The Environmental Costs of Civil War: A Synthetic Comparison of the Congolese Forests with and without the Great War of Africa

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Despite the fact that, between 1950 and 2000, more than 80% of wars occurred within biodiversity hot spots, we do not fully understand the environmental costs of war. This study conducts one of the first systematic evaluations of the costs of civil war for forest environments. The analysis, however, requires a proper counterfactual: the forest coverage if it were not for civil war. Moreover, instead of estimating an average cost of diverse civil wars, it would be better to tailor the estimate to each war. I address these problems by applying the synthetic control method to the case of the Great War of Africa in the Democratic Republic of the Congo. The analysis shows that the civil war caused a 1.61% loss of the forests, which is more than the entire territory of Belgium and nearly a half of Sierra Leone, over five years. The finding calls further attention to "conflict timber" problems.

Despite increasing attention to environmental changes and civil war, we do not yet fully understand the environmental costs of civil war. The absence of systematic analysis is rather surprising, given the substantial overlap between the locations of civil war and environmental reservoirs. Between 1950 and 2000, for instance, "118 out of 146 wars (81%) took place wholly or partially within biodiversity hotspots" (Hanson et al. 2009, 580), and 23 out of 34 hot spots (68%) experienced at least one war. Moreover, over half of tropical rainforests (55%) are located in countries that experienced civil war since 1945, while over one-third of civil wars (39%) happened in countries with tropical rainforests.[1] Given these overlaps, it is critically important to understand possible costs of civil war for forest environments.

In fact, the policy community has warned of the environmental consequences of "conflict timber" (UNSC 2001b, 44), which is defined as "wood that has been traded or taxed at some point in the chain of custody by armed groups, be they rebel factions or state militaries, or by a civilian administration involved in armed conflict to finance hostilities or otherwise perpetuate conflicts" (Price, Donovan, and De Jong 2007, 117). The issue of conflict timber is also reported for the case of the ongoing civil war in the Central African Republic (Global Witness 2015). Despite these facts, we still do not have systematic answers about the environmental costs of civil war.

In this study, I conduct one of the first systematic evaluations of the environmental costs of civil war. Although previous studies examine whether and why deforestation occurs at particular locations within a country with an ongoing civil war, they do not consider the possibility that civil war can cause deforestation in the locations where no violent events are occurring, or they do not evaluate the overall costs of a civil war for forest environments. The cost evaluation, nonetheless, has substantive importance. Without knowing how much forest is lost because of civil war, for instance, we cannot properly incorporate civil war into forest and climate change forecasts (Field and Van Aalst 2014), which provide a scientific basis for a number of international accords, including the 2015 Paris Agreement. Furthermore, understanding the environmental costs of civil war is essential for considering possible policies for environmental recovery

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1. See app. 1 (apps. 1–13 are available online) for the details.

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after civil war (Lambin and Meyfroidt 2011). This article fills this gap between the academic research and substantive needs by analyzing the environmental costs of civil war at the country level.

The evaluation of environmental costs, however, poses empirical challenges. First and foremost, the environmental costs of civil war cannot be estimated without knowing what would have happened if it were not for the civil war. Only by comparing the so-called counterfactual (in statistics and political science) or baseline (in ecological sciences and policy evaluation) to the observed forest changes can we properly understand how much deforestation civil war causes. The field-level or satellite-based description in geographical sciences, however, does not properly account for the counterfactual problems, leaving us little clue about the causal effects of civil war. Moreover, while previous studies tend to examine an average effect across different cases (Reuveny, Mihalache-O'Keef, and Li 2010), the evaluation of environmental costs needs to be tailored for each civil war. In fact, it is possible that even if the average effect is reforestation, a particular civil war causes substantial deforestation. Previous studies about human and economic costs of civil war indeed emphasize costs of particular civil wars rather than estimating global effects (Horiuchi and Mayerson 2015; Straus 2004).

In this article, I address these problems by applying the synthetic control method. The synthetic control method (Abadie, Diamond, and Hainmueller 2010, 2015) allows me to explicitly and empirically construct a plausible counterfactual. Furthermore, unlike the regression-based methods, the synthetic control can yield an estimate of a particular civil war without assuming that the effect would be constant across different cases. I apply the method to the case of the Great War of Africa in the Democratic Republic of the Congo (DRC), using satellite-based data of forest coverage, which are available twice a month from 1987 to 2008 for all tropical countries.

The analysis shows that the Great War of Africa caused a 1.61% loss of forest coverage on average over five years, which amounts to a forest loss of over 37,000 square kilometers—more than the entire territory of Belgium and nearly a half of Sierra Leone. Based on a previous study of the carbon absorption rate in the Congolese forests, the result implies an increase of over 36 million tons of CO_2 per year, which is equivalent to the annual emission from over 8 million typical automobiles. These estimates suggest a potentially devastating effect of civil war on forest environments. At the end of this article, I discuss empirical challenges for examining causal mechanisms at a subnational level and also present preliminary evidence.

CIVIL WAR AND FOREST ENVIRONMENTS

Despite the absence of attention, a handful of studies across different fields—including political science, economics, conservation science, and geographic information system (GIS) image analyses—examine the effect of political violence on forest environments. While some studies rather counterintuitively state that violence reduces the rate of deforestation or even results in reforestation, other scholars consider the adverse impacts of political violence on forest environments. These studies, however, do not consider the possibility that civil war can result in deforestation in nonwar zones, or they do not evaluate the net costs of civil war for forest environments at a national scale.

Migration and counterinsurgency

Some studies have made the counterintuitive argument that political violence causes reforestation (Alix-Garcia, Bartlett, and Saah 2013; Martin and Szuter 1999). This proposition, which I call the *migration thesis*, has been built inductively from field observations, including those of Lewis and Clark on the American Indian Wars (Martin and Szuter 1999), the El Salvadoran Civil War (Hecht et al. 2006), the Sri Lankan Civil War (Suthakar and Bui 2008), and the Sierra Leonean Civil War (Lindsell, Klop, and Siaka 2011). In general, the migration hypothesis maintains that political violence threatens lives and property, resulting in migration from the affected territories, which in turn reduces human pressure on local forests. Political violence and the resultant insecurity, for instance, entail death, refugees, reduction in firewood demand, abandonment of farmlands, and cessation of slash-and-burn agriculture. These demographic changes can decrease human pressure on forests, providing de facto protection of the environment. Recently, Burgess, Miguel, and Stanton (2015) provided some of the first systematic evidence for the migration hypothesis. They find that chiefdoms that experienced violence during the Sierra Leonean Civil War have lower rates of deforestation.

By contrast, other studies highlight the adverse consequences of political violence. In insurgent wars, rebels can use dense forests as hideouts (Collier and Hoeffler 1998), which gives government forces a tactical incentive to cut or burn down the forests. During the Vietnam War, for example, the United States extensively used chemical defoliates to destroy Vietcong hideouts (Nakamura 2007). Recently, Fergusson, Romero, and Vargas (2014) extend this perspective by incorporating the possibility that the government actively uses violence to dislocate rebels and their potential supporters. They find that during the Colombian Civil War, municipalities that had paramilitary violence experienced accelerated rates of deforestation.

Seeing the forest for the trees? Resource mobilization theory

Despite the attention to subnational dynamics, no study of which I am aware analyzes the overall costs of civil war on a country. Importantly, while previous studies examine the effect of violent events,[2] they do not assess the effect of civil war.[3] That is, although previous studies analyze how individual events during civil war can affect forest coverage, they provide little clue about how civil war, as a collective phenomenon, can affect forest environments.

This distinction has particular importance, as civil war can cause deforestation in the locations where no violent events are occurring. From the perspective of resource mobilization theory (Hazen 2013; Ross 2004; Rustad et al. 2008; Theisen 2008), which mostly examines the effect of natural resources on civil war, it is possible that parties exploit timber in safer areas in order to fund their activities. In countries of ongoing civil war, there is little reason to expect that parties exploit timber in frontline regions: if other conditions are constant, it is safer and hence more efficient to exploit timber on the home front. During the Cambodian War, for instance, the Khmer Rouge exploited timber in the Thai border region, which was relatively free from violence. The timber exploitation led to a fivefold increase in timber exports to its ally, Thailand (Le Billon and Springer 2007).

Given this possibility that civil war can cause deforestation in home fronts, the subnational analysis of the effect of violent events on deforestation cannot directly tell us the net effect of civil war. In fact, even when violent events cause reforestation in frontline regions as predicted by the migration mechanisms, if parties exploit timber in home fronts, the net effect can be deforestation. For instance, Burgess et al. (2015) compare the inside and outside of the Sierra Leone–Guinea border to make an inference about the effect of the Sierra Leonean Civil War on forest coverage. Although their identification strategy seems plausible, we cannot infer from the finding that the net effect of the civil war is reforestation; even if the border areas experienced reforestation, it is still possible that the nonborder areas experienced deforestation. Thus, even if their findings are internally valid, we cannot easily extend them to the national level or evaluate the net environmental costs of the civil war, which I believe has substantive importance.

Moreover, the subnational analysis also risks comparing treated units (battle zones) to other treated units (nonbattle zones) and hence making a misleading inference. For instance, although Burgess et al. (2015) compare the municipalities with and without violent events and find null results, this may not imply that the net effect of the Sierra Leonean Civil War is also null. In fact, if both the counterinsurgency and resource mobilization mechanisms are at work, civil war should result in deforestation across the country regardless of the presence of violent events. As a result, we should observe no difference in deforestation rates between battle and nonbattle zones (especially when the counterinsurgency and resource mobilization have similar effect sizes). The observed countrywide deforestation itself also does not provide credence to either of the mechanisms, since such a countrywide deforestation is indistinguishable from a generic country-level trend. In this article, I address these problems in previous studies by first estimating the net environmental costs of civil war at a country level and also providing preliminary evidence about forest changes at subnational levels.

RESEARCH DESIGN

Estimating the environmental costs of civil war raises empirical challenges; without a proper counterfactual (i.e., the forest coverage if it were not for civil war), we cannot validly evaluate the causal effect of civil war on forest environments. For instance, although field-level qualitative observations and GIS image descriptions can be useful, they are usually insufficient for making a causal claim. In fact, even when we see some forest degradation during a civil war, it does not mean that the civil war causes deforestation; without the civil war, people might have cut trees on an even larger scale. Thus, depending on the counterfactual scenarios that we assume, it is possible to draw different, or even opposite, causal claims from the same observations.

Previous quantitative analyses have attempted to address the counterfactual problem with difference-in-difference (DID) approaches. Although the DID can be useful when it is combined with a careful research design (see, e.g., Burgess et al. 2015), the approach crucially depends on the assumption that both areas would have experienced the same rate of deforestation if it were not for the war (the so-called parallel trend assumption). This assumption poses a challenge, especially when we analyze the deforestation at the country level. Even though the assumption can hold for certain subnational areas, such as the Sierra Leone–Guinea border regions (Burgess et al. 2015), the deforestation trajectories at a country level are likely to be different due in part to different conservation policies

2. Violent events are defined as "an incident where armed force was [used] by an organised actor against another organized actor, or against civilians . . . at a specific location and a specific date" (Sundberg, Lindgren, and Padskocimaite 2010, 2).

3. Civil war refers to "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties . . . results in at least 25 battle-related deaths in one calendar year" (Themner 2012, 1).

and timber consumption patterns. This makes it difficult to distinguish the effect of civil war from country-level deforestation trends.

An alternative approach might be a more model-driven method, such as the regression with a lagged outcome variable and fixed effects seen in Reuveny et al. (2010). Although the model-based approach might be useful for statistical inference, it has a problem of model-dependent extrapolation. Depending on how we model the temporal structure, the inference can be substantially different (Abadie et al. 2015). Since the deforestation trajectories are likely to be different across countries, it is extremely difficult to find a single correct specification of temporal dependency. The country-specific trend variables could potentially address this concern (Carey and Horiuchi 2017), but in practice this approach tends to impose relatively strong constraints on the functional form (Abadie et al. 2015).[4] The parametric approaches also tend to be less explicit about the counterfactuals (Abadie et al. 2015).

In addition, extant studies, including Reuveny et al. (2010), often use the Food and Agriculture Organization of the United Nations forest database, which relies on government self-reporting and is subject to nonresponses and biases. Although these reporting problems are widely acknowledged both in political and geographic sciences (Theisen 2008), the absence of the data that are available in a sufficient number of countries for a long time period makes it difficult to avoid the reporting problems. In this article, I address these empirical problems by applying a synthetic control method and a satellite-based measurement to a historically unique event in the DRC, the so-called Great War of Africa.

Case: The Great War of Africa in the DRC

I analyze the Great War of Africa for its unprecedentedly large scale and the uniqueness of the geographical characteristics.[5] The DRC, at the heart of sub-Saharan Africa, shares borders with 10 countries and has large tropical rainforests in the north as well as dry forests in the south. The country is endowed with abundant timber species including African mahogany (sapele), African teak (afrormosia), African cherry (makore), wenge, and ebony in the Congo Basin, which stretches from the north to the south of the country, and also beli and bloodwood in the Miombo woodlands in the southeast. The DRC is also known for rich mineral deposits, including gold, diamonds, copper, coltan, and cobalt.

The 1994 Rwandan genocide and the resultant refugees and exiles destabilized the DRC's long-time president, Mobutu Sese Seko, who was already facing waning support from Western countries.[6] The First Congo War broke out on October 24, 1996, when Laurent-Désiré Kabila led the Alliance of Democratic Forces for the Liberation of Congo-Zaire to overthrow the Mobutu regime. With the cooperation of Rwanda, Uganda, Angola, and Burundi, Kabila quickly established a new government in the following year, but peace was not brought to the DRC. The continued presence of the Rwandan forces strained the relationship between Kabila and his former foreign allies. On August 2, 1998, when the rebels, backed by the Rwandan Army, crossed the border into the DRC, the tension escalated into a war, known as the Second Congo War.

The invasion followed a spiral of escalation and international involvement. At least seven countries had a military presence in the DRC: Angola, Namibia, Zimbabwe, and Chad on the government's side and Rwanda, Uganda, and Burundi on the rebels' side. Both sides battled along the front line extending from the northwest to the east of the country. In the later phases of the war, the eastern border areas were also characterized by the fragmentation of the rebel groups and prevalence of insurgent violence. By the official end of the war in the 2002 Sun City Agreement and the following Global and All-Inclusive Agreement, the violence had resulted in a large loss of human life and economic collapse. The total death toll climbed to 3–5 million (the largest of any war since World War II; Lacina and Gleditsch 2005), over 2 million people were displaced, and per capita income plummeted from US $630 in 1980 to US$78–US$88 in 2002 (Nest, Grignon, and Kisangani 2006; Prunier 2008).

Measurement: Long-term forest coverage changes

In this study, the unit of analysis is country half months. The outcome and explanatory variables are forest coverage and the onset of the Great War of Africa, respectively. Following the standard definition of civil war "onset" (Sambanis 2004; Themner 2012) and a conventional reading of Congolese history (Prunier 2008), I consider the First and Second Congo Wars as a single sequence of armed conflicts, which I call the Great War of Africa. Following the Uppsala Conflict

4. In a later robustness check, I estimate a model of country fixed effects and country-specific trend variables.

5. The other possible cases are the civil wars in Angola, Liberia, and Sierra Leone, which all happened in countries with dense rainforests and entailed large numbers of refugees, enormous mobilization, and intense military confrontations. However, because my forest coverage measure is available only after 1987 and the synthetic control method requires a sufficient number of prewar observations, I cannot analyze these cases.

6. In a later section, I discuss that the Rwandan genocide cannot explain my findings.

Data Program Armed Conflict Dataset, I define its onset as October 24, 1996 (Themner 2012).

As a measure of long-term forest coverage changes, I propose data that are derived from satellite images available every half month from 1981 to 2014 for all tropical countries. The forest coverage index is based on the Advanced Very High Resolution Radiometer (AVHRR) satellite images. The AVHRR images are spatially coarse but cover a long time period, which makes them particularly useful for the panel data analysis at an aggregated level (Beck et al. 2011).[7] Among many indexes of the AVHRR images, one of the most widely used is the Normalized Difference Vegetation Index (NDVI).[8] The NDVI is the surface reflectance of near-infrared light, which leaf cells strongly reflect, minus visible light, which leaf pigments strongly absorb. The NDVI index is shown to highly correlate with actual forest coverage (Beck et al. 2011; Fensholt and Proud 2012). In particular, this study uses AVHRR NDVI images that are calibrated and reprojected by the Global Inventory Modeling and Mapping Studies (GIMMS). The GIMMS NDVI images are available globally at the spatial resolution of 8-by-8 kilometers (Pinzon and Tucker 2014).[9]

Because the NDVI series contains substantial noise and seasonal variation (du Plessis 1999; Hird and McDermid 2009), I deseason the series and apply a recursive Savitzky-Golay filter suggested by Chen et al. (2004).[10] The Savitzky-Golay filter is a simple extension of a moving average, and the algorithm is one of the most widely used in the literature on NDVI smoothing.[11] The recursive algorithm gives larger weights to values in a growing season, while the weights themselves are calculated from smoothed values in the previous iteration. Because the filter works poorly at the beginning and end of the time series, I drop those observations and hence limit the time period of the measurement to 1987–2008.[12]

In addition, since the NDVI values cannot be directly interpreted as forest coverage, I calculate the percentage of forest coverage predicted by the NDVI.[13] Specifically, I first estimate a regression of the AVHRR Tree Cover Continuous Fields (Defries et al. 2000), a global satellite-based image of the tree coverage percentage in the 1992–93 period, on the NDVI values in the same period.[14] The estimated regression model is used to predict the percentages of forest coverage.[15]

A caveat is that even though GIMMS makes corrections for sensor switches, orbital drifts, and atmospheric noise, there are still system-wide temporal fluctuations in the NDVI series. Because a decrease in the absolute values estimates means either an actual forest loss or a system-wide fluctuation, we need to be extremely careful about interpreting the absolute values. Thus, similar to other satellite-based data, my forest coverage estimates should be primarily used for comparative purposes with proper statistical methods.

Method: Synthetic control

The synthetic control method creates a baseline estimate, while accounting for both country-specific characteristics and time-varying trends (for details, see Abadie et al. 2010, 2015). Thus, unlike the methods in previous studies, the synthetic control can account for both time-invariant and time-variant confounders. In particular, the method matches observations by all past values of an outcome variable. The algorithm first assigns an optimal weight to each comparison country (a so-called control country), so that the weighted averages of the control countries are the closest to the observed forest coverage in the DRC for the entire prewar period. The trajectory of the weighted averages constitutes a baseline, or "synthetic," unit. If the observed and synthetic DRC are sufficiently similar in the prewar period, they should also not be different in terms of time-varying confounders; otherwise, the forest coverage would have been different even before the Great War of Africa. Thus, any differences after the war should be attributed to some event that happened in the second half of October 1996, which is most likely the Great War of Africa. In contrast, if the trajectories are different even before the war, the synthetic DRC is systematically different from the observed DRC so that we cannot

7. For details of the AVHRR and other satellite images, see app. 2, "Data Source."

8. For details, see app. 2, "Data Source," and Warner, Foody, and Nellis (2009).

9. For details, see app. 2, "Data Source," and Beck et al. (2011).

10. For details of the Savitzky-Golay filter, refer to Savitzky and Golay (1964). For details of the recursive algorithm, see app. 2, "Smoothing," and Chen et al. (2004).

11. For a review of the literature, refer to Hird and McDermid (2009) and Pettorelli et al. (2005).

12. For details about the smoothing, see app. 2, "Smoothing."

13. In a robustness check, I conduct analyses without the transformation.

14. The AVHRR Tree Cover Continuous Fields are available only for the 1992–93 period. For details on the transformation, see app. 2, "Transformation and Aggregation."

15. I compare the forest coverage data to the Moderate Resolution Imaging Spectroradiometer (MODIS) Vegetation Continuous Fields in 2000 (Sexton et al. 2013), another standard satellite-based image of the tree cover percentage after the 2000s. The two indexes have a very high correlation of 0.826, indicating the validity of my measurement. See app. 2, "Validation." Note also that this measurement is based on predicted values, and hence, rigorously speaking, the uncertainties in the measurement need to be incorporated into the later statistical analysis. As I will detail later, however, the statistical inference of the synthetic control method is based on placebo tests, and incorporating the measurement uncertainties into the placebo tests with methodological rigor is beyond the scope of this article, so I leave it for future studies.

draw a valid causal inference. In this way, the synthetic control method not only allows us to make a more rigorous causal inference, but the method also makes it possible to evaluate the plausibility of the core assumption.

Strictly speaking, if some events that are independent or causes of the Great War of Africa would have affected the Congolese forests at the same time of the civil war, the synthetic control analysis could not isolate the effect of the war from those of the confounding events. However, I find no such confounding event. The *Africa Research Bulletin*, an academic periodical based on hundreds of international and local sources, does not mention any independent or causal events that affected the Congolese forests in the time period. In addition, the Rwandan genocide and the resultant refugees to the DRC cannot explain the findings. By 1998, the inflow and outflow of refugees between the DRC and Rwanda were balanced, and after 1999, more refugees went from the DRC to Rwanda (UNHCR 2000). Thus, the refugee inflows and the increased human pressures on forests cannot explain deforestation in the DRC after 1998. Finally, there is no record of a large-scale wildfire or drought.

The synthetic control method requires control countries to be comparable to the DRC (Abadie et al. 2015, 500). To select appropriate control countries, I use two criteria: (1) the total area of tropical climate zones comprises more than three-quarters of the country; (2) the total area of the country is more than 64,000 kilometers squared (1,000 NDVI cells). The first condition keeps the treated and control units comparable.[16] The second condition reduces the noise, as forest coverage in small countries is sensitive to minor errors and exogenous events. In total, the control pool consists of 22 countries: Bangladesh, Brazil, Cameroon, Central African Republic (CAR), Colombia, Cote d'Ivoire, Cuba, Gabon, Ghana, Guinea, Guyana, Indonesia, Liberia, Malaysia, Nigeria, Philippines, Republic of the Congo (Congo), Suriname, Tanzania, Thailand, Uganda, and Venezuela.[17]

A potential problem in the synthetic control analysis is that if parties cut forests in order to prepare for the Great War of Africa, the prewar period would not constitute a clean baseline. However, the First Congo War ended after six months with the decisive victory of Kabila, and people could hardly foresee the escalation to the devastating Second Congo War. Thus, I assume that before the First Congo War, parties did not mobilize forest resources or destroy forests in anticipation of the future wars. To the best of my knowledge, there is no field report that observes such anticipatory behaviors. Moreover, if parties would have cut trees before 1996, there should be a gap between the observed and synthetic DRC before October 1996, which is not consistent with my later findings.

Finally, in order to address the concerns of overfitting (the synthetic DRC might overfit to the observed DRC in the pretreatment period, making the results highly sensitive to small errors), I apply the cross-validation technique suggested by Abadie et al. (2015). I divide the data points into a *training period* from the beginning of the data to the first half of October 1991, a *validation period* from the second half of October 1991 to the first half of October 1996, and a *posttreatment period* for 10 years after the treatment. Using predictors averaged over the training period, I first calculate the weights of predictors that minimize the mean square predictor error (MSPE) over the validation period. Then, using the weights and the predictors averaged over the whole pretreatment period, I construct the synthetic DRC. Although the cross-validation technique requires many data points over time, my forest coverage index is available twice a month, and hence I have over 110 data points in the pretreatment period.

In the analysis, I also include a standard set of covariates that are used in previous studies (Combes Motel, Pirard, and Combes 2009; Culas 2007). The predictors are gross domestic product per capita, population density (World Bank 2015), democracy index (Marshall 2013), the proportion of croplands (FAO 2015), proportion of protected areas (UNEP, WCMC, and IUCN 2014), average temperature, and precipitation (UEACRU 2014). Following convention (Abadie et al. 2015), I also include the prewar outcome variable as a predictor. All of the predictors are averaged over the pretreatment period.

Since the synthetic control method is nonparametric (i.e., it makes no assumption about the distribution of the outcome variable), I present the estimates graphically as the differences between the observed and synthetic DRCs. Intuitively, the synthetic DRC is the forest coverage change if it had not been for the Great War of Africa. Thus, if the method succeeds to construct an appropriate synthetic DRC, its trajectory should overlap with that of the observed DRC in the prewar period. Otherwise, the observed and synthetic DRC may systematically differ, and the systematic difference may explain forest changes after the war. In contrast, if the prewar trajectories closely overlap and the postwar trajectories substantially differ, the cause of the difference should be attributed to the Great War of Africa. If the postwar forest

16. All countries that receive positive weights in the synthetic control analysis are well above and hence insensitive to these criteria.

17. Cambodia was dropped because of missing values in its predictors. Later, in order to address potential spatial interference among countries, I also drop all neighboring countries and rerun the analysis (see "Robustness checks" subsection).

coverage of the observed DRC is lower (higher) than that of the synthetic DRC, it indicates that the effect of the Great War of Africa is deforestation (reforestation).

RESULTS: THE ENVIRONMENTAL COSTS OF THE GREAT WAR OF AFRICA

Figure 1 shows the main result of the analysis, presenting the trajectories of the forest coverage in the observed and synthetic DRC over time. Figure 1*B* shows the difference in forest coverage between the observed and synthetic DRC. The left white area is the training period, the gray area is the validation period, and the right white area is the posttreatment period. In the pretreatment period, the two trajectories closely overlap, suggesting the synthetic control estimator successfully constructs a valid counterfactual unit.[18] In contrast, the observed DRC departed from the synthetic DRC after the war. The outcome values of the observed DRC are lower than those of the synthetic DRC, suggesting that the war led to deforestation.

For the five years following the onset of the Great War of Africa, the average difference is 1.61% of the country.[19] This implies that the environmental costs of the Great War of Africa were the loss of 37,754 square kilometers of forest coverage, which is even larger than the entire territory of Belgium or nearly half of Sierra Leone, on average annually over those five years. According to an estimate (Galford et al. 2015), this amounts to the increase of over 36 million tons of CO_2 in a year, which is equivalent to the annual emissions from over 8 million typical automobiles (EPA 2017). Moreover, given an estimate about the effect of deforestation on flood risks (Bradshaw et al. 2007), the war-driven deforestation can increase the annual flood frequency by 0.56 to 4.52%. Although this corresponds to an increase of less than one incidence of flooding per year in the DRC, and the effect of the deforestation on flood-related deaths is still under debate, it does not deny the possibility that war-led deforestation might beget secondary disasters, which in turn would further degrade the environmental, security, and economic conditions of a conflict country.

The observed and synthetic DRC tend to diverge until late 2004, with a small bump in 2002 (fig. 1*B*). Although the Great War of Africa formally ended on April 19, 2002, the peace agreement was largely due to “a military stalemate rather than any kind of genuine desire for ‘peace’” (Prunier 2008, 305). Thus, even after the agreement, the parties had incentives to continue resource mobilization or tactical destruction of forests until they could be confident that peace would last. Although it is hard to determine such a moment, qualitative studies indicate that this happened sometime between late 2004 and December 10, 2005, when the new constitution was passed in a referendum (Stearns 2012) and “the ghost of the transition began to gain substance” (Prunier 2008, 303). This period corresponds to the dates on which the trends of the observed and synthetic DRC in figure 1 start converging.

The synthetic unit is composed of three countries: Suriname (0.71), Uganda (0.21), and the CAR (0.08).[20] Nearly three-quarters of the weights are assigned to Suriname, while Uganda and the CAR share the remaining weights. This means that these three countries are sufficient to create a synthetic unit and that the remaining 19 countries are actually redundant. Suriname has as high a forest coverage as the DRC, which makes it an appropriate basis for comparison. In addition, Suriname is dependent on mineral resources and located at similar latitudes, while the country is free from civil war after democratization in 1991 (Hoefte 2013). In contrast, Uganda and the CAR are conflict-prone countries and share political characteristics with the DRC. Although one may worry that the estimate relies on a too few countries, this is common in most applications of synthetic control and is even advantageous, as the estimate is less reliant on dissimilar units.[21]

Placebo tests

Because the synthetic control method is nonparametric, a standard statistical test is a permutation or so-called placebo test. In an in-space placebo test, I assign the treatment to each control unit as if it had experienced the Great War of Africa.[22] Because no country other than the DRC experienced such a large-scale war in reality, the treatment effect in the DRC should be larger than those in the placebo cases. I use the ratio of the MSPEs in the posttreatment periods to those in the validation period as a test statistic (Abadie et al. 2015). Intuitively, the MSPE represents the average squared difference between the solid and dashed lines in figure 1. When the estimate is precise (small pretreatment MSPE) and the treatment effect is large (large posttreatment MSPE), the MSPE ratio is large.

18. Note that because the NDVI has system-wide temporal trends, we should look at the differences between the observed and synthetic DRCs instead of their own values.

19. This is the average difference between the observed and synthetic DRC for the 1996–2001 period.

20. The weights are in parentheses.

21. For instance, Abadie et al. (2010, 2015) assign positive weights to 5 out of 38 units and 5 out of 16 units, respectively. Later, I also conduct a robustness check by dropping each of the three countries.

22. I also conduct in-time placebo tests. See fig. A4.1.

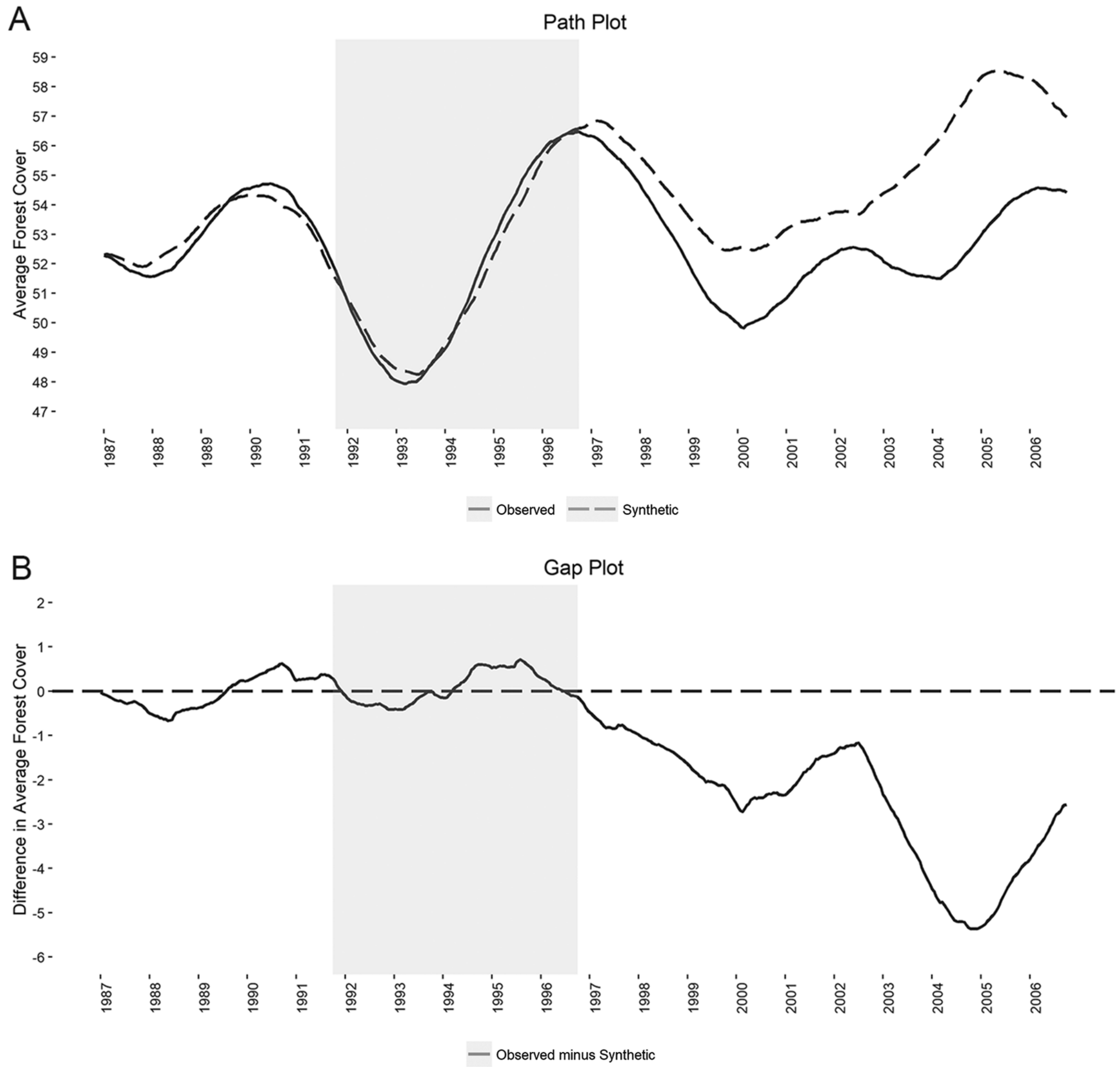

Figure 1. Effect of the Great War of Africa on forest coverage in the DRC. *A*, Forest coverage of the observed and synthetic DRC over the time period; *B*, their differences. Because the forest coverage estimates are subject to system-wide temporal fluctuations, we should look at the differences between the observed and synthetic units instead of their absolute values. First white area is the training period, and the gray region corresponds to the validation period, with which the predictor weights are optimized. Last white area is the posttreatment period.

Figure 2 shows the results of the in-space placebo tests.[23] The horizontal axis shows the posttreatment MSPE divided by the pretreatment MSPE. I also add the pretreatment MSPE values on the right side. As seen in the graph, the MSPE ratio of the Great War of Africa is larger than those of the placebos. Because there are 23 cases, the "p-value" is $1/23 = .043 < .05$.[24] Several countries, including the CAR, Philippines, and Colombia, have reasonably small MSPEs in the pretreatment period, but the posttreatment MSPEs are small as well. This indicates that the effect of the Great War of Africa is indeed statistically significant.

Causal mechanisms

Although this article is not intended to identify specific causal mechanisms, it is useful to consider possible approaches to analyze the underlying mechanisms. In particular, the counterinsurgency explanation expects that government forces destroy forests to eliminate insurgents' hideouts. From this perspective, we should observe more intense deforestation in areas of possible violent confrontations. By contrast, if parties exploit timber in their resource mobilization efforts, they are unlikely to cut trees in frontline regions. Given the abundance

23. See app. 3 for the gap plot for each placebo case.

24. The p-value in a permutation test is conceptually different from a conventional p-value.

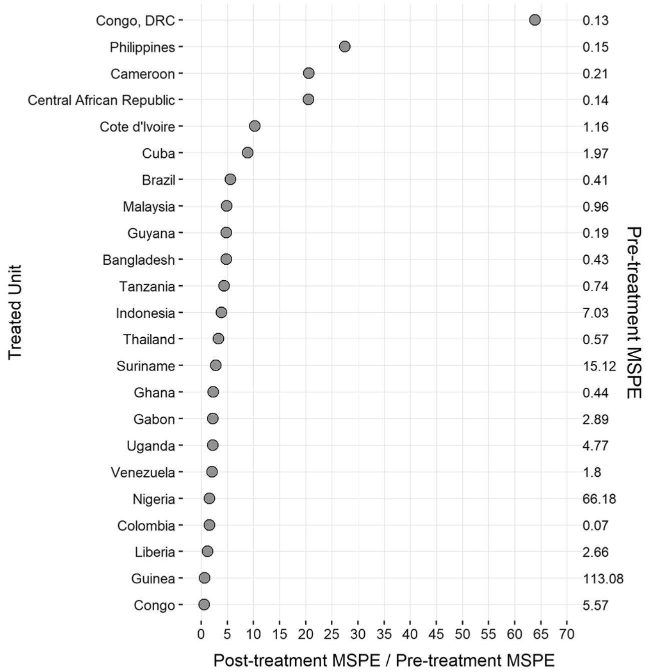

Figure 2. In-space placebo tests. *X*-axis, mean square prediction error (MSPE) in the posttreatment period (five years after the Great War of Africa) divided by the MSPE in the validation period (five years before the Great War of Africa). A larger value on the axis means a large difference between the observed and synthetic DRCs in the posttreatment period relative to the pretreatment period. *Right*, pretreatment MSPEs. A lower pretreatment MSPE indicates that the observed and synthetic units are similar in the pretreatment period, and hence the estimate is precise.

of forests in the DRC, it is safer and more productive to cut trees in the regions free from violence. Thus, while the counterinsurgency perspective expects deforestation in war zones, the resource mobilization approach predicts deforestation in nonbattle zones.

Note that a subnational analysis is insufficient for properly testing these hypotheses. In both accounts, the counterfactuals are the absence of civil war; battle and nonbattle zones are predicted to experience higher rates of deforestation than if it were not for the civil war at the national level. As far as civil war affects both battle and nonbattle zones, both are "treated" observations and thus cannot be counterfactuals of each other. For instance, even when there is no difference in the forest changes between battle and nonbattle zones, the result can indicate either (i) neither counterinsurgency nor resource mobilization mechanism works or (ii) both of the mechanisms are at work. Given this possibility, we need to create proper counterfactuals from countries that are not affected by civil war.

To this end, I split the DRC into battle and nonbattle zones, calculate their forest coverage, and create a synthetic unit for each zone from the control countries.[25] Admittedly, this is a rather informal test, as I do not properly account for potential interactions between battle and nonbattle zones. In addition, the analysis relies on a somewhat uneasy comparison between subnational units and other countries. Thus, the results should be taken with caution and considered as only preliminary evidence for the causal mechanisms. I leave it to future studies to develop methods that can create theoretically appropriate counterfactuals while accounting for possible interference between battle and nonbattle zones.

Figure 3 shows that both battle and nonbattle zones experienced deforestation to some extent. While the effect on the battle zone is statistically significant at a 0.1 threshold ($p = 2/23 = .09$), the effect on the nonbattle zone has a larger p-value ($p = 5/23 = .22$), which is probably due to the worse fit in the pretreatment period. Given the similarity in the effect sizes, however, the null results should not be interpreted as no effect. Thus, while the quantitative analysis provides some support for the counterinsurgency mechanism, we cannot draw a definite inference about the resource mobilization mechanism.

By contrast, qualitative studies provide some support for the resource mobilization explanation, while I do not find reports or witnesses that are consistent with the counterinsurgency mechanism. For instance, Baker et al. (2003) visited the DRC in March 2003 and reported that "active logging is taking place in southern Equatéur and Bandundu province" (22), both of which were under the government's firm control. Other sources also indicate government logging activities in Katanga province (*Africa Confidential* 2000; Global Witness 2001; UNSC 2001a) and rebels' timber exploitation in the eastern border areas (UNSC 2001a). There are also reports of war-related mining activities and resultant clearance of rainforests, especially in the eastern provinces (Butsic et al. 2015; Institute for Environmental Security 2008). Thus, using both quantitative and qualitative evidence, I conjecture that both resource mobilization and counterinsurgency mechanisms exist to some extent. However, given the indeterminate results and the limitation of qualitative sources, I hesitate to draw a definite conclusion.

Robustness checks

Finally, I conduct robustness checks. First, although the forest coverage index rests on the recursive Savitzky-Golay filter, which requires a specific value for a smoothing parameter, the

25. See app. 5 for the detail of the battle zone data. Both the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED) Polygon and the Peace Research Institute Oslo Grid (PRIO-GRID) are too inclusive and thus cannot be used in the present analysis.

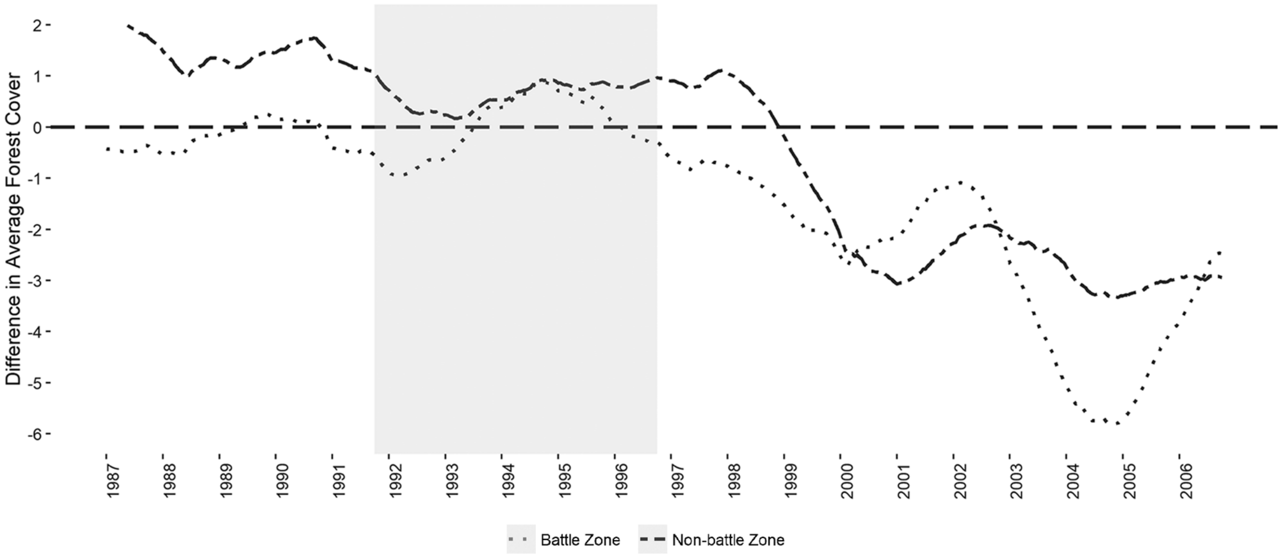

Figure 3. Effects on the battle and nonbattle zones: differences between the forest coverage of the observed and synthetic DRC over the time period. *Dot-dashed line*, differences between the observed forest coverage in the nonbattle zones and the corresponding values of the synthetic unit. *Dotted line*, differences between the observed forest coverage in the battle zones and the corresponding values of the synthetic unit. First white area is the training period, and the gray region corresponds to the validation period, with which the predictor weights are optimized. Last white area is the posttreatment period.

results are consistent with the main finding and the nature of the outcome variable (see fig. A6.1). Second, while I transformed the raw NDVI values to percentages of forest coverage, even without the transformation, the synthetic control estimates are similar to those in figure 1 (see fig. A7.1). Third, I conduct leave-one-out tests, in which I repeat the synthetic control analyses, removing each of the control units that have positive weights (Suriname, Uganda, and the CAR). Although the effect sizes are generally similar to the main finding, removing a unit of the largest or second-largest weight (Suriname or Uganda) results in lower statistical significance. This indicates that Suriname (which usually does not receive much attention in conflict studies) and Uganda (which retains geographically and environmentally similar characteristics) are actually essential for constructing a valid baseline estimate (see fig. A8.1).[26] Fourth, I drop all neighboring countries (the CAR, Uganda, Tanzania, and Congo) at once in order to address potential spatial interferences. Although this entails the omission of Uganda and hence lowers the statistical significance, the effect sizes remain similar (see fig. A8.2). Fifth, I incorporate recent critiques and refinements of the synthetic control methods, including the generalized synthetic control (Xu 2017), a synthetic control analysis with all pretreatment lags but without any other covariates (Ferman, Pinto, and Possebom 2018), an analysis without the two-step validation procedure (Klößner et al. 2018), and a sensitivity analysis proposed by Firpo and Possebom (2018; see apps. 9–12).[27] Overall, the results are quite robust to these changes. Sixth, I estimate parametric regressions with country fixed effects and country-specific trend variables. The results are similar to the synthetic control estimate (see table A13.1).

CONCLUSION

In this article, I have conducted one of the first systematic evaluations of the environmental consequences of civil war. A theoretical implication of this study is that the adverse effects of natural resources, commonly called the "resource curse" (Ross 2004), are only one side of the story; the issue of "resource curse reversed" requires further attention. Although a few studies have examined this possibility in the contexts of political institution and economic development (Tyburski 2012), our focus is still largely restricted to the effects of, not on, natural resources in studies of civil war. Nevertheless, even though forest resources may be less relevant as causes of civil war (Rustad et al. 2008; Theisen 2008), once a civil war has happened, warring parties have

26. The sensitivity to a unit of a large weight is, unfortunately, a common problem with the synthetic control method. For instance, the estimate of Abadie et al. (2015) is sensitive to the omission of the United States, which has the second-largest weight.

27. There are other working papers, such as Doudchenko and Imbens (2016) and Imai, Kim, and Wang (2019). I recommend that readers refer to those studies.

strategic incentives to cut trees. A similar logic can potentially be applied to the effects of civil wars on other natural resources, including mineral, soil, animal, and fishery resources.

It is important for future studies to integrate the subnational and cross-national analyses of war-related deforestation. Since civil war can affect both battle and nonbattle zones, a narrow focus on the subnational variation risks comparing a treated unit to another treated unit. However, as I have suggested in the analysis, the subnational variation is useful, and perhaps necessary, to conduct a more detailed analysis of the causal mechanisms. This creates a methodological problem: If we could not validly compare units within a conflict country, to which unit should they be compared? Although my preliminary answer is other countries, this may or may not be appropriate. It is a task for future studies to address the counterfactual problem and provide new insights about the causal mechanisms.

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