%	1.50	NO
37	Botswana	Sub-Saharan Africa	1979	76.60	58.29	48.29	-6.08%	-9.79%	0.49	NO
38	Mozambique	Sub-Saharan Africa	1995	208.40	165.70	131.50	3.09%	-11.58%	2.62	NO
39	Mauritania	Sub-Saharan Africa	1995	118.60	110.50	101.70	-7.58%	-42.26%	0.48	YES
40	South Africa	Sub-Saharan Africa	1991	59.30	61.70	76.70	-22.17%	-80.59%	0.22	YES

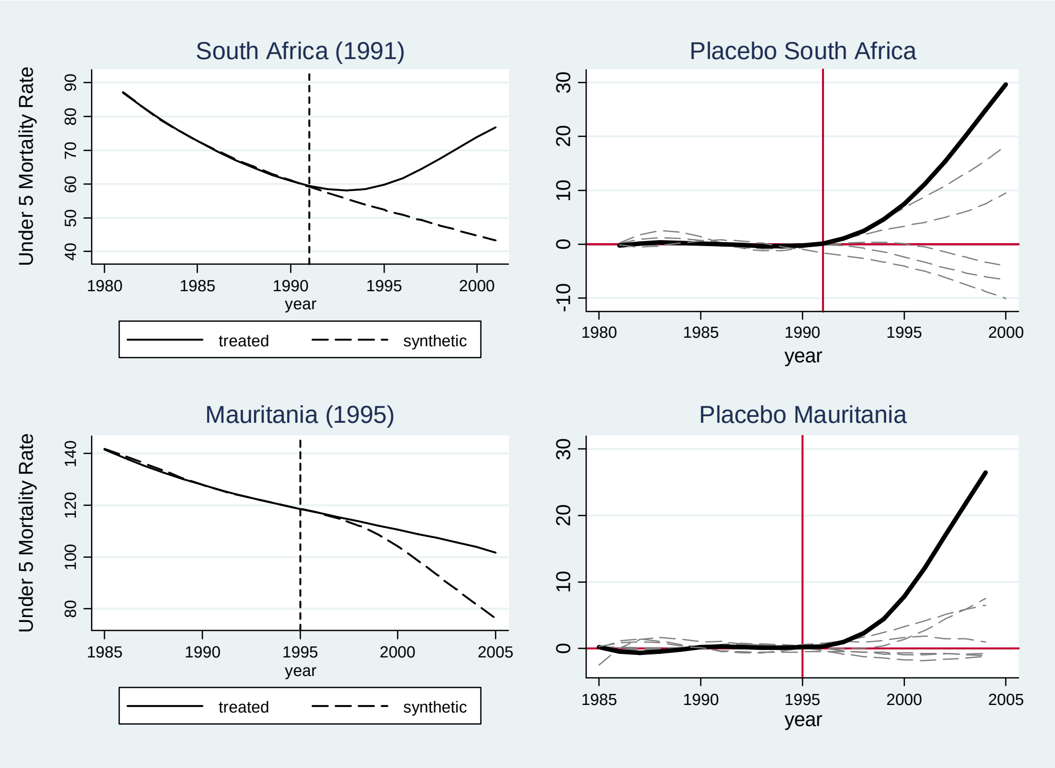
Notes: The Table summarizes the main SCM results at the country level reported in details in Table A1-A4 of Appendix A. The magnitude of the "average treatment effect" of trade liberalization on the U5MR is measured as the % deviation of the treated country in comparison to the (counterfactual) synthetic control. See Text.

Figure 1. Average Treatment Effect Aggregated at Regional level



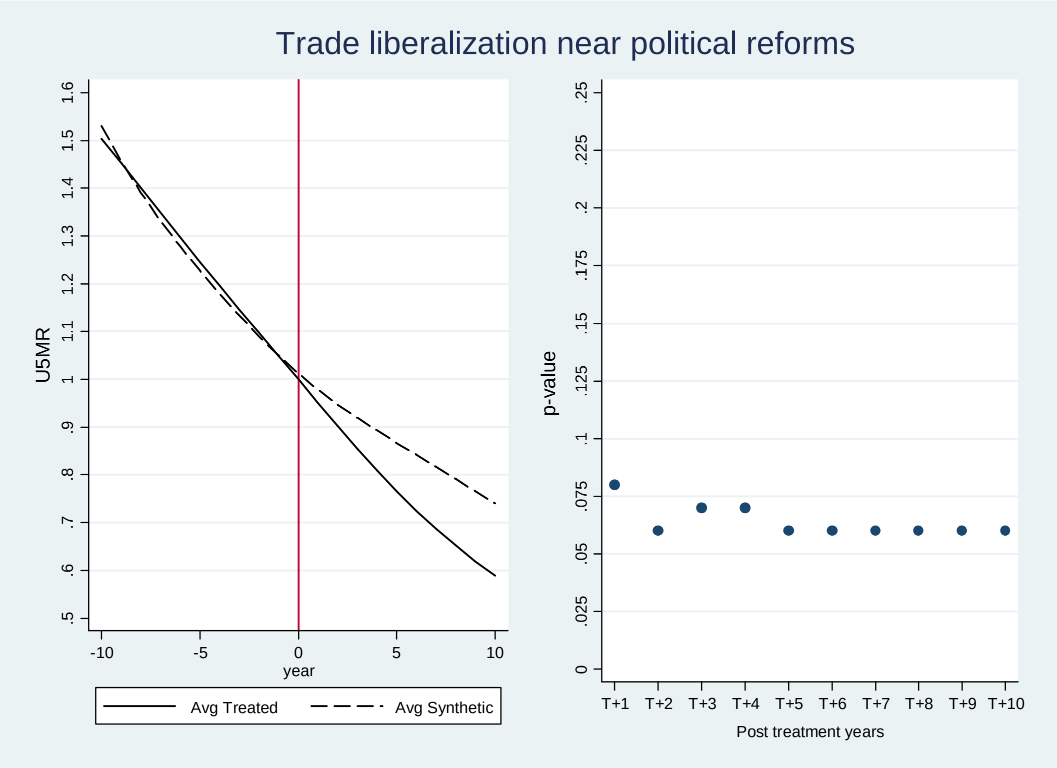
Notes: The figure reports the dynamic treatment effects aggregated at regions level using the equation (5). Each regional effect is obtained by averaging the contribution of all the treated countries within the same region in terms of yearly deviation of the outcome variable (U5MR) with respect to the one of the respective synthetic control. Countries used for estimating the average regional effects for Asian countries correspond to those reported in Table A1, while countries reported in Table A3 and Table A4 have been used to estimate the average regional effect for Latin America and North Africa and Middle East, respectively. Countries used to estimate the average regional effect for Africa are those reported in tablel A2a and A2b, with the exception of Uganda, Zambia and Burkina Faso, which have been excluded due to their extremely high value of RMSPE, which make them potential outliers in the estimation of the average regional effect.

Figure 2. SCM results and Placebo Tests for "negative" Reform Effects



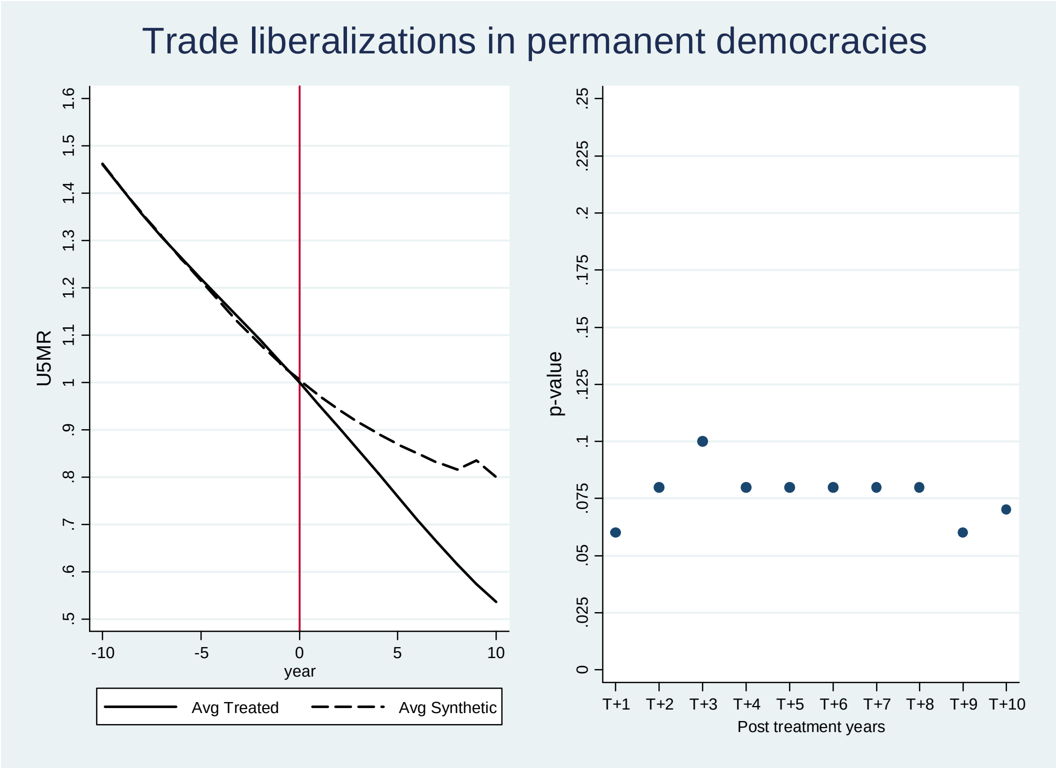
Notes: The Figure reports examples of SCM results and their respective placebo tests for two case studies, where the effect of the trade reforms on child mortality resulted to be negative. For each SCM experiment, the left panel reports the outcome variable for the treated unit (*solid line*) and the synthetic control (*dashed line*); the vertical dashed line coincides with the year of the trade reform, T_0. Instead, in the right panel the bold line reports the outcome difference between each treated unit and the synthetic control, while the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

Figure 3. Average Treatment Effect and *p*-value for Trade Reforms Near Political Reforms



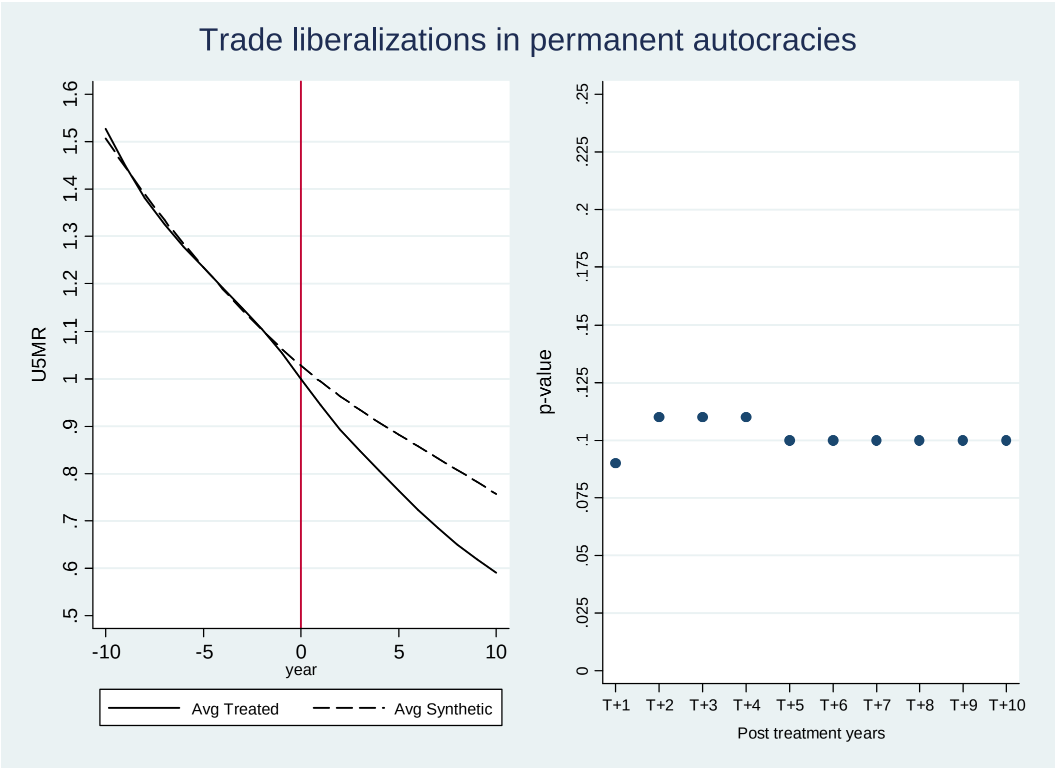
Notes: The Figure reports estimates of the average treatment effect and the corresponding *p*-value of trade liberalizations that occur near political reforms (democratization). The estimates are obtained by applying relations 5 and 6 to trade liberalizations in the following five countries (in parenthesis the year of democratization): Guatemala 1988 (1986); Mexico 1986 (1994); Nicaragua 1991 (1990); Perù 1991 (1993); and Philippines 1988 (1987). See text.

Figure 4. Average Treatment Effect and *p*-value for Trade Reforms in Permanent Democracies



Notes: The Figure reports estimates of the average treatment effect and the corresponding *p*-value of trade liberalizations that occur in permanent democracies. The estimates are obtained by applying relations 5 and 6 to trade liberalizations of the following four countries: Brazil, Turkey, Sri Lanka, and Gambia.

Figure 5. Average Treatment Effect and *p*-value for Trade Reforms in Permanent Autocracies



Notes: The Figure reports estimates of the average treatment effect and the corresponding *p*-value of trade liberalizations that occur in permanent democracies. The estimates are obtained by applying relations 5 and 6 to trade liberalizations of the following seven countries: Indonesia, Morocco, Tunisia, Egypt, Chile, Guinea, and Tanzania.

Appendix A. Detailed results of the Synthetic Control case studies

Table A1. SCM results: Covariates and average effects for Asian countries

Asia	Indonesia 1970	Synthetic Control	Thailand 1970	Synthetic Control	Sri Lanka 1977	Synthetic Control
War	0.10	0.08	0.00	0.05	0.06	0.01
Log GDP per-capita	6.52	6.82	7.05	7.86	6.68	8.58
Rurale population	0.84	0.79	0.80	0.67	0.79	0.63
Population growth	0.03	0.03	0.03	0.03	0.02	0.02
Primary school	8.85	6.82	27.40	16.39	9.49	24.79
U5MR T_0	165.20	165.23	99.40	99.41	59.30	59.35
U5MR T+5	139.89	148.30	77.90	81.86	42.10	48.10
U5MR T+10	120.00	135.02	61.79	68.85	24.40	38.72
RMSPE		0.23		0.08		0.50
	Philippines 1988	Synthetic Control	Bangladesh 1996	Synthetic Control	Pakistan 2001	Synthetic Control
War	0.29	0.08	0.00	0.03	0.10	0.10
Log GDP per-capita	7.58	7.07	6.58	6.61	7.05	7.00
Rurale population	0.64	0.71	0.87	0.89	0.72	0.73
Population growth	0.03	0.03	0.02	0.02	0.03	0.03
Primary school	20.98	6.40	9.49	3.61	5.12	5.22
U5MR T_0	65.60	66.73	108.10	109.43	109.60	109.50
U5MR T+5	49.90	59.72	83.59	82.95	98.40	97.18
U5MR T+10	42.09	51.48	63.40	59.66	90.00	85.38
RMSPE		3.18		1.28		0.27

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Indonesia**: Cameroon (0.249); Honduras (0.043); India (0.077); Pakistan (0.226); Trinidad and Tobago (0.052); Tunisia (0.101); Zimbabwe (0.252). **Synthetic Thailand**: Cameroon (0.108); Panama (0.251); Siria (0.364); Trinidad and Tobago (0.207); Tunisia (0.038); Zimbabwe (0.032). **Synthetic Sri Lanka**: Algeria (0.1084); Nicaragua (0.016); Panama (0.181); Korea (0.171); Trinidad and Tobago (0.361); Venezuela (0.177). **Synthetic Philippines**: China (0.317); Pakistan (0.132); Papua New Guinea (0.158); Siria (0.393). **Synthetic Bangladesh**: China (0.082); Iran (0.088); Malawi (0.154); Nepal (0.677). **Synthetic Pakistan**: Rep. Dem. Congo (0.274); Iran (0194); Malawi (0.089); Nepal (0.173); Siria (0.038); Togo (0.231)

Table A2a. SCM results: Covariates and average effects for African countries (1)

	Botswana 1979	Synthetic Control	Ghana 1985	Synthetic Control	Gambia 1985	Synthetic Control
War	0.00	0.08	0.00	0.00	0.00	0.01
Log GDP per-capita	7.21	7.32	7.19	7.06	7.12	6.77
Rurale population	0.92	0.63	0.71	0.71	0.78	0.81
Population growth	0.03	0.03	0.03	0.03	0.03	0.03
Primary school	4.92	6.68	3.52	4.21	0.54	4.46
U5MR T_0	76.60	76.64	154.70	154.67	203.30	203.41
U5MR T+5	58.29	54.95	128.10	148.55	169.70	188.72
U5MR T+10	48.20	43.90	113.30	146.07	141.00	214.06
RMSPE		0.49		1.78		3.06
	Guinea 1986	Synthetic Control	Guinea-Biss. 1987	Synthetic Control	Uganda 1988	Synthetic Control
War	0.00	0.00	0.00	0.00	0.25	0.07
Log GDP per-capita	6.67	6.60	6.88	6.72	6.50	6.80
Rurale population	0.82	0.75	0.83	0.72	0.93	0.74
Population growth	0.01	0.02	0.02	0.02	0.03	0.03
Primary school					5.22	4.36
U5MR T_0	259.60	264.60	211.70	211.06	180.40	182.85
U5MR T+5	235.30	253.74	201.60	204.82	169.60	171.53
U5MR T+10	201.50	244.16	185.00	198.62	157.39	159.93
RMSPE		3.36		1.94		5.137
	Benin 1990	Synthetic Control	Cape Verde 1991	Synthetic Control	South Africa 1991	Synthetic Control
War	0.00	0.02	0.00	0.07	0.26	0.17
Log GDP per-capita	6.76	6.69	6.97	7.08	8.55	7.89
Rurale population	0.78	0.80	0.76	0.70	0.52	0.60
Population growth	0.02	0.03	0.02	0.03	0.02	0.03
Primary school	2.51	7.47			8.98	6.36
U5MR T_0	180.70	181.65	59.10	59.41	59.30	59.11
U5MR T+5	158.20	165.89	47.50	47.44	61.70	50.50
U5MR T+10	147.39	143.66	35.50	36.21	76.70	42.47
RMSPE		1.464		0.15		0.217

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Botswana**: Argentina (0.008); China (0.226); Algeria (0.114); Panama (0.084); Rwanda (0.004); Siria (0.564). **Synthetic Ghana**: Rep Centrafricana (0.212); Rep Dem Congo (0.341); Malawi (0.079); Panama (0.033); Papua New Guinea (0.145); Sierra Leone (0.172); Siria (0.017). **Synthetic Gambia**: Burkina Faso (0.207); Algeria (0.243); Malawi (0.131); Nigeria (0.092); Sierra Leone (0.326). **Synthetic Guinea**: Algeria (0.008); Sierra Leone (0.992). **Synthetic Guinea Bissau**: Rep Centrafricana (0.583); Sierra Leone (0.417). **Synthetic Uganda**: Pakistan (0.61); Senegal (0.059); Sierra Leone (0.331). **Synthetic Benin**: Malawi (0.4); Nepal (0.045); Pakistan (0.245); Senegal (0.309). **Synthetic Cape Verde**: China (0.281); Algeria (0.048); Nepal (0.181); Siria (0.49). **Synthetic South Africa**: Central Africa Republic (0.102); China (0.128); Iran (0.406); Siria (0.364).

Table A2b. SCM results: Covariates and average effects for African countries (2)

	Cameroon 1993	Synthetic Control	Zambia 1993	Synthetic Control	Cote d'Ivory 1994	Synthetic Control
War	0.03	0.06	0.00	0.00	0.00	0.02
Log GDP per-capita	7.47	6.86	7.15	6.69	7.25	6.62
Rurale population	0.72	0.69	0.66	0.72	0.67	0.71
Population growth	0.03	0.03	0.03	0.02	0.04	0.02
Primary school	10.32	2.80	8.92	4.67	3.73	5.50
U5MR T_0	143.50	136.54	192.40	177.02	152.3	148.99
U5MR T+5	155.10	134.24	179.10	169.54	147.40	145.24
U5MR T+10	134.60	131.42	143.30	157.48	134.50	138.90
RMSPE		2.995		12.422		1.680
	Mauritania 1995	Synthetic Control	Mozambique 1995	Synthetic Control	Tanzania 1995	Synthetic Control
War	0.00	0.04	0.37	0.00	0.00	0.00
Log GDP per-capita	7.21	6.78	5.86	6.35	6.37	6.70
Rurale population	0.76	0.75	0.88	0.89	0.87	0.73
Population growth	0.03	0.03	0.02	0.03	0.03	0.02
Primary school	13.45	11.77	5.32	8.22	12.82	7.90
U5MR T_0	118.60	118.45	208.40	206.69	159.60	159.56
U5MR T+5	110.50	102.72	165.70	170.99	131.50	149.06
U5MR T+10	101.70	71.49	131.50	117.85	90.10	125.98
RMSPE		0.475		2.617		0.608
	Ethiopia 1996	Synthetic Control	Madagascar 1996	Synthetic Control	Burkina Faso 1998	Synthetic Control
War	0.44	0.05	0.00	0.02	0.00	0.05
Log GDP per-capita	6.06	6.42	6.89	6.86	6.37	6.32
Rurale population	0.90	0.89	0.82	0.82	0.91	0.82
Population growth	0.02	0.03	0.03	0.03	0.02	0.03
Primary school						
U5MR T_0	167.70	169.41	131.80	131.82	191.40	182.44
U5MR T+5	139.70	133.25	102.60	104.43	174.00	154.88
U5MR T+10	101.90	94.37	76.69	74.39	131.60	128.47
RMSPE		1.50		0.62		6.44

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Cameroon**: Rep. Dem. Congo (0.72); Algeria (0.206); China (0.074). **Synthetic Zambia:** Central Africa Republic (0.859); Malawi (0.141). **Synthetic Cote d'Ivore**: Central African Republic (0.676); Rep. Dem. Congo (0.159); China (0.165). **Synthetic Mauritania**: China (0.347); Iran (0.084); Malawi (0.231); Senegal (0.337). **Synthetic Mozambique**: Malawi (0.902); Senegal (0.096). **Synthetic Tanzania**: Central Africa Republic (0.52); Malawi (0.163); China (0.086); Senegal (0.231). **Synthetic Ethiopia**: India (0.195); Malawi (0.633); Nepal (0.172). **Synthetic Madagascar**: Algeria (0.195); Haiti (0.05); Malawi (0.338); Nepal (0.309); Papua New Guinea (0.002); Senegal (0.106). **Synthetic Burkina Faso**: Rep. Dem. Congo (0.482); Malawi (0.518).

Table A3. SCM results: Covariates and average effects for Latin American countries

	Chile 1976	Synthetic Control	Colombia 1970	Synthetic Control	Mexico 1986	Synthetic Control
War	0.00	0.05	0.00	0.12	0.00	0.11
Log GDP per-capita	8.32	7.81	8.28	7.71	8.90	7.51
Rurale population	0.26	0.56	0.43	0.56	0.39	0.59
Population growth	0.02	0.03	0.02	0.03	0.03	0.03
Primary school	24.97	16.44	20.42	3.41	15.69	3.96
U5MR T_0	57.10	62.67	40.40	45.70	56.20	57.86
U5MR T+5	**30.00**	**50.03**	**34.09**	**35.90**	**43.80**	**47.90**
U5MR T+10	**22.10**	**40.07**	**28.90**	**29.79**	**32.70**	**40.31**
RMSPE		3.62		5.56		0.66

	Guyana 1988	Synthetic Control	Guatemala 1988	Synthetic Control	Paraguay 1989	Synthetic Control
War	0.00	0.06	0.00	0.08	0.00	0.09
Log GDP per-capita	7.93	6.35	8.35	7.64	7.82	7.14
Rurale population	0.70	0.82	0.64	0.66	0.60	0.66
Population growth	0.01	0.02	0.03	0.03	0.03	0.03
Primary school	41.30	10.68	7.34	6.72	15.14	6.82
U5MR T_0	63.10	62.75	88.40	88.76	47.20	47.47
U5MR T+5	**55.29**	**59.19**	**69.50**	**79.53**	**39.59**	**40.55**
U5MR T+10	**48.79**	**50.11**	**55.09**	**76.32**	**33.79**	**32.87**
RMSPE		3.32		0.67		1.38

	Brazil 1991	Synthetic Control	Honduras 1991	Synthetic Control	Nicaragua 1991	Synthetic Control
War	0.00	0.18	0.03	0.13	0.26	0.23
Log GDP per-capita	8.46	8.09	7.91	7.67	8.05	8.40
Rurale population	0.39	0.57	0.68	0.63	0.52	0.57
Population growth	0.02	0.03	0.03	0.03	0.03	0.03
Primary school	15.51	5.39	10.79	4.64	9.10	6.84
U5MR T_0	59.20	59.23	56.20	56.43	63.30	63.43
U5MR T+5	**44.20**	**49.08**	**45.09**	**44.37**	**49.70**	**53.90**
U5MR T+10	**30.79**	**39.39**	**36.29**	**34.03**	**38.10**	**44.81**
RMSPE		0.30		0.22		0.25

	Perù 1991	Synthetic Control	Dominican Rep. 1992	Synthetic Control	Panama 1996	Synthetic Control
War	0.26	0.17	0.00	0.15	0.00	0.01
Log GDP per-capita	8.46	7.85	8.18	7.91	6.58	6.59
Rurale population	0.40	0.67	0.54	0.59	0.87	0.89
Population growth	0.03	0.03	0.03	0.03	0.02	0.02
Primary school	14.18	4.55	7.72	5.52		
U5MR T_0	74.90	76.78	55.00	55.23	108.10	108.42
U5MR T+5	**53.59**	**60.50**	**44.70**	**45.12**	**83.59**	**82.26**
U5MR T+10	**37.00**	**46.07**	**37.29**	**34.64**	**63.40**	**60.27**
RMSPE		1.63		0.44		0.36

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Chile**: Panama (0.58); Siria (0.42). **Synthetic Colombia**: China (0.219); Rep. Dem. Congo (0.066); Algeria (0.084); Iran (0.104); Siria (0.494); Swaziland (0.033); **Synthetic Mexico**: China (0.063); Central African Republic (0.038); Burundi (0.017); Siria (0.846); Pakistan (0.036). **Synthetic Guyana**: China (0.809); Papua New Guinea (0.156); Pakistan (0.035). **Synthetic Guatemala**: Iran (0.12); Papua New Guinea (0.183); Senegal (0.324); Siria (0.373). **Synthetic Paraguay**: China (0.358); Pakistan (0.003); Papua New Guinea (0.049); Siria (0.59). **Synthetic Brazil**: Central African Republic (0.035); Iran (0.423); Malawi (0.055); Siria (0.487). **Synthetic Honduras**: China (0.066); Iran (0.228); Nepal (0.155); Siria (0.55). **Synthetic Nicaragua**: Central African Republic (0.089); China (0.04); Iran (0.735); Senegal (0.017); Siria (0.12). **Synthetic Perù**: Iran (0.459); Malawi (0.03); Nepal (0.269); Siria (0.243). **Synthetic Dominican Republic**: China (0.033); Iran (0.307); Malawi (0.077); Siria (0.583). **Synthetic Panama**: Haiti (0.03); Iran (0.03); Malawi (0.338); Nepal (0.309); Siria (0.071).

Table A4. Covariates and average effects for Middle East and North Africa countries

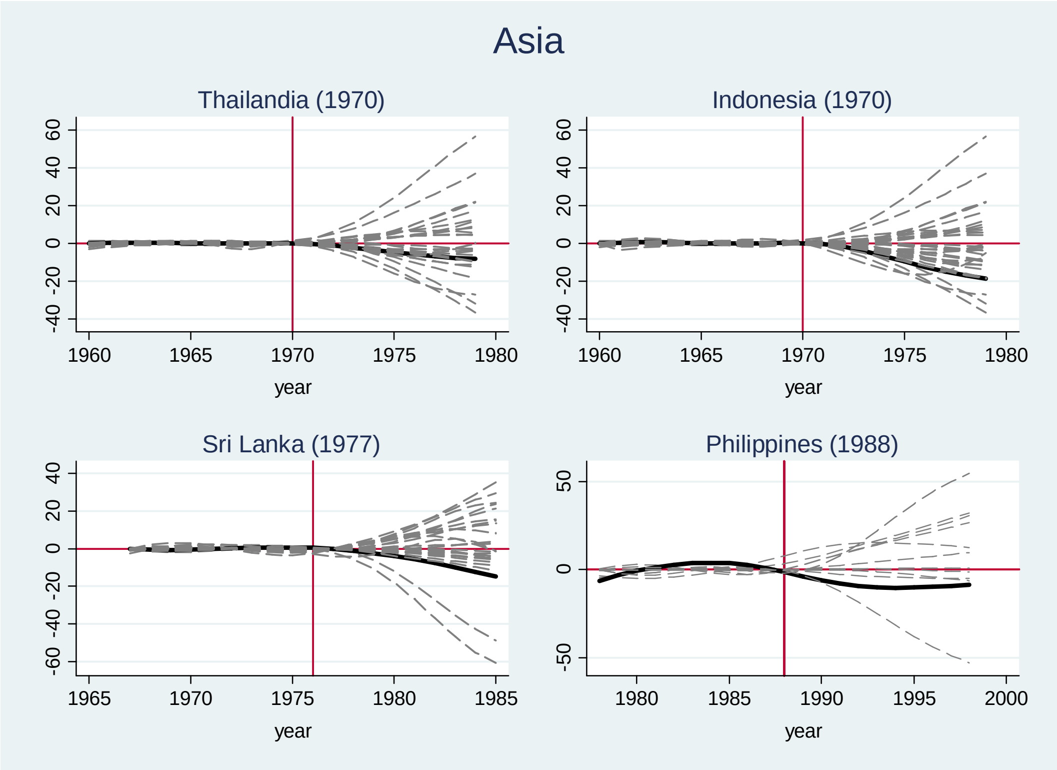
	Morocco 1984	Synthetic Control	Tunisia 1989	Synthetic Control	Turkey 1989	Synthetic Control
War	0.08	0.08	0.03	0.14	0.03	0.19
Log GDP per-capita	7.29	7.27	7.99	7.96	8.44	8.11
Rurale population	0.64	0.64	0.53	0.57	0.58	0.59
Population growth	0.03	0.03	0.02	0.03	0.02	0.03
Primary school	1.94	4.37	5.88	4.86	18.12	7.56
U5MR T_0	108.40	108.51	53.90	54.31	78.10	79.47
U5MR T+5	**83.80**	**89.07**	**41.40**	**45.89**	**58.00**	**70.92**
U5MR T+10	**66.40**	**77.40**	**31.50**	**40.63**	**40.59**	**64.53**
RMSPE		0.200		0.390		0.984

	Egypt 1995	Synthetic Control
War	0.06	0.06
Log GDP per-capita	7.30	7.65
Rurale population	0.57	0.71
Population growth	0.02	0.03
Primary school	3.54	2.09
U5MR T_0	64.20	68.57
U5MR T+5	**45.09**	**52.66**
U5MR T+10	**31.20**	**39.26**
RMSPE		2.41

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Morocco**: Central African Republic (0.171); China (0.054); Algeria (0.113); Egypt (0.258); India (0.13); Iran (0.012); Panama (0.034); Siria (0.229). **Synthetic Tunisia**: Algeria (0.106); Iran (0.193); Senegal (0.091); Siria (0.611). **Synthetic Turkey**: Algeria (0.022); Iran (0.477); Senegal (0.285); Siria (0.216). **Synthetic Egypt**: Algeria (0.563); Iran (0.057); Nepal (0.38).

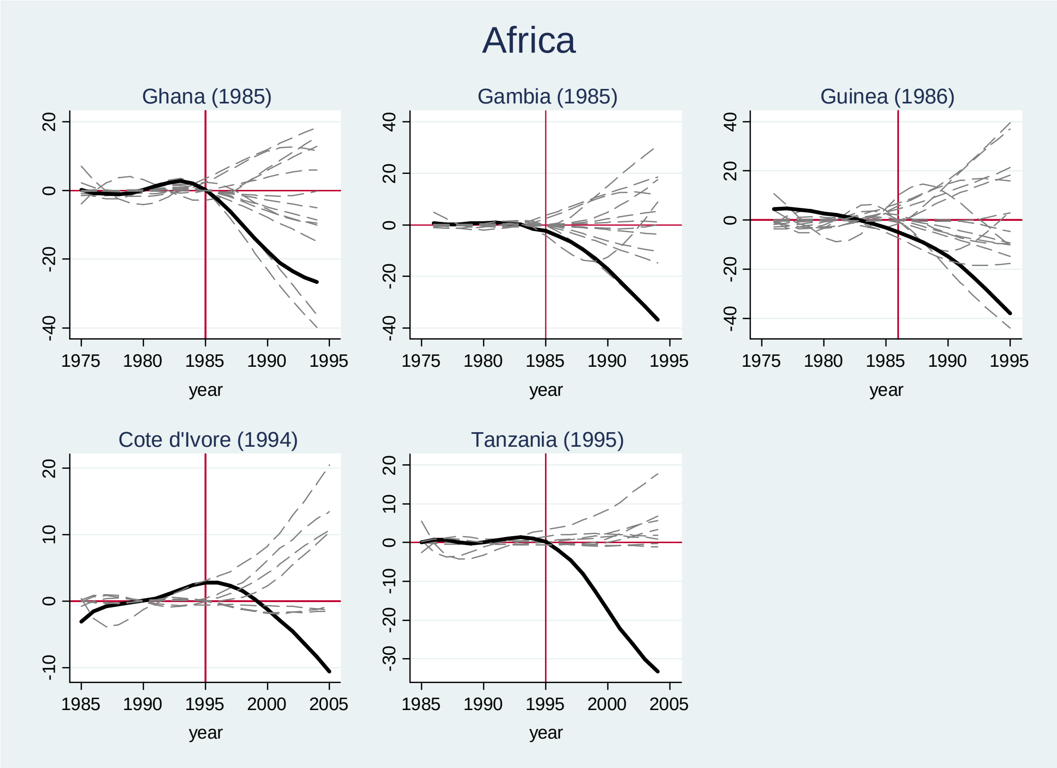
Appendix B. Placebo Tests

Figure B1 Placebo tests for Asian SCM Experiments with "Positive" Reform Effects



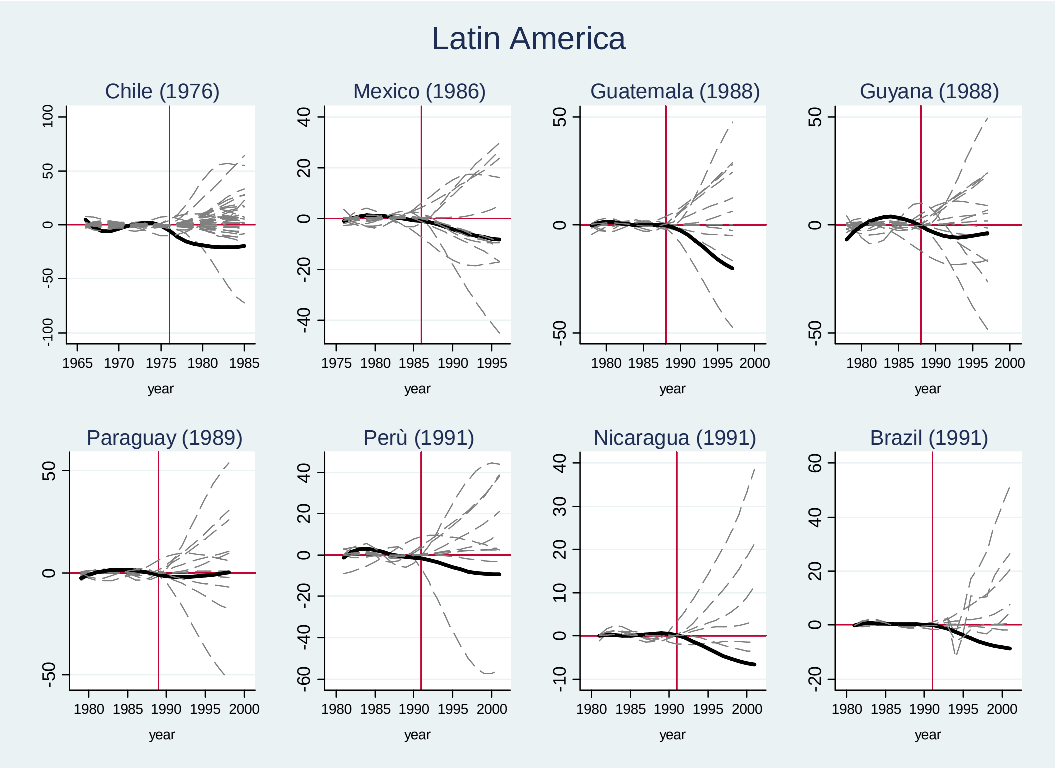
Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

Figure B2. Placebo tests for African SCM Experiments with "Positive" Reform Effects



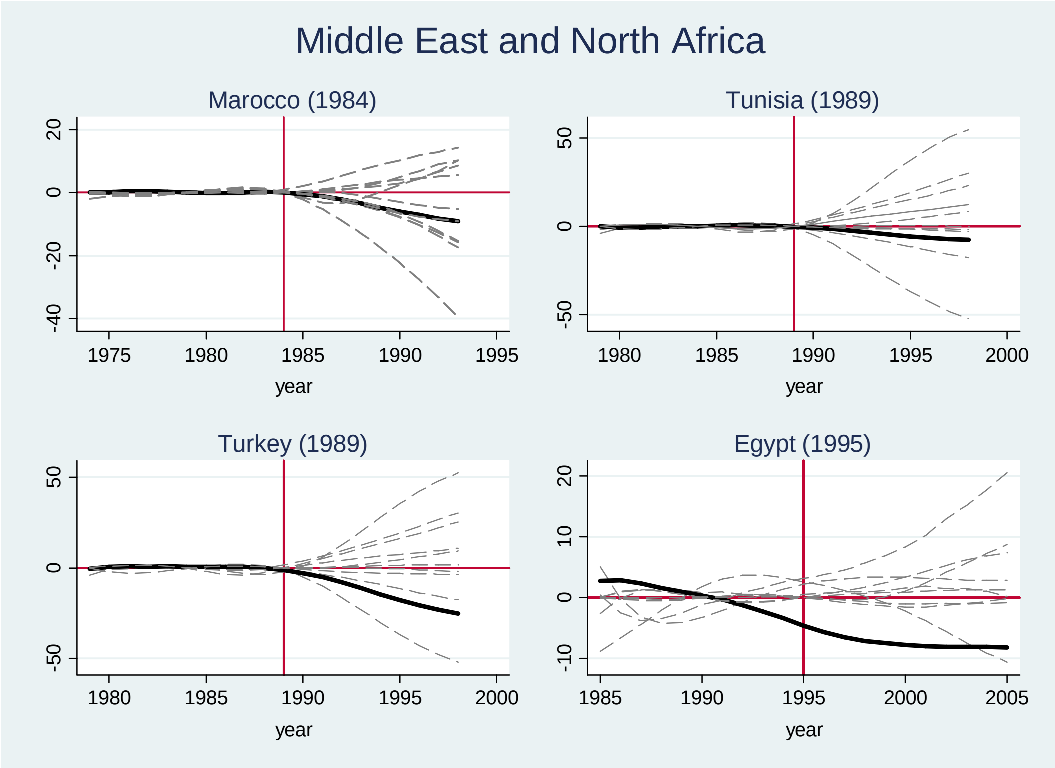
Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

Figure B3. Placebo tests for Latin America Experiments with "Positive" Reform Effects



Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

Figure B4. Placebo tests for Middles Esat and N. Africa SCM experiments with "Positive" Reform Effects



Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dashed lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.