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Trade Liberalization and Child Mortality: A Synthetic Control Method*

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Abstract. We study the causal effect of trade liberalization on child mortality by exploiting 41 policy reform experiments in the 1960-2010 period. The Synthetic Control Method for comparative case studies allows to compare at the country level the trajectory of post-reform health outcomes of treated countries (those which experienced trade liberalization) with the trajectory of a combination of similar but untreated countries. In contrast with previous findings, we find that the effect of trade liberalization on health outcomes displays a huge heterogeneity, both in the direction and the magnitude of the estimated effect. Among the 41 investigated cases, 19 displayed a significant reduction in child mortality after trade liberalization. In 19 cases there was no significant effect, while in three cases we found a significant worsening in child mortality after trade liberalization. Trade reforms in democracies, in middle income countries and which reduced taxation in agriculture reduce child mortality more.

Keywords: Trade liberalization, Child Mortality, Synthetic Control Method.

JEL Classification: Q18, O24, O57, I15, F13, F14.

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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 economic 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. Goldberg and Pavcnik, 2007; Topalova, 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 poverty (Deaton, 2003), it is also an important end in its own right (Sen, 1999). Moreover child health is also itself important for economic growth and development (Levine and Rothman, 2006).

There is an extensive literature addressing the issue and the mechanisms through which trade may affect health, and in particular child mortality (see Blouin et al. 2009 for a survey). These include the impact on economic growth, poverty and inequality (Pritchett and Summers 1996; Deaton, 2003), public health expenditures (Kumar et al. 2013; Filmer and Pritchett, 1999), knowledge spillovers (Deaton, 2004; Owen and Wu, 2007), dietary changes (Cornia et al. 2008; Chege et al. 2015; Oberländer et al. 2016), food prices (Headey, 2014; Fledderjohann et al. 2016), fertility and the labour market (Anukriti and Kumler 2014). Not only are there many ways that trade may affect people’s health, the impact may be both positive and negative.

Despite the importance of this topic there are only two published economic studies that quantitatively assess the impact of trade on health on a global basis. 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 by 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 and some other covariates. The authors

¹ See also Goldberg and Pavcnik (2004; 2007) for extensive reviews on the poverty and distributional effects of trade liberalization in developing countries.

conclude that one of the main channels 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 correlations between trade and health can be attributed 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 the sub-sample of only developing countries, the trade effect on health 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 effect as measured by previous cross-country studies may hide important heterogeneity among countries and regions.

To analyze this issue we use a different methodology with respect to previous studies, namely 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 relationship between trade liberalization and growth. Our approach follows their application of the SCM by considering also aggregation of the units under investigation across specific dimensions (see Cavallo et al. 2013).

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 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 impact of 41 trade liberalization events which occurred during the 1960-2010 period. Among other things, this approach offers the key advantage over the other methodologies of allowing the explicit identification of the heterogeneity of the reform effects.

The SCM methodology allows flexibility and transparency in the selection of the counterfactual, and thus improves the comparability between treated and untreated units. Importantly, the SCM also accounts for endogeneity bias due to omitted variables by accounting for the presence of *time-varying* unobservable confounders. Moreover, 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).

We find that in the 41 investigated cases, about 19 (46%) showed a short- and long-run significant reduction in the child mortality after trade liberalization, with an average effect of about 22% in the long-run. For 19 other cases we do not find any effect of trade liberalization. In 3 cases (7%) we find a significant increase in child mortality after trade liberalization. Our results are robust when controlling for potential confounding effect, and in particular for the concomitant occurrence of political reforms (i.e. democratic transition), and when considering potential spill over effects.

In our analysis of the potential channels through which trade liberalization affects child health, we do find evidence supporting the idea that child mortality declines more when trade liberalization happened in democracies, in middle income countries and when it causes a reduction of taxation in the agricultural sector, where many of the poorest people are employed.

The remainder of the paper is organized as follows. In the next Section the methodology – the synthetic control approach – 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 robustness checks and some extensions, while in Section 6 we further investigate potential mechanisms. Section 7 concludes.

2. Methodology

The empirical identification of the causal effect of trade policies on health outcomes is difficult because trade policies tend to be correlated with many other social, political and economic factors. Moreover, the effect of trade policies on inequality and poverty in developing countries tends to be country-, time- and case-specific (see Goldberg and Pavcnik, 2004; 2007).

Previous quantitative studies do not fully account for all these issues simultaneously. The instrumental variable approach of Levine and Rothman (2006), relies on the assumption that the estimated trade share from gravity model is not correlated with other factors, such as institutions or growth, that by themselves could affect child mortality (see Nunn and Trefler, 2014). The panel fixed effects approach proposed by

Owen and Wu (2007) assumed that in absence of trade reforms, health outcomes for the treated and control groups would have followed parallel trajectories over time, an assumption often violated and sensitive to the fixed effects specification (Bertrand et al. 2004; Ryan et al. 2015).² In addition, both these approaches do not provide insights on the potential heterogeneity of the trade reforms effects on poverty and inequality.

To overcome the identification problem we use the synthetic control method (SCM) proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). The SCM is an approach for programme evaluation, developed in the context of comparative case studies, that relaxes the parallel trends assumption of the difference-in-difference method.³ The SCM, besides accounting for time varying unobserved effects, is particularly suitable for those contexts where the effect of the policy under investigation is supposed to be heterogeneous across the investigated units. Moreover, as the SCM offers a dynamic estimate of the average effects, its results add additional insights on the dynamic effect of trade policy reforms on health outcomes, as some of the effects may require some time to emerge (Billmeier and Nannicini, 2013). Finally, The SCM estimator is both externally and internally valid, as it combines properties of large cross-country studies, which often lack internal validity, and of single country case studies, that often cannot be generalized.

In what follows we summarize the SCM approach following Abadie et al. (2010) and Billmeier and Nannicini (2013) who studied the relation between trade liberalization and growth. We also discuss the problem of aggregation of the units of investigation based on Cavallo et al. (2013).

2.1 The Synthetic Control Method

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

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

² In fact, Owen and Wu pooled together developed and developing countries in the same fixed effects regression. In so doing, as an effect of the Preston curve (see Preston, 1975) in the relation between health and income, the probability that the parallel assumption inherent in fixed effects model is violated, appears high in this context.

³ See Ryan et al. (2015) for an in depth discussion about the plausibility of the parallel assumption of the difference-in-difference (DiD) estimator, and Kreif et al. (2016) for a comparison of DiD with the synthetic control method in the context of health policy.

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 $(\tau_{i,T_0+1}, \dots, \tau_{i,T})$. As is well known from the program evaluation 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):

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

where δ_t is an unknown common term 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 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 later on, the variables that we include in the vector X_j (real per capita GDP, population growth, fraction of rural population, frequency of wars and conflicts, female primary education, and child mortality) refer to the pre-treatment period. Hence, we are assuming that they are exogenous, and thus not affected by the treatment (trade liberalization). Put differently, we are ruling out any kind of “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 $\sum w_j = 1$. Every value of W represents a possible counterfactual for country i . Moreover, define $\bar{Y}_j^k = \sum_{s=1}^{T_0} k_s Y_{js}$ as a linear combination of pre-treatment outcomes. Abadie et al. (2010) showed that, as long as one can choose W^* such that

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

then

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

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 $[(\bar{Y}_1^k, X_1), \dots, (\bar{Y}_{I_C}^k, X_{I_C})]$. However, in practice, the synthetic control W^* is selected so

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.

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.⁷

The SCM has three key advantages in comparison with the DiD and other estimators normally used in the program evaluation literature. First, it is more transparent, as the weights W^* clearly identify the countries that are used to estimate the counterfactual. Second, it is more flexible because the set of I_C potential controls can be restricted to make the underlying country comparisons more appropriate. Third, it is based on identification assumptions that are weaker, as it allows for the effect of unobservable confounding factors to be time variant. Yet, identification is still based on the assumption that the attribution of a given treatment to one country does not affect the other countries, and/or that there are not spillover effects (stable unit treatment value assumption (SUTVA)).⁸

The SCM methodology has two main drawbacks. First, it does not distinguish between direct and indirect causal effects, a standard weakness of the program evaluation literature. Second, the small number of observations often involved in such case studies translates into the impossibility to use standard inferential techniques. Following Abadie et al. (2010) we try to address this problem by making use of placebo tests. These tests compare 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.

2.2 Measuring Average Effect

In previous SCM applications the analysis of the results have been largely conducted at the level of (each) single unit of investigation, e.g. at the country level.

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. We followed the same approach.

⁷ In particular, one of the key outcomes of the SCM procedure is the estimate of the root mean square predicted error (RMSPE) between the treated and the synthetic control, measured in the pre-treatment period.

⁸ Working with macro data and trade reforms, the probability that the treatment assignment to one country may have – partial or general equilibrium – effects on the others could not, a priori, be ruled out. However, as we will argue in the results section, in our specific context this problem does not appear particularly severe.

However, when the analysis covers many countries, as in the present study, may be interesting to measure the average treatment effects for specific groups of countries. To do this, we follow the approach by Cavallo et al. (2013). Denote by $(\hat{\tau}_{1,T0+1}, \dots, \hat{\tau}_{1,T})$ a specific estimation of the trade liberalization effects on child mortality of the country of interest 1. The average trade liberalization effects across G countries of interest. The estimated average effect across these G trade reforms can then be computed as:

$$(5) \quad \bar{\tau} = (\bar{\tau}_{T0+1}, \dots, \bar{\tau}_T) = G^{-1} \sum_{g=1}^G (\hat{\tau}_{g,T0+1}, \dots, \hat{\tau}_{g,T}).^9$$

To estimate whether this (dynamic) average treatment effect is statistically significant, Cavallo et al. (2013) proposed an approach that allows consistent inference measurement regardless of the number of available controls or pre-treatment periods, although the precision of inference clearly increases with their number. The underlying logic of this methodology is to first apply the SCM algorithm to every potential control in the donor pool to evaluate whether the estimated effect of the treated country outperforms the ones of the fake experiments.¹⁰

Furthermore, because we are interested in valid inferences on the $\bar{\tau}$ average effect, we need to construct the distribution of the average placebo effects to compute the year t average specific p -value. Following Cavallo et al. (2013), we first compute all the placebo effects for the treated countries, as summarized in footnote 10. As we are interested in computing the p -value of the average effect, we then consider at each year of the post-treatment period, all the possible average placebo effects for any possible aggregation of placebos, G . The number of possible placebo averages is computed as follows:

$$(6) \quad N_{\overline{PA}} = \prod_{g=1}^G J_g.$$

The comparison between the average effect of the group of treated countries, with the average effect of all the possible groups deriving from any potential combination of non-

⁹ 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. This is done by setting the child mortality of the treated country equal to 1 in the year of trade reform, T_0 .

¹⁰ 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 significance level, namely the p -value, for the estimated trade reform effect as follows: $p\text{-value}_t = \Pr(\hat{\tau}_{1,t}^{PL} < \hat{\tau}_{1,t}) = \frac{\sum_{j=2}^{J+1} I(\hat{\tau}_{1,t}^{PLj} < \hat{\tau}_{1,t})}{\# \text{ of controls}}$, where $\hat{\tau}_{1,t}^{PL}$ 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 each 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.

treated countries yields the p -value for the average effects. After ranking each year-specific average trade reform effect in the placebo distribution, the yearly p -value of the average effect is thus computed as the ratio between the number of average placebo groups that display a higher effect than the actual group of treated countries, over the number of possible placebo averages.¹¹

3. Data, Measures and Sample Selection

The first issue to address in our empirical analysis is the measurement of 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). 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. Finally, as discussed in Billmeier and Nannicini (2013), trade reforms may not occur suddenly, but there 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. Note that this problem will introduce attenuation bias in our estimated reform effects, 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.¹² The choice of this indicator of health is based on several grounds. First, as discussed extensively by Deaton (2006), it represents a better health indicator in comparison to life expectancy. Second, the U5MR has the key advantage of being available on a *yearly* basis from 1960 for almost all the countries in the world. This is a key property for our identification strategy, because the SCM works with yearly data, and the dataset covers a period when many trade reforms happened. Third, from a conceptual

¹¹ For a more formal derivation of this methodology, see Cavallo et al. (2013).

¹² See: <http://www.childmortality.org>.

point of view, the U5MR represents a key index of the United Nations Millennium Development Goals (see Alkema et al. 2014), and because improvements in child mortality happen at the bottom of the income distribution (Acemoglu et al. 2014), which made it especially relevant in this respect.

The vector of covariates X_j used to identify the synthetic controls has been selected on the basis of previous (cross-country) studies on the determinants of health and child mortality (see, e.g., Chamarbagwala 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); the fraction of rural population into total population (source: FAO); years of wars and conflicts based on Kudamatsu (2012) (source: *Armed Conflict database*, Gleditsch et al. 2002); female primary education (source: Barro and Lee, 2010); the average U5MR in the pre-treatment period (source: United Nations). Finally, in the robustness checks we also consider the Polity2 index from the Polity IV data set (see Marshall and Jaggers, 2007), to classify countries as autocracy or democracy,¹³ and data for agricultural policy distortions from the World Bank “Agricultural Distortion database” (see Anderson and Nelgen, 2008).

Concerning the sample of countries, we started from a dataset of about 130 developing countries. However, for about 33 of them, information related to the trade policy reform index is missing (see Wacziarg and Welch, 2008 for details). A further selection was based on the 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.¹⁴ Second, there exist a sufficient number of countries with similar characteristics that remain closed to international trade (untreated countries) 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 and the placebo tests. Moreover, as

¹³ 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.

¹⁴ Abadie et 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.

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.¹⁵

A final critical issue is related to the criteria used to select the donor pool, namely the potential controls used to build each synthetic control. From this perspective we face a non-trivial trade-off. On the one hand, by considering in the donor pool only countries belonging to the same region of the treated unit could be a strategy that would allow having countries with a relatively strong degree of similarity with the treated unit, and that are likely to be affected by the same regional shocks as the treated unit. On the other hand, in our specific context this approach could present some problems. First, because it would imply few control countries in several SCM experiments, and would thus worsen the pre-treatment fit and prevent the placebo tests. Second, the use of a donor pool with only countries that belong to the same region in an exercise that studies the macro effects of trade reforms, may violate the SUTVA assumption, because the spillover effects of trade liberalization in neighboring countries are likely more severe. Given these considerations, we do not impose further constraints in the selection of the donor pool, leaving the selection of the best synthetic control to the SCM algorithm. However, as a robustness check, we also discuss the results obtained by imposing more restrictions in the choice of the donor pool.

Using these criteria, we ended up with a usable data set of 80 developing countries, of which 41 experienced a trade liberalization episode (see Tables A1-A4, in the Appendix A).¹⁶ The dataset has data from 1960 to 2010. However, the time span used in the SCM is different for each country case-study based on the year of the liberalization. For each 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 evaluating the outcome, where T_0 is the year of trade liberalization.

4. Results

This section summarizes the results obtained from our 41 SCM experiments. We first present the effect of trade liberalization on child mortality aggregated over all experiments and by regions and then the detailed results at the country level.

¹⁵ Countries excluded from the donor pool due to anomalous spikes in child mortality are: the Republic of Congo, Lesotho, Rwanda and Zimbabwe. Note, the inclusion of these country do not change at all the final outcomes and conclusions.

¹⁶ More precisely, using these criteria we end up with 45 usable treated countries. However, for three countries it has been impossible to find a good counterfactual, due to their extreme high level of child mortality in comparison to the donor pool. These countries are: Mali, Niger, and Sierra Leone.

4.1 Average Effects

Figure 1 illustrates the aggregated average effect of trade liberalization on child mortality computed using equation (5) for the 41 trade liberalizations for which a good counterfactual could be constructed and that met our inclusion criteria. The solid line represents the average child mortality of the treated units and the dashed line shows the evolution of child mortality for the average synthetic control. The vertical line represents the year of trade liberalization (T_0). Before trade openness, the average treated and synthetic control are very close, consistent with a good fit between them. On average, trade liberalization reduced child mortality. After trade liberalization, average child mortality rates of the treated countries falls below the child mortality rates of the synthetic control. Five years after trade openness, child mortality is on average 6.7% percent lower in the treated countries than in their synthetic control (p -value < 0.01), an effect that increases to 9.5 (p -value < 0.01) after 10 years.

Figure 2 reports the dynamic treatment effect by regions computed in a similar way than before, namely by aggregating each country-year treatment effect at the regional level. In order to make the graph more readable, each regional effect is now obtained by averaging the contribution of all the treated countries within the same region in terms of yearly deviation of the outcome variable with respect to the one of the respective synthetic control.¹⁷ Before the year of the treatment T_0 , the lines are close to zero, meaning that also at regional level the treated countries and their synthetic controls behave quite similarly. In the year of the treatment T_0 , each regional line starts to become negative, and more so moving away from T_0 , except in the case of African countries where, instead, the line approaches zero. On average, in the Middle East and North Africa (MENA), Latin America and Asian countries child mortality reduced more (or increased less) after trade liberalization than in the respective synthetic control, but not in Africa. The average effect of trade liberalization on child mortality is strongest in the sample of MENA and Asian countries. In the long run (T_0+10) child mortality is 23% lower than in the synthetic control, an effect that is significant for both regions (p -value < 0.01 for MENA and p -value < 0.05 for Asian countries). The average effect for Latin American countries is

¹⁷ As discussed at the end of section 2 (see footnote 9), we normalize the estimates before aggregating the individual country effects, by setting the child mortality of the treated unit equal to 1 in the year of trade reform, T_0 . Thus, the difference in the outcome variable between the treated and the synthetic counterfactual in the post-reform period represents an estimate of the average treatment effect.

lower (around 14%) but still strongly significant (p -value < 0.01). Interestingly, the gap between these two groups grows over time. While the effect increases over the 10 year period for the MENA and Asian countries, most of the impact is reached after 5-6 years in the Latin American group (as the treatment effect line flattens out). For the sample of African countries, on average, there is no significant difference between treated countries and their synthetic control: the average increase in child mortality of +0.4% at time T_0+10 is not significant (p -value = 0.34).

In summary, these averages indicate that trade liberalization reduced child mortality, but there is regional heterogeneity.

4.2 Country Level Effects

Table 1 reports the numerical comparison of the outcome variable between the treated and the respective synthetic control for each country that implemented trade liberalization in our dataset. 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 (17 have *RMSPE* < 1 , 18 have *RMSPE* between $1 \div 5$, and only 6 have *RMSPE* > 5).

In Table 1 the results of the significance of the Placebo tests (p -value) are reported in the last column of Table 1. We refer to Appendix A and B for more details on the covariates and the synthetic controls for each of the countries and a series of placebo tests. The comparisons 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 (dynamic) treatment effect. Countries are ranked based on the magnitude of the 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 +41% to -52%. The country case studies where the p -value is lower than 0.15 are at the top and the bottom of the table. More than half of the country case studies (22) have a p -value lower than 0.15 (and for 17 the p -value < 0.10). From these 22 significant effects, 19 are positive (i.e. trade liberalization reduced child mortality) and 3 have a negative effect (i.e. it worsened child mortality). With a p -value cut-off of 10%, 15 are positive and 2 negative.

In all five Asian SCM experiments trade liberalizing countries experienced a reduction in U5MR that significantly (p -value < 0.10) outperforms the one of the respective synthetic control. These five countries are Indonesia (reform in 1970), Sri

Lanka (1977), Philippines (1988), Nepal (1991) and Bangladesh (1996). Among these countries. The strongest effects were in Nepal and Sri Lanka, where the U5MR is, respectively, 41% and 28% lower than the estimated counterfactual after ten years.

In Latin America, for most trade liberalization episodes (seven out of eleven) the treated countries outperform the U5MR reduction of the respective synthetic control. The strongest improvements following trade reforms were in Chile (1976) and Perù (1988). Ten years after the trade reform, the U5MR was about 31% lower than that of synthetic control in Chile and 34% in Perù. In other cases the effect of trade liberalization is not significant.

The large majority of SSA countries are concentrated at the bottom half of Table 1, meaning that the health effect of these trade liberalization episodes has been small or negative. In some SSA countries child health also benefited from trade liberalization: Gambia (year of reform 1985), Ghana (1985), Tanzania (1995), and Burundi (1999) displayed all a positive and significant effect of trade liberalization on child mortality. However, in most SSA countries the effect was not significant (13 out of 20). Moreover, the three countries where there was a significant increase of child mortality after trade liberalization are all in SSA: Kenya (−23%), Mauritania (−24%) and South Africa (−52%).

In all MENA countries (Morocco (1984), Tunisia (1989), Turkey (1989) and Egypt (1995)) trade liberalization reduced child mortality. The U5MR dynamic of the treated country outperforms that of the respective synthetic control, with a magnitude ranging from 8% for Morocco to 33% for Turkey. In all cases except Morocco, the reduction of child mortality is statistically significant at the 15% level (see Table 1).

In summary, these results indicate that trade liberalization has contributed to reducing child mortality in almost half of the countries in our sample. In most other countries, there was no significant impact. In three countries there was a negative effect, meaning that trade liberalization seems to have increased child mortality. This of course raises the question what are the reasons for these different effects. In the rest of this paper we first check (Section 5) whether our findings may be due to problems with the methodology or confounding effects which our approach has not sufficiently covered. Next (in Section 6) we look at a few additional factors which either the literature or occasional observations suggest may be influencing the impact of trade liberalization.

5. Robustness Tests

We will now discuss and try to test whether our results could be driven (or influenced) by specific assumptions or other shocks which occurred around the trade reform or in the post-treatment period.

5.1 Stable Unit Treatment Value Assumption (SUTVA)

A first issue of our identification strategy is the possible violation of the SUTVA assumption, namely that the treatment status of one unit does not affect the potential outcomes of the other (control) units. If this circumstance is not satisfied, the size of the effects could be either over or under estimated.

However, in our specific context the existence of these spillover effects are not so obvious a priori, since trade liberalization can exert an effect on child mortality only indirectly. Clearly, the existence of spillover effects would be more likely if the outcome variable under investigation would be, for example, trade flows or foreign direct investment, instead of child mortality. In fact, if trade liberalization in one country has led to a successful attraction of trade flows, other geographical proximate countries may have received lower trade flows. However, this reasoning cannot be applied to child mortality, at least directly, because the relationship between trade and child mortality is, a priori, difficult to establish.

At any rate, to be on the safe side, we re-ran the SCM experiments by excluding from the donor pool those countries that share a national border with the treated unit, so that the possible spillover effects will be attenuated. The results for those SCM experiments where the SUTVA may be violated are presented in bold in Table C1 (see Appendix C). As is evident from the figures, the size of the effect is only slightly affected by the exclusion of countries sharing a common border with the treated unit. The only cases where the size of the effect changes significantly are those of Mauritania and Mozambique. However, in the first case, the negative effect of trade liberalization on child mortality previously detected, shrinks to almost zero, and remains insignificant. In the case of Mozambique, the SCM experiments resulting from the exclusion of the border countries has a very high value of *RMSPE* (i.e. 59.2), suggesting that this experiment is not reliable. Hence, our main results and conclusions do not appear to be affected by the possible violation of the SUTVA.

5.2 Political Reforms

If another important change which affects child health (and which is not (fully) captured by the SCM) occurred around the trade reform, our estimated impacts could be

the result of these “other changes” rather than of trade reforms. One factor which has been identified in the literature as affecting child mortality is the political system of a country, and particularly the change in the political system. Several studies show that political reforms (in particular the move from autocracy to democracy) affect health outcomes (Besley and Kudamatsu, 2006; Kudamatsu, 2012; Pieters et al., 2016). Other studies argue theoretically (Zissimos, 2014) and show empirically (Giavazzi and Tabellini, 2005) that trade and political reforms are often interrelated in developing countries. In several of the countries in our dataset there have been important political reforms, which sometimes have occurred around the same time as the trade liberalization.

A related, but distinct, issue is that the nature of the political system could affect the trade liberalization effects. In case there would be no confounding effects due to political changes (and thus no bias in our estimated numbers) it may be that some political systems are more conducive to e.g. protecting the poor against potential negative effects of trade liberalization or enhance the poor’s capacity to benefit from new opportunities due to trade liberalization. This could then affect child mortality. Standard political economy arguments based on the median voter model suggest that, on average, democracies are more likely to contribute to pro-poor outcomes than autocracies.

We will consider both issues. A simple way to check whether our findings suggest that the nature of the political regime interacts with the trade reforms is to aggregate the SCM results according to the countries’ political regime. We therefore aggregate the nineteen countries which displayed a significant improvement in child mortality after the trade liberalization in three not overlapping groups, using the Polity 2 index of democracy. In order to classify these countries, we considered the political regime in place in the years “close to” the economic transition, which we define as the five years before and after trade liberalization.¹⁸

For this purpose we compare the trade reform effects which occurred under three different political regimes: (i) trade reforms close to political reforms (for all countries in our analysis “political reform” means democratization, i.e. the move from autocracy to democracy), (ii) trade reform in consolidated democracy and (iii) trade reform in autocracy. In the first group (G_1) there are five countries where democratization occurred

¹⁸ The choice of use five years before and five years after trade liberalization, instead of the whole period of each analysis (i.e. ten years before and ten years after trade liberalization) has been taken to better isolate the political condition near the treatment period. However, even classifying the treated countries using the whole period, the main results are not affected.

close to the trade reforms.¹⁹ The second (G_2) and third (G_3) groups include countries that during the considered period (five years before and five years after the trade reform) were permanent democracies or permanent autocracies, respectively.²⁰

Figure 3 presents the results of the (dynamic) average effect across each country group presented above.²¹ The three lines represent the average effect in countries that experienced the trade reform near political reforms (circle line), in democracies (square line) and in autocracies (triangle line). There is a significant average reduction in child mortality in all three groups (p -value < 0.05 for all groups), and the difference between the groups is relatively small.

Democratic countries experiencing trade liberalization have an average reduction in child mortality of 25% at T_0+10 , which is the highest. For the group of countries where trade liberalization occurred close to political reforms the average reduction in child mortality was 22%.²² In the group of autocracies the average reduction is 18%.

Thus, first of all, these findings do not suggest that political reforms (democratizations), *per se*, are driving the effect of trade liberalization on child mortality, *ceteris paribus*. In fact, the average reduction in child mortality is relatively similar in the three groups, and the reduction in permanent democracies is higher than the one in the group where political reforms and trade reforms are occurring simultaneously.

Second, the finding that the impact of trade liberalization on child mortality is more positive on average (meaning lead to a stronger reduction in child mortality) in democracies than in autocracies are consistent with the hypothesis that the poor are more likely to benefit from trade liberalization in a democracy, although, as already mentioned, the difference is not very large. This result is somewhat different than earlier findings of Giavazzi and Tabellini (2005) who found that when an economic liberalization preceded the political reform, countries perform better in term of GDP growth, although we know

¹⁹ 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). However, only two countries, Burundi and Guatemala switches to democracy two years before trade liberalization, while other countries switch one year before (Nepal, Philippines and Nicaragua). In order to determine the year of democratization using the polity2 variable, we follow Persson and Tabellini (2008).

²⁰ The composition of the three groups is as follows: G_1 (Burundi, Guatemala, Nepal, Nicaragua and Philippines); G_2 (Bangladesh, Brazil, El Salvador, Gambia, Perú, Sri Lanka and Turkey); G_3 (Egypt, Ghana, Indonesia, Mexico, Tanzania and Tunisia). Note that, the only country displaying a positive and significant effect that has been excluded from these aggregation is Chile as, according to the polity 2 variable, is the only country experiencing a transition to autocracy near trade liberalization.

²¹ Once again the aggregation is based on equation (5) and 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).

²² Note that, ever considering all the cases (not only those individually positive and significant) the main results are very close.

that there is not necessarily a direct link between GDP growth and child mortality (see Deaton, 2003).

5.3 HIV/AIDS

The fact that the large majority of SSA countries are concentrated at the bottom half of Table 1, meaning that the health effect of these trade liberalization episodes has been less positive (and sometimes negative) than in other regions, suggests that there may be an SSA-specific effect. One factor is income. SSA is the poorest region and income may influence the trade impacts. We will discuss and analyze the income factor in the next section.

Another potential factor is the spread of HIV, a disease which has affected overall mortality around the world, and which has been particularly devastating in some African countries. Intuitively it seems possible that the spread of HIV could influence our results. Oster (2012) explains how trade liberalization may have stimulated the HIV/AIDS spread in SSA countries.

Several countries for which the trade liberalization effects are insignificant or negative (those at the bottom of Table 1) have seen a deterioration in child health due to the spread of HIV/AIDS infections in the mid-1990s. Two countries with significant negative trade effects (South Africa (−52%) and Kenya (−25%)) have been strongly affected by the spread of HIV/AIDS infections. In South Africa seroprevalence increased from 1 % in 1990 to 25 % in 2000 (Karim and Karim, 1999; South Africa Department of Health, 2005). This may obviously influence the trade liberalization effects since trade liberalization occurred in South Africa during the same decade. However, not all cases of negative trade liberalization effects are correlated with the spread of HIV. For example, in Mauritania (−24%) trade liberalization occurred during the 1990s and the spread of HIV/AIDS was low in comparison to other SSA countries.²³

The problem with testing whether the spread of HIV has affected our results is that we do not have a consistent dataset for HIV infections in the pre-treatment period. Data on HIV are only available in a consistent way since 1990 which makes it impossible to integrate it into the SCM analysis.

Table 2 presents the average HIV infections in the post-treatment period of our SCM analysis for the three groups of countries (significant positive trade liberalization effect,

²³ Mandzik and Young (2014), attributed the low HIV/AIDS diffusion in Mauritania to religion, i.e. the large prevalence of Muslim in that country.

no significant effect, and significant negative effect). The HIV prevalence (as a share of the population between 15 and 49 years) is much higher in the significant negative effect (at 5.8%) than in the not significant group (at 3.3%) and even more compared to the positive significant group (at 1.1%). While this comparison obviously does not provide a real test of the HIV effect, the data in Table 2 are consistent with the hypothesis that the strong negative effects of trade liberalization in some of the African countries can be partially explained by the spread of HIV which occurred around the same period.

6. Other Factors and the Heterogeneity of Effects

In Section 5 we documented differences in trade effects between different political regimes. We also searched for correlations of the trade liberalization effects with other factors that could potentially explain the heterogeneity of the trade reform effects on child health. We found interesting correlations with country income level and with agricultural policy (reforms).

6.1 Income Level

As mentioned already, the fact that SSA countries perform so poorly compared to other regions and that SSA is the poorest region raises the question whether the differences in effects of trade liberalization may be caused by income differences. A country's income level, or level of development more generally, may influence the trade reform effects because low income countries typically have weak institutions and poor infrastructure. A weak institutional framework, poor infrastructure, and limited private and public resources in general may constrain the reallocation of production factors (including poor people's labor) to be more efficiently used in order to realize the gains from trade (see Bardhan, 2006). For the poor for whom child mortality is highest, these factors may also constrain health policies to be effective in response to a changed economic and social environment.

To check whether our findings are consistent with the argument that the effect of trade liberalization on child mortality may be influenced by the level of development (income), we divided the sample of treated countries in two groups: countries with below median ("lower") income levels and countries with above average ("higher") income levels at the time of the liberalization.

The results in Figure 4 show that the reduction of child mortality in countries with higher income at the time of liberalization was indeed significantly stronger than in lower

income countries. After 5 years, child mortality in the higher-income group is 9% lower than the counterfactual (p -value < 0.04), and after 10 years is 12.4% lower (p -value < 0.01). The lower-income country group instead experienced an average reduction of only 2.8% after 5 years (p -value < 0.01), which increases to 6.9% after 10 years (p -value < 0.01). These results are consistent with the argument that the effect of trade liberalization on child mortality is more positive in countries that, at the time of the reform, have a better institutional framework, better infrastructure, and more resources to allow the reallocation of production factors (including poor people's labor) to be more efficient to realize the gains from trade.

6.2 Agricultural Policy Reform

Many of the poorest people in the world, which is the main social location of child mortality, are employed in agriculture, either as smallholder farmers or as farm workers. One can therefore imagine that the profitability in agriculture would affect child mortality by influencing an important source of poor people's income. Trade liberalization may affect or may coincide with changes in agricultural incomes.

In many countries in the world governments heavily intervene in food and agricultural markets. Studies have shown that these government interventions are not random but follow a pattern: rich countries subsidize their farmers while poor countries tend to tax their agricultural sectors (Anderson 2010; Krueger et al. 1988). This was especially the case in the 1970s and 1980s before many agricultural policy reforms were implemented around the world (Anderson et al. 2013). Since many agricultural products are traded, these policy reforms have often coincided with trade policy reforms. Therefore the extent to which trade liberalization has affected farmers, e.g. through the removal of export taxes, may help to explain the impact on the poor, and thus on child mortality.

There is casual evidence from our country results to support this argument. For example, in Ghana, one of the few SSA countries which benefited significantly from trade liberalization, the reform of Ghana's trade policy reduced export taxation on key agricultural commodities (in particular cocoa which is a very important commodity for Ghana) and this coincided with an overall liberalizing of its agricultural policy (see Thomas 2006). These reforms reduced agricultural taxation and contributed to a significant reduction in poverty and inequality in Ghana's rural areas (Coulombe and

Wodon, 2007).²⁴ Similarly, in Sri Lanka, which has one of the most positive effects of trade liberalization in Asia, the trade liberalization caused a reduction of the taxation of agricultural export crops, especially tea, coconuts and rubber. Taxation of these main export products fell from around 40% before the trade liberalization to 20% after the reforms, contributing to agricultural productivity growth and significant poverty reduction (De Silva et al. 2013; Karunagoda et al. 2011). Also in Latin America, several countries where the impacts of trade liberalization on child mortality have been very positive according to our estimations, the trade liberalizations strongly reduced taxation of agriculture. This was, among others, the case in Chile (31%),²⁵ Mexico (18%) and Brazil (27%) (Anderson and Nelgen, 2013). Finally, in MENA the trade liberalizations with positive impacts on child mortality in, for example, Tunisia (17%), Turkey (33%) and Egypt (25%) have coincided with a reduction in taxation of agriculture and reduced rural poverty (Anderson and Nelgen, 2013; Chemingui and Thabet 2003).

In order to test more systematically whether our estimated trade liberalization effects are correlated with agricultural policy reform, we make use of the Nominal Rate of Assistance to agriculture (NRA) from Anderson (2009) and Anderson and Nelgen (2013). The NRA is an indicator of the extent of subsidization (positive NRA) or taxation (negative NRA) of the agricultural sector through government policies (including border trade policies, such as tariffs and non-tariff barriers). To check the role of and relationship with agricultural policy, we classify our trade liberalizing countries in two different groups: one group where the NRA increased more than the mean NRA change (and where farmers (at least potentially) benefited more than average from trade liberalization) and one group where the NRA increased less than the mean.

The results in Figure 5 show that the reduction of child mortality in countries with higher NRA growth – hence a stronger reduction in agricultural taxation after the reform – was significantly stronger than in lower NRA growth countries. After 5 years, child mortality in the higher-NRA group is 10% lower than the counterfactual (p -value < 0.01), and after 10 years is 13% lower (p -value < 0.01). The lower-NRA group experienced an

²⁴ For example, 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).

²⁵ In Chile, the nominal rate of assistance in agriculture shifted from 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 in depth discussion about agricultural policy reforms in Chile, see Anderson and Valdés (2008).

average reduction of only 2.8% after 5 years (p -value < 0.01) and 5.4% after 10 years (p -value < 0.01).

These results are consistent with the argument that poor people in developing countries (which are the ones primarily confronted with child mortality) benefit from trade liberalization if it benefits the sectors they work in, in this case agriculture as the poor are still mainly concentrated in rural areas and depending on agriculture for their income. This is also consistent with the more general argument that what matters for the reduction of child mortality (and reduction of poverty more generally) is not just trade reform *per se*, but the nature of the trade reform and which sectors it affects.

7. Concluding remarks

There are few empirical studies on the effects of trade liberalization on health, and these studies have empirical limitations. We hope to contribute to this literature (and this question) by using a different methodology: the synthetic control method. We analyzed the effect of trade liberalization on child mortality, exploiting 41 trade reform episodes during the last half-century. The use of this method allows the construction of better counterfactuals and to control for the time-varying nature of unobserved heterogeneity.

Our results show that on average trade liberalizations reduced child mortality, but the effects differ significantly across countries and regions. In all regions of the developing world, except SSA, there have been significant benefits from trade liberalization, on average. At the country level, almost half of the countries experienced a significant positive effect, almost half experienced no effect and in three countries, all in SSA, there was a negative effect.

Robustness tests indicate that the results are not due to spill-over effects and not driven by the simultaneous occurrence of political reforms (i.e. democratization).

Our additional analysis suggests that the negative effects of trade reforms in a few SSA countries is correlated with the simultaneous spread of HIV/AIDS in these countries. However, there are insufficient data to draw strong conclusions on this, and we cannot test whether this correlation is due to the independent simultaneous occurrence of both or whether trade has contributed to HIV/AIDS spread and thus worsened child mortality.

Overall the heterogeneity of the trade liberalization impact on child mortality is correlated with country income levels (higher income countries have stronger positive effects), their political systems (democracies show better impacts than autocracies) and

the extent to which trade liberalization reduced taxation on the agricultural sector, the main source of employment and income for the poorest people who are disproportionately confronted with child mortality. The last findings are consistent with the argument that when trade reform improves the conditions in agriculture, the effect of trade reform on child mortality appears to be better.

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Table 1. Summary of the SCM Results at the Country Level

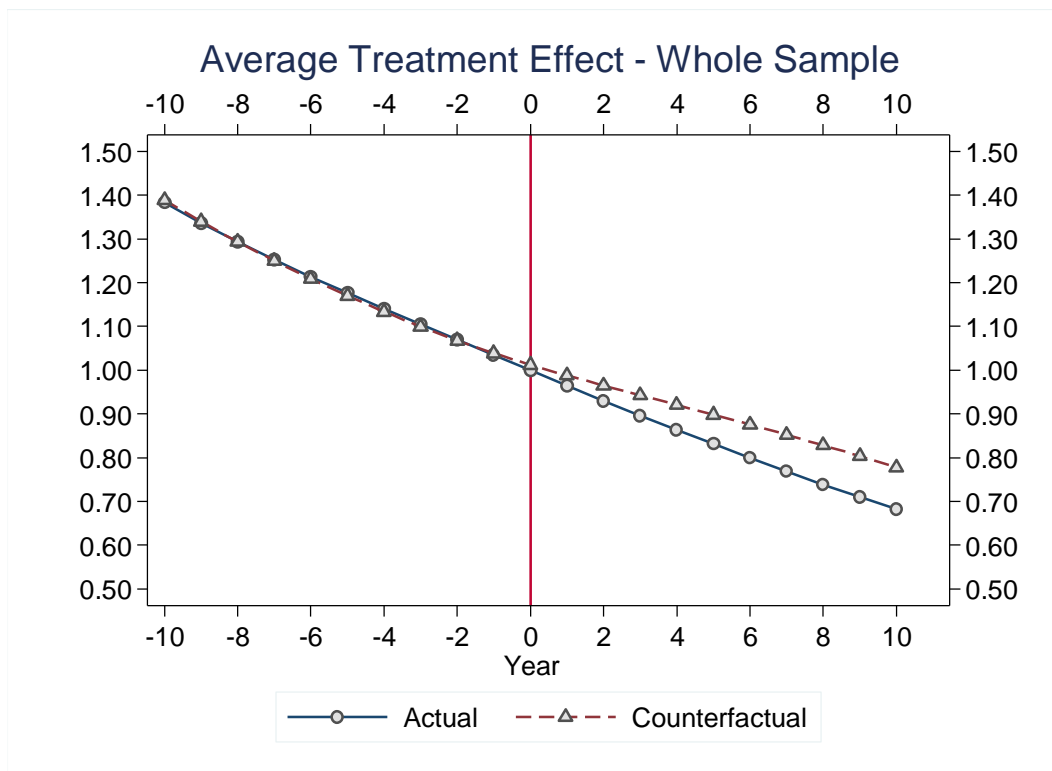
#	Country	Region	Year of Reform (T_0)	Average Treatment Effect		RMSPE	p-value
				$T+5$ (%)	$T+10$ (%)		
1	Nepal	Asia	1991	32.42%	41.21%	3.07	0.00
2	Perù	Latin America	1991	26.09%	34.46%	1.74	0.00
3	Turkey	MENA	1989	18.58%	33.95%	0.80	0.01
4	Chile	Latin America	1976	35.11%	31.50%	3.65	0.01
5	Egypt	MENA	1995	24.64%	29.67%	5.70	0.01
6	Sri Lanka	Asia	1977	11.65%	28.11%	0.47	0.07
7	Brazil	Latin America	1991	17.05%	27.27%	0.51	0.02
8	Guatemala	Latin America	1988	12.69%	25.64%	0.56	0.06
9	Tanzania	Sub-Saharan Africa	1995	11.35%	23.80%	0.61	0.01
10	Philippines	Asia	1988	17.63%	22.07%	3.08	0.08
11	Bangladesh	Asia	1996	14.76%	21.02%	6.00	0.08
12	El Salvador	Latin America	1989	11.76%	19.98%	1.25	0.02
13	Gambia	Sub-Saharan Africa	1985	12.97%	19.13%	2.50	0.06
14	Mexico	Latin America	1986	12.15%	18.50%	0.66	0.11
15	Ghana	Sub-Saharan Africa	1985	11.60%	17.97%	1.40	0.11
16	Tunisia	MENA	1989	10.54%	17.55%	0.65	0.12
17	Nicaragua	Latin America	1991	12.39%	17.38%	0.72	0.01
18	Indonesia	Asia	1970	7.10%	15.20%	0.94	0.07
19	Madagascar	Sub-Saharan Africa	1996	6.04%	11.23%	1.41	0.18
20	Burkina Faso	Sub-Saharan Africa	1998	-1.90%	8.47%	1.14	0.39
21	Morocco	MENA	1984	3.46%	8.01%	0.21	0.37
22	Zambia	Sub-Saharan Africa	1993	-4.97%	7.37%	12.42	n.a.
23	Cape Verde	Sub-Saharan Africa	1991	3.45%	5.52%	0.52	0.16
24	Guyana	Latin America	1988	9.04%	4.82%	3.25	0.20
25	Honduras	Latin America	1991	4.40%	4.80%	0.52	0.21
26	Uganda	Sub-Saharan Africa	1988	2.07%	4.76%	6.27	n.a.
27	Burundi	Sub-Saharan Africa	1999	0.66%	4.08%	0.25	0.09
28	Cote d'Ivoire	Sub-Saharan Africa	1994	-1.39%	2.94%	1.68	0.39
29	Guinea-Bissau	Sub-Saharan Africa	1987	-1.35%	2.86%	1.96	0.54
30	Benin	Sub-Saharan Africa	1990	6.88%	2.66%	1.52	0.17
31	Guinea	Sub-Saharan Africa	1986	1.08%	2.12%	23.20	n.a.
32	Ethiopia	Sub-Saharan Africa	1996	-0.98%	0.09%	1.76	0.17
33	Paraguay	Latin America	1989	4.30%	-0.32%	1.34	0.25
34	Colombia	Latin America	1970	4.46%	-1.24%	5.56	0.40
35	Cameroon	Sub-Saharan Africa	1993	-14.66%	-2.34%	3.04	0.20
36	Dominican Republic	Latin America	1992	0.91%	-4.72%	0.44	0.26
37	Mozambique	Sub-Saharan Africa	1995	2.33%	-6.57%	2.62	0.32
38	Botswana	Sub-Saharan Africa	1979	-4.90%	-7.33%	0.51	0.34
39	Kenya	Sub-Saharan Africa	1994	-22.71%	-22.67%	4.55	0.00
40	Mauritania	Sub-Saharan Africa	1995	-6.13%	-24.14%	0.40	0.12
41	South Africa	Sub-Saharan Africa	1991	-15.85%	-51.97%	0.22	0.09

Notes: The Table summarizes the key SCM results at the country level reported in details in Table A1-A4 of the 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. p -value is not available (n.a.) for those countries showing a RMSPE > 6, where a good counterfactual could not be constructed. See Text.

Table 2. Average HIV infections in the post-treatment period by group of countries

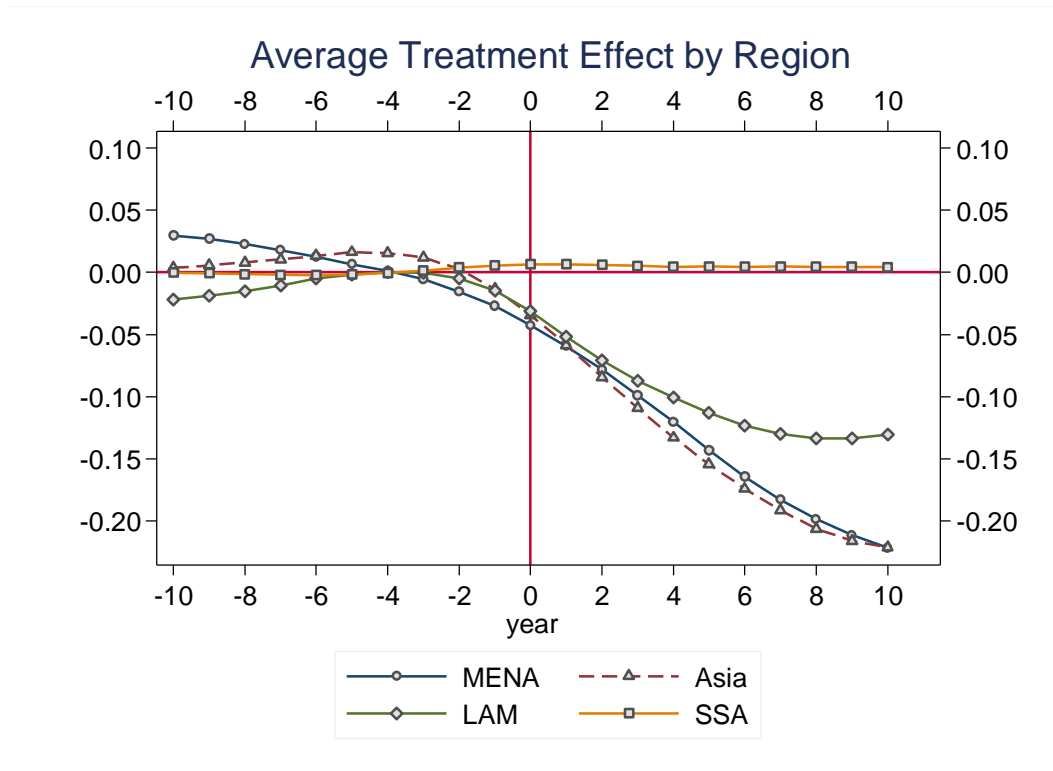
	Average HIV prevalence (% of pop. ages 15-49)	Std. Error	Number of countries
Positive and Significant Effect	1.1%	2.03	19
No Significant Effect	3.3%	4.23	19
Negative and Significant Effect	5.8%	5.50	3

Note: Data on HIV prevalence are taken from The World Bank and refer for each country to the post treatment period.

Figure 1. Average Treatment Effect Across all 41 SCM experiments

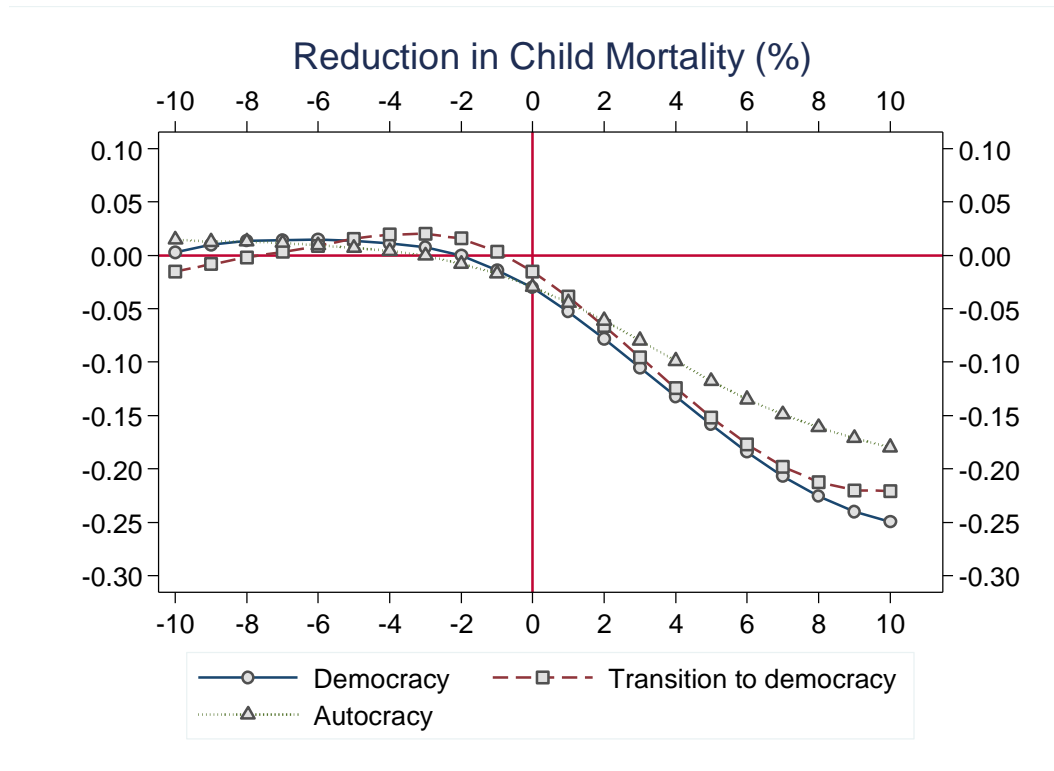
Notes: The figure reports the dynamic treatment effects aggregated across all the 41 reform effects using the equation (5); the corresponding p -value, computed as discussed in section 2.2, is reported in the text (in bracket). Before aggregating the individual country estimates are normalized by setting the child mortality of the treated country equal to 1 in the year of trade reform, T_0 .

Figure 2. Average Treatment Effect Aggregated at Regional level



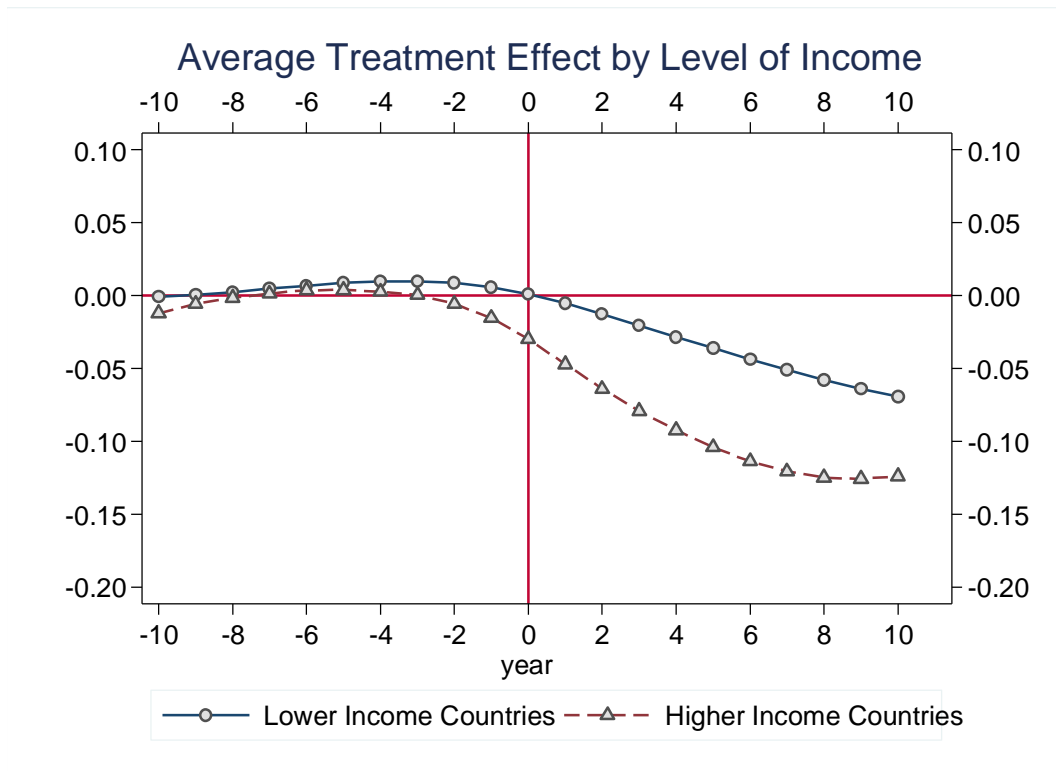
Notes: The figure reports the dynamic treatment effects aggregated at regions level using the equation (5); the corresponding p -values, computed as discussed in section 2.2, are reported in the text (in bracket). Each regional effect is obtained by averaging the contribution of all the treated countries 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 (MENA), respectively. Countries used to estimate the average regional effect for Africa are those reported in table 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. See Text.

Figure 3. Average Treatment Effect Across Different Political Regimes



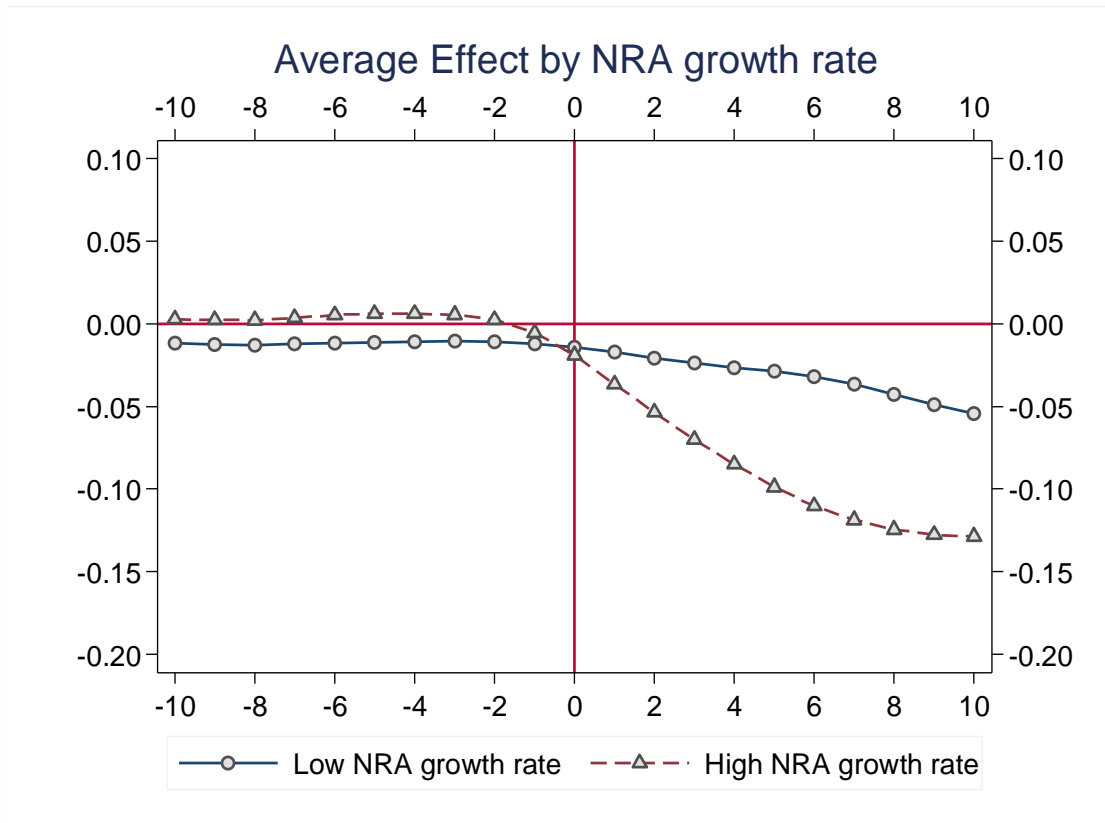
Notes: The Figure reports the average treatment effect of trade reforms aggregated across different political regimes using equation (5); the corresponding p -values, computed as discussed in section 2.2, are reported in the text (in bracket). “Transition to democratizations” corresponds to trade liberalizations that occurred near political reforms in the following five countries (in parenthesis the year of democratization): Burundi 1999 (2001); Guatemala 1988 (1986); Nepal 1991 (1990); Nicaragua 1991 (1990); and Philippines 1988 (1987). “Democracy” correspond to trade liberalization that occurred in consolidated democracies in the following seven countries: Bangladesh, Brazil, El Salvador, Gambia, Perù, Sri Lanka and Turkey. Finally, “Autocracy” correspond to trade liberalizations that occurred in permanent autocracies in the following six countries: Egypt, Ghana, Indonesia, Mexico, Tanzania and Tunisia. See text.

Figure 4. Average Treatment Effect in High vs. Low income Groups



Notes: The figure reports the dynamic treatment effects aggregated across income groups (High vs. Low) using the equation (5); the corresponding p -values, computed as discussed in section 2.2, are reported in the text (in bracket). Each income group effect is obtained by averaging the contribution of all the treated countries within the same group in terms of yearly deviation of the outcome variable (U5MR) with respect to the one of the respective synthetic control.

Figure 5. Average Treatment Effect in High vs. Low NRA Growth Rate



Notes: The figure reports the dynamic treatment effects aggregated across NRA growth rate groups (High vs. Low) using the equation (5); the corresponding p -values, computed as discussed in section 2.2, are reported in the text (in bracket). Each NRA growth rate group effect is obtained by averaging the contribution of all the treated countries (with NRA data) within the same group in terms of yearly deviation of the outcome variable (U5MR) with respect to the one of the respective synthetic control. Countries belonging to the Low-NRA growth rate group are: Bangladesh, Benin, Burkina Faso, Côté d'Ivoire, Cameroon, Colombia, Ethiopia, Nicaragua, Sri Lanka, South Africa, Tanzania, and Zambia. Countries belonging to the High-NRA growth rate group are: Brazil, Chile, Dominican Republic, Egypt, Ghana, Indonesia, Kenya, Madagascar, Mexico, Mozambique, Philippines, Turkey, and Uganda.

Appendix A. Detailed results of the Synthetic Control case studies

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

	Indonesia 1970	Synthetic Control	Sri Lanka 1977	Synthetic Control	Philippines 1988	Synthetic Control
War	0.10	0.05	0.06	0.00	0.29	0.36
Log GDP per-capita	6.52	7.26	6.68	8.89	7.58	7.81
Rurale population	0.84	0.82	0.79	0.70	0.64	0.60
Population growth	0.03	0.03	0.02	0.02	0.03	0.03
Primary school	8.85	5.68	9.49	27.47	20.98	3.98
U5MR T_0	165.20	165.06	59.30	59.42	65.60	66.83
U5MR T+5	139.89	151.62	42.10	49.01	49.90	61.46
U5MR T+10	120.00	145.11	24.40	41.07	42.09	56.58
RMSPE		0.94		0.47		3.08
	Nepal 1991	Synthetic Control	Bangladesh 1996	Synthetic Control		
War	0	0	0.00	0.01		
Log GDP per-capita	6.39	7.12	6.58	7.31		
Rurale population	0.95	0.68	0.87	0.77		
Population growth	0.02	0.03	0.02	0.02		
Primary school	0.46	12.68	9.49	2.78		
U5MR T_0	135.00	141.57	108.10	117.50		
U5MR T+5	103.50	147.26	83.59	99.56		
U5MR T+10	77.20	132.83	63.40	86.12		
RMSPE		3.07		6.00		

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Indonesia:** Cameroon (0.127); India (0.049); Philippines (0.066); Papua Nuova Guinea (0.117); Trinidad and Tobago (0.07); Tunisia (0.232); Uganda (0.338). **Synthetic Sri Lanka:** Algeria (0.019); Nicaragua (0.098); Paraguay (0.125); Trinidad and Tobago (0.585); Venezuela (0.173). **Synthetic Philippines:** China (0.105); Iraq (0.58); Papua New Guinea (0.315). **Synthetic Nepal:** Malawi (0.011); Senegal (0.989); **Synthetic Bangladesh:** Haiti (0.969); Iran (0.031).

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	0.09	0.00	0.01	0.00	0
Log GDP per-capita	7.21	7.44	7.19	7.01	7.12	6.13
Rurale population	0.92	0.61	0.71	0.71	0.78	0.95
Population growth	0.03	0.03	0.03	0.03	0.03	0.02
Primary school	4.92	6.19	3.52	3.93	0.54	4.61
U5MR T ₀	76.60	76.71	154.70	154.83	203.30	206.42
U5MR T+5	58.29	54.54	128.10	146.05	169.70	196.06
U5MR T+10	48.20	42.58	113.30	141.10	141.00	179.89
RMSPE		0.51		1.40		2.50
	Guinea 1986	Synthetic Control	Guinea- Biss. 1987	Synthetic Control	Uganda 1988	Synthetic Control
War	0.00	0.00	0.00	0.09	0.25	0.00
Log GDP per-capita	6.67	6.27	6.88	6.66	6.50	6.78
Rurale population	0.82	0.93	0.83	0.83	0.93	0.74
Population growth	0.01	0.03	0.02	0.02	0.03	0.03
Primary school					5.22	6.42
U5MR T ₀	259.60	252.70	211.70	209.05	180.40	183.24
U5MR T+5	235.30	238.10	201.60	198.73	169.60	173.33
U5MR T+10	201.50	207.00	185.00	191.06	157.39	165.99
RMSPE		23.20		1.96		6.27
	Benin 1990	Synthetic Control	Cape Verde 1991	Synthetic Control	South Africa 1991	Synthetic Control
War	0.00	0.00	0.00	0.20	0.26	0.27
Log GDP per-capita	6.76	6.78	6.97	7.66	8.55	8.47
Rurale population	0.78	0.79	0.76	0.65	0.52	0.52
Population growth	0.02	0.03	0.02	0.03	0.02	0.03
Primary school	2.51	8.39			8.98	6.67
U5MR T ₀	180.70	181.85	59.1	59.93	59.30	59.41
U5MR T+5	158.20	170.63	47.50	49.54	61.70	52.30
U5MR T+10	147.40	152.21	35.50	38.76	76.70	45.88
RMSPE		1.52		0.52		0.225

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Botswana:** Argentina (0.001); China (0.158); Algeria (0.12); Panama (0.092); Syria (0.629). **Synthetic Ghana:** Algeria (0.016); Central African Republic (0.588); Panama (0.006); Papua New Guinea (0.027); Senegal (0.083); Syria (0.096); Togo

(0.185). **Synthetic Gambia:** Burundi (0.597); Malawi (0.403). **Synthetic Guinea:** Malawi (1.0). **Synthetic Guinea Bissau:** Central African Republic (0.171); Chad (0.829). **Synthetic Uganda:** Central African Republic (0.519); Malawi (0.207); Senegal (0.274). **Synthetic Benin:** Central African Republic (0.144); Malawi (0.383); Papua Nuova Guinea (0.063); Senegal (0.41). **Synthetic Cape Verde:** China (0.301); India(0.124); Iran (0.453); Syria (0.122). **Synthetic South Africa:** China (0.016); Gabon (0.219); Haiti (0.016); Iran (0.305); Iraq (0.243); Syria (0.2).

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

	Cameroon 1993	Synthetic Control	Kenya 1993	Synthetic Control	Zambia 1993	Synthetic Control
War	0.03	0.06	0.00	0.04	0.00	0.00
Log GDP per-capita	7.47	6.86	6.99	6.42	7.15	6.69
Rurale population	0.72	0.69	0.87	0.76	0.66	0.72
Population growth	0.03	0.03	0.04	0.02	0.03	0.02
Primary school	10.32	2.77	16.32	9.76	8.92	4.67
U5MR T ₀	143.50	136.36	106.00	95.84	192.40	177.02
U5MR T+5	155.10	134.06	112.90	88.83	179.10	169.54
U5MR T+10	134.60	131.25	103.50	79.47	143.30	157.48
RMSPE		3.039		4.55		12.422
	Cote d'Ivoire 1994	Synthetic Control	Mauritania 1995	Synthetic Control	Mozambique 1995	Synthetic Control
War	0.00	0.02	0.00	0.04	0.37	0.00
Log GDP per-capita	7.25	6.63	7.21	6.96	5.86	6.35
Rurale population	0.67	0.71	0.76	0.74	0.88	0.89
Population growth	0.04	0.02	0.03	0.03	0.02	0.03
Primary school	3.73	5.49	13.45	11.69	5.32	8.22
U5MR T ₀	152.3	149.01	118.60	118.50	208.40	206.69
U5MR T+5	147.40	145.28	110.50	103.23	165.70	170.99
U5MR T+10	134.50	138.98	101.70	73.07	131.50	117.85
RMSPE		1.680		0.402		2.617
	Tanzania 1995	Synthetic Control	Ethiopia 1996	Synthetic Control	Madagascar 1996	Synthetic Control
War	0.00	0.00	0.44	0.00	0.00	0.00
Log GDP per-capita	6.37	7.08	6.06	6.66	6.89	7.11
Rurale population	0.87	0.70	0.90	0.86	0.82	0.80
Population growth	0.03	0.03	0.02	0.03	0.03	0.02
Primary school	12.82	7.98				
U5MR T ₀	159.60	159.38	167.70	172.07	131.80	132.62
U5MR T+5	131.50	149.62	139.70	136.17	138.06	110.56
U5MR T+10	90.10	128.09	101.90	102.06	76.69	91.50
RMSPE		0.613		1.76		1.41
	Burkina Faso 1998	Synthetic Control	Burundi 1999	Synthetic Control		
War	0.00	0.10	0.00	0.03		
Log GDP per-capita	6.37	6.98	6.12	6.56		
Rurale population	0.91	0.74	0.96	0.73		
Population growth	0.02	0.03	0.02	0.02		
Primary school			6.26	6.21		
U5MR T ₀	191.40	191.59	151.30	151.39		
U5MR T+5	174.00	170.37	138.5	140.12		
U5MR T+10	131.60	147.82	115.8	124.57		
RMSPE		1.14		0.25		

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 Kenya:** China (0.3); Gabon (0.061); Iran (0.093); Malawi (0.228); Senegal (0.317); **Synthetic Zambia:** Central Africa Republic (0.859); Malawi (0.141). **Synthetic Cote d'Ivoire:** Central African Republic (0.676); Rep. Dem. Congo (0.165); China (0.165). **Synthetic Mauritania:**

China (0.3); Gabon (0.061); Iran (0.093); Malawi (0.228); Senegal (0.317). **Synthetic Mozambique:** Malawi (0.902); Senegal (0.096). **Synthetic Tanzania:** Central African Republic (0.495); Gabon (0.141); Malawi (0.164); Senegal (0.199). **Synthetic Ethiopia:** Haiti (0.401); Malawi (0.599). **Synthetic Madagascar:** Haiti (0.854); Malawi (0.146). **Synthetic Burkina Faso:** Central African Republic (0.106); Rep. Dem. Congo (0.101); Malawi (0.018); Nigeria (0.621); Chad (0.155). **Synthetic Burundi:** Central African Republic (0.594); China (0.134); Rep. Dem. Congo (0.155); Malawi (0.117).

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	0.09
Log GDP per-capita	8.32	7.81	8.28	7.71	8.90	7.13
Rurale population	0.26	0.56	0.43	0.56	0.39	0.68
Population growth	0.02	0.03	0.02	0.03	0.03	0.03
Primary school	24.97	16.42	20.42	3.41	15.69	5.86
U5MR T ₀	57.10	62.70	40.40	45.70	56.20	57.26
U5MR T+5	30.00	50.05	34.09	35.90	43.80	50.63
U5MR T+10	22.10	40.08	28.90	29.79	32.70	43.10
RMSPE		3.65		5.56		0.66
	Guyana 1988	Synthetic Control	Guatemala 1988	Synthetic Control	El Salvador 1989	Synthetic Control
War	0.00	0.07	0.00	0.05	0.31	0.23
Log GDP per-capita	7.93	6.15	8.35	7.87	8.31	8.32
Rurale population	0.70	0.80	0.64	0.61	0.58	0.57
Population growth	0.01	0.02	0.03	0.03	0.02	0.03
Primary school	41.30	11.60	7.34	7.89	10.37	6.40
U5MR T ₀	63.10	63.61	88.40	88.92	62.60	64.23
U5MR T+5	55.29	61.00	69.50	80.72	46.90	54.26
U5MR T+10	48.79	51.84	55.09	77.77	33.90	46.41
RMSPE		3.25		0.56		1.25
	Paraguay 1989	Synthetic Control	Brazil 1991	Synthetic Control	Honduras 1991	Synthetic Control
War	0.00	0.09	0.00	0.32	0.03	0.16
Log GDP per-capita	7.82	7.00	8.46	8.09	7.91	8.05
Rurale population	0.60	0.67	0.39	0.51	0.68	0.59
Population growth	0.03	0.03	0.02	0.03	0.03	0.03
Primary school	15.14	7.54	15.51	5.59	10.79	5.09
U5MR T ₀	47.20	48.22	59.20	59.62	56.20	57.08
U5MR T+5	39.59	41.63	44.20	54.29	45.09	47.57
U5MR T+10	33.79	33.65	30.79	46.95	36.29	39.00
RMSPE		1.34		0.51		0.52
	Nicaragua 1991	Synthetic Control	Perù 1991	Synthetic Control	Dominican Rep. 1992	Synthetic Control
War	0.26	0.28	0.26	0.19	0.00	0.15
Log GDP per-capita	8.05	8.18	8.46	8.08	8.18	7.93
Rurale population	0.52	0.54	0.40	0.58	0.54	0.59
Population growth	0.03	0.03	0.03	0.03	0.03	0.03
Primary school	9.10	6.44	14.18	8.19	7.72	5.54
U5MR T ₀	63.30	63.40	74.90	77.84	55.00	55.33
U5MR T+5	49.70	57.54	53.60	73.14	44.70	45.20
U5MR T+10	38.10	49.10	37.00	62.81	37.29	34.70
RMSPE		0.72		1.74		0.44

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Chile:** Panama (0.58); Syria (0.42). **Synthetic Colombia:** China (0.219); Rep. Dem. Congo (0.066); Algeria (0.084); Iran (0.104); Syria (0.494); Swaziland (0.033); **Synthetic Mexico:** Burundi (0.051); China (0.288); Papua Nuova Guinea (0.061); Syria (0.6). **Synthetic Guyana:** Central African Republic (0.084); China (0.916). **Synthetic Guatemala:** Algeria (0.027); Gabon (0.221); Haiti (0.025); Senegal (0.298); Syria (0.429). **Synthetic El Salvador:** Algeria (0.038); Iran (0.57); Senegal

(0.122); Syria (0.27). **Synthetic Paraguay:** Central African Republic (0.018); China (0.44); Syria (0.542). **Synthetic Brazil:** Iran (0.237); Iraq (0.353); Senegal (0.132); Syria (0.277). **Synthetic Honduras:** Haiti (0.095); Iran (0.349); Papua Nuova Guinea (0.055); Senegal (0.022); Syria (0.478). **Synthetic Nicaragua:** Algeria (0.034); China (0.011); Gabon (0.009); Iran (0.366); Iraq (0.224); Senegal (0.155); Syria (0.202). **Synthetic Perù:** Iran (0.465); Iraq (0.04); Senegal (0.316); Syria (0.179). **Synthetic Dominican Republic:** China (0.033); Iran (0.311); Malawi (0.077); Syria (0.58).

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

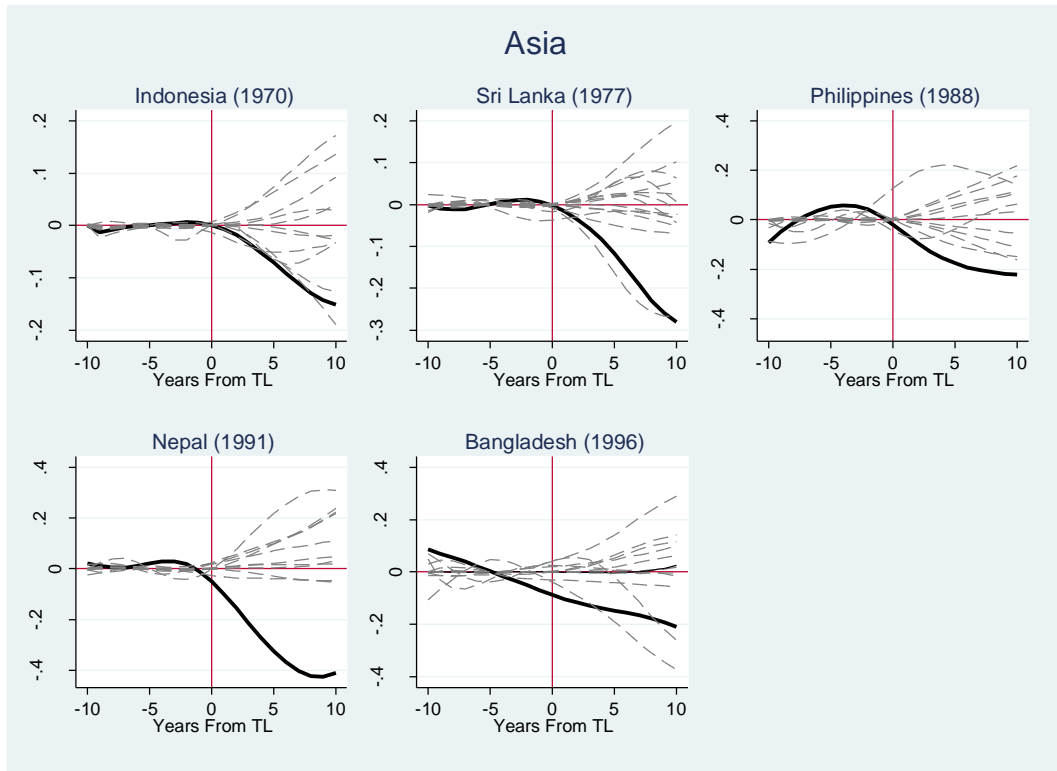
	Morocco 1984	Synthetic Control	Tunisia 1989	Synthetic Control	Turkey 1989	Synthetic Control
War	0.08	0.10	0.03	0.08	0.03	0.12
Log GDP per-capita	7.29	7.30	7.99	7.70	8.44	7.80
Rurale population	0.64	0.64	0.53	0.58	0.58	0.60
Population growth	0.03	0.03	0.02	0.03	0.02	0.03
Primary school	1.94	2.89	5.88	4.73	18.12	7.23
U5MR T ₀	108.40	108.34	53.90	54.26	78.10	79.47
U5MR T+5	83.80	87.55	41.40	47.08	58.00	72.51
U5MR T+10	66.40	75.09	31.50	40.96	40.59	67.11
RMSPE		0.208		0.648		0.798

	Egypt 1995	Synthetic Control
War	0.06	0.05
Log GDP per-capita	7.30	7.95
Rurale population	0.57	0.64
Population growth	0.02	0.03
Primary school	3.54	2.47
U5MR T ₀	64.20	73.49
U5MR T+5	45.10	60.92
U5MR T+10	31.20	50.25
RMSPE		5.70

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Morocco:** Algeria (0.038); Burundi (0.046); Central African Republic (0.112); China (0.008); Egypt (0.396); Iraq (0.058); Papua Nuova Guinea (0.117); Syria (0.225). **Synthetic Tunisia:** Algeria (0.161); China (0.036); Senegal (0.116); Syria (0.687). **Synthetic Turkey:** Algeria (0.092); China (0.027); Iran (0.216); Senegal (0.322); Syria (0.344). **Synthetic Egypt:** Algeria (0.627); Haiti (0.373).

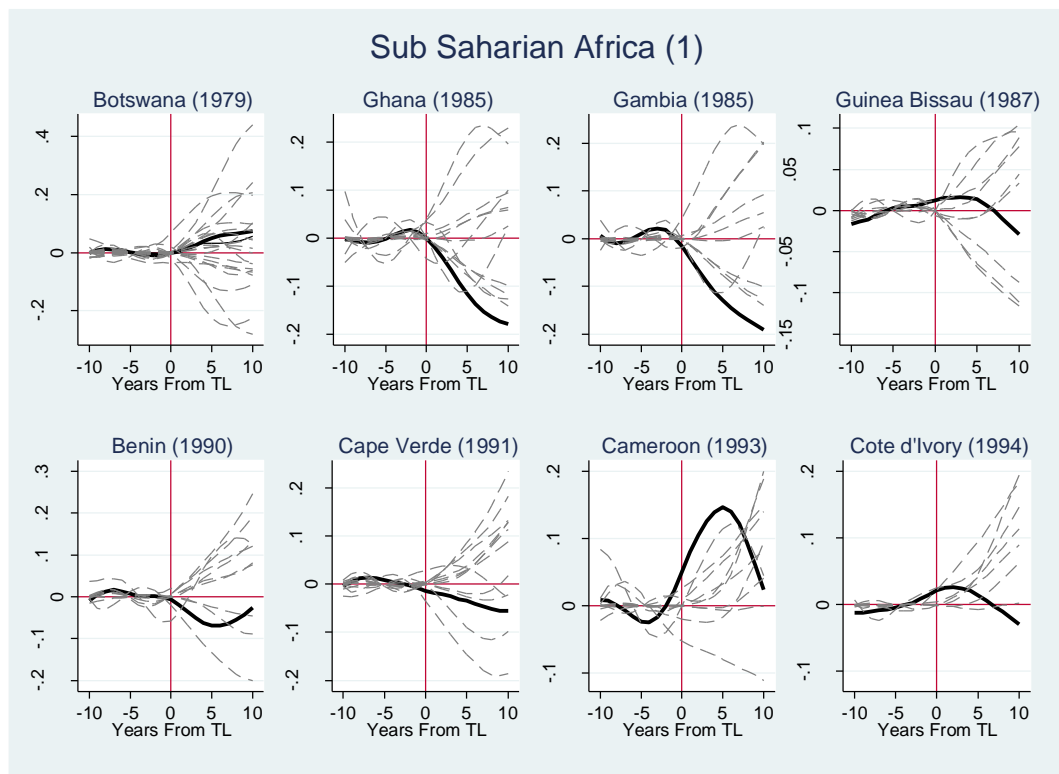
Appendix B. Placebo Tests

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



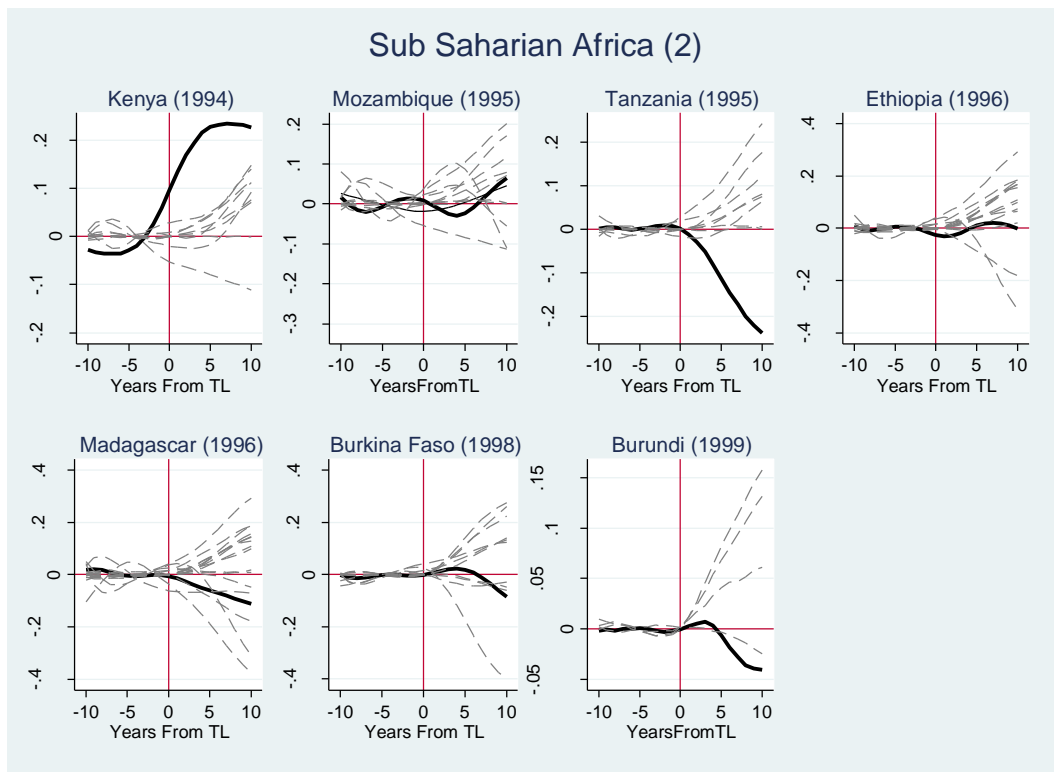
Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

Figure B2a. Placebo tests for African SCM Experiments with “Positive” Reform Effects



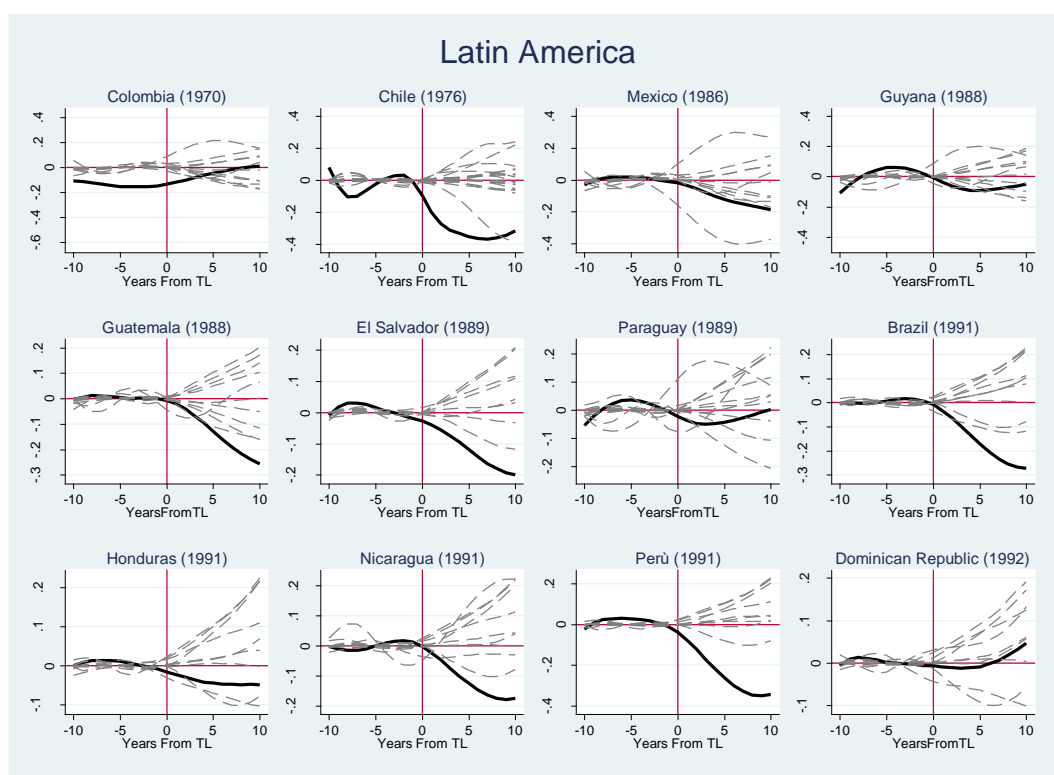
Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

Figure B2b. Placebo tests for African SCM Experiments with “Positive” Reform Effects



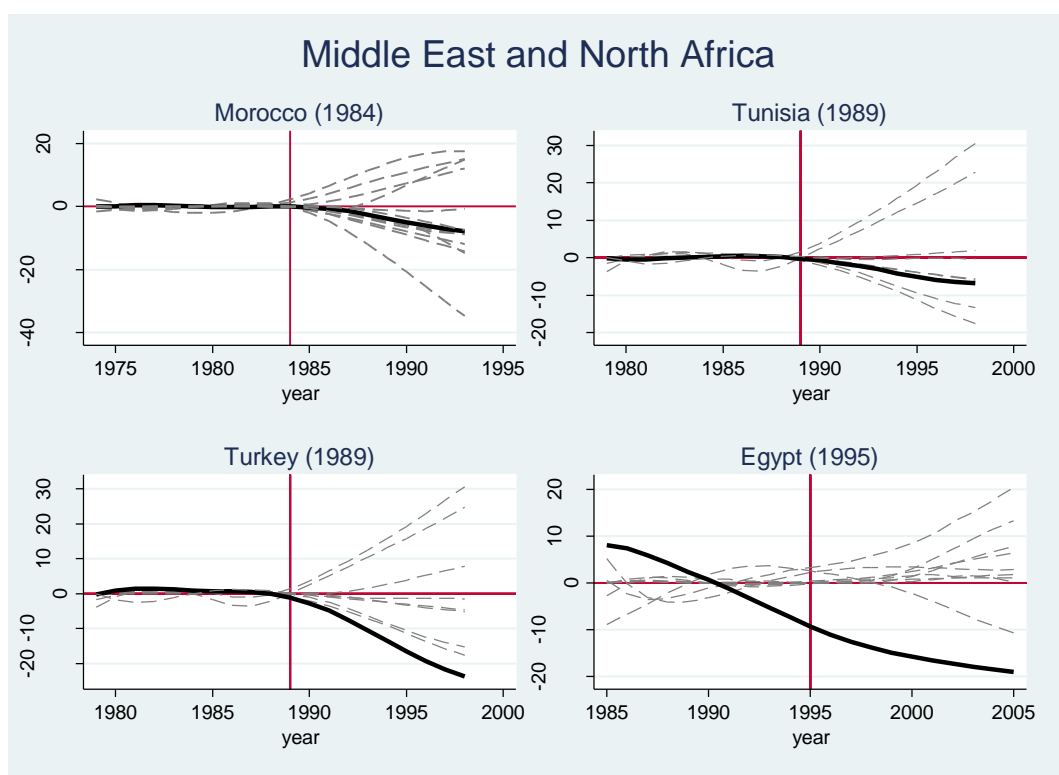
Notes: the bold line reports the outcome difference between each treated unit and the synthetic control; instead the grey dash lines report the outcome differences between each (false) treated country (from the donor pool) and their synthetic control in the placebo tests.

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 MENA 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.

Appendix C: Robustness Check to SUTVA

Table C1: Summary of the SCM Results Accounting for Possible SUTVA Violation

#	Country	Region	Year of Reform (T_0)	Baseline Model				Considering SUTVA			
				Average Treatment Effect		RMSPE	p-value	Average Treatment Effect		RMSPE	p-value
				T+5	T+10			T+5 (%)	T+10 (%)		
1	Nepal	Asia	1991	32.42%	41.21%	3.07	0.00	32.42%	41.21%	3.07	0.00
2	Perù	Latin America	1991	26.09%	34.46%	1.74	0.00	26.09%	34.46%	1.74	0.00
3	Turkey	MENA	1989	18.58%	33.95%	0.80	0.01	23.14%	40.76%	1.63	0.01
4	Chile	Latin America	1976	35.11%	31.50%	3.65	0.01	35.11%	31.50%	3.65	0.01
5	Egypt	MENA	1995	24.64%	29.67%	5.70	0.01	24.64%	29.67%	5.70	0.01
6	Sri Lanka	Asia	1977	11.65%	28.11%	0.47	0.07	11.65%	28.11%	0.47	0.07
7	Brazil	Latin America	1991	17.05%	27.27%	0.51	0.02	17.05%	27.27%	0.51	0.02
8	Guatemala	Latin America	1988	12.69%	25.64%	0.56	0.06	12.69%	25.64%	0.56	0.06
9	Tanzania	Sub-Saharan Africa	1995	11.35%	23.80%	0.61	0.01	12.95%	33.88%	1.83	0.04
10	Philippines	Asia	1988	17.63%	22.07%	3.08	0.08	19.06%	24.16%	3.10	0.09
11	Bangladesh	Asia	1996	14.76%	21.02%	6.00	0.08	14.76%	21.02%	6.00	0.08
12	El Salvador	Latin America	1989	11.76%	19.98%	1.25	0.02	11.76%	19.98%	1.25	0.02
13	Gambia	Sub-Saharan Africa	1985	12.97%	19.13%	2.50	0.06	12.97%	19.13%	2.50	0.06
14	Mexico	Latin America	1986	12.15%	18.50%	0.66	0.11	12.15%	18.50%	0.66	0.11
15	Ghana	Sub-Saharan Africa	1985	11.60%	17.97%	1.40	0.11	6.73%	12.00%	1.37	0.13
16	Tunisia	MENA	1989	10.54%	17.55%	0.65	0.12	4.51%	6.97%	0.47	0.10
17	Nicaragua	Latin America	1991	12.39%	17.38%	0.72	0.01	12.39%	17.38%	0.72	0.01
18	Indonesia	Asia	1970	7.10%	15.20%	0.94	0.07	8.72%	19.03%	1.06	0.04
19	Madagascar	Sub-Saharan Africa	1996	6.04%	11.23%	1.41	0.18	6.04%	11.23%	1.41	0.18
20	Burkina Faso	Sub-Saharan Africa	1998	-1.90%	8.47%	1.14	0.39	-1.90%	8.47%	1.14	0.39
21	Morocco	MENA	1984	3.46%	8.01%	0.21	0.37	3.22%	7.91%	0.20	0.25
22	Zambia	Sub-Saharan Africa	1993	-4.97%	7.37%	12.42	n.a.	-4.24%	0.96%	15.97	n.a.
23	Cape Verde	Sub-Saharan Africa	1991	3.45%	5.52%	0.52	0.16	3.45%	5.52%	0.52	0.16
24	Guyana	Latin America	1988	9.04%	4.82%	3.25	0.20	9.04%	4.82%	3.25	0.20
25	Honduras	Latin America	1991	4.40%	4.80%	0.52	0.21	4.40%	4.80%	0.52	0.21
26	Uganda	Sub-Saharan Africa	1988	2.07%	4.76%	6.27	n.a.	2.07%	4.76%	6.27	n.a.
27	Burundi	Sub-Saharan Africa	1999	0.66%	4.08%	0.25	0.10	0.96%	5.70%	0.30	0.10
28	Cote d'Ivoire	Sub-Saharan Africa	1994	-1.39%	2.94%	1.68	0.39	-1.39%	2.94%	1.68	0.39
29	Guinea-Bissau	Sub-Saharan Africa	1987	-1.35%	2.86%	1.96	0.54	-1.35%	2.86%	1.96	0.54
30	Benin	Sub-Saharan Africa	1990	6.88%	2.66%	1.52	0.17	6.88%	2.66%	1.52	0.17
31	Guinea	Sub-Saharan Africa	1986	1.08%	2.12%	23.20	n.a.	1.08%	2.12%	23.20	n.a.
32	Ethiopia	Sub-Saharan Africa	1996	-0.98%	0.09%	1.76	0.17	-0.98%	0.09%	1.76	0.17
33	Paraguay	Latin America	1989	4.30%	-0.32%	1.34	0.25	4.30%	-0.32%	1.34	0.25
34	Colombia	Latin America	1970	4.46%	-1.24%	5.56	0.40	4.46%	-1.24%	5.56	0.40
35	Cameroon	Sub-Saharan Africa	1993	-14.66%	-2.34%	3.04	0.20	-14.64%	-2.3%	3.04	0.17
36	Dominican Republic	Latin America	1992	0.91%	-4.72%	0.44	0.26	0.91%	-4.72%	0.44	0.26
37	Mozambique	Sub-Saharan Africa	1995	2.33%	-6.57%	2.62	0.32	18.09%	30.09%	59.21	n.a.
38	Botswana	Sub-Saharan Africa	1979	-4.90%	-7.33%	0.51	0.34	-4.90%	-7.33%	0.51	0.34
39	Kenya	Sub-Saharan Africa	1994	-22.71%	-22.67%	4.55	0.00	-22.71%	-22.67%	4.55	0.00
40	Mauritania	Sub-Saharan Africa	1995	-6.13%	-24.14%	0.40	0.12	-0.98%	-0.85%	0.26	0.41
41	South Africa	Sub-Saharan Africa	1991	-15.85%	-51.97%	0.22	0.09	-15.85%	-51.97%	0.22	0.09

Notes: The Table summarizes the key SCM results at the country level by excluding from the donor pool those countries that share a national border with the treated unit in order to account for the possible violation of the SUTVA. The SCM experiments presented in bold are those where border countries were included in the synthetic control in the previous (baseline) experiments. The right hand side of the table presents the SCM results for these countries when excluding from the donor pool the border ones. 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. The detailed results for the SCM experiment in bold are not shown to save space, but are available upon request. Percentage differences between the average treatment effect in treated and untreated countries are obtained by considering normalized values of child mortality (i.e. child mortality in treated countries equal to 1 at time T_0).