

The Economic Effects of Genocide: Evidence from Rwanda[†]

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Abstract

Cross-country studies on the economic consequences of internal political violence typically find short-run effects that are not very large, and no evidence for full economic recovery. We study the economic impact of the Rwandan genocide in 1994, which has been one of the most intense events of political violence since World War II. More precisely, we estimate its effect on economic development using the synthetic control method and addressing data quality issues that have been a concern in the literature. We find a 58% decrease in GDP in 1994. This effect still corresponds to a decrease in GDP per capita of around 31% when taking into account that 800,000 people were killed and that around two million fled the country during the genocide. We further provide strong evidence that Rwanda's economy was then catching up with the estimated counterfactual GDP it would have had in absence of the genocide, with the gap closing after 17 years. When focusing on the effects on the Rwandan exports as reported by the importing countries, we find similar short-run effects but somewhat slower recovery. We finally show that agriculture was less severely hit by the genocide than the industry and service sectors, and that it also recovered much more quickly.

Key words: genocide, growth, economic recovery, Rwanda

JEL classification: O11, O55

1. Introduction

Civil wars and other forms of internal political violence were common in the second half of the last century (Blattman and Miguel, 2010). In this century, the news are again dominated by reports on civil wars and major political violence in a diverse set of countries from Eastern Europe, the Middle East, North Africa, South Asia, and Sub-Saharan Africa. Given

the ubiquity of political violence, it is surprising that their consequences for economic prosperity have not received more attention in the literature so far. Economic theory provides limited guidance. Standard neoclassical growth models predict relatively fast growth in the first years after civil war and major political violence 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 similarly prominent cross-country growth study, Cerra and Saxena (2008) find that civil conflicts cause GDP to drop by 6% on average, and that GDP only partially rebounds in the medium- to long-run. In contrast, Mueller (2012) finds an average drop of 18% and no evidence for economic recovery. More recently, researchers acknowledge the fact that each conflict is unique in its consequences for the affected territories. Using the synthetic control method to estimate individual effects for different countries, Bove *et al.* (2017), Costalli *et al.* (2017), and Gardeazabal and Vega-Bayo (2014) all find heterogeneous effects across countries.¹

Our study contributes to the literature on the economic consequences of internal political violence by focusing on the genocide in Rwanda in 1994. The Rwandan genocide was the result of tensions between Hutus and Tutsis dating back to the times of colonialism. The Belgian colonialists granted preferential treatment to members of the Tutsi population (Prunier, 1995). After independence in 1962, the Hutu majority tried to marginalise the Tutsi population, thereby causing a significant stream of Tutsi refugees into Uganda and other neighbouring countries. Then, in the early 1990s, Tutsi refugees tried to gain power over Rwandan territory resulting in a civil war from 1991 to 1993. The start of the genocide itself is tied to the death of Rwandan president Juvenal Habyarimana, who was a member of the Hutu majority. His plane was shot down near the capital city of Rwanda, Kigali, on 6 April 1994. Although it is still unclear who was responsible for the shooting down, Hutu extremists blamed the Tutsi rebels and initiated an orchestrated mass-killing of Tutsis and moderate Hutus, which had been systemically planned long before (Prunier, 1995). During a period of approximately 100 days, extremists of the Hutu majority slaughtered approximately 800,000 Tutsis and moderate Hutus (UNHCR, 2000). In addition, there was an exodus of at least two million refugees (UNHCR, 2000). Due to its speed and

1 The recent empirical contributions by, e.g., Miguel and Roland (2011), Rogall and Yanagizawa-Drott (2013), Rohner *et al.* (2013) and Serneels and Verpoorten (2015) compare the economic consequences of internal political violence across different regions of a conflict-torn country that have experienced different degrees of violence. In addition, Singhala and Nilakantan (2016) use the synthetic control method to estimate the economic effects of a counterinsurgency policy in an Indian state. 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. For example, Miguel and Roland (2011) find no long-term differences in economic and social outcomes between more and less intensively bombed Vietnamese districts. This absence of conflict-induced local poverty traps does however not necessarily imply full recovery at the country level.

intensity, this genocide has doubtlessly been one of the most intense events of political violence since World War II.²

We aim to identify the country-wide short- and long-run economic effects of the Rwandan genocide on economic output. Complementary to the cross-country studies mentioned beforehand, which rely on a uniform design for all conflicts under investigation, we focus on just this one country. This focus allows us to tailor our analysis to account for the specific characteristics of Rwanda and the genocide in 1994.³

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.⁴ 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.

We find that the Rwandan genocide led to an immediate drop in GDP by 58%. Taking into account the death toll suggests that GDP per survivor dropped by 53%. Further taking into account the exodus of refugees suggests that GDP per survivor staying in Rwanda still dropped by 31%. These effects are large, but not unprecedented. [Barro \(2006\)](#) reports a drop of 64% in the German GDP per capita in just two years during World War II. Since the end of the genocide, the Rwandan economy has 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.

These results differ from those in the previous literature. [Lopez and Wodon \(2005\)](#) find a permanent negative effect on GDP per capita of about 25–30% when using time series data. A major advantage of the synthetic control method is that it 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. Moreover, [Lopez and Wodon \(2005\)](#) find no evidence for convergence (possibly due to limited post-treatment information available at the time), while we find strong evidence that Rwanda's GDP has caught up successfully. More recently, [Costalli et al. \(2017\)](#) find a negative short-run effect of around 14% on Rwandan GDP per capita using the synthetic control method. There are many differences between our application of this method and their uniform design for a relatively large set of countries, e.g., differences related to the

- 2 Another remarkable aspect of the Rwandan genocide was the scale of civilian involvement at the local level. [McDoom \(2013\)](#) shows that this involvement was determined by the social structure of the household and the neighbourhood.
- 3 See [Verpoorten \(2014\)](#) for a broad discussion of growth, poverty and inequality in Rwanda.
- 4 Other prominent applications of the synthetic control method include [Billmeier and Nannicini \(2013\)](#) and [Cavallo et al. \(2013\)](#). [Gardeazabal and Vega-Bayo \(2017\)](#) compare the synthetic control method and a panel data approach in case of a single treated unit. They find that the synthetic control method tends to lead to more consistent results.

selection of the donor pool and the use of standard population data.⁵ They study neither long-run effects nor the effects on other outcomes.⁶

In contrast to these studies, we deliberately use GDP rather than GDP per capita as the outcome of interest. There are two reasons why we find using GDP per capita problematic when studying the effects of mass-killings. First, mass-killings lead to a substantial death toll and often also to a large number of people fleeing the country. As a consequence, a zero or positive effect on GDP per capita cannot serve as an indication that there was no economic disruption. This issue is particularly salient for the Rwandan genocide with its death toll of 800,000 and the two million people who left the country at least temporarily. The sudden drop in the population size of around 40% implies that a zero effect on GDP per capita could well be consistent with a massive drop in economic activity. In fact, if the population data were accurate, a zero effect on GDP per capita might mask a 40% drop in economic activity. Here, the second concern comes into play: Annual population statistics tend to be heavily smoothed over time. In case of the Rwandan genocide, standard data sources have stretched the mass-killings happening in 1994 over a period of almost 10 years in their annual population data (see Figure A1 in the Appendix). This smoothing of the population data makes GDP per capita a problematic outcome variable in the aftermath of the genocide.

We also explicitly address the concerns over the quality of the GDP data in developing countries as pointed out by, e.g., [Henderson et al. \(2012\)](#) or [Johnson et al. \(2013\)](#). To corroborate our findings, we additionally use a sectoral decomposition of GDP (i.e., value added in agriculture, industry and services), look at trade statistics, and the production value of the major drivers of exports in Rwanda: tea and coffee. In doing so, we rely on a variety of different data sources which mitigates the risk of measurement error. When looking at the development of the various sectors, we find an interesting pattern: Value added in agriculture dropped less than value added in industry and services, and it also rebounded more quickly to its counterfactual level. We argue that this pattern is reasonable and can be explained by the high priority given to subsistence consumption needs directly after the

- 5 There are three main differences in the donor pool. First, [Costalli et al. \(2017\)](#) include countries from other continents, while we exclude countries from outside Sub-Saharan Africa, whose economic and political (post-treatment) development may have been shaped by very different forces. Second, they include neighbouring countries, which we exclude because they were 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. Third, they exclude countries that experienced a civil conflict during their sample period (1970–2008). We keep these countries in the donor pool, because we do not want to lose too many Sub-Saharan African countries, and because Rwanda may have experienced some conflict events even in the absence of the genocide, making it inappropriate to construct the counterfactual/synthetic Rwanda as a weighted average of peaceful countries. Going further, we use information on civil conflict events in the years up to 1994 when constructing the counterfactual/synthetic Rwanda. See below for why we do not use standard population data.
- 6 [Bove et al. \(2017\)](#) also apply the synthetic control method with a uniform design to study the effects of civil conflict on GDP per capita in a relatively large set of countries. For Rwanda, they define all years from 1990–1994 to 1996–2000 as conflict years, and report a mean effect that is positive, but small and statistically insignificant. This mean effect is consistent with a large negative effect in 1994 and positive effects from 1996 to 2000.

genocide; the high population density and the associated difference in labour productivity across sectors; and the targeted killing of educated citizens during the genocide. It is also consistent with our findings regarding the reaction of trade flows in the aftermath of the genocide. While we find a long-lasting effect on exports in general which substantially shrinks after around 15 years, there is a permanent negative effect in the overall value of goods and services exported to high income countries. Finally, we find a pronounced short-term effect on tea production and a more long-lasting effect on coffee production.

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 generalises 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 (for which data is available). We exclude countries from outside Sub-Saharan Africa, as their economic and political (post-treatment) development may have been shaped by very different forces. Moreover, we exclude Rwanda's neighbouring 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 minimised 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 most 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 minimise 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.⁷

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).

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 the different dependent variables used in our analysis turn out to be negligible. Therefore, these placebos do not invalidate the treatment effects found in the core analysis (see Figure 2 and Figure A3 in the Appendix).

3. Data

The main outcome variable, GDP, is real GDP at chained PPPs from Penn World Table (PWT) 8.0 published by the Groningen Growth and Development Centre (GGDC) at the University of Groningen. Further outcome variables are the value added in agriculture, industry and services in constant USD from the World Development Indicators (WDI) published by the World Bank.⁸

In line with Abadie and Gardeazabal (2003) and Abadie *et al.* (2015), we use different types of economic and political predictors when looking at GDP and value added in industry and services: the human capital index per worker from PWT 8.0, which is based on years of schooling and an assumed rate of return for primary, secondary and tertiary

7 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 criterion in our setting where we only want to exclude extreme outliers on one side of the distribution. Figure 2 (left graph) below 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).

8 We choose PWT as the source for the GDP data as this has a strong positive impact on the sample size. Data for value in agriculture, industry and services are only available from WDI.

education; 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 is a combined indicator measuring the quality of political institutions, i.e., how well developed a country's democratic traits are relative to its autocratic traits; the political rights rating from Freedom House measuring the quality of the electoral process, political pluralism and participation, and functioning of government; and a variable indicating the number of civil conflict/war events in a particular country and year from the Uppsala Conflict Data Program (UCDP, see [Gleditsch et al. 2002](#)).

In addition, we also use average daily temperature and precipitation aggregated on the country and year level 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. We use population data from PWT 8.0, and gross production value for coffee and tea from the FAO.⁹ Finally, the bilateral trade data stem from the correlates of war (COW) project ([Barbieri and Keshk, 2012](#)). For the empirical analysis, we created two variables: (i) the overall value of goods and services exported to all countries and (ii) the value of goods and services exported to high income countries (World Bank classification). More specifically, we focus on the imported value of goods and services that reporting countries recorded with Rwanda or any other SSA country being the originating country.¹⁰ Table 1 presents summary statistics for each of the variables used in the empirical analysis.

When computing the weights of the different countries in the donor pool, we use the 5-year average of the outcome variable of interest (e.g., the GDP in levels averaged over the years 1985–1990) and its normalised value for every odd year (e.g., normalised GDP in 1971, 1973, 1975,..., 1993) as predictors. The reason for this choice is that we want to construct a synthetic Rwanda that is not just similar to the actual Rwanda in terms of the outcome level, but also in terms of outcome growth and outcome fluctuations.¹¹

For most of the other predictors, we use the average values over the years 1985–1990 when computing the weights of the different countries in the donor pool. However, 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 3 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.

In doing so, we identify a synthetic control which is as similar as possible to the true Rwanda in terms of its economic and political conditions and the degree of political violence in the years

9 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).

10 We focus on imports in other countries rather than directly on exports as the data quality of imports tends to be higher due to the higher incentives of accurately recording imports (e.g., due to tariffs as well as security and health issues).

11 The reason for only using every odd year is that using every year would result in a dominance of outcome information in the matching process, such that other important predictors would receive insufficient weighting in the construction of the synthetic Rwanda.

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
Mear 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	
Trade ¹	1489.73	5055.67	0	76564.47	1602	COW
Trade HI ¹	1245.8	4122.86	0	61236.43	1602	

Note: ¹In million USD (not normalised). ²In millions. ³Production in 1,000 tonnes. ⁴Gross production values in 1,000 international dollars. VA, value added.

prior to the treatment, i.e., the genocide. For the estimates focusing on GDP, 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).¹² Table 2 compares the true (i.e., treated) and the synthetic Rwanda.

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 share of countries in the donor pool with a higher RMSPE ratio in parenthesis. The value after D (bottom left) corresponds to the number of outliers in the placebo study that have

12 The table notes provide the corresponding weights for all the other estimates.

Table 2: Predictor Balance

Variable	Treated	Synthetic
GDP	7,357	11,377
Openess	86.53	81.00
Human capital	1.31	1.65
Investment	14.09	20.96
Inflation	3.20	3.97
Political rights	6.00	5.49
Polity2	-7.00	-6.32
Conflicts	0.17	0.06
Conflicts (1991–1993)	1.00	0.31

Note: Predictor balance for all predictors used to estimate the effect on GDP (see Figure 1). Values constitute averages for the period 1985–1990 for the true Rwanda (Treated) and the synthetic Rwanda (Synthetic) unless indicated otherwise.

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.

The estimates show that the genocide reduced GDP by 58% 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% if we take the 800,000 deaths into account, and still by around 31% if we take moreover into account that two million people fled the country.¹³ 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. We thus argue that Rwanda’s economy fully recovered from the genocide after around 17 years.

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, there is no effect.

The share of countries in the donor pool with a higher RMSPE ratio reported in Figure 1 is based on placebo studies for the 22 Sub-Saharan African countries in the donor

13 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% in 1994. As discussed in Section 1, we however advise against using per capita time series, partly because estimated time series for population are all extremely smooth. While around 11% of Rwanda’s population was killed in a short period of time (and many more left the country at least temporarily), the effect of the genocide is smoothed over ten years in the estimated population time series (see Figure A1 in the Appendix).

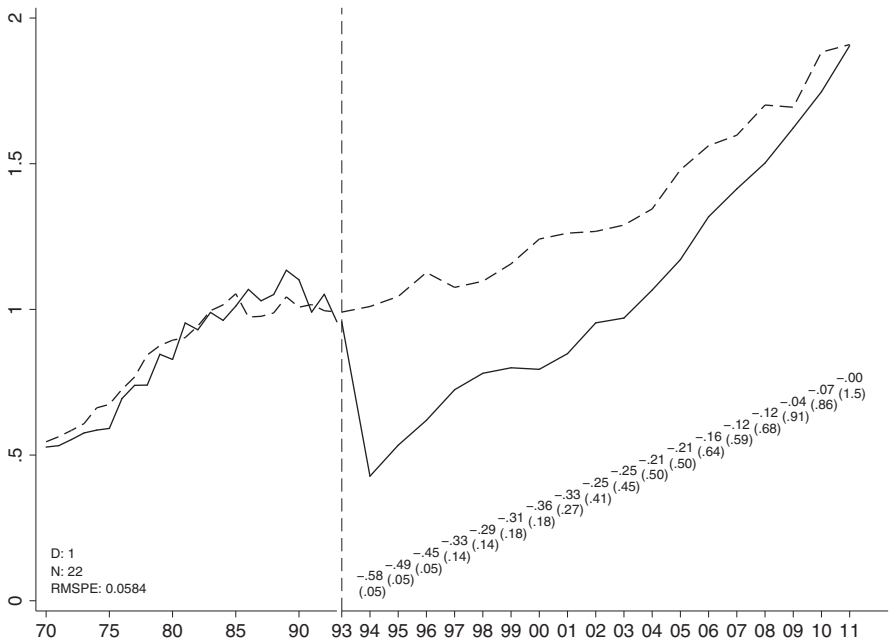


Figure 1: GDP

Note: Normalised 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 share of countries in the donor pool with a higher RMSPE ratio 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 normalised 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.

pool. The values of $1/22 = 0.05$ for the years 1994–1996 indicates that the difference between the 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 this share 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, this share becomes $3/22 = 0.14$ in 1997.

Figure A2 in the Appendix presents an important robustness test. It shows the estimated effects when sequentially dropping all the predictors used to construct the synthetic Rwanda, except the information of pre-treatment GDP. The results remain qualitatively unchanged and quantitatively similar.¹⁴

14 As a further robustness test, Figure A3 in the Appendix presents the estimated effects after excluding Cameroon, which has the largest weight, from the donor pool. The results remain very similar.

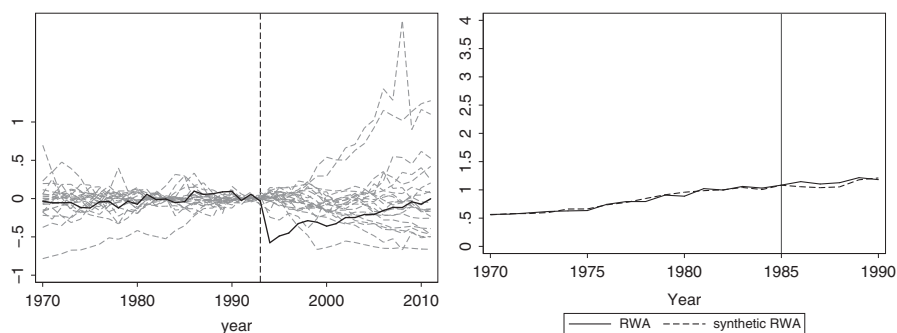


Figure 2: Placebo studies for GDP

Note: The left graph shows the results of the placebo studies with the gap between the actual GDP and the synthetic GDP, i.e., $(\text{actual GDP} - \text{synthetic GDP}) / (\text{synthetic GDP})$, depicted on the vertical axis. To minimise 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 shows the placebo in time for GDP. The placebo treatment year is 1985.

Figures 3–5 present the results for valued added in agriculture, industry and services, respectively.¹⁵ The reduction in value added in 1994 was 40% in agriculture, 66% in industry and 59% in services. Moreover, the process of catching up with the counterfactual value added lasted around 8 years in agriculture, 13 years in industry, and 17 years in services.¹⁶ These results suggest that agriculture was less severely hit by the genocide than the other sectors, and recovered much more quickly.

We can think of various possible reasons for these heterogeneous effects across sectors: First, people probably give the 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 over 90% of the population work in smallholder agriculture (Mutebi *et al.*, 2003).¹⁷ The negative effects of population density on agricultural output per capita are even seen as a major cause of the genocide (Prunier, 1995). 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, labour productivity was probably much higher (Caselli, 2005), such that a drop in the workforce had more negative effects.

- 15 Figure A4 in the Appendix presents the same placebo tests as Figure 2, but for valued added in agriculture, industry and services rather than GDP.
- 16 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.
- 17 While over 90% of the population work in the agricultural sector (even in the post-genocide period), its contribution to overall GDP was only 32.5% in both 1990 and 2010 (according to data from the WDI).

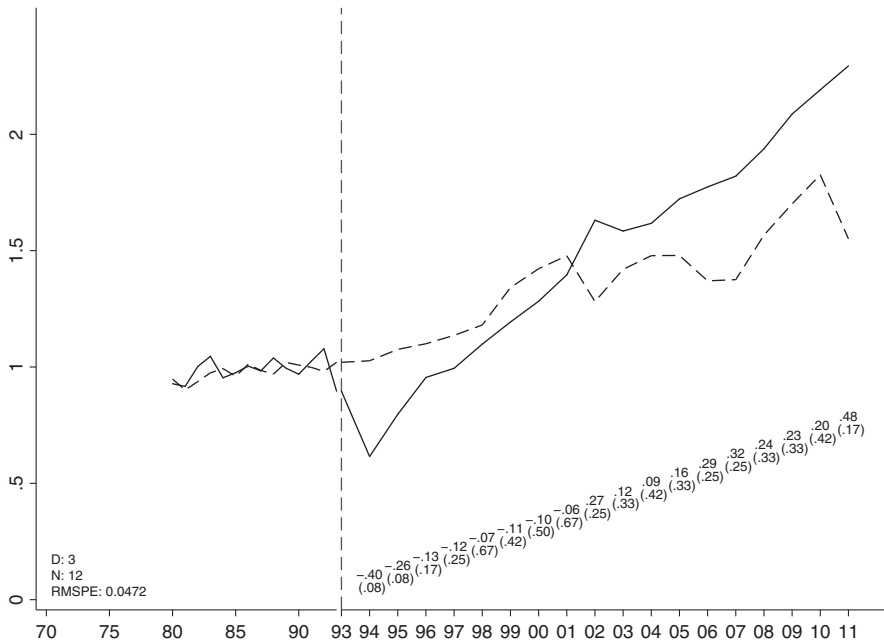


Figure 3: Value added in agriculture
Note: Normalised value added in agriculture (=1 for average over 1991–1993) for Rwanda. The solid line displays the actual value added for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) value added for Rwanda based on the synthetic 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 normalised 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.

Third, De Walque and Verwimp (2010) find that citizens with an urban and more educated background were more likely to die during the genocide. This result probably reflects that Tutsis were on average more educated than Hutus, and also that the moderate Hutus killed by the Hutu extremists tended to be relatively rich and well educated as well. One of the underlying reasons for this pattern is that the perpetrators could often keep assets and properties of their victims. The disproportional loss of urban and educated citizens and, thereby, a substantial part of the country’s economic elite most likely hit the industry and service sectors more severely than agriculture, which is less reliant on skilled workers. More generally, De Walque and Verwimp (2010) review a number of studies showing that the likelihood of being killed during mass-killings is higher for wealthy and well-educated people. As a consequence, targeted killings often lead to a substantial loss of human capital which can have more far-reaching consequences than the loss of physical capital.

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% 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 A5 in

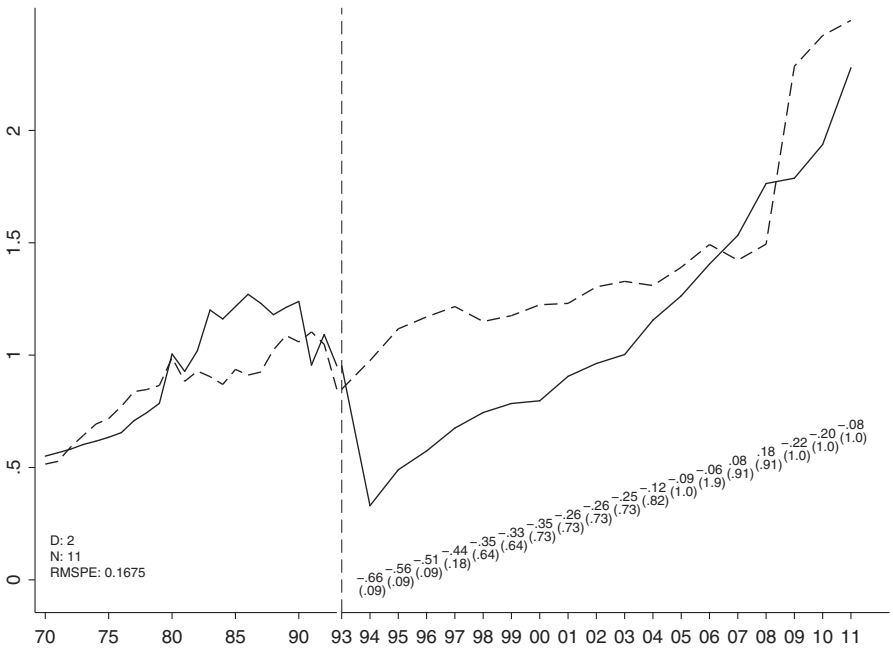


Figure 4: Value added in industry
Note: Normalised value added in industry (=1 for average over 1991–1993) for Rwanda. The solid line displays the actual value added for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) value added for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the share of countries in the donor pool with a higher RMSPE ratio 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 normalised 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.

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% compared to the counterfactual. However, the production value exceeds its counterfactual from 2006 onwards, and does so by more than 100% in 2011. We also find a dramatic short-run effect on the production value of coffee with a decrease of close to 100% 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.¹⁸

18 Data on undernourishment are available from the FAO from 1992 onwards, but only as 3-year averages. Undernourishment was around 56% in the 3 years prior to the genocide, and around 64% in the 3 years thereafter.

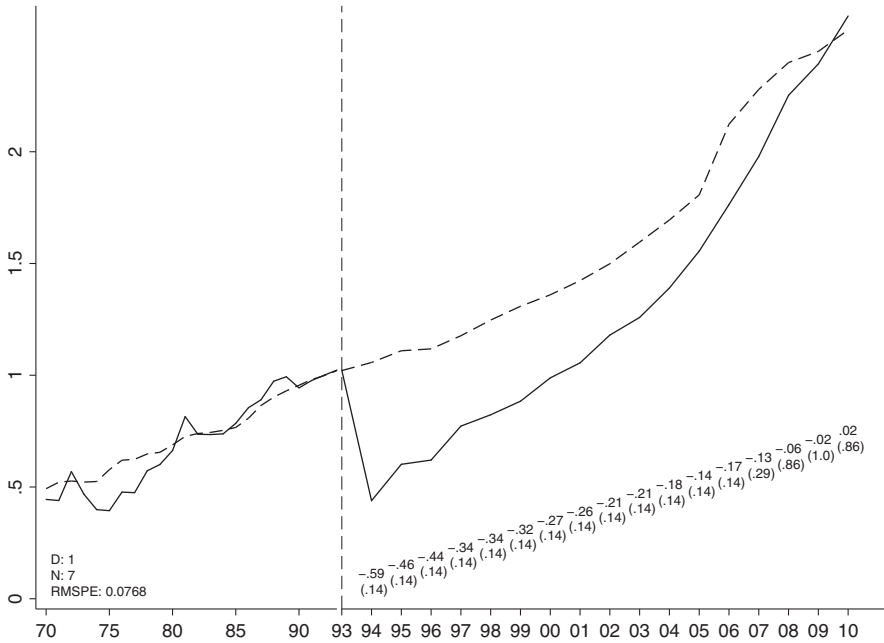


Figure 5: Value added in services
Note: Normalised value added in services (=1 for average over 1991–1993) for Rwanda. The solid line displays the actual value added for Rwanda after the genocide. The dashed line represents the counterfactual (synthetic) value added for Rwanda based on the synthetic control method. The numbers below these lines display the estimated yearly treatment effects in percent and the share of countries in the donor pool with a higher RMSPE ratio 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 normalised 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.

Henderson *et al.* (2012) remind us of the rather limited quality of GDP data (including sectoral data) for developing countries such as Rwanda. They, therefore, suggest using satellite data on night-time light intensity instead. Unfortunately, these data only start in 1992 and therefore leaves us with insufficient pre-treatment information. Given these limitations, we use trade flows as an alternative. Of course, trade flows are not a perfect substitute for GDP data as a substantial part of the economic output of Rwanda is consumed domestically. Even worse, the quality of trade data for developing countries can also be rather poor as pointed out by, e.g., Rozanski and Yeats (1994) or Gleditsch (2002). However, we believe that by combining the results from aggregate GDP (PWT), sectoral value added (WDI) and trade statistics (COW) we offer the most comprehensive analysis of the economic consequences of the Rwandan genocide. When looking at Figure A6 in the Appendix, it becomes clear that there has been a long-lasting negative effect on the value of goods and services imported by all Rwandan trade partners with a substantial decrease of the effect in the period 2007–2009. The effect on imports reported by all Rwandan trade partners (left graph) is therefore in line with our original findings for GDP. More interestingly, however, are the findings when focusing on records from high income countries only (right graph). Exports

to high income countries decreased after the outbreak of the mass-killings and remain at a low level relative to the sharp increase of the counterfactual (i.e., a combination of similar Sub-Saharan African countries).

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 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% 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 previous studies. First, the negative short-run effects can be much higher than the average effects found in previous 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 was contributing to the recovery of the entire economy.

Supplementary material

Supplementary material is available at *Journal of African Economies* online.

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