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Abstract

Studies on the economic consequences of internal political violence typically find negative short-run effects that are not very large, and no evidence for full economic recovery. We study the impact of the Rwandan genocide in 1994 on economic development using the synthetic control method. We find a 58 percent decrease in GDP in 1994, and strong evidence that Rwanda's economy was then catching up with the estimated counterfactual GDP it would have had in the absence of the genocide, with the gap closing after 17 years. The negative effects were more pronounced in the industry and service sectors than in agriculture.

JEL Codes: O11, O55

Keywords: Genocide, Growth, Economic Recovery, Rwanda

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1 Introduction

Civil conflicts and other forms of internal political violence were common in the second half of the last century (Blattman and Miguel, 2010); and nowadays the news are again dominated by reports on civil conflicts in a diverse set of countries, including countries in Eastern Europe, the Middle East, North Africa, and Sub-Saharan Africa. Given the ubiquity of civil conflicts, it is surprising how little we know about the short- and long-run effects of civil conflicts on a country's economic development. Economic theory provides limited guidance. Standard neoclassical growth models predict relatively fast growth in the first years after civil conflict when the economy is still far below its steady state, and fully recovery in the long run (Solow, 1956). Alternative theoretical models however imply that major political violence may tip countries into poverty traps (Azariadis and Drazen, 1990).

The empirical literature on the economic consequences of civil conflicts started with the cross-country growth study by Collier (1999). In a more recent and similarly prominent cross-country growth study, Cerra and Saxena (2008) find that civil conflicts cause GDP to drop by six percent on average, and that GDP only partially rebounds in the medium- to long-run. A concern with cross-country growth studies is that, due to data availability, countries with small-scale events and fast recoveries are more likely to be in the sample than countries with large-scale political violence and a potentially chaotic experience in the aftermath. This sample selection bias may lead to an underestimation of the negative short-run effects, and an overestimation of the speed of recovery. The recent empirical contributions by, e.g., Rogall and Yanagizawa-Drott (2013), Rohner et al. (2013), and Serneels and Verpoorten (2013) compare the economic consequences of internal political violence across different regions of a conflict-torn country that have experienced different degrees of violence. These within-country studies all provide interesting insights, but by design they cannot inform us about the country-wide (or average) short- and long-term consequences of major political violence.

Our study contributes to the literature on the economic consequences of internal political violence by focusing on the country-wide short- and long-run economic effects of the genocide in Rwanda in 1994. This genocide has been one of the most intense events of political violence since World War II. During a period of approximately 100 days, extremists of the Hutu majority slaughtered around 800,000 Tutsis and moderate Hutus. In addition, there was an exodus of at least two million refugees (UNHCR, 2000).

The main challenge for estimating the effect of violent events in a single country is to find the appropriate counterfactual development, i.e., the hypothetical development of Rwanda's economy had it not experienced the genocide. We use the synthetic control method exactly because it allows determining an appropriate counterfactual development. This method was pioneered by Abadie and Gardeazabal (2003) in a study on the economic costs of terrorism in the Basque country, and further refined by Abadie et al. (2010, 2015), who apply it to study the counterfactual development of states and countries.[1] In the context of the Rwandan genocide, this method allows the construction of a synthetic Rwanda as a counterfactual that is composed of countries from a donor pool of other Sub-Saharan African countries. The synthetic control method uses pre-genocide information of the developmental outcome of interest and additional economic and political predictors that are important determinants of the outcome to construct the synthetic Rwanda that develops as similar as possible as the true Rwanda prior to the genocide. The short- and long-run economic effects of the Rwandan genocide can then be estimated by comparing the development of the true and the synthetic Rwanda.

We find that the Rwandan genocide led to an immediate drop in GDP by 58 percent. Taking into account the death toll suggests that GDP per survivor dropped by 53 percent. Further taking into account the exodus of refugees suggests that GDP per survivor staying in Rwanda still dropped by 31 percent. The Rwandan economy has since been catching up with the synthetic Rwanda. However it took around 17 years until its GDP was equal to its counterfactual GDP, which it would have experienced in the absence of the genocide. When looking at the development of the various sectors, we find an interesting and, as we argue below, reasonable pattern: Value added in agriculture dropped less than value added in industry and services, and it also rebounded more quickly to its counterfactual

[1]Other recent applications using the synthetic control method to determine a country's counterfactual development include Billmeier and Nannicini (2013), and Cavallo et al. (2013).

level.

While our paper contributes to the entire literature on the short- and long-term economic consequences of internal political violence, it is of course particularly close to previous contributions focusing on the Rwandan genocide. Like us, Lopez and Wodon (2005) also study at the effect on the country's GDP. There are three main differences between their study and ours. First, they use a methodology for the identification and correction of outliers in time series while we use the synthetic control method. The main advantage of our approach is that the synthetic control method can account for post-genocide events that affected Rwanda, and that would have affected Rwanda even if the genocide had never occurred, which is difficult when solely relying on time series information. Second, Lopez and Wodon (2005) find no evidence for convergence, while we find strong evidence that Rwanda's GDP has caught up successfully.[2] Third, we also study how the effects differ across sectors. Complementarily, the aforementioned contributions of Rogall and Yanagizawa-Drott (2013), and Serneels and Verpoorten (2013) study how the intensity of the violence experienced during the genocide affects today's living standards.[3]

The remainder of our paper is structured as follows: Section 2 presents the methodology, section 3 the data, and section 4 our findings. Section 5 briefly concludes.

2 Methodology

The synthetic control method was first used by Abadie and Gardeazabal (2003) and further developed by Abadie et al. (2010, 2015). This method generalizes the idea of difference-in-differences in several ways and has been tailored for the analysis of case studies where both the treated and the control group may be very small. Studying the economic impact of the genocide in Rwanda, we make use of country-level panel data which leaves us with the country exposed to the treatment, i.e., the genocide in 1994, and a control group, called donor pool, which consists of all Sub-Saharan African countries

[2] This difference in results is due to the different methods applied as well as the fact that we can use ten more years of data.

[3] See Verpoorten (2014) for a broader, more descriptive discussion of growth, poverty and inequality in Rwanda.

(for which data is available). We however exclude Rwanda's neighboring countries, i.e., Burundi, the Democratic Republic of the Congo, Tanzania, and Uganda. These countries have been affected by the genocide as well, e.g., through the exodus of refugees and the subsequent involvement of Rwandan forces in conflicts in the Democratic Republic of the Congo.

The main idea behind the synthetic control method is to use countries included in the donor pool, which have not been exposed to the treatment, to build the counterfactual development for Rwanda in the post-treatment period. This method accounts for the fact that different countries share a different degree of similarity with Rwanda by using country weights ω_d for each country d in the donor pool, with $0 \leq \omega_d \leq 1$ and $\sum_{d=1}^{D} \omega_d = 1$. To find the best possible synthetic Rwanda among all the possible combinations of countries in the donor pool it uses pre-treatment information of the outcome of interest Y_t and additional predictors Z_t that are important determinants of Y_t. In particular, the synthetic Rwanda is estimated by choosing weights ω_d such that $Y_t - \sum_{d=1}^{D} \omega_d^* Y_{dt}$ and $Z_t - \sum_{d=1}^{D} \omega_d^* Z_{dt}$ are minimized for the years prior to the treatment, i.e., in our case for $t < 1994$. The treatment effect α_t is then calculated as $\alpha_t = Y_t - \sum_{d=1}^{D} \omega_d^* Y_{dt}$ for $t \geq 1994$ (Abadie et al., 2010, 2015).

By applying the synthetic control method one does not obtain classical standard errors to make judgments about the statistical significance of the treatment effect α_t. Instead, one can rely on placebo studies (Abadie and Gardeazabal, 2003). That is, one runs the same analysis for the other countries in the donor pool, which are not exposed to the treatment, and then compares the resulting α_{dt} for each placebo with the original α_t. A treatment effect may then only be considered as being significantly different from 0 if it is larger than the "treatment effects" obtained from the placebos.

However, placebos typically also result in large α_{dt} if the fit between the synthetic donor country and the actual donor country is poor, i.e., if the pre-treatment root mean square prediction errors (RMSPE) are high. Consequently, the main inference approach used below is based on a refinement of the placebo studies. In particular, we take two additional measures: First, we exclude placebos with very high pre-treatment RMSPE to minimize the influence of outliers. We do so based on a simple rule: We exclude all placebos for which the pre-treatment RMSPE is larger than the median plus one standard deviation in the sample.[4]

Second, we based our inference analysis on the RMSPE ratios, i.e., the ratios between the prediction error (or RMSPE) for individual post-treatment years and the pre-treatment RMSPE (Abadie et al., 2010, 2015). The RMSPE ratios allow comparing the size of the treatment effects relative to the quality of the fit. High RMSPE ratios for the treated country relative to the countries from the donor pool indicate that the treatment effect is exceptional given the pre-treatment fit, or, in other words, that it is unlikely that one would obtain a similar effect by randomly assigning the treatment to a non-treated country from the donor pool. We indicate the share of countries in the donor pool for which we got a higher RMSPE ratio in our main figures (in parenthesis below the treatment effects). These share or probabilities, respectively, allow assessing the statistical significance of the treatment effects.

In addition to the placebo studies based on countries in the donor pool, we also conduct placebo studies in time. That is, we apply the synthetic control method under the false assumption that the genocide already happened in 1985 instead of the actual occurrence in 1994. The underlying idea is that there should be no treatment effect happening before the actual treatment. Finding an effect for the placebos in time would therefore invalidate any effect found in the core analysis. However, the results for all four dependent variables in our analysis turn out to be negligible and therefore do not invalidate the treatment effects found in the core analysis (see Figure 2 and Figure A.3 in the Appendix).

3 Data

The main outcome variable, GDP, is real GDP at chained PPPs from Penn World Table (PWT) 8.0. Further outcome variables are the value added in agriculture, industry and

[4]A reason for excluding outliers is that placebos with high pre-treatment RMSPE usually show a high fake treatment effect which is increasing over time. There are more sophisticated methods to detect outliers in the literature. However, our simple rule proves to be a very effective criteria in our setting where we only want to exclude extreme outliers on one side of the distribution. Figure 2 (left graph) shows the placebo study for GDP which serves as the basis for calculating the pre-treatment RMSPE. Only one placebo has been detected as outlier and therefore been removed (see also Figure 1).

services in constant USD from the World Development Indicators (WDI).[5]

We use different types of economic and political predictors when looking at GDP and value added in industry and services: The human capital index from PWT 8.0, which is based on years of schooling and an estimate of the rate of return; the investment share of GDP at constant prices from PWT 7.1; openness defined as exports plus imports divided by GDP at constant prices from PWT 7.1; inflation defined as the annual change in the GDP deflator from WDI; the Polity2 score from Polity IV, which measures the democratic quality of political institutions; the political rights rating from Freedom House; and a variable indicating the number of civil conflict/war events in a particular country and year from the Uppsala Conflict Data Program (UCDP). In addition, we also use average daily temperature and precipitation from the Climatic Research Unit (CRU), and information on the production of meat, cereals, pulses, vegetables and fruits in tonnes from the Food and Agriculture Organization (FAO) when looking at value added in agriculture. For most of these predictors we use the average values over the years 1985–1990. There are two exceptions: First, for the conflict variable, we additionally use the average over the years 1991–1993 due to the conflict events in Rwanda in the three years prior to the genocide. Second, for temperature and precipitation, we use the average not only over the years 1985–1990, but also over the post-treatment years, as temperature and precipitation are exogenous.

Moreover, we use population data from PWT 8.0, and gross production value for coffee and tea from the FAO.[6] Table 1 presents summary statistics.

Table 1 around here

[5] We choose PWT as the source for the GDP data as this has a strong positive impact on the sample size. However, data for value in agriculture, industry and services is only available from WDI.

[6] In the Appendix, we use the gross production value for coffee and tea to study the impact of the genocide on tea and coffee production. Due to very limited data availability, we only use temperature and precipitation as additional predictors for tea and coffee production (plus the civil conflicts variable for coffee).

4 Results

Figure 1 presents the estimated effects of the Rwandan genocide on Rwanda's economic development. The graph shows the GDP of the true (solid line) and the synthetic (dashed line) Rwanda from 1970 to 2011. The prediction error is reasonably small as indicated by the almost overlapping lines for the period up to 1993. The estimated effect of the genocide corresponds to the difference between the GDP of the true and the synthetic Rwanda from 1994 onwards. The numbers underneath the solid line in Figure 1 indicate this difference for all post-genocide years and the corresponding statistical significance in parenthesis. The value after D (bottom left) corresponds to the number of outliers in the placebo study that have been dropped. In this case only one country/placebo has been dropped form the inference analysis. Consequently, N refers to the number of countries included in the placebos study, and RMSPE stands for the root mean squared prediction error.

Figure 1 around here

The estimates show that the genocide reduced GDP by 58 percent in 1994. Rwanda's total population was 7.1 million in 1991 according to the Second Rwanda General Census of Population and Housing. Hence, GDP per capita dropped by around 53 percent if we take the 800,000 deaths into account, and still by around 31 percent if we take moreover into account that two million fled the country.[7] The estimates further show that Rwanda has been steadily catching up to the counterfactual GDP since 1994. At some point between the years 2001 and 2002 the negative effect of the genocide became around half as large as it was in 1994. Finally, in 2011, Rwanda's GDP coincided with the GDP of the synthetic Rwanda, suggesting that Rwanda's GDP was no longer any lower than it would have been in the absence of the genocide. Hence, Rwanda's economy fully recovered from

[7] When applying the synthetic control method to GDP per capita rather than total GDP, the estimates imply that the genocide reduced GDP per capita by 51 percent in 1994. Generally, we are however hesitant to trust results using per capita time series because estimated time series for population are all extremely smooth. While around 11 percent of Rwanda's population was killed in a short period of time, the effect of the genocide is smoothed over five years in the estimated population time series (see Figure A.1 in the Appendix).

the genocide after around 17 years.[8]

Figure 2 presents two types of placebo studies. The left graph shows the effects for all the 22 other countries in the donor pool. We see that the difference between the countries' true and synthetic GDP was larger for Rwanda than for all countries in the donor pool in the first years from 1994 onwards, which is what one would expect given that these countries were not hit by such a large negative shock as a genocide. The right graph shows the development of the true and the synthetic Rwanda when preponing the treatment to 1985. As expected, we see absolutely no effect.

Figure 2 around here

The statistical significance of the treatment effects reports in Figure 1 is based on placebo studies for the 22 Sub-Saharan African countries in the donor pool. The values of 0.00 for the years 1994 to 1996 indicates that the difference between the countries' true and synthetic GDP remains larger for Rwanda than for all countries in the donor pool during these years even if we condition on the pre-treatment fit. Given the economic and political volatility of many Sub-Saharan African countries, it is not surprising that the statistical significance falls quickly over time. For the years 1997 and 1998 we already find two countries in the donor pool with a larger absolute difference between the true and the synthetic GDP than Rwanda. Hence, the probability to assess the statistical significance of the effect for Rwanda becomes 2/22=0.09 in 1997. From 1999 onwards, this probability exceeds 10 percent, which is a common level of significance in cross-country studies, but remains reasonably close to 10 percent for at least two more years.

Figures 3–5 present the results for valued added in agriculture, industry and services, respectively.[9] The reduction in value added in 1994 was 40 percent in agriculture, 66 percent in industry, and 59 percent in services. Moreover, the process of catching up with the counterfactual value added lasted around 8 years in agriculture, 13 years in industry,

[8]The notes to Figure 1 indicate that Cameroon has a large weight in the construction of the synthetic Rwanda. Figure A.2 in the Appendix presents the estimated effects after excluding Cameroon from the donor pool. The results remain very similar.

[9]Figure A.3 in the Appendix presents the same placebo tests as Figure 2, but for valued added in agriculture, industry and services rather than GDP.

and 17 years in services.[10] These results suggest that agriculture was less severely hit by the genocide than the other sectors, and recovered much more quickly.

Figures 3, 4 and 5 around here

We can think of various possible reasons for these heterogeneous effects across sectors: First, people probably give highest priority to subsistence consumption needs and, therefore, agricultural production during and after such dramatic events. This priority may explain both the smaller initial decline in agricultural production and the faster recovery thereafter. Second, Rwanda was (and still is) very densely populated and most people were working in agriculture. The negative effects on agricultural output per capita are even seen as a major cause of the genocide (Prunier, 2005). Therefore, it is no surprise that a large drop in the population did not have a strong and lasting negative effect on value added in agriculture. In the industry and service sectors, labor productivity was probably much higher (e.g., Caselli, 2005), such that a drop in the workforce had more negative effects. Third, de Walque and Verwimp (2010) find that more educated citizens were more likely to die during the genocide. The disproportional loss of educated citizens probably hit agriculture less severely than the industry and service sectors, which are more reliant on skilled workers.

Figure 3 suggests one further interesting finding: Rwanda's value added in agriculture did not only catch up relatively quickly, but was even 20 to 50 percent above its counterfactual value from 2006 onwards. To further investigate this effect, we study the two most important agricultural products for Rwanda in terms of exports, Tea and Coffee (see Figure A.4 in the Appendix). The effects on the production value of tea look like the effect on agricultural value added except that the effects on tea are substantially larger. In 1994, the production value of tea decreased by 69 percent compared to the counterfactual. However, the production value exceeds its counterfactual from 2006 onwards, and does so by more than 100 percent in 2011. We also find a dramatic short-run effect on

[10]While the true Rwanda's value added in agriculture and services stayed above the corresponding values of the synthetic Rwanda after having caught up, the same is not true for value added in industry: The synthetic Rwanda's value added in industry has again been higher than the true Rwanda's value in the most recent years.

the production value of coffee with a decrease of close to 100 percent in the year of the genocide. Since then the production value for coffee has been steadily catching up with its counterfactual. Given the relatively moderate initial effect on the agricultural sector as a whole (relative to industry and services), and the drastic effects on the agricultural export goods tea and coffee, we suspect that the focus has been on subsistence farming in the aftermath of the genocide. This suspicion is in line with the rather moderate increase in undernourishment after the genocide.[11]

5 Concluding Remarks

We have employed the synthetic control method to study the short- and long-term economic consequences of the genocide in Rwanda in 1994, which has been one of the the most intense events of political violence since World War II. We find a large negative effect on economic performance in the short run. In particular, we estimate that GDP dropped by 58 percent below its counterfactual level in 1994. Looking at the long run we find that full recovery to the counterfactual level of development is possible and happened in Rwanda after 17 years. Our analysis therefore challenges two findings from cross-country growth studies. First, the negative short-run effects can be much higher than the average effects found in cross-country growth studies. This insight may not be surprising as countries with large-scale events tend to be under-represented in cross-country growth studies. The second difference is more upbeat in that we show that even countries suffering from very intense internal political violence can fully recover – at least in economic terms. This finding is consistent with standard neoclassical growth models (e.g., Solow, 1956).

In addition, we show that the magnitude of the short-run effect and the speed of recovery are both sector-specific. In case of Rwanda the drop in agricultural production was smaller and recovery was faster in agriculture than in the industry and service sectors. Arguably, the relatively fast recovery in agriculture may have been an important prerequisite for the recovery of the entire economy.

[11] Data on undernourishment is available from the FAO from 1992 onwards, but only as 3-year averages. Undernourishment was around 56 percent in the 3 years prior to the genocide, and around 64 percent in the three years thereafter.

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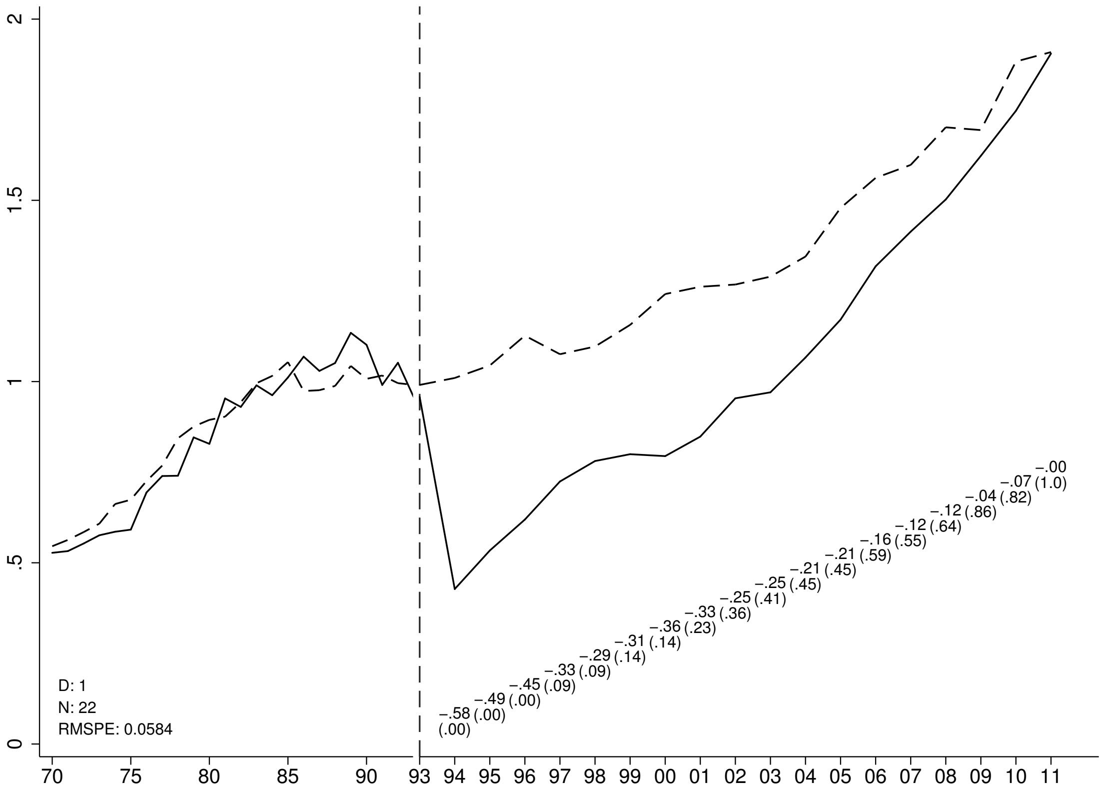
Tables and Figures

Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N	Source
GDP[1]	18687.34	44364.65	96.69	426762.59	1680	PWT
Openess	71.06	36.77	10.08	217.14	1600	
Human Capital	1.67	0.41	1.04	2.85	1176	
Investment	20.89	12.57	-33.14	85.17	1600	
Population[2]	10.88	19	0.07	162.47	1680	
VA Agriculture[1]	841.32	1147.55	9.29	7422.31	1320	WDI
VA Industry[1]	2003.27	6481.84	8.02	49513.01	1278	
VA Services[1]	4544.25	14616.57	52.16	122639.28	936	
Inflation	23.02	187.56	-33.79	5399.53	1531	
Political Rights	5.05	1.81	1	7	1535	FH
Polity2	-2.32	5.99	-10	10	1498	Polity IV
Conflicts	0.12	0.36	0	3	1620	UCDP
Meat[3]	139.16	280.59	0.22	2859.62	1718	FAO
Cereals[3]	1619.61	3524.25	0.11	30209.00	1634	
Pulses[3]	135.02	336.87	0.05	3422.25	1550	
Vegetables[3]	403.74	1119.72	0.40	11846.48	1676	
Fruits[3]	670.15	1420.86	1.13	11212.06	1718	
Tea[4]	23990.32	62985.11	11	424334	528	
Coffee[4]	36812.77	69749.48	8	408257	1011	
Temperature	24.21	4.15	6.51	29.39	1640	CRU 3.1
Precipitation	81.71	52.28	4.17	257.6	1640	

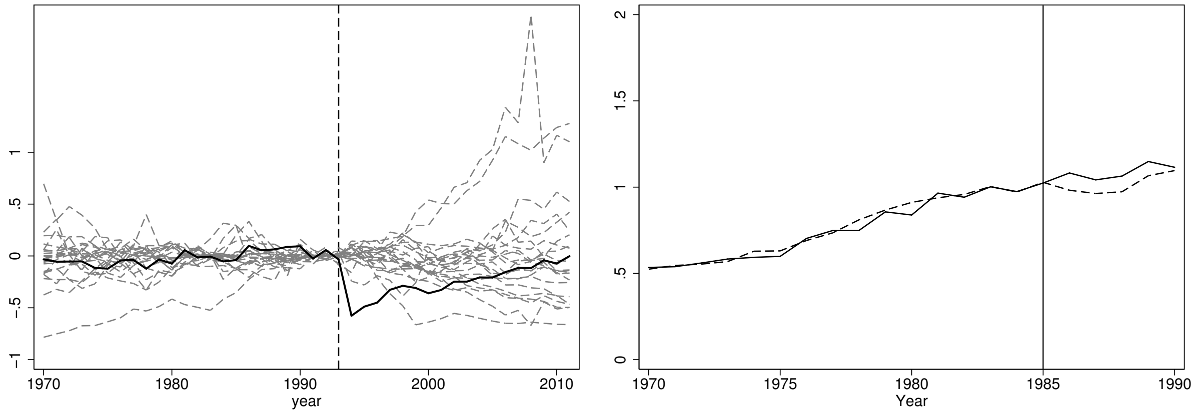
Notes: [1] in million USD. [2] in millions. [3] production in 1,000 tonnes. [4] gross production values in 1,000 international dollars. VA: value added.

Figure 1: GDP



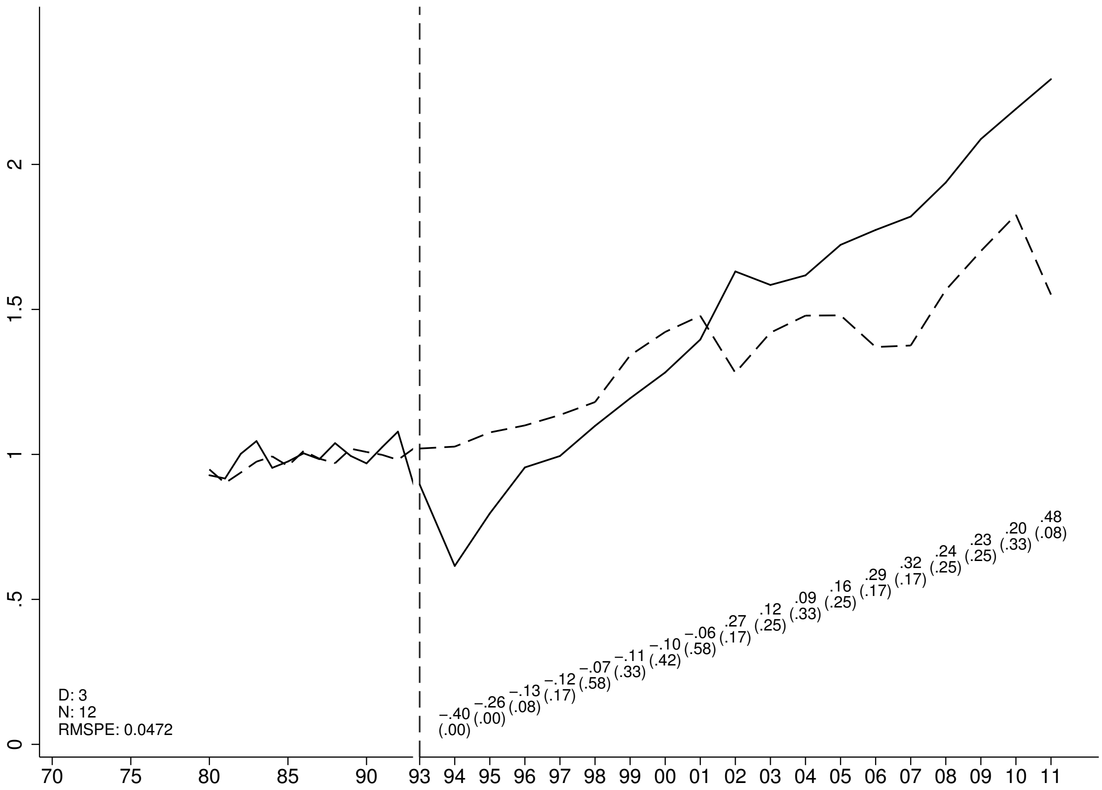
Notes: Normalized GDP (=1 for average over 1991-1993) for Rwanda. The solid line displays the actual GDP for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) GDP for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the probabilities to assess the statistical significance of these effects in parenthesis. The country weights for the synthetic Rwanda are: Cameroon (0.254), Republic of the Congo (0.061), Gabon (0.149), Liberia (0.032), Lesotho (0.192), Mali (0.016), Niger (0.108), Sudan (0.014), and Senegal (0.175). These weights are based on normalized GDP for odd years up to 1993, the 5 year average (1985-1990) of GDP in levels, and the additional predictors discussed in Section 3.

Figure 2: Placebo Studies for GDP



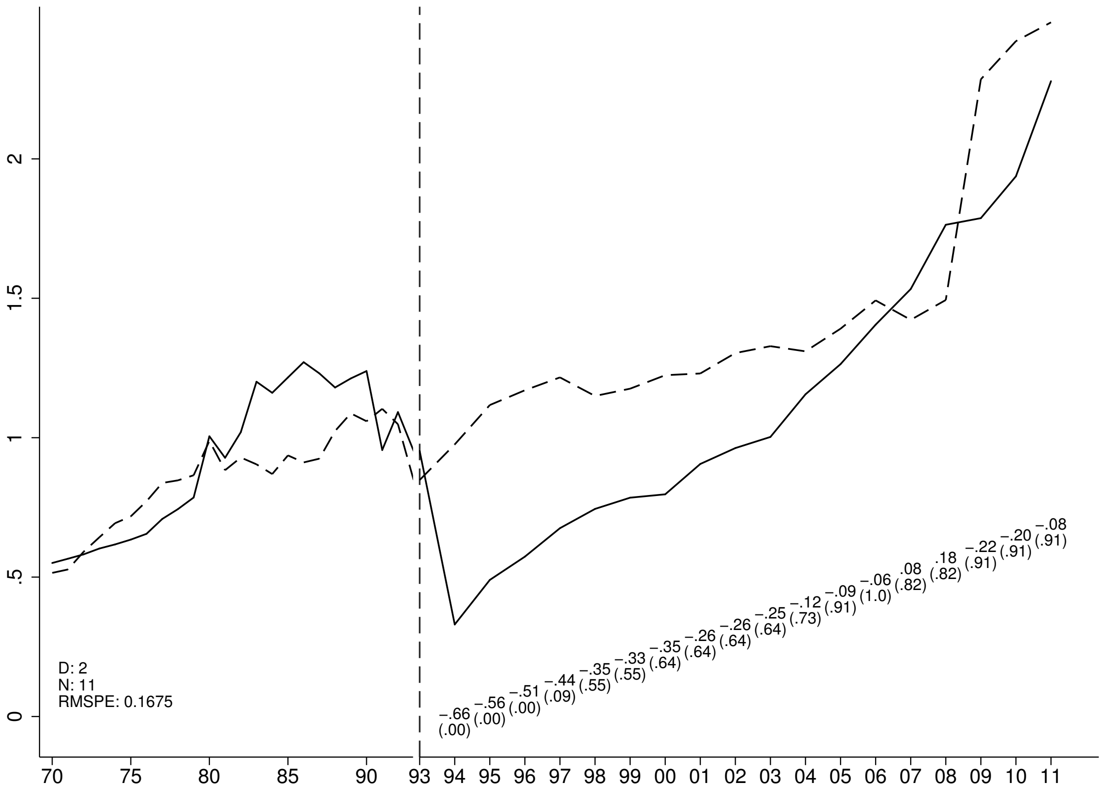
Note: The left graph shows the results of the placebo studies with the gap between the actual GDP and the synthetic GDP, i.e., (actual GDP-synthetic GDP)/(synthetic GDP), depicted on the vertical axis. To minimize the influence of outliers, we applied the rule as described in section 2. The solid line indicates the gap for Rwanda whereas the dashed lines the results for the placebo studies. The right graph show the placebo in time for GDP. The placebo treatment year is 1985.

Figure 3: Value added in agriculture



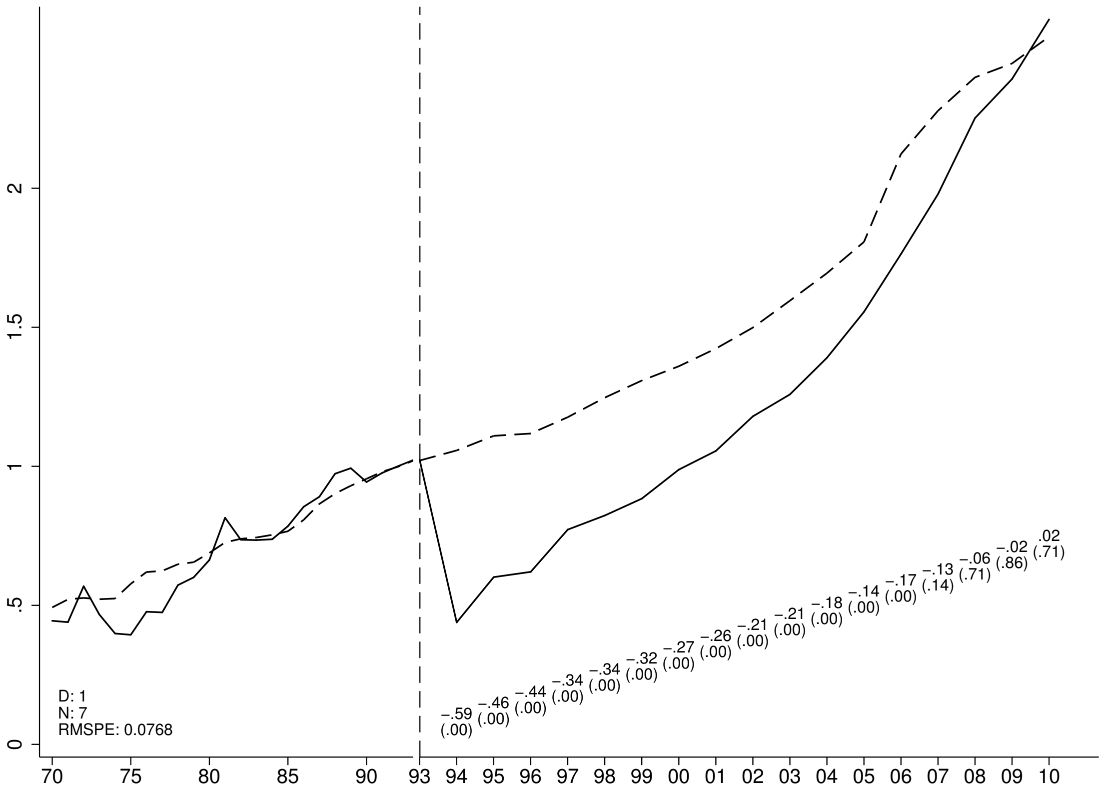
Notes: Normalized value added in agriculture (=1 for average over 1991-1993) for Rwanda. The solid line displays the actual value added for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) value added for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the probabilities to assess the statistical significance of these effects in parenthesis. The country weights for the synthetic Rwanda are: Ivory Coast (0.258), Gambia (0.354), Lesotho (0.123), and Senegal (0.165). These weights are based on normalized value added for odd years up to 1993, the 5 year average (1985-1990) of value added in levels, and the additional predictors discussed in Section 3.

Figure 4: Value added in industry



Notes: Normalized value added in industry (=1 for average over 1991-1993) for Rwanda. The solid line displays the actual value added for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) value added for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the probabilities to assess the statistical significance of these effects in parenthesis. The country weights for the synthetic Rwanda are: Republic of the Congo (0.158), Togo (0.654), South Africa (0.169), and Zambia (0.019). These weights are based on normalized value added for odd years up to 1993, the 5 year average (1985-1990) of value added in levels, and the additional predictors discussed in Section 3.

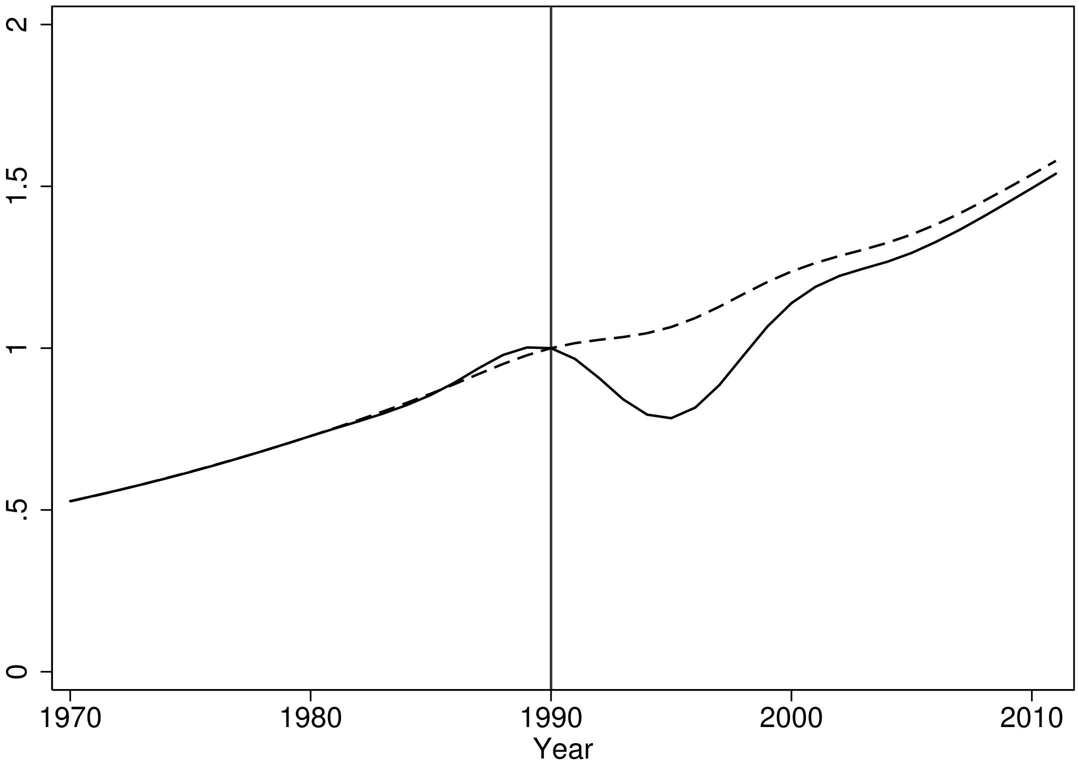
Figure 5: Value added in services



Notes: Normalized value added in services (=1 for average over 1991-1993) for Rwanda. The solid line displays the actual value added for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) value added for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the probabilities to assess the statistical significance of these effects in parenthesis. The country weights for the synthetic Rwanda are: Benin (0.126), Botswana (0.139), Mauritania (0.032), Sudan (0.159), and Senegal (0.544). These weights are based on normalized value added for odd years up to 1993, the 5 year average (1985-1990) of value added in levels, and the additional predictors discussed in Section 3.

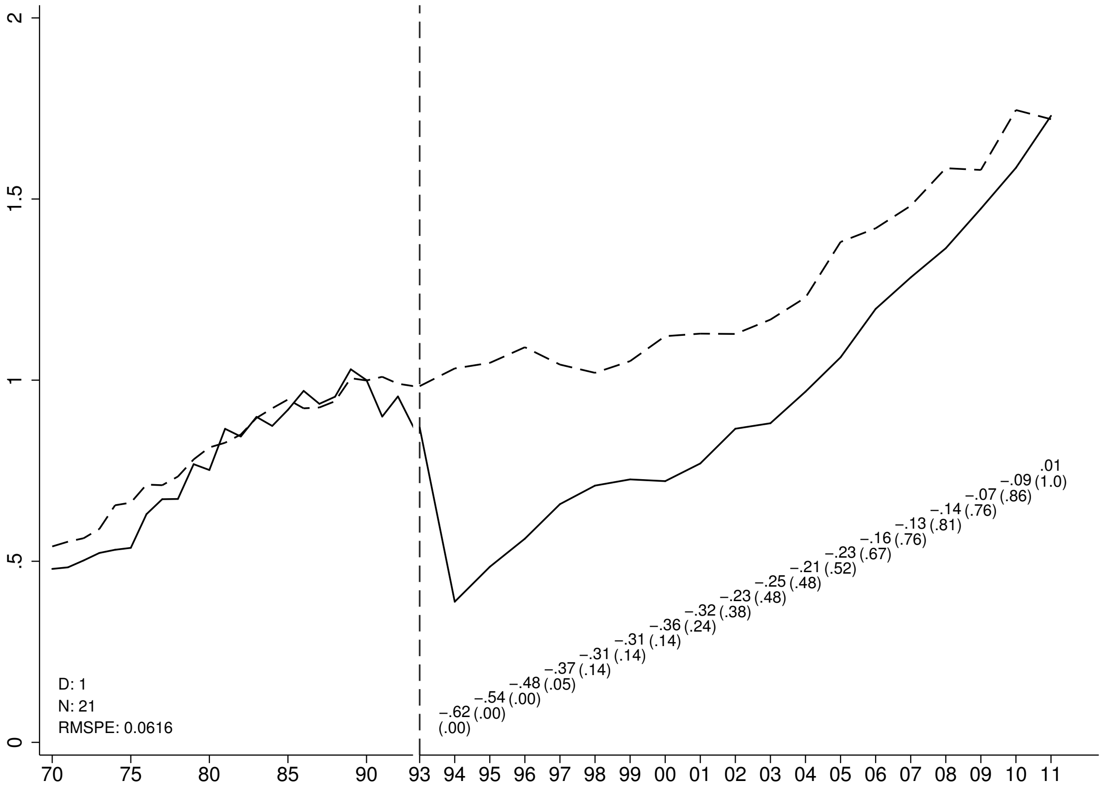
Appendix

Figure A.1: Population Smoothing



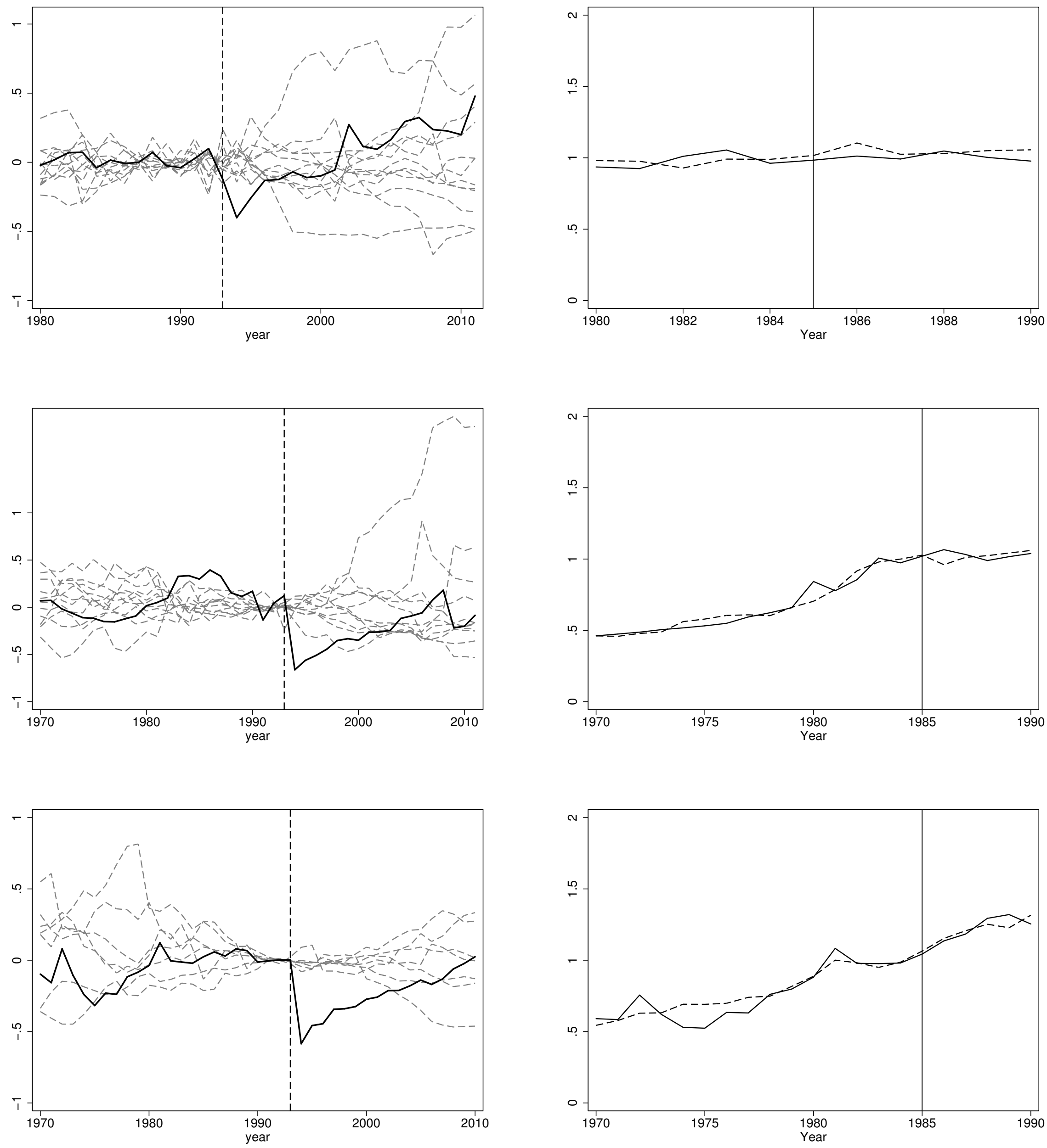
Notes: Normalized total population (=1 for average over 1990-1993) for Rwanda from PWT 8.0. The solid line displays the actual development of total population for Rwanda. The dashed line represents the counterfactual (synthetic) development for Rwanda based on the synthetic control method. This Figure shows how population data included in the Penn World Tables is smoothed over time. The depicted development is rather smooth with no indication of a drastic event in 1994, implying that it does not represent the true development in case of the Rwandan genocide. We have checked population data from other sources including WDI and FAO. Although there are some differences, the general pattern is very similar to the one shown in this figure.

Figure A.2: GDP without Cameroon



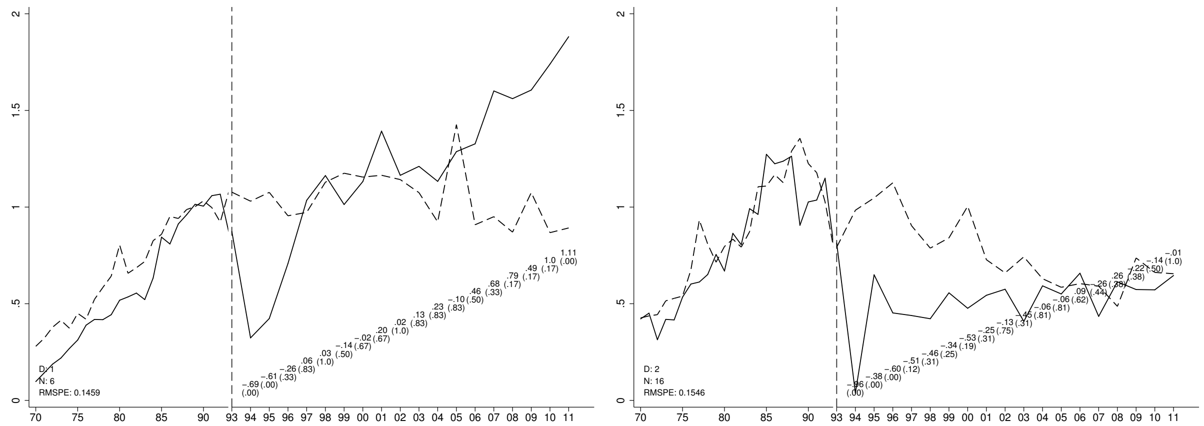
Notes: Identical to Figure 1, except that Cameroon has been excluded from the donor pool.

Figure A.3: Placebo Studies for value added in agriculture, industry, and services



Notes: The upper graph shows the two types of placebo studies for value added in agriculture. The graphs are therefore the equivalent to Figure 2 for GDP. Likewise, the middle and lower graphs show the placebos studies for value added in industry and services, respectively. See Figure 2 for further details.

Figure A.4: Production Values for Tea and Coffee



Notes: The Figure shows results for normalized gross production value (=1 for average over 1991-1993) for tea (left) and coffee (right). The solid line displays the actual gross production value for Rwanda. The dashed line represents the counterfactual (synthetic) for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the probabilities to assess the statistical significance of these effects in parenthesis. The country weights for the synthetic Rwanda are: Tea: Cameroon (0.467), Kenya (0.093), Mauritius (0.217), and South Africa (0.222). Coffee: Republic of the Congo (0.061), Kenya (0.439), Malawi (0.247), Sierra Leone (0.089), Togo (0.026), and Zimbabwe (0.137). In addition to several years of normalized production value, we also use several years of temperature and precipitation data, and the count variable for civil conflicts for coffee. The reason for not including more predictors is the limited data availability. Given the large number of missing observation for each of the variables (outcome of interest and predictors), each additional predictor decreases the number of countries in the donor pool. Even by using this very restrictive set of predictors, we are already down to six countries in the donor pool in case of tea.