Food Security, Health and Trade Liberalization*

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Abstract
This paper investigates the effect of trade liberalization on food security outcomes, measured as children mortality rate, by exploiting 40 policy reform episodes observed in the 1960-2010. We use a new approach – the Synthetic Control Method – that provides a way to choose the "best" comparison units in comparative case studies. Using this method, we compare the trajectory of post-reform food security/health outcomes of treated countries, which experienced a trade liberalization, with the trajectory of a combination of similar but untreated countries. Among 40 investigated case studies, in about 20 we find a significant (short-) and long-run reduction in children mortality after the trade reform; in 19 cases we do not detect any relevant effect; only in one case − South Africa − we find a significant increase in child mortality, though largely attributable to the AIDS infection diffusion.

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JEL Classification: Q17, Q18, O13, O24, O57, I15, F13, F14.

1. Introduction

Food insecurity and health problems are emerging as increasingly relevant issues at international level. Indeed, although the past half-century has seen a marked improvement in food production, as well as in sanitation, more than one in seven people still suffers from undernourishment, and even more from malnourishment (Charles et al., 2010; Wheeler and von Braun, 2013). So far, a vast research on the factors influencing food security and malnutrition has been carried out at micro-level (e.g. Bhutta et al. 2008;

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Bashir et al., 2012; Kassie et al., 2012; Bertelli and Macours, 2013). However, less attention has been paid to studying the macro determinants of food security and health outcomes, such as macro-economic shocks, trade liberalization, and institutional and political reforms (Headey, 2013). In particular, the link between international trade and food security has become crucial after the recent commodity price spikes in the world market. Moreover, the Doha Development Round of the World Trade Organization (WTO) has shown its interest in predicting the implications of a further trade liberalization on food security in a more uncertain world.

Understanding the relationship between trade liberalization and food security has become crucial in a world where there is growing consensus on the idea that trade could disrupts more than contributes to resolve food security and health issues (see Matthews, 2014 for a discussion). Worse, this consensus is not only limited to development activists or prominent NGOs, and it is not only the consequence of the financial crisis. In fact, months before Lehman Brothers failed in 2008, the WTO’s Doha trade talks collapsed in Geneva largely because India and China wanted bigger safeguards against agricultural imports than America felt able to accept.[1]

A problem in studying the relation between trade liberalization, food security and health outcomes is that the predictions from international trade theory are somewhat controversial and difficult to establish univocally (e.g. Panagariya, 2005; Swinnen, 2011). International trade is the mechanism that balances domestic supply and demand. Hence, trade and access to markets are fundamental to ensure global food security (Matthews, 2014). Overall, trade liberalization increases the efficiency allocation and the use of scarce resources. Moreover, as predicted by the endogenous growth models (Grossman and Helpman, 1991), the participation in international trade also creates new opportunities for innovation and stronger productivity growth due to the improvements in the techniques and practices available to farmers and food companies (Fleming and Abler, 2013).

However, as a matter of fact, trade liberalization makes some individuals better off and others worse off and this income redistribution can be systematically biased in favor or against poor individuals. For example, trade can pose challenges to food security and malnutrition in developing countries, because agricultural trade liberalization can adversely affect the income of consumers and/or of producers of import-competing crops. Moreover, trade can lead to increased risk through imported price volatility (Matthews,

[1] For an update discussion about the evidence and the determinants of the “anti-globalization” waves of the last years, see the recent special report of the Economist (Economist, 12 Oct. 2013).

2014). Thus, the direction of the net effects of an increase in food price could be difficult to be established properly, and this effects are often country- or even region-specific.[2]

The ambiguity of the relation between (agricultural) trade liberalization and food security is well documented by the current empirical evidence. For example, McCorriston et al. (2013) in a recent systematic review of the empirical studies showed that across 34 case studies addressing this issue, 13 suggested that agricultural trade reform has led to an improvement in food security, 10 reported a decline, and the remaining 11 indicated a mixed outcome.

Thus, whether trade liberalization affects food security and health outcomes is ultimately an empirical question, precisely the one we address in this study. We contribute to this literature by bringing a new methodological approach which allows us to assess the sign and magnitude of the short- and long-run effects of trade liberalization on food security and health. More specifically, we use the Synthetic Control Method (SCM) recently proposed by Abadie et al. (2010) as an econometric tool providing a way to choose the *best* comparison units in comparative case studies. Using this framework, we compare the trajectory of post-reform food security outcomes of countries that experienced a trade liberalization episode – called *treated* countries – with the trajectory of a combination of similar, but *untreated* countries. Using this method, we investigated the food security/health outcomes of 40 trade liberalization episodes during the 1960-2010 period. Given the country-specificity of the outcomes related to the question we investigate, we believe this methodology provides the best feasible identification strategy of the parameter of interest. However, it is important to stress that we are not testing among alternative trade theories on the relationship between trade liberalization and food security. Instead, we attempt to rigorously establish the direction and the magnitude of the average causal effect of trade liberalization on food security/health outcomes, an important piece of evidence not well established yet in the literature.

The main findings can be summarized as follow. Across 40 investigated case studies, 20 showed a significant (short-) and long-run reduction in child mortality rate after the trade reform, with an average effect of about 17 percent points; in 19 case studies

[2] The reduction in tariffs and non-tariff barriers affects consumer and producer prices, which in turn affect household production, household consumption, labor earnings, and transfers (Nicita et al., 2012). Thus, price increases have mixed effects on poverty and hunger. On the one hand, they increase the cost of food for consumers, but on the other hand they raise the income of farmers, who represent the bulk of the world's poor. Net effects will differ depending on whether poor households or countries buy or import, or sell or export food. Moreover, several other conditions, like infrastructure, institutions, and market imperfections will also play a role (Swinnen and Squicciarini, 2011).

we do not detect any relevant effect; only in one case – South Africa – we find a significant increase in child mortality, though largely attributable to the AIDS infection diffusion. Finally, though we showed that the contemporaneous occurrences of political reforms does not affect our results, we find that when trade liberalization occurred in a consolidated democracies, the magnitude of the estimated effect is two time higher than in an autocratic regime.

As documented in Mc Corriston et al. (2013), we are certainly not the first who tried to investigated this relationship. However, our analysis is first of all related to the few papers that using cross-sectional regression tools tried to established the (causal) link between indicators of food security and trade policy. Arcand and Hombres (2004), using both panel data and time series econometrics, found weak association between the prevalence of underweight in children and indicators of trade openness, like the trade to GDP ratio and the Sachs-Warner index, although the depreciations of the real exchange rate appears to reduce children underweight. Similarly, Bezuneh and Yiheyis (2009), using panel data econometric found a negative effect of trade liberalization episodes occurred during the '80 and '90 on indicators of food availability, although this relationship turns to weakly positive in the long-run.

Second, our analysis is also related to the literature that investigated the effect of trade liberalization on growth, poverty and health in developing countries, a literature too vast to be summarized here.[3] However, within this literature, some recent papers are worth noting especially because they used more credible identification strategies by exploiting variation at (rural) district level of the exposure to trade liberalization. For example, Topalova (2010) exploiting India's trade liberalization occurred during the nineties, finds that poverty declined at a relatively slower rate in rural districts that were more exposed to tariff reform.[4] Similarly, and interestingly, using retrospective birth histories, Anukriti and Kumler (2012), among other things found that India's rural districts experiencing a relative decline in tariff protection and, at the same time, a relative increase in infant

[3] Recent survey on trade liberalization and growth can be find in Winters and Masters (2013). Winters et al. (2004) and Goldberg et al. (2007), review the literature of the impact of trade on poverty and inequality. Overall the main message from this literature is that while trade is good for growth at aggregated country level, its effect appears heterogeneous and can increase inequality within country. Cornia et al. (2008) and Blouin et al. (2009) instead, review the literature on globalization and health, concluding that trade liberalization in developing countries may be not good for health, especially because trade has facilitated availability of highly processed, calorie-rich and nutrient-poor food, inducing a dietary deterioration.

[4] However, using state-level data, Hasan et al. (2007) conclude that greater exposure to trade openness is not associated with slower reduction in poverty in rural India.

mortality, confirming that trade reform may have important distributional consequences.[5] In comparison to these findings, our results tend to be more optimistic about the (average) effect of trade liberalization on indicators of food security and health.

The remainder of the paper is organized as follows. The next section presents the methodology, the synthetic control method. In Section 3, we discuss how we measure trade policy reforms and food security outcomes, as well as the covariates used to select the synthetic controls, and the country sample. Section 4 presents and discusses the results at the regional level, while in Section 5 the magnitude and robustness of the estimated average effect is investigated taking into consideration the possible confounding effects induced by the simultaneously occurrence of political reforms. Finally, Section 6 concludes.

2. The Empirical Method

2.1 Synthetic Control Approach for Comparative Case Studies

The Synthetic control method has been firstly proposed by Abadie and Gardeazabal (2003) and then further refined by Abadie et al. (2010), to which the interested reader can refer. Billmeier and Nannicini (2013) applied the SCM to studying the relation between trade liberalization and growth. Our approach follow their application of the SCM.

Formally, suppose that we observe a panel of $I_C + 1$ countries over T periods, where only country i experiences the treatment, namely it opens to trade at time $T_0 < T$. 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 a closed economy (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 it 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):

5 However, the same authors highlight that their results do not suggest that trade liberalization overall increases infant mortality. Indeed, during the trade liberalization period, India experienced a decreasing trend in fertility and infant mortality.

(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 vector of parameters; μ_j is a country specific unobservable, with λ_t representing the unknown common factor;[6] finally, ε_{jt} are transitory shocks with zero mean. As better explained in the data section, the variables included in the vector X_j (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 reform) means that we have ruled out "anticipation" effects.[7]

Next, define $W = (w_1, \ldots, w_{I_C})'$ as a generic ($I_C \times 1$) vector of weights such that $w_j \geq 0$ and $\sum w_j = 1$. Each 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 we 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 τ_{it}.

Note that condition (3) can hold exactly only if $(\bar{Y}_j^k, X_j)$ belongs to the convex hull of $[(\bar{Y}_1^k, X_j), \ldots, (\bar{Y}_{I_C}^k, X_{I_C})]$. 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.[8] Then, any deviation from condition (3) imposed by this procedure can be evaluated in the data, and represents a part of the SCM output.

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[6] 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.

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

[8] 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.

Hence, the general idea behind the SCM is to select a weighted combination of (untreated) control 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 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 presents 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 – namely it allows the effect of unobservable confounding factors to be time variant.

However, it is worth noting also some shortcomings of the SCM. First, as in the program evaluation literature, our estimator does not distinguish between direct and indirect causal effects of trade liberalization on food security/health outcomes, a distinction that is of valuable interest for drawing policy implications. A second issue is due to the small number of observations involved in such comparative case studies, which leads 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 in a comparison of the magnitude of the estimated effect for the treated country, with the size of those obtained by assigning the treatment randomly to any (untreated) country of the donor pool. In what follows, we applied both the standard Abadie et al. (2010) placebo tests, than a generalization as proposed recently by Cavallo et al. (2013).

2.2 Average Effect and Inference with SCM

In the previous SCM applications the analysis of the results and the respective placebo tests, has been largely conducted at the level of (each) single country case-study. However, when the analysis covers many countries, as in the present study, it might be of valuable interest to try to generalize the results of the investigation, by measuring the average treatment effect over some particular group of countries (Cavallo et al. 2013). In particular, we are interested in combining the results of the placebo tests with the estimation of the average effect over the country-specific comparative case studies.

Denote with $\left(\hat{\tau}_{1,T0+1}, \dots, \hat{\tau}_{1,T}\right)$ a specific estimate of the trade liberalization effects on the country of interest 1. Then, consider the average trade liberalization effects across *G* countries of interest, for example the ones that are democracies (or autocracies). The estimated average effect for these *G* trade reforms, can be computed as:

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

Note also that, because the size of the country specific effect will depend on the level of child mortality rate, the same decline in child mortality is more important in poorest countries. Due to these scale effects, in order to applied correctly the relation (5) we need to normalize the estimates before aggregating the individual country effects.[9]

In order to estimate the extent to which this (dynamic) average treatment effect is statistically significant, we follow Cavallo et al. (2013). These authors, like Abadie et al. (2010), used exact inference techniques similar to permutation tests, that allow consistent inference 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, namely the countries that are not exposed to the treatment. Then we evaluate whether the estimated effect of the treated country outperform the ones of these fake experiments.

Formally, supposed that we want to do inference for the negative trade liberalization effect on child mortality for each of the ten post-reform years. Then, it is possible to compute the year-specific significant level, namely the *p*–value, for the estimated trade reform effect as follow:

(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 a placebo reform is assigned to the control country *j* at the same time then the treated country 1. In this case, the fake treatment effect is calculated using the same algorithm outlined for $\hat{\tau}_{1,t}$. The operation is run for every country *j* of the donor pool, in order to build the distribution of the fake

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

experiments, with the aim of evaluating how the estimate $\hat{\tau}_{1,t}$ is positioned in that distribution. Finally, because we are interested to do valid inference on $\bar{\tau}$, we compute the year *t* specific *p*-value for the average effect as

(7) $$p\text{−value}_t = \Pr\left(G^{-1}\sum_{g=1}^{G}\hat{\tau}_{g,t}^{PL} < \bar{\tau}_{1,t}\right) = \Pr\ (\bar{\tau}_t^{PL} < \bar{\tau}_t).$$ [10]

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), 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 previous literature (Giavazzi and Tabellini, 2005; Billmeier and Nannicini, 2013) 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.

Using this index, we capture liberalization episodes of the *overall* economy, not just the one of the agricultural sector. However, several arguments can justify this choice. First, working on a developing country sample, the agricultural and food sectors represents the bulk of these economies (see Nicita et al. 2012, on this point). Second, as recently shown by Olper et al. (2014), there exists a strong cross-country and within country correlation between the Sachs and Warner index and the protection level in agriculture. This is because agricultural trade liberalization is often introduced as part of wider reforms which make it difficult to isolate the agricultural trade liberalization impacts alone (McCorriston et al. 2013). Third, it is well known that in developing countries the protection of the manufacturing sector represents an important source of distortion and indirect taxation of agriculture (see Krueger, 1992; Anderson, 2010). Finally, it is important to emphasize that the accessibility and stability dimension of food

[10] Further details on this computation approach can be find in Cavallo et al. (2013).

security can be directly (and indirectly) affected by overall trade liberalization, through its effect on growth and development (see Hertel, 2006).

To measure food (in)security at national level, use was made of the under-5 mortality rate (per 1,000 live births), hereafter U5MR for brevity. U5MR is the result of the United Nation Inter-agency Group for Child Mortality estimation,[11] and it represents the outcome variable of interest (Y_{it}). Different practical and conceptual reasons justify the choice of this variable. From a practical point of view, although U5MR is a health variable and, as such, affected also by factors which are not strictly related to the disposability and/or accessibility of safe food, like for example sanitation and infectious diseases, it 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 also because many of the disposable trade reform episodes happened before or during the eighties. Moreover, from a conceptual point of view, U5MR represents a key index of the United Nations Millennium Development Goal, an aspect that recently contributed to improve the quality of its estimation (see Alkema and New 2013 on this point). In addition to this, U5MR is a key component of the IFPRI Global Hunger Index (GHI) that, however, is disposable only from the eighties and not on a yearly basis. Finally, Caulfield et al. (2004) showed that about 54% of child mortality in the developing world is explained by malnutrition issues, a result that confirms how children mortality, although imperfectly, captures important dimensions of food (in-)security. Yet, to partially address this shortcoming, in the results discussion attention will be given also to contrast our key findings based on child mortality with existing evidence based on more direct food security and undernourishment indicators.

The vector of covariates X_j used to identify the synthetic controls has been selected on the basis of previous evidence on the (cross-country) determinants of food security and health (see Deaton, 2006; Besley and Kudamatsu, 2006; Owen and Wu, 2007; Headey, 2013; Pieters et al., 2013). 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 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

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

Lee, 2010); and, finally, the average U5MR in the pre-treatment period (source: United Nations).

Finally, in order to study whether the effect of trade reform is higher in democracies or autocracies, and in particular whether the occurrence of trade reforms interact with political reforms, we also classify countries in democracies and autocracies using the Polity2 index from the Polity IV data set (Marshall and Jaggers, 2013).[12]

Concerning the country sample, we start from a dataset of about 130 developing countries. However, for about 33 of them, information related to the trade policy index is missing (see Wacziarg and Welch, 2003, 2008 for details). The final number of developing countries eligible for the policy experiments were identified taking into account the following criteria. Firstly, the treated countries have liberalized at the earliest around 1970, so as to have at least 10 years of pre-treatment observations to match with the synthetic control. Secondly, there exists a sufficient number of countries that remain closed to international trade for at least 10 years before and after each liberalization episodes, in order to provide a sufficient donor pool of potential comparison units. Using these criteria, we end up with a usable data set of about 80 developing countries, of which 40 experienced a trade liberalization episode.[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, where the outcome is evaluated, while T_0 is the year of trade liberalization.

4. Results

This section summarizes the results obtained from our 40 SCM experiments. The results are mainly presented and discussed by region, making use of tables. Moreover, we also display a few graphical representations (see Figures 1 and 2)[14] with their placebo tests for representative case studies, with the aim of better explaining how the SCM

[12] Specifically, 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 (or out of) 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.

[13] The 40 treated countries used to run the SCM experiments and the respective years of trade liberalization are reported in Tables 1-4.

[14] The graphs for all the 40 SCM experiments and the relevant placebo tests are disposable from the authors upon request.

works, and to understand the significance of the key results. Next, in section 5, the issue of inference and average effect will be analyzed considering the robustness of our identification assumption considering the effect of possible confounder effects.

The Tables from 1 to 4, report for each country case study, the numerical comparison between the treated and the respective synthetic control of the explanatory variables used to select the counterfactual in the pre-treatment period. The overall pre-treatment fit is measured by the root mean square prediction error (RMSPE) of the under-five mortality rate, reported at the bottom of each country case study. In the estimation of each Synthetic control, use was made of more values of the pre-treatment U5MR (typically three, corresponding to T_0−10, T_0−5 and T_0). However, to make the Tables more readable, we decide to report only the U5MR value in the year of the reform, called U5MR T_0.

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, represents two estimates of the dynamic treatment effects. Finally, the note reported under the Tables specifies the countries used to build each Synthetic control, and their respective estimated weights (in parenthesis).

Figures 1 and 2, instead, display a graphical representation of the outcome variable, U5MR, for the treated unit (solid line) and the synthetic control (dashed line), considering ten years before and ten years after the treatment. The vertical dotted line coincides with the year of the trade reform, T_0. By comparing the solid and dashed lines in the pre-treatment period (before T_0), it is possible to have an idea of the quality of the synthetic control extracted by the SCM algorithm, namely the extent to which the estimated synthetic control behaves closely to the treated unit. Instead, the comparison in the post-treatment period gives a precise overview of the dynamic treatment effect. The Figures also report the placebo tests, which have the aim of assessing whether the estimated dynamic effects are robust. Here the bold line reports, for each year before and after the treatment, the difference between the U5MR of the treated unit and that of the synthetic control. Thus, any departure from the horizontal zero line gives an idea of the direction and the magnitude of the estimated dynamic effects. The fake experiments, obtained by assigning the treatment randomly to any (untreated) country of the donor pool, are represented by the grey lines. If the magnitude of the effect of the treated country (bold line) is larger than the majority of the placebo tests (grey line), then we can conclude that the result is robust.

Finally, in the discussion of the results, we provide some contextual background for particular countries in each macro region. More specifically, we compare the SCM result in some specific countries with information on both agricultural trade policy and direct food security outcomes, based on data from Anderson and Nelgen (2013), FAO food security and trade policy case studies (see Thomas, 2006; Sharma and Morrison, 2011), and IFPRI information on other food security indicators (see Wiesman, 2006). This allows the possibility to better understand whether the SCM results are corroborated also with qualitative and quantitative information about what really happens around the trade reform year.

4.1 SCM results for Asia

Starting from Asia (Table 1), in four out of six SCM experiments we find that trade liberalizing countries display a reduction in U5MR that outperforms the one of the respective synthetic control. This is particularly true for Indonesia (reform in 1970), Thailand (1970), Sri Lanka (1977) and Philippines (1988). If we consider Sri Lanka, we can note that, in the reform year, the U5MR was virtually identical to that of the synthetic control (59.30 vs. 59.35), which is based on a combination of Algeria (0.1084), Nicaragua (0.016), Panama (0.181), Korea (0.171), Trinidad and Tobago (0.361), and Venezuela (0.177). In the post-reform period, the U5MR in Sri Lanka turns out to be 12% lower than the estimated counterfactual after five years, a value that jumps to 37% after ten years.[15] Interestingly, this relevant effect coincides with the reduction of the taxation of agricultural export crops, especially tea, coconuts and rubber. In particular, the taxation of traditional export products was in fact over 40 percent in the 1960s and 1970s, but gradually fell to about 20 percent in the 1980s (Karunagoda et al. 2011, 245).

The magnitude of the reform's effect in Thailand, Indonesia and Philippines was a little bit lower, but still positive in both the short (U5MR T_0+5) and long run (U5MR T_0+10). Moreover, as it is evident from the low values of RMSPE of these experiments, in the majority of the cases lower than 1, the precision of the counterfactual selection gives credence to the SCM results, which indeed tend to survive to the placebo tests (results not shown).

For countries having available data, it could be of some interest to see whether other indicators of food security followed the same path. For Sri Lanka, the percentage of the

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

prevalence of underweight in children under five, a component of the IFPRI Global Hunger Index (GHI), shrinks from 47.5% to 37.3%, ten years after the reform. However, in the same period, the percentage of undernourished population increases from 22% to 28% (Wiesman, 2006, Table 18), though the GHI still shrinks from 25 to 22.4 points. For Philippines (reform in 1988), all the component of the GHI improved in the years after the reform. Thus, overall, these numbers tend to support the notion that in Asia trade reforms have contributed to improve both health and food security conditions.

4.2 SCM results for Africa

Moving to Africa, the results are less clear cut and heterogeneous (see Tables 2a- 2b). First, among the eighteen SCM experiments, six display a higher reduction of U5MR after trade liberalization with respect to the counterfactual (Ghana, Gambia, Guinea, Guinea Bissau, Uganda, Tanzania), six showed a dynamic in the child mortality rate that is worse than that of the synthetic control (Benin, South Africa, Mauritania, Ethiopia and Burkin Faso), while in the remaining six countries we do not find any appreciable difference between the treated and the synthetic control. However, it is important to note that the majority of cases where U5MR increases after trade liberalization correspond to reforms which occurred after 1990. These results mimic, at least partially, a similar finding of Billmeier and Nannicini (2013) who, using an analogue methodology, studied the reform's effects on the level of development. Thus, in Africa, the trade liberalization events of the '90s not only gave low or negative contribution to GDP growth, but also worsened or did not improve children's health conditions.

However, an important difference between the African liberalization events before and after the '90 can be appreciated from the graphical analysis reported in Figure 1 and 2. In Figure 1, we displayed examples of successful reform's effects with their placebo tests. When considering the two African reforms, those of Ghana (1985) and of Guinea (1988), the positive reform's effect on U5MR is clearly confirmed by the placebo tests. Indeed, the treated unit (bold line) clearly outperforms the large majority of the placebo ones (grey lines), suggesting that these results are statistically significant. If we consider, instead, the reforms after the '90, here the situation is different (see Figure 2). Indeed, we find a significant deterioration of the U5MR just for the South Africa reform of 1991, as it is apparent from the placebo tests. However, this can be hardly attributable to a direct negative effect of trade liberalization on food security/health issues, as there is a strong overlapping between the post-reform years and the South Africa severe HIV/AIDS epidemic spread. Indeed, national antenatal clinic data show a rise in seroprevalence from 1 percent in 1990 to 25 percent in 2000 (see Karim and Karim, 1999; South Africa Department of Health, 2005).[16] By contrast, the “negative” effect of the 1995 trade reform in Mauritania, is not supported by the placebo test, because in several fake experiments (grey lines), the U5MR rate deteriorates more than in Mauritania (bold line). Yet, the positive reform effect in Tanzania (1995) is strongly confirmed by the placebo tests (see Figure 2). Thus, although we have some confirmation of the idea that in Africa only the early liberalizations had a positive impact on health and food security, a result that mimics the GDP reform effects studied by Billmeier and Nannicini (2013), when health indicators are considered, it is not always true that all the trade reforms of the nineties have had a negative effects.

Among the African case-studies, the one of Ghana is worth noting and turns out to be interesting. First, a close analysis of the reforms put forward after 1985 (the year of trade liberalization) shows that this country reformed simultaneously its overall macroeconomic policies (fiscal and monetary policy), its trade policy (especially by changing exchange rate policy), as well as its agricultural trade policy (see Thomas 2006, Table 4 p. 10). More specifically, Ghana gradually reformed its output price policy, reduced exports taxation on key commodities (cocoa), agricultural input price, import tariffs, addressing also important institutional change at the level of marketing board. These reforms translated to an impressive reduction in the level of protection for import-competing commodities, like rice and maize after the 1985. Yet, the percent of undernourished population dropped from 37% in 1990 to 18% in 1995-97, and the GHI shrunk substantially, going from 35 (1981) to 18 (1995). This represents a broad confirmation of our SCM results.

For the other countries that showed a significant reduction of the U5MR with respect to the counterfactual, the dynamic of other indicators of food security strongly confirms our findings for Gambia, Guinea Bissau and Tanzania. These are all countries which experienced a significant increase in the GHI after the trade reform, as an effect of an

[16] This shortcoming cannot be properly addressed in this SCM experiment, due to the lack of quantitative information about the epidemic spread of HIV/AIDS in the pre-treatment period. A look at the composition of the synthetic South Africa (see the bottom of Table 2) clarifies this point. Indeed, the Synthetic South Africa is build also with countries that suffer HIV/AIDS epidemic spread, albeit less severe than South Africa, like the Central African Rep. (0.095) and Congo Rep. (0.07). However, the failure in controlling for this (post-treatment) shock, translated to a very low weight attributed to these countries, in comparison to the high weight attributed by the algorithm to countries that do not experienced a similar HIV/AIDS shock, like Iran (0.318) and Siria (0.50).

improvement in the proportion of undernourished people and the prevalence of underweight in children. Instead, we find less clear cut consistency for Guinea and Uganda, where there is still an improvement in GHI which, however, seems to be more driven by a reduction in child mortality than by other more direct indicators of food security. However, the not perfect overlapping of the indicators in hand makes these comparisons difficult.[17]

4.3 SCM results for Latin America

Considering now Latin America trade liberalization episodes (see Table 3), in about half of the experiments (six 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 nil, but never negative. In particular, the trade reforms that worked better have been especially those of Chile in 1976, Mexico in 1986, and Guatemala in 1988. Here, ten years after the liberalization, the U5MR is about 45% lower than that of synthetic control in Chile, 32% in Mexico and 28% in Guatemala. The placebo tests reported in Figure 1 for Chile and Guatemala, as representative examples, show that only very rarely (1 case out of 30 and 16, respectively) the fake experiments in the potential controls are below the effect in the treated countries. Thus, it clearly emerges that the trade reforms in Latin America, once again especially those before the nineties, had their contribution in accelerating the reduction in children mortality rate.

These results are also often corroborated by other information. For example, the Chile trade reform of 1976 has been followed by a strong shift from agricultural taxation to agricultural subsidization, that has been accompanied by a progressive shift from subsistence crops to high value added export productions. However, and interestingly, the FAO case studies on trade and food security (Thomas, 2006) also highlighted that the main impact of economic reforms on food insecurity, more than on food prices *per se*, has been largely due to off-farm income generation opportunities derived from growth in the agricultural sector and the broader economy.

Here, the situation of Guatemala is of particular interest. Indeed, although our estimate shows a significant reduction in U5MR rate in comparison to the counterfactual, this finding is not confirmed by other analysis (see, e.g., Thomas, 2006) who instead

[17] For example, for Guinea (reform 1986) the IFPRI data show a deterioration of the proportion of undernourishment (from 30 to 39) and the prevalence of underweight in children (from 23 to 24) when passing from 1980 to 1990. However, both indicators improve substantially when passing from 1990 to 1995, and almost the same happens for Uganda (see Wiesman, 2006, Table 18).

documented a deterioration of the percentage of undernourished population after the trade and agricultural reforms started in 1988. However, the IFPRI data on underweight in children go in an opposite direction, showing some improvement. This discrepancy in the results seems at least partially attributable to the specificity of the agricultural sector in Guatemala. Indeed, reforms have shown mixed results also due to external factors. In particular, the fall in the price of coffee has been the principal reason for the poor performance, and growth in non-traditional products has not been sufficient to compensate for the employment loss in the coffee sector (Thomas, 2006, p. 74). Moreover, the FAO report also stressed that Guatemala may be a case in which greater transparency and accountability in government are necessary conditions for improvement of the lot of the poor, confirming the notion that institutional reforms are complementary to the ability of trade liberalization to exert its effect, especially on the poor (see Rodrik, 2000; Goldberg and Pavcnik, 2007).

Among the other case studies with positive effect of trade reform on U5MR, the evolution of the other food security indicators strongly confirms our results for Brazil and Perù, but not for Nicaragua which experienced a slight deterioration in other food security indicators, at least in the short period (see IFPRI, 2006). Yet, and interestingly, one underlying reason of that results could be found precisely in the adverse pattern of agricultural trade policy which, after the 1991 overall trade reform, displays a significantly increase in the taxation of agricultural export products (see Anderson and Nelgen, 2013).

4.4 SCM results for Middle East and North Africa Countries

Finally, we consider the four reform experiments conducted on Middle East and North Africa countries (Morocco (1984), Tunisia (1989), Turkey (1989) and Egypt (1995)). As it is clear from a close inspection of Table 4, in all the cases analyzed the U5MR dynamic of the treated country outperforms that of the respective synthetic control, with a magnitude of the estimated treatment effect at T_0 +10 quite similar across the experiments, and equals to a child mortality reduction of about 10 percent points. In Figure 2, as representative example, we report the SCM of Turkey with its placebo test, which clearly demonstrates the robustness of the result. Overall these findings appear in contrast with those of Billmeier and Nannicini (2013) who, instead, found no significant GDP growth after trade liberalization in these countries. The last result has some interest, as it suggests that, while GDP growth induced by (overall) trade liberalization may represent one of the key factor responsible for the improvement in the U5MR, probably through the poverty reduction channel, in these specific cases factors different from income growth appear to be at work.

The SCM results of this group of countries tend to be corroborated by considering other indicators of food security which, with the only exception of Morocco, improved substantially in the years after the trade reforms (Wiesmann, 2006, Table 18). Finally, when looking at the patterns of agricultural trade policies, the case of Turkey appears of some interest. Indeed, in the years after the trade liberalization, there has been a significantly switch from taxation to subsidization of the agricultural sector (see Anderson and Nelgen, 2013),. This results supports the idea that, when overall trade reform is also followed by agricultural trade policy reform, the effect on food security outcome appears stronger.

5. Robustness check and the effect of political reforms

An important property of the synthetic control method shared with other non-experimental methods assumes that the main results are not affected by other relevant events occurring simultaneously (or after) the reform (Cavallo et al. 2013). In our specific case, the most important potential confounding effect is the occurrence of political reforms. Indeed, as shown by Giavazzi and Tabellini (2005), trade and political reforms (democratizations) may be interrelated in developing countries. For example, a liberalized economic regime may fosters a transition towards democracy, in order to increase the economic well being and the economic power of the middle classes (see Acemoglu and Robinson, 2006; Rajan and Zingales, 2003). Yet, Giavazzi and Tabellini (2005) also find evidence that countries which first liberalize the economy, and then switch to a democratic regime, perform better, both in terms of growth, and other several macro policies, than those which adopt the two reforms in the reverse order.

From this perspective, it is important to understand whether or not political reforms affect our results. In doing this, we start by observing that in the 40 treated countries, being a democracy or an autocracy does not seem to be a relevant factor in driving the results. In other words, trade liberalizing countries which displayed an improvement in child mortality are not systematically different in terms of regime type (democracy vs.

autocracy) from those countries where trade reforms did not exert any effect.[18] This preliminary observation is consistent with the results reported in Pieters et al. (2014) who, using a similar approach, do not find a systematic effect of political reforms on the improvement of child mortality.

In order to study formally whether or not political reforms affect trade reforms outcomes on the child mortality rate, we divided the twenty countries which displayed a reduction in the child mortality rate after the trade reforms in three not overlapped groups: in the first one (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;[19] the second group (G_2) instead considers four countries that were already democracy at the time of the trade reform, namely where the political reform anticipated the trade reform; finally the third group (G_3) considers seven autocratic countries, where the democratic transition never take place in the ten years after the trade reform.[20]

Figures 3-5 present the analysis obtained by apply to these groups the relation (5) and (7), in order to measure the average treatment effect and their respective significant level. Note that the left panel of these figures are equivalent to the one reported in figures 1 and 2, namely they display a graphical representation of the outcome variable, U5MR, for the treated unit (solid line) and the synthetic control (dashed line), considering ten years before and ten years after the treatment. The only difference is that in this case we are considering the average effect obtained using equation 5. Differently, the right panel reported the corresponding *p*-values based on equation 6.

In Figure 3, we can observe that when we restrict the analysis to the subset of countries where political reforms came simultaneously or follow trade liberalization, the short-run effect, though still positive, is insignificant at conventional statistical level (*p*-value = 0.13). However, four years after the trade liberalization the average effect turns out to be significant, and more so when approaching the year T_0+10, where the magnitude

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[18] In fact, in countries where trade reform reduced child mortality rate, we have 4 democracies, 9 autocracies and 5 countries that switch from autocracy to democracy in the observed period (from T_0−10 to T_0+10). By contrast, in the group where nothing happened, we have 7 democracies, 4 autocracies and 10 countries that switch from autocracy to democracy.

[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) until eight years later (T_0+8). Small changes in these rules do not affect our conclusions.

[20] The composition of these three groups is as follows: 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 they 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).

of the average effect is of about 18 percent points. In Figure 4, where trade reforms occur in consolidated democracies, we observe a large and significant effect. Here both the short- and long-run effects are statistically significant, and the magnitude at the year T_0+10 reacts about 26 percent points, hence showing a relevant economic effect. Finally, in Figure 5 considering trade reforms that occurred in autocratic regimes, we find that the average effect at T_0+10, equal to 15 percent points, other than smaller it is also barely significant starting from the seventh year after the reform.

Taken together these results, on the one hand seem to suggest that political reforms, *per se*, are not driving the effect of trade liberalization on child mortality, *ceteris paribus*. On the other hand, there is some evidence that when food security and health issues are considered, liberalizing trade after that a country have reached a certain level of political rights, performs better, a result not in line with the Giavazzi and Tabellini (2005) findings.

5. Concluding remarks

In this paper we have analyzed the effect of trade liberalization on food security/health outcomes, exploiting 40 reform episodes occurred 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.

Main results show that trade liberalization tends to have, for about half of our experiments, a positive and significant impact on the reduction trajectory of children mortality, here used as proxy for food security and health conditions. In the other half of the investigated case studies, the trade reform effect has been always non-negative, with the only exception of the liberalization in South Africa in the 1991, though largely attributable to the overlap with the HIV/AIDS epidemic spread. Moreover, we also find that, for the majority of case studies that displayed a positive reform effect of trade liberalization on children mortality, trade liberalization is often associated with a reduction of taxation in agriculture, and that the reduction in child mortality is also associate with improvements of other direct indicators of food security.

We showed that these results are fairly robust, and are not driven by the simultaneity occurrence of political reforms, and also that trade reforms that happened in democracy work better both in term of the magnitude of the estimated effect, and their significant level. Finally, we find less or conflicting evidence to the idea that reform episodes that happened in the nineties, do not exert any relevant effect on the improvement of child mortality, as emerged recently in similar exercise with respect to GDP growth. This results appears particular true for the reforms in the Middle East and North Africa countries. This is of some interest, because it may suggest that, while GDP growth induced by (overall) trade liberalization may represent one of the key factor responsible for the improvement in children mortality through the poverty reduction channel, in these specific cases factors different from income growth appear to be at work.

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Table 1. SCM results: Covariates and average effects for Asian countries

	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	0.11
Log GDP per-capita	7.58	7.07	6.58	6.61	7.05	7.04
Rurale population	0.64	0.71	0.87	0.89	0.72	0.72
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	5.14
U5MR T_0	65.60	66.73	108.10	109.43	109.60	109.47
U5MR T+5	49.90	59.72	83.59	82.95	98.40	96.54
U5MR T+10	42.09	51.48	63.40	59.66	87.90	81.21
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**: Congo Rep (0.307); Iran (0.207); Malawi (0.118); Nepal (0.156); Siria (0.067); Togo (0.146).

Table 2a. 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	0.15
Log GDP per-capita	6.76	6.69	6.97	7.08	8.55	7.95
Rurale population	0.78	0.80	0.76	0.70	0.52	0.57
Population growth	0.02	0.03	0.02	0.03	0.02	0.03
Primary school	2.51	7.47			8.98	5.07
U5MR T_0	180.70	181.65	59.10	59.41	59.30	59.22
U5MR T+5	158.20	165.89	47.50	47.44	61.70	52.28
U5MR T+10	147.39	143.66	35.50	36.21	76.69	45.97
RMSPE		1.464		0.15		0.149

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**: Rep Centrafricana (0.095); China (0.017); Congo Rep (0.07); Iran (0.318); Siria (0.50).

Table 2b. 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	0.02	0.00	0.00	0.00	0.00
Log GDP per-capita	7.47	6.97	7.15	6.68	7.25	7.00
Rurale population	0.72	0.76	0.66	0.74	0.67	0.69
Population growth	0.03	0.03	0.03	0.02	0.04	0.03
Primary school	10.32	7.84	8.92	4.63	3.73	5.32
U5MR T_0	143.50	143.53	192.40	177.62	152.30	151.93
U5MR T+5	155.10	166.49	179.10	172.78	147.39	143.33
U5MR T+10	134.60	119.65	143.30	156.22	134.50	119.78
RMSPE		1.774		12.4		2.121
	Mauritania 1995	Synthetic Control	Mozambique 1995	Synthetic Control	Tanzania 1995	Synthetic Control
War	0.00	0.05	0.37	0.00	0.00	0.02
Log GDP per-capita	7.21	7.05	5.86	6.34	6.37	6.36
Rurale population	0.76	0.76	0.88	0.90	0.87	0.86
Population growth	0.03	0.03	0.02	0.03	0.03	0.03
Primary school	13.45	10.35	5.32	8.12	12.82	11.01
U5MR T_0	118.60	118.76	208.40	207.85	159.60	159.76
U5MR T+5	110.50	104.46	165.70	170.99	131.50	144.92
U5MR T+10	101.70	76.66	131.50	117.85	90.09	114.06
RMSPE		0.457		2.364		1.109
	Ethiopia 1996	Synthetic Control	Madagascar 1996	Synthetic Control	Burkina Faso 1998	Synthetic Control
War	0.44	0.05	0.00	0.02	0.00	0.02
Log GDP per-capita	6.06	6.42	6.89	6.86	6.37	6.37
Rurale population	0.90	0.89	0.82	0.82	0.91	0.90
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	192.59
U5MR T+5	139.70	133.25	102.60	104.43	174.00	133.33
U5MR T+10	101.90	94.37	76.69	74.39	131.60	88.73
RMSPE		1.50		0.62		8.42

Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Cameroon**: Congo Rep (0.17); Algeria (0.034); Rwanda (0.361); Senegal (0.395); Zimbabwe (0.040). **Synthetic Zambia:** Rep Centrafricana (0.808); Malawi (0.143); Rwanda (0.049). **Synthetic Cote d'Ivore**: Congo Rep (0.626); Malawi (0.336); Rwanda (0.038). **Synthetic Mauritania**: China (0.204); Congo Rep (0.031); Iran (0.12); Malawi (0.223); Papua New Guinea (0.14); Senegal (0.283). **Synthetic Mozambique**: Malawi (0.902); Rwanda (0.012); Senegal (0.086). **Synthetic Tanzania**: Lesotho (0.223); Malawi (0.474); Rwanda (0.024); Senegal (0.113); Zimbabwe (0.167). **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 Centrafricana (0.06); China (0.06); Malawi (0.612); Rwanda (0.269).

Table 3. 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	0.10
Log GDP per-capita	8.32	7.81	8.28	7.71	8.90	7.21
Rurale population	0.26	0.56	0.43	0.56	0.39	0.63
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	5.95
U5MR T_0	57.10	62.67	40.40	45.70	56.20	56.40
U5MR T+5	**30.00**	**50.03**	**34.09**	**35.90**	**43.79**	**50.34**
U5MR T+10	**22.10**	**40.07**	**28.90**	**29.79**	**32.70**	**48.41**
RMSPE		3.62		5.56		0.33

	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	0.22
Log GDP per-capita	8.46	8.09	7.91	7.67	8.05	8.24
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	7.05
U5MR T_0	59.20	59.23	56.20	56.43	63.30	63.28
U5MR T+5	**44.20**	**49.08**	**45.09**	**44.37**	**49.70**	**55.62**
U5MR T+10	**30.79**	**39.39**	**36.29**	**34.03**	**38.09**	**46.85**
RMSPE		0.30		0.22		0.47

	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); Congo Rep (0.066); Algeria (0.084); Iran (0.104); Siria (0.494); Swaziland (0.033); **Synthetic Mexico**: China (0.128); Rep Dem Congo (0.105); Siria (0.638); Zimbabwe (0.129). **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 (0.035); China (0.048); Rep del Congo (0.041); Iran (0.608); Senegal (0.08); Siria (0.188). **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).

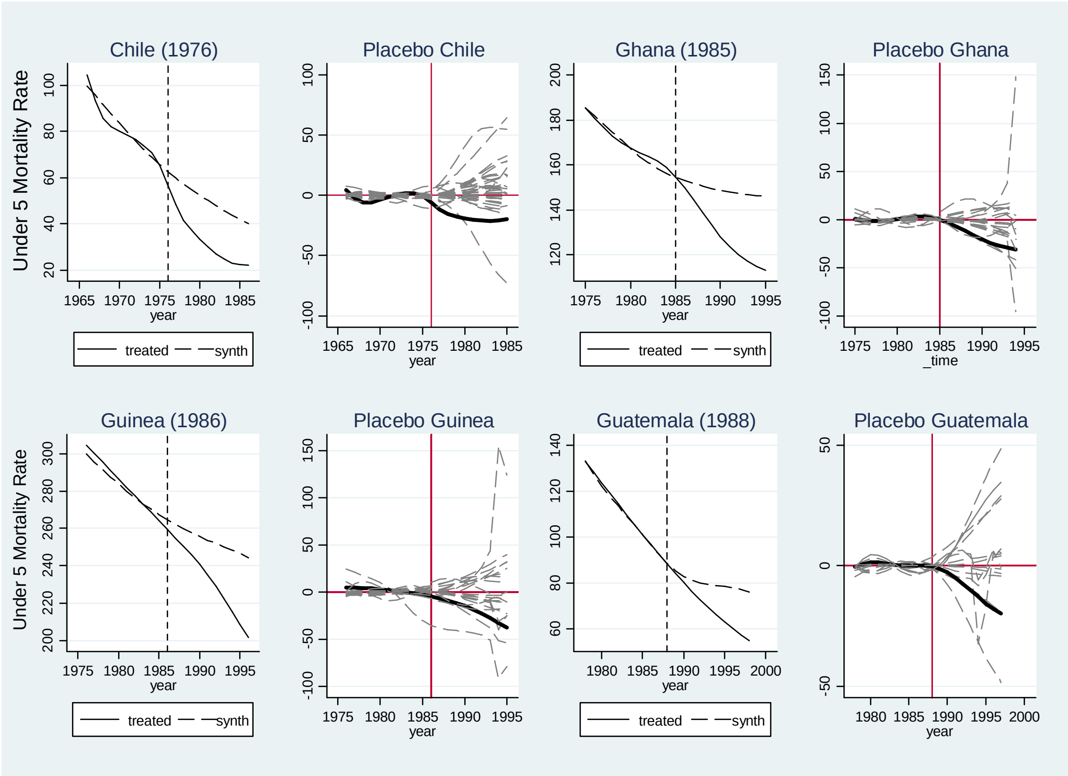
Table 4. Covariates and average effects for Middle East and North Africa countries

	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

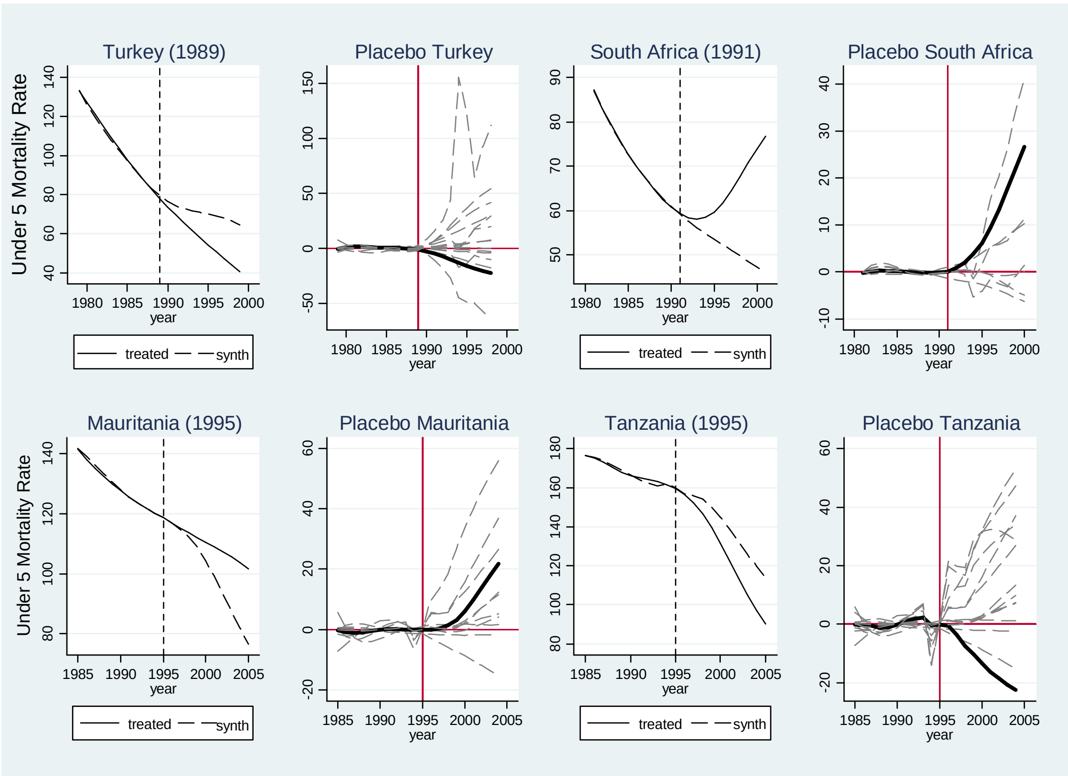
Notes: Countries used to build each Synthetic control, and relative weights in parenthesis. **Synthetic Morocco**: Central African Republic (0.171); China (0.054); Algeria (0.113); Egypt (0.258); India (0.13); Iran (0.012); Panama (0.034); Siria (0.229). **Synthetic Tunisia**: Algeria (0.106); Iran (0.193); 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).

Figure 1. Treated vs. Synthetic Control and Placebo Tests for a Selection of Negative Reform Effects



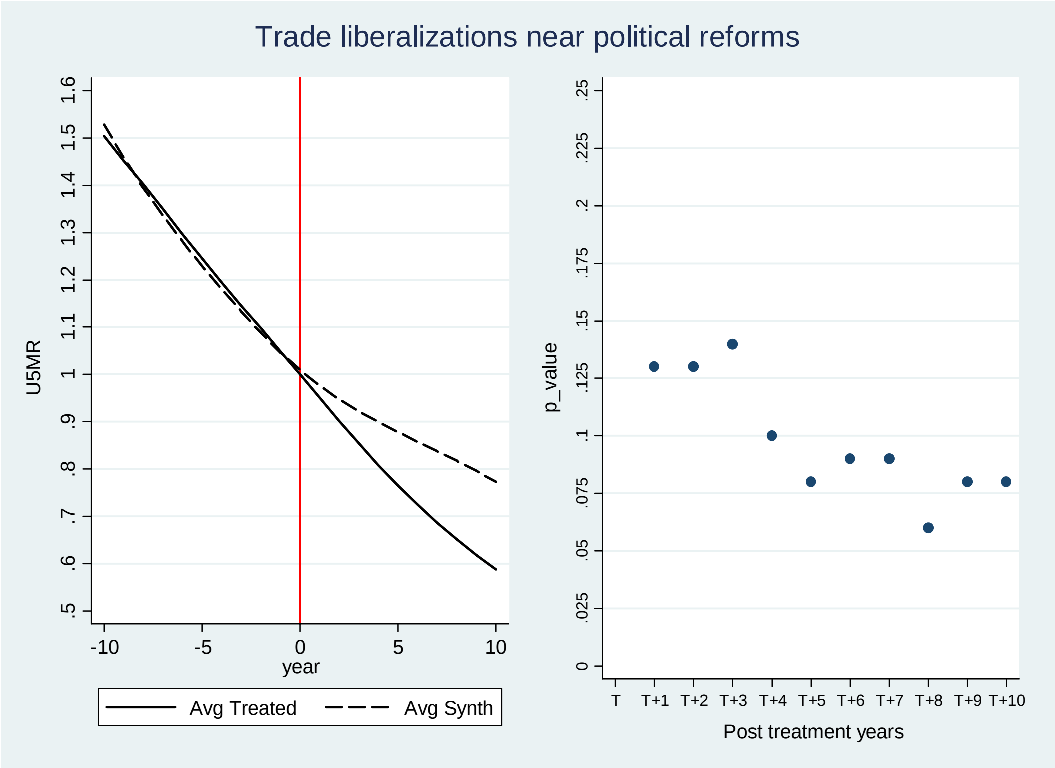
Notes: The figure reports examples of SCM results and their respective Placebo tests for cases of positive trade reform effect (see text).

Figure 1. Treated vs. Synthetic Control and Placebo Tests for Mixed Reform Effects



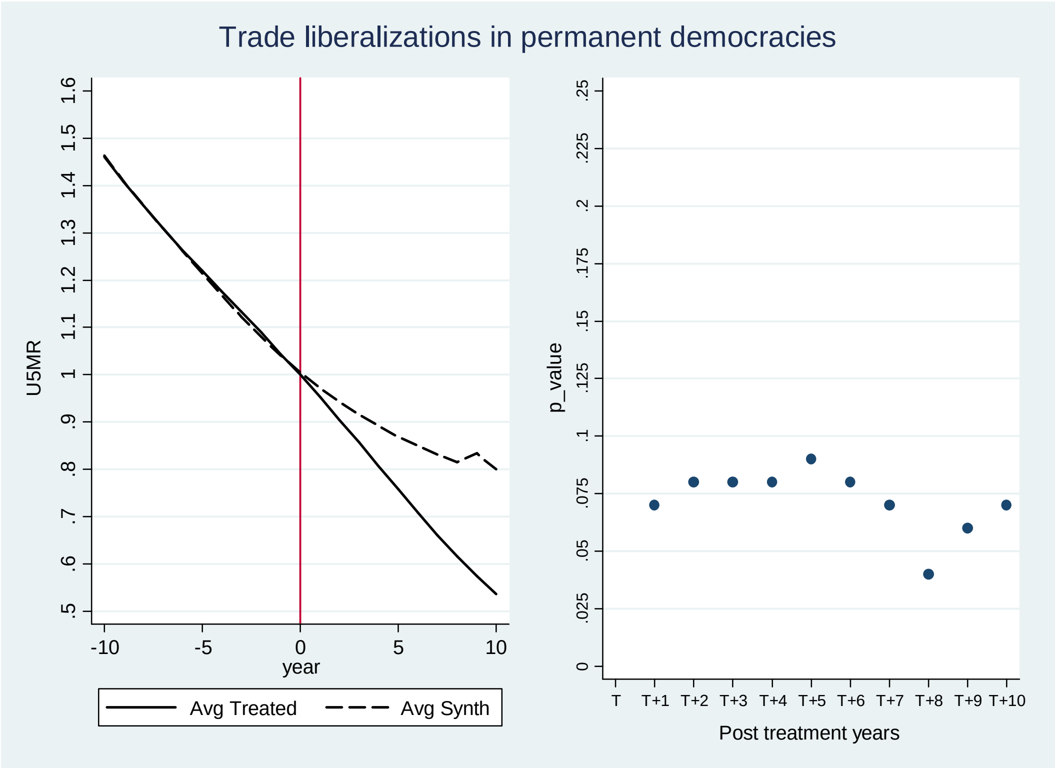
Notes: The Figure reports examples of SCM results and their respective placebo tests for experiments with mixed trade reform effects (see text).

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



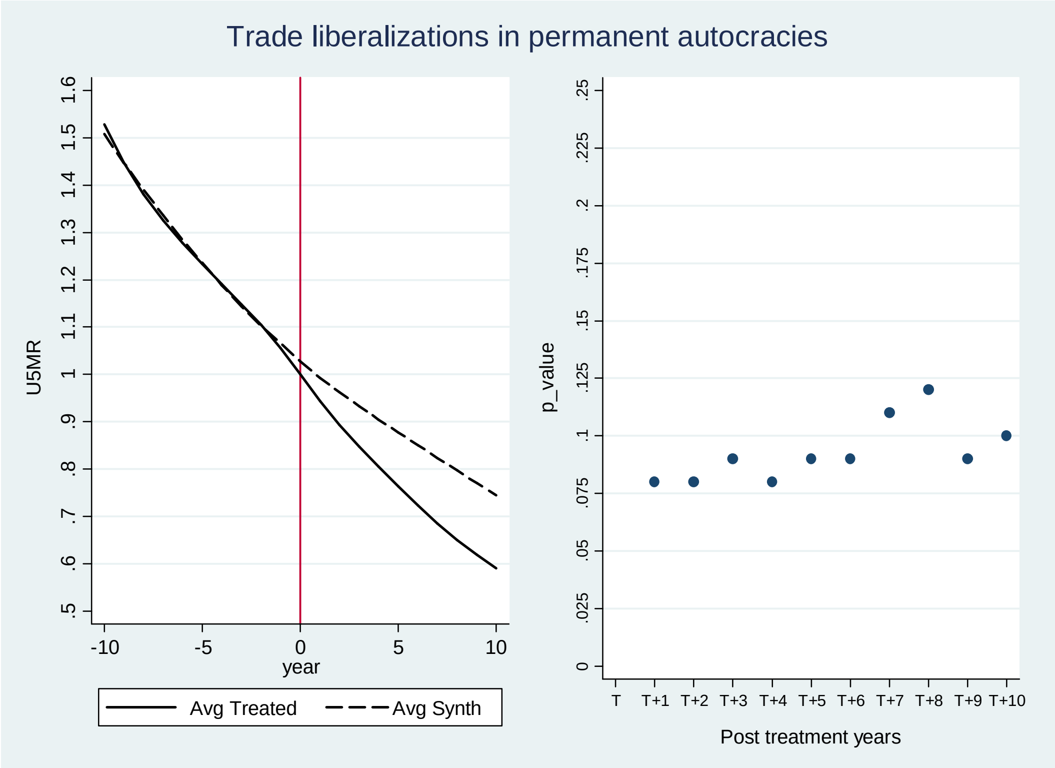
Notes: The Figure reports estimates of the average treatment effect and the corresponding *p*-value of trade liberalizations that occur near political reforms (democratization). The estimates are obtained by applying relations 5 and 6 to trade liberalizations in the following five countries (in parenthesis 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 4. 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.