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The economic costs of civil war: Synthetic counterfactual evidence and the effects of ethnic fractionalization

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Abstract

There is a consensus that civil wars entail enormous economic costs, but there is little systematic analysis of the determinants of their heterogeneous destructiveness. Moreover, reliably estimating these costs has proven challenging, due to the complexity of the relationship between violence and socio-economic conditions. In this article, we study the effect of ethnic fractionalization of war-torn countries on the economic consequences of civil war. Building on an emerging literature on the relationships between ethnicity, trust, economic outcomes, and conflict processes, we argue that civil wars erode interethnic trust and highly fractionalized societies pay an especially high price, as they rely heavily on interethnic business relations. We use the synthetic control method to construct appropriate counterfactuals and measure the economic impact of civil war. Our focus is on the years of armed conflict in a sample of 20 countries for which we observe an average annual loss of local GDP per capita of 17.5%, though with remarkable variation across cases. The empirical analysis provides supporting evidence in the form of a robust positive association between ethnic fractionalization and our measures of war-induced economic costs.

Keywords

costs of civil war, ethnic fractionalization, synthetic control method

Introduction

Observers, participants, and victims generally agree that civil wars cause enormous economic damage. However, there is a dearth of systematic analysis of the determinants of the variation in civil war's destructiveness and thus we know little about underlying mechanisms. This is perhaps unsurprising as we also lack reliable estimates of the costs of civil war, due to the complexity of the nexus between violence and socio-economic conditions as well as measurement and aggregation challenges. This article studies how a country's ethnic heterogeneity affects the economic impact of civil war. The latter is an unobserved entity that we estimate with the synthetic control method (SCM) as the yearly distance (gap) between the country's observed GDP per capita and its constructed counterfactual in the absence of war.

A large body of literature explores the effects of politicized ethnic identities and ethnic demography on the

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risk of civil wars (Montalvo & Reynal-Querol, 2005; Cederman, Wimmer & Min, 2010), their duration (Wucherpfennig et al., 2012) and conflict dynamics, like violence severity (Costalli & Moro, 2012). However, scholars have paid little attention to the impact of ethnic heterogeneity on the economic effects of civil wars. Our argument about the relationship between ethnic fractionalization and destructiveness of civil war builds on the existing findings that interpersonal trust is a key enabler of economic activity (Zak & Knack, 2001; Algan & Cahuc, 2010) and that civil war erodes interethnic trust, while leaving intra-ethnic trust unaltered or even bolstering it (Dercon & Gutiérrez-Romero, 2012; Bauer et al., 2014). Highly fractionalized societies should pay an especially high economic price for the destruction of interethnic trust, as on average they rely heavily on interethnic business relations, which violence tends to undermine. Moreover, we expect the detrimental effects of ethnic fractionalization to intensify as the number of war casualties increases, given that more and more individuals and communities are exposed to the trust-eroding effects of violence.

We test this hypothesis on a sample of 20 countries, for which we find that civil war causes an average annual loss of 17.5% of GDP per capita in the years of armed conflict.[1] With a series of panel regressions, we provide consistent evidence of a robust negative association between ethnic fractionalization and our measure of war-induced GDP gap (i.e. the more fractionalized a country, the larger its economic losses tend to be). In keeping with our theoretical expectations, we also find that the negative impact of fractionalization increases with the intensity of conflict.

The use of SCM for assessing the economic effects of civil wars is not a mere exercise in causal inference virtuosity, as accurate measures are necessary for systematic analyses of their determinants. These in turn can help policymakers efficiently allocate scarce resources for conflict prevention and post-war recovery where they could have the largest returns.[2]

[1] Note that, as we are interested in studying the effects of a country's ethnic fractionalization, our dependent variable measures the impact of civil war on the national economy, not likely negative international spillovers. Thus, our estimates of the economic costs of the civil wars in these 20 countries could plausibly be interpreted as a lower bound of their *global* costs.

[2] Concerns about cost–benefit ratios of various development initiatives (including conflict prevention and post-conflict reconstruction) have been at the heart of recent debates on the post-2015 development agenda (High-Level Panel of Eminent Persons on the Post-2015 Development Agenda, 2013; Hoeffler & Fearon, 2014).

The article proceeds as follows. In the next section, we discuss the limits of the existing empirical literature on the impact of armed conflict on the national economy of conflict-torn countries, detail the advantages and limitations of our method (SCM), and present our dependent variable – the yearly gap between the observed GDP per capita and its estimated counterfactual in the absence of war for each country in our sample. In the subsequent section, we put forth our theoretical argument about the effect of ethnic fractionalization on civil war-induced economic losses and report the corresponding empirical results. We then present a set of robustness checks and conclude by summarizing our findings.

The economic costs of civil war: Measuring the dependent variable

War is about killing people and breaking things. It is thus unsurprising that civil wars entail significant economic costs. At the most basic level, internal armed conflict leads to the depletion of a country's stock of productive factors – labor, human and physical capital – through the killing, maiming, and displacing of individuals as well as the destruction of infrastructure, productive equipment, and household assets. Moreover, in wartime public resources are diverted from productive activities and social services to fighting; financial and human capital flee the conflict-ridden country; opportunism and distrust increase as individuals experience violence and time horizons shorten; and a shift occurs away from war-vulnerable economic activities (e.g. construction, finance, and manufacturing) towards activities that are less vulnerable, but also less productive, such as subsistence agriculture (Collier, 1999; Koubi, 2005; Gates et al., 2012; Mueller, 2013).

After discussing the main methodological challenges associated with measuring the economic effects of civil war, we present our country-specific, time-varying measure. This will serve as dependent variable for our analysis of the role of ethnic fractionalization.

Methodological challenges

While the observation that civil wars entail major economic costs is not controversial, reliably estimating those costs has proven challenging due to endogeneity. As Blattman & Miguel (2010: 39) note, 'assessing the economic consequences of civil war is complicated by a central identification problem: war-torn countries are different than peaceful ones' – that is, unobserved factors

could drive both economic performance and conflict. Moreover, economic development and civil war are linked in a circular relationship: violence negatively affects the economy and poor economic conditions in turn increase the risk of civil war (Fearon & Laitin, 2003; Miguel, Satyanath & Sergenti, 2004). Thus, wartime economic decline could reflect the deteriorating economic situation that contributed to cause conflict, in addition to being its consequence. Put differently, assessing the costs of civil war requires identifying a counterfactual – that is, answering the difficult question of what a country's GDP per capita would have been had it not experienced war.[3]

The existing literature offers important contributions but does not satisfactorily address the challenges involved in finding appropriate counterfactuals. Case study analyses tend to compare prewar and wartime economic conditions or conflict-affected countries' growth trajectories with neighbors' and regional averages (Stewart et al., 2001). However, these are not necessarily appropriate counterfactuals as the assumption that, in the absence of armed conflict, a country's economy would have performed as in the past or similarly to a peaceful neighbor's may not be warranted.[4] Quantitative works on the economic effects of civil conflict often rely on country-fixed effects (Collier, 1999; Hoeffler & Reynal-Querol, 2003; Gates et al., 2012), which cannot fully tackle endogeneity, given the presence of time-varying unobservable confounders. Some micro-level empirical studies creatively address endogeneity (Miguel & Roland, 2011). However, as Blattman & Miguel (2010: 41) observe, the limitation of subnational studies is that they may underestimate the country-wide effects of war if even largely peaceful areas are adversely affected by war-related disruptions.

To obtain a country-level time-varying measure of the economic impact of civil war, we use SCM, which allows us to create data-driven counterfactuals for 20 war-torn countries. Abadie & Gardeazabal (2003) first applied this method to study the economic effects of terrorism in the Basque country, while Dorsett (2013) and Bilgel & Karahasan (2015) recently used it to investigate the economic impact of terrorism in Northern Ireland and Turkey, respectively.[5] As social scientists have increasingly employed SCM, in the next section we present the basic intuition underlying the method and the details of our specific application, while referring to Abadie, Diamond & Hainmueller (2010), Pinotti (2015), and our Online appendix for a formal presentation.

The synthetic control method

We measure the economic impact of civil war by constructing a counterfactual of the path of the GDP per capita for each of the conflict-ridden countries in our sample (i.e. an estimate of the GDP per capita in the years in which the conflict took place, had it not occurred). As other impact evaluation methodologies, SCM compares the outcome of a treated country against that of control units. The control unit is called 'synthetic' because it is a weighted combination of a sample of control countries, which are not exposed to the treatment (i.e. they are at peace). Taking into account predictors of the outcome variable, the weights assigned to each control country are selected by an algorithm to minimize the pre-treatment differences between the country of interest and its synthetic counterpart. As Abadie, Diamond & Heinmueller (2010) show, if there are no appreciable differences in the pre-treatment characteristics and evolution of the outcome variable between the treated unit and the synthetic control, and the pre-treatment period is sufficiently long compared to the treatment period, the outcome for the synthetic country in the treatment period represents an unbiased estimation of the counterfactual for the treated country. Thus the difference (gap) between the outcome variable of the treated unit and the synthetic control in the treatment period is an unbiased estimation of the treatment effect.

This method can deal with omitted variable bias by accounting for the presence of time-varying unobservable confounders, while fixed effects and difference-in-differences can account only for confounders that are time-invariant or share a common trend, respectively (Billmeier & Nannicini, 2013).[6] More specifically, SCM can also address the endogeneity concern that an underlying trend of economic decline in the country under

[3] Gates et al. (2012: 1715) and Smith (2014) stress the importance of (and challenges involved in) finding counterfactuals to assess the developmental impact of civil conflict.

[4] The same problem plagues Chen, Loayza & Reynal-Querol's (2008) study, which juxtaposes war-torn countries' growth trajectories with those of neighbors and peaceful developing countries.

[5] In recent working papers, Gardeazabal & Vega-Bayo (2015) and Bove, Elia & Smith (2014) use SCM to assess the economic impact of civil war.

[6] See Bove, Elia & Smith (2014) for a discussion of the differences between panel fixed effects and SCM as applied to the economic effects of civil war.

examination could be causing both the economic effect that we observe during the conflict and the conflict itself: a good pre-treatment fit would account for any such trend, thus increasing our confidence that the difference in the outcome of interest in the treatment period can be attributed to the conflict.[7]

An additional benefit of SCM, as Abadie, Diamond & Hainmueller (2010) observe, is that it helps to create the bridge between qualitative and quantitative methods advocated by several scholars (e.g. Tarrow, 1995). In fact, SCM allows us to employ a comparative case study research design, conducive to a more detailed description and analysis of the differences between the cases of interest and the comparison units than regression analysis, while preserving the benefit of precise numerical results comparable across cases. In addition, unlike other statistical tools for impact evaluation (including most forms of matching techniques), the method enables us to assess the dynamic effects of the treatment, that is, to explore the evolution of the war-induced GDP gap over time for each country, instead of simply focusing on an average treatment effect. SCM also precludes extreme counterfactuals produced when researchers extrapolate the estimated effects from their empirical models outside the support of the data (King & Zeng, 2006).

Sample and data

The advantages of SCM just highlighted come with some limitations. Constructing reliable counterfactuals requires a good pre-treatment match between treated country and synthetic counterfactual (Abadie, Diamond & Heinmueller, 2010: 495; Cavallo et al., 2013: 1555). To satisfy this requirement, and to address missing data problems (pervasive in the context of low-income, war-torn countries), we adopt straightforward and transparent inclusion criteria, which result in a sample of 20 countries experiencing a civil war between 1970 and 2008,[8] drawn from the list of conflicts assembled by Kalyvas & Balcells (2010).[9]

The Penn World Tables version 7.0 (PWT; Heston, Summers & Aten, 2011) is our source for economic data. We use GDP per capita (in purchasing power parity, PPP) as the outcome variable.[10] We include predictors commonly used in the growth literature: lagged

[7] If civil war is triggered by economic agents' anticipation of future economic decline that is not captured in the unobserved heterogeneity included in the model, there could be reverse causality bias (Billmeier & Nannicini, 2013). However, existing theoretical arguments do not support this kind of dynamic, as they envision observed (rather than anticipated) poor economic performance as affecting conflict risk by weakening state capacity and/or lowering the opportunity cost of fighting for individuals. A more plausible anticipation dynamic would occur if expectations of conflict outbreak prompt actions that negatively affect the economy before fighting erupts (e.g. disinvestment). These effects should be attributed to the subsequent eruption of conflict but would not be reflected in the treatment effect. Therefore, our estimated effects are likely to be an underestimation of the actual effect.

[8] Our criteria are the following. (i) Data availability constrains our sample because SCM requires a sufficiently long pre-treatment period with no missing values in the outcome series for the entire period of analysis and at least one observation for each of the covariates in the pre-treatment period. Other things being equal, a good fit for a longer pre-treatment period increases confidence on the post-treatment projection of the synthetic unit's outcome. Thus, if a war lasts more than ten years, we require that the pre-treatment GDP series be at least as long as the treatment period. For wars lasting less than ten years, we require a pre-treatment period of at least ten years (we apply these criteria with some flexibility, allowing for a two-year 'grace period' around the thresholds, so as not to lose interesting cases that almost matched the criteria). (ii) To examine the effects of the treatment over an appreciable period, we require at least three years of ongoing conflict. (iii) As very poor countries display extremely volatile paths of GDP per capita over time, making it more difficult to construct good synthetic counterfactuals, we require a minimum average GDP per capita of $2 (PPP) per day in the pre-treatment period for each treated country. (iv) Due to similar concerns of substantial GDP volatility, we exclude countries that experienced dramatic currency devaluations. (v) To avoid capturing the effect of a different form of political violence, the treated country must not have been involved in an international war in the five years before the outbreak of the civil conflict. However, we do not exclude countries that during the treatment period experienced international war lasting no more than one year and causing less than 1,000 battle-related deaths (Gleditsch et al., 2002). Including cases in which international conflict is likely to have had a minor impact compared to the civil war allows us to maximize the size of our treatment group. (vi) We require the pre-treatment analysis to start after 1960 to reduce the number of missing values and thus ensure a large number of countries in the control sample.

[9] We use the list from Kalyvas & Balcells (2010) because the relatively high threshold of combat casualties for inclusion in their dataset ensures that our analysis focuses on clearcut instances of large-scale fighting, which one would expect to have a noticeable impact on the national economy. The full list of definitional criteria in the Kalyvas & Balcells (2010) dataset is: (i) more than 1,000 war-related deaths during the entire war and in at least one single year of it; (ii) the war challenged the sovereignty of an internationally recognized state; (iii) it occurred within the territory of that state; (iv) the state was one of the principal combatants; and (v) the challengers mounted an organized military opposition.

[10] Official GDP data for poor, conflict-ridden countries are far from perfect (Henderson, Storeygard & Weil, 2012). This has prompted several scholars to use nightlight information to measure the economic effects of armed conflict in contexts of low-quality GDP data (e.g. Rohner, Thoening & Zilibotti, 2013a; Lopes da Fonseca & Baskaran, 2015). However, nightlight data are not a viable alternative for our purposes as they are available only from the early 1990s on,

GDP per capita (year by year, as, for instance, in Billmeier & Nannicini, 2013 and Bilgel & Karahasan, 2015), the pre-treatment average of the investment share, trade openness, and population growth rates (all from PWT), and the level of secondary school enrollment from the World Development Indicators (World Bank, 2014) as a measure of education. These variables enter the algorithm for the choice of the weights to be assigned to each country in the control pool (see Abadie, Diamond & Heinmueller, 2010).[11] Our pool of potential control units includes countries that did not experience civil and international wars during the period of analysis and for which macroeconomic data from the PWT are available.[12]

Estimated economic effects of civil war

As Table I shows, the average annual per capita GDP gap induced by civil war for the 20 countries in our sample[13] is –17.5% during the years of war (the average war duration is 9.5 years).[14] This estimate is in the same ballpark as Paul Collier's (1999: 175–176) oft-cited finding that a 15-year civil war would reduce a country's GDP per capita by about 30% on average. Collier also finds that civil wars tend to reduce annual economic growth by 2.2%, which is roughly comparable to our corresponding estimate of about 1.5% (calculated as the average across countries of the average annual gap in GDP growth between the actual country and its synthetic match during war). Similarly, in a more recent study, Mueller (2012) finds a persistent loss of roughly 18% of GDP per capita caused by ongoing civil wars.

Figure 1 shows that the economic effects of civil war vary substantially across cases and over time. This heterogeneity is not necessarily surprising, but our

Table I. Impact of civil war on GDP per capita

Country	*Percentage effect*	*Ethnic fractionalization*
	(1)	*(2)*
Cote d'Ivoire	–16.1	0.87
Congo, Republic of	–0.4	0.72
Djibouti	–27.9	0.80
Algeria	–3.0	0.30
Egypt	–1.8	0.25
Haiti	–13.4	0.10
Kenya	–3.2	0.89
Liberia	–74.0	0.89
Nigeria	–6.5	0.89
Nicaragua	–22.4	0.50
Nepal	–14.2	0.68
Peru	–14.1	0.66
Rwanda	–14.4	0.22
Senegal	–2.8	0.81
Sierra Leone	–24.2	0.79
El Salvador	–21.6	0.15
Somalia	–51.9	0.39
Thailand	–5.1	0.36
Turkey	–1.6	0.19
Uganda	–31.7	0.93
Average	–17.5	0.57
Correlation	–0.23	

Column 1 reports the percentage difference between the observed GDP per capita and its synthetic counterfactual averaged during the treatment period. Column 2 reports the ethnic fractionalization index.

which would restrict the application of our approach to only the most recent civil wars in our sample.

[11] Our findings are robust to different sets of growth predictors (e.g. the GDP share of industry and agriculture, population density) and to the exclusion of pre-treatment values of the GDP per capita among the predictors of the outcome. Overall our main model specification ensures (for any single country) a better pre-treatment fit compared to alternative specifications excluding the GDP lags, either using the same group of control countries or a subsample of countries with less dissimilar pre-treatment characteristics compared to the treated country (results available upon request).

[12] A rationale for not limiting the control pool to neighboring countries is that they may be affected by the treatment through conflict diffusion/contagion (Murdoch & Sandler, 2002). If neighboring countries take positive weights in the construction of the synthetic counterfactual and the GDP per capita of these countries is negatively affected by spillover effects, our results would underestimate the detrimental effect of war on the treated country. As it turns out, in our main estimations neighboring countries obtain appreciable weights only for a handful of civil war countries. Our results are virtually unchanged if we exclude neighbors from the pool of potential control countries (results available upon request).

[13] We do not make any claims about the validity of our results beyond the sample of civil wars analyzed. However, as a preliminary exploration, we compared average values of the predictors of the war-induced GDP gap (used in the regression analysis below) for our cases against the other civil wars in Kalyvas & Balcells's (2010) list. The wars in our sample are not significantly different, besides being less likely to be fought along ethnic lines and with conventional warfare, and more likely to occur after 1989 (see Table A.II in the Online appendix).

[14] This is the average across 20 countries of their mean annual economic impact, calculated as the percentage ratio between the average observed GDP per capita and the average of its synthetic counterfactual over the treatment period. See Online appendix B for the weights assigned to the countries in the control sample and for the predictor balance obtained in the construction of each counterfactual.

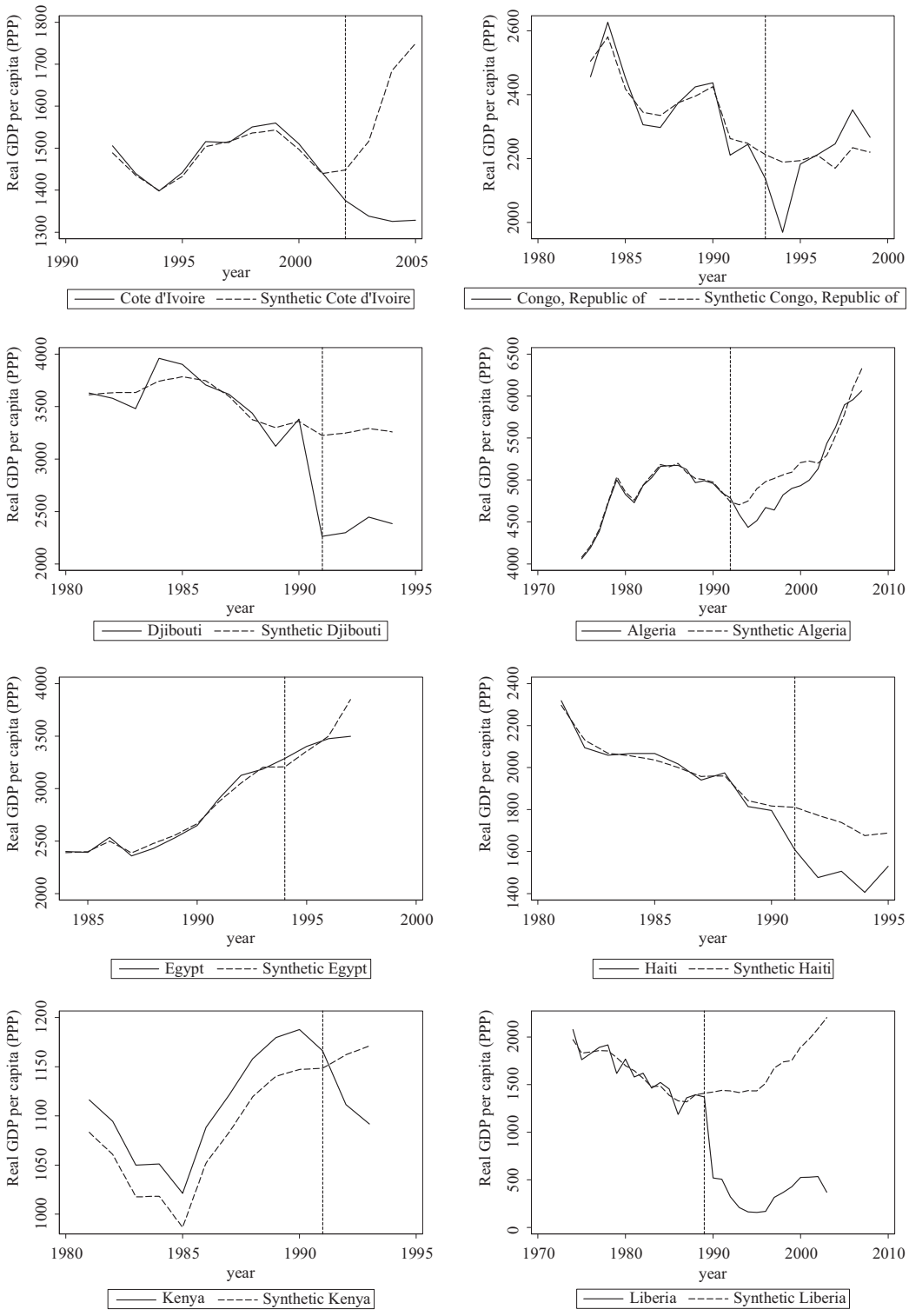

Figure 1. Real GDP per capita and its synthetic counterfactual

Each graph plots two series. The continuous line represents the observed GDP per capita for each of the 20 war-torn countries in our sample, while the dashed line is its synthetic counterfactual. The vertical line corresponds to the first year of the war; all years to its right indicate periods of war.

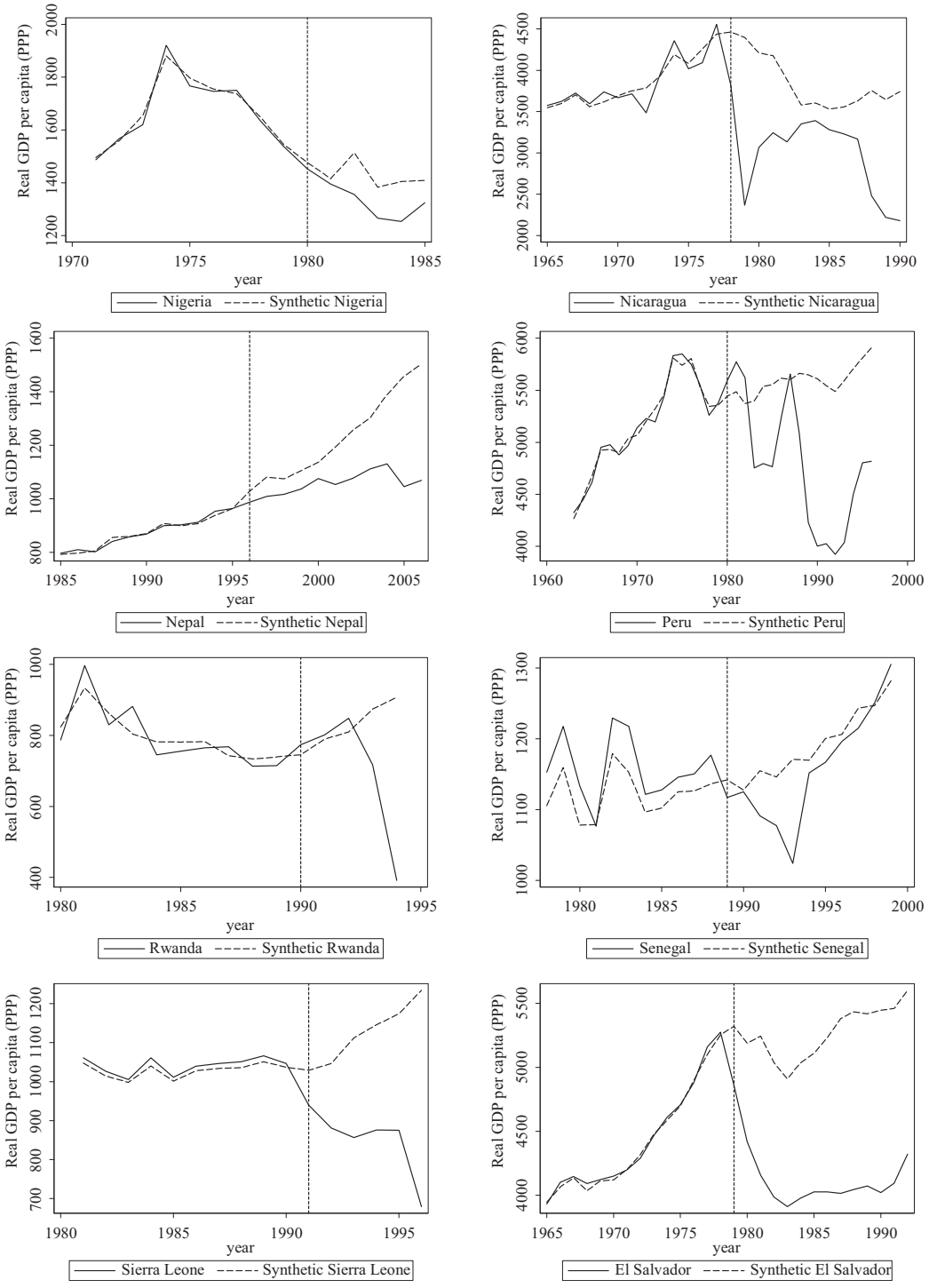

Figure 1. (continued)

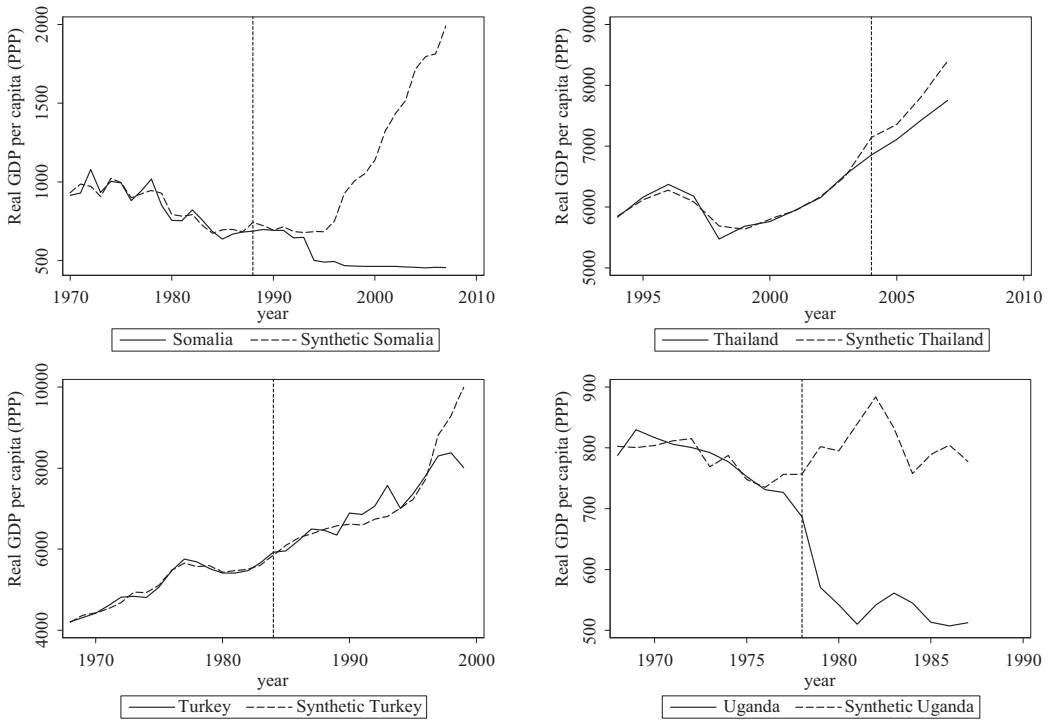

Figure 1. (continued)

approach allows us to reveal it, unlike cross-sectional and panel regression analyses. Most countries display a sharp drop in GDP per capita early in the war, but some diverge from this pattern. In the case of Turkey, there is no evidence of an appreciable economic loss until 1997–99, when we observe a decline of about 12% of GDP per capita. The overall limited impact may be due to the fact that the violence mostly affected the Kurdish southeast of the country, while the losses of the last two years of conflict may reflect the significant escalation of counterinsurgency operations from the mid-1990s, causing the depopulation of many Kurdish villages (Marcus, 2007). In their SCM analysis of the economic effects of the civil war in Turkey's southeastern provinces, Bilgel & Karahasan (2015) also find the largest impact in the late 1990s. Rwanda displays a somewhat similar trend, with no appreciable economic loss in the first three years of the war – when intermittent, low-level fighting was limited to the north of the country – followed by a very sharp downturn in 1993 and 1994 – when a major rebel offensive and the genocide occurred (Jones, 2001: 28–38).

Another discernible pattern is that the gap between the observed GDP and its counterfactual tends to widen in most cases as the war drags on, suggesting that the detrimental effects of war may intensify rather than dissipate over time (we discuss the effects of war duration in our regression analysis below).[15]

Ethnic heterogeneity and the economic costs of civil war

What explains the heterogeneity of the effects of civil war that the synthetic counterfactual evidence highlighted? The scale of the economic impact of civil war is clearly a function of the intensity of the fighting and the ensuing physical devastation. However, social features of war-torn countries may also play an important

[15] Looking at cumulative effects of conflict over time, Liberia, Somalia, Uganda, El Salvador, and Nicaragua belong to the quartile of overall costliest wars; the least destructive quartile includes Thailand, Turkey, Kenya, Egypt, and the Republic of Congo. Figure A.1 in the Online appendix reports tentative evidence of a positive correlation between prewar GDP per capita and average monetary value of war-induced GDP per capita loss, and of a negative correlation between GDP per capita and average war-induced loss in percentage terms. This is not surprising, as we would expect poorer countries to have less to lose, so to speak, in armed conflict, but also that these small monetary losses would amount to relatively large shares of their smaller 'pie'. Furthermore, geographically larger countries seem to experience smaller losses. This may plausibly reflect the fact that small countries have fewer opportunities to adjust to war through the relocation of productive factors and economic activities to areas relatively unaffected by the violence.

role. Building on an emerging literature on the relationships between ethnicity, trust, economic outcomes, and violent conflict, in this section we posit that civil war erodes interethnic trust and that highly fractionalized societies pay an especially high price, as they rely heavily on interethnic business relations. The empirical evidence presented below supports this hypothesis as the negative correlation between ethnic fractionalization and war-induced economic gap retains statistical significance after controlling for the intensity of war and a broad set of country/war characteristics.

Theory and testable hypotheses

The literature has made important strides in recent years in unraveling the relationships between conflict, trust, ethnicity and the economy, but crucial debates remain open. A body of works explores the relationship between ethnic heterogeneity and several socio-economic outcomes, typically finding that heterogeneity has a negative impact on the quality of policies and institutions (La Porta et al., 1999), public goods provision (e.g. Miguel & Gugerty, 2005), participation in social activities and trust (Alesina & La Ferrara, 2000), and economic growth (Montalvo & Reynal-Querol, 2005). A second strand of literature finds that social capital and trust have positive effects on economic outcomes such as growth (Zak & Knack, 2001; Algan & Cahuc, 2010), financial development, and trade (Guiso, Sapienza & Zingales, 2009).

A burgeoning third line of research looks at the effects of violent conflict on pro-social behavior, but reaches disparate conclusions. Some studies on the behavioral legacies of conflict report enhanced pro-social behavior of individuals after violence. Bellows & Miguel (2009) find that people more affected by war in Sierra Leone display higher levels of social and political involvement. Similarly, Blattman (2009) documents higher levels of political activism among abductees of the Lord's Resistance Army than in the general population of northern Uganda. Voors et al. (2012) report that members of communities exposed to higher levels of violence in Burundi exhibit more altruistic behavior, while Gilligan, Pasquale & Samii (2013) find higher levels of social cohesion and trust in Nepalese communities more affected by civil war. By contrast, some studies support the conventional wisdom that violence undermines social cohesion and trust while increasing the salience of ethnic identities. Rohner, Thoenig & Zilibotti (2013a) find that individuals in locales more exposed to violence in Uganda subsequently exhibit lower levels of generalized trust and stronger ethnic identities. Becchetti, Conzo & Romeo (2014) report lower trustworthiness among individuals most affected by electoral violence in Kenya in 2007. Consistently, Cassar, Grosjean & Witt (2013) find that victims of civil war violence in Tajikistan display lower trust and willingness to enter into market transactions as well as stronger kinship ties. However, the same individuals also participate at higher rates in community and religious associations.

The longstanding observation (Portes, 1998) that there exist different types of social capital, with distinct implications for social outcomes, goes a long way in explaining the literature's contradictory findings on the relationship between conflict and pro-social behavior. The experience of violence may both erode generalized trust and enhance sociopolitical participation, as the two are distinct phenomena. Generalized trust amounts to willingness to cooperate with strangers despite the risk of exploitation, while sociopolitical involvement may well occur within friendship, kinship or ethnic networks and may coexist with distrust, exclusion, and discrimination of outsiders. Moreover, some of the divergent findings could be explained by the fact that the effects of conflict on pro-social behavior are likely to depend on a country's ethnic structure (relevant for answering the question: trust toward whom?), the main cleavage of conflict, and the patterns of violence (does the conflict pit ethnic group X against group Y?). Violence may increase victims' interethnic distrust and harden identities while enhancing in-group trust, with an ambiguous net impact on a country's overall stock of trust. Consistently, Bauer et al. (2014) find that in Georgia and Sierra Leone greater exposure to war spurred egalitarian motivations among children and young adults towards in-groups, but not out-groups. Similarly, Dercon & Gutiérrez-Romero (2012) report that victims of electoral violence in Kenya display reduced trust towards members of other ethnic groups but not towards co-ethnics.

Taking stock of these various findings, we posit that armed conflict affects the economy through its differential impact on intra- and interethnic trust. Generalized trust is a fundamental ingredient for a functioning economic system. Much economic exchange occurs in a context of asymmetric information about the reliability of one's anonymous counterparts. Trust enables economic agents to operate more efficiently (e.g. by invoicing for goods that they have delivered) and reduces the need to devote resources to monitoring and protection against exploitation. Consistent with findings from Dercon & Gutiérrez-Romero (2012) and Bauer et al. (2014), we expect civil war to erode interethnic trust, while leaving intra-ethnic trust unaltered or even bolstering it. Violent conflict can thus be seen as exacerbating the

Figure 2. Ethnic fractionalization and GDP loss

observed tendency for individuals to cooperate and reciprocate cooperation more frequently when dealing with co-ethnics than ethnic-others (Habyarimana et al., 2007).

Domestic trade is likely to be an important mechanism through which war-induced mistrust affects the economy, due to its trust-sensitive nature and its immediate impact on the economic system (Rohner, Thoenig & Zilibotti, 2013b).[16] Moreover, mistrust could affect economic performance by undermining public good provision, in particular when it relies heavily on individual contributions – a common occurrence when state capacity is low.

We expect the negative economic effects of civil war to be more pronounced the more ethnically heterogeneous a country is. In highly fractionalized countries (i.e. with many small ethnic groups), a large number of economic exchanges would normally occur between ethnic-others, but they risk being encumbered or deterred by conflict-induced mistrust. Economic inefficiency is bound to be high when markets look like small and isolated 'ethnic islands', even if there are high levels of intra-ethnic trust. Figure 2 summarizes the logic of the argument with a flow diagram.

The empirical test

To test our hypothesis about the relationship between ethnic fractionalization and war-induced impact on GDP per capita, we estimate a set of reduced-form equation models. Our dependent variable is the yearly percentage gap of GDP per capita calculated with SCM for each of the 20 countries in the sample. The measure of ethnic fractionalization (*Ethnic Fract.*) for each country is our key independent variable (Reynal-Querol, 2002). Ethnic fractionalization indicates the probability that two randomly selected individuals from a country's population belong to different ethnic groups; it increases monotonically with the number of groups.[17] We expect *Ethnic Fract.* to display a negative sign, as more fractionalized countries should experience larger negative gaps (i.e. larger losses).

We include in the estimated equations controls corresponding to factors that are likely to affect the economic impact of civil war. We control for the severity of violence as measured by the number of victims (in log form, *log(Deaths)*) in each country-year (Lacina & Gleditsch, 2005) and the duration of conflict (*Years at war*) (Gleditsch et al., 2002), as severity and duration are likely correlated with destructiveness. Moreover, we control for regime type using the *Polity2* index (Marshall, Gurr & Jaggers, 2013). Given that previous studies found non-linear relationships between democracy and conflict (Hegre et al., 2001) as well as between regime type and growth (Papaioannou & Siourounis, 2008), we include the squared value of *Polity2*. We also add a dummy for ethnic civil wars (*Ethnic war*) (Kalyvas & Balcells, 2010) to capture the potentially distinct impact on the economy of wars fought along ethnic lines. Finally, we control for different technologies of warfare, using the typology from Kalyvas & Balcells (2010) of 'conventional', 'irregular' (guerrilla), and 'symmetric non-conventional' civil wars.[18]

We estimate all our models with panel-corrected standard errors (Beck & Katz, 1995) with Prais-

[16] Colletta & Cullen (2000) report persistently lower levels of Hutu–Tutsi trade compared to before Rwanda's genocide; Guiso, Sapienza & Zingales (2009) show that interstate war has a suppressive effect on bilateral international trade. Rohner, Thoening & Zilibotti (2013b) argue that interethnic trade requires specific human capital investment by two ethnic communities (e.g. learning the other group's language/customs, maintaining an interethnic social network), so that each group will invest only if it trusts the other to do the same. Trust may also affect trade through the mechanism of trade-credit (Fisman & Love, 2003).

[17] We are cognizant of the critiques of indexes of ethnic fractionalization (Posner, 2004; Chandra & Wilkinson, 2008). The absence of a tight match between the theoretical concepts of interest and measures of ethnic heterogeneity, which plagues several studies, does not apply to our analysis as we offer a theoretical argument explicitly linking heterogeneity and the economy during civil war, rather than advancing generic claims about the effects of 'ethnicity' (Chandra & Wilkinson, 2008). The fact that indexes of ethnic fractionalization are time-invariant while, in reality, ethnic structures change over time is potentially more problematic. In practice, however, this results in a measurement error of our independent variable, which entails an attenuation bias in our estimates. Moreover, Posner's (2004) measure of ethnic fractionalization in Africa, which was developed taking into account these insights, suggests that the measurement error in our measure of fractionalization may not be pervasive: Posner records only eight changes in his measure of fractionalization for 42 African countries over four decades.

[18] In symmetric non-conventional civil wars states are unable to deploy an organized military against poorly equipped insurgents.

Table II. Explaining the impact of civil war

	Dependent variable: Percentage GDP per capita gap between observed and synthetic series during war							
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Ethnic fract.	–0.298**	–0.267**	0.252	–0.068**	–0.065*	0.260†		
	(0.086)	(0.079)	(0.180)	(0.026)	(0.030)	(0.140)		
Ethnic fract.*(log)Deaths			–0.064**			–0.044*	–0.076*	–0.093**
			(0.021)			(0.017)	(0.030)	(0.020)
Polity2	–0.043**	–0.041**	–0.038**	–0.013**	–0.019**	–0.023**	–0.041**	–0.022**
	(0.009)	(0.011)	(0.011)	(0.004)	(0.005)	(0.007)	(0.009)	(0.008)
Polity2, squared	0.002**	0.002**	0.002**	0.001**	0.001**	0.001**	0.002**	0.001**
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
(log)Deaths		–0.012†	0.024*		–0.006	0.021**	0.031*	0.037**
		(0.007)	(0.011)		(0.005)	(0.007)	(0.013)	(0.008)
Years at war	–0.022**	–0.018**	–0.019**	–0.004*	–0.001	–0.001	–0.018**	–0.009**
	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)
Ethnic war	–0.044	–0.047	–0.019	–0.003	–0.004	–0.003		
	(0.036)	(0.040)	(0.039)	(0.012)	(0.012)	(0.016)		
Guerrilla war	0.005	–0.000	0.010	0.059*	0.020	0.004		
	(0.049)	(0.052)	(0.050)	(0.027)	(0.027)	(0.030)		
SNC war	–0.089	–0.133*	–0.141*	–0.060*	–0.077*	–0.074*		
	(0.074)	(0.066)	(0.067)	(0.025)	(0.031)	(0.030)		
Lag percentage gap				0.791**	0.825**	0.816**		0.670**
				(0.056)	(0.074)	(0.069)		(0.080)
Decade dummy	X	X	X	X	X	X	X	X
Country dummy							X	X
Observations	190	153	153	170	137	137	153	137
R-squared	0.395	0.492	0.543	0.935	0.920	0.925	0.747	0.947
Number of countries	20	18	18	20	17	17	18	17
Mean outcome	–0.200	–0.185	–0.185	–0.218	–0.199	–0.199	–0.185	–0.199
Mean ethnic fract.	0.536	0.512	0.512	0.532	0.512	0.512	0.512	0.512
SD ethnic fract.	0.276	0.270	0.270	0.275	0.268	0.268	0.270	0.268

Panel corrected standard errors in parentheses. Inference: ** $p < 0.01$, * $p < 0.05$, † $p < 0.1$. This is a panel of the 20 war-torn countries in all years of war. Columns 2, 3, 5, 6, 7, and 8 include a lower number of countries due to missing values on war casualties for three countries (Kenya, Nigeria, and Haiti).

Winsten transformation for panel-specific AR1 autocorrelation and decade-fixed effects. In some specifications, we also add the one-year lagged value of the dependent variable (*Lag.percentage gap*) to control for inertia, and country-fixed effects to pick up time-invariant country-specific features and thus reduce concerns of omitted variable bias.

In the country-fixed effects models, we do not include the single term of the fractionalization index, since it would not be identified due to its perfect collinearity with the country dummies. Instead, we focus on the interaction between the country's ethnic heterogeneity and the intensity of the civil war, proxied by the number of deaths in each country-year. This interaction allows us to conduct a nuanced test of our argument by examining a channel through which violence erodes interethnic trust, thus affecting the economy. Interethnic trust should decline more steeply as the conflict becomes more intense and a growing number of individuals are exposed to violence. Thus, fractionalization should have a more detrimental impact the more intense the war, which should be reflected in the negative coefficient of the interaction term.

Results

Table II reports estimates of the effects of ethnic fractionalization on the economic costs of civil war. In column 1, the significant negative coefficient of ethnic fractionalization is consistent with our hypothesis that highly heterogeneous countries experience more severe economic losses during civil war. Our results show that, everything else held equal, one standard deviation higher ethnic fractionalization corresponds to a war-induced

GDP per capita loss about eight percentage points larger (i.e. 40% of the average annual economic loss). We find the same substantive result in column 2, after controlling for war intensity (*log(Deaths)*).

The significant negative coefficient of the interaction term between fractionalization and war intensity (column 3) also supports our theoretical expectations about the mechanism linking civil war to economic effects: the detrimental impact of fractionalization grows stronger as the number of victims increases. Estimation results indicate that a one standard deviation increase in the fractionalization index corresponds to a GDP per capita loss of four percentage points more for countries with a number of casualties at the 75th percentile of the variable's distribution than for countries at the 25th percentile. This difference is about 20% of the average impact on per capita GDP caused by civil war.

These results on the effect of ethnic fractionalization are robust to the inclusion of lagged values of the dependent variable (columns 4–6 and 8) and country-fixed effects (columns 7 and 8). The positive coefficient of the lagged dependent variable suggests a form of persistence over time of the impact of civil war, which is consistent with the pattern of a widening GDP gap during the war noted above.

Concerning the control variables, it is worth noting that there is some evidence of a non-linear relationship between level of democracy and the economic costs of civil war: for lower levels of democracy, an upward movement in the Polity scale is associated with relatively larger marginal losses of wealth, while for higher levels the same increase in democracy corresponds to smaller losses. However, the underlying theoretical mechanisms require further investigation, and in any case the result should be taken with much caution as it loses significance when lagged measures of regime type (one and five years) are used.[19] Moreover, the results in Table II confirm the intuition that longer and more intense wars cause larger economic damage.[20] The technologies of warfare adopted by belligerents also seem to matter: while there is no statistical difference between conventional and guerrilla civil wars, symmetric non-conventional wars seem to be more destructive than conventional wars (cf. Balcells & Kalyvas, 2014, who find that conventional civil wars are more lethal). Finally, civil wars fought along ethnic lines do not appear to be statistically more destructive than ideological conflicts.

Robustness checks and further evidence

In this section, we discuss robustness checks on the SCM we used to construct our dependent variable and on the regression analysis of the effects of ethnic fragmentation. We also present additional evidence on the relationship between ethnicity and the economic costs of civil war.

Alternative control samples

To assuage concerns that our estimates of the gap of GDP per capita computed with SCM may be driven by the specific composition of our sample of control countries, we adopt a robustness check introduced by Campos, Coricelli & Moretti (2015, 2016). By construction, the weight assigned to each control country and the resulting synthetic counterfactual are influenced by the composition of the control sample. Therefore, the GDP gap between the conflict-torn country and its synthetic counterfactual may be influenced by the inclusion in the control pool of specific countries experiencing idiosyncratic economic shocks during the treatment period. If this were the case, part of the corresponding GDP gap would be unrelated to the war in the treated country. We thus check the robustness of our results to random changes in the composition of the control sample with the following procedure. For all treated countries, we iteratively re-estimate the synthetic counterfactual using 100 alternative control samples, each containing a random draw of half the countries in the main analysis. This exercise allows us to compare our main estimates with those based on 100 alternative control samples with random probabilities of including countries affected by local shocks in the treatment period.

Figure 3 shows that, when compared to most of the estimations with the alternative control samples (gray lines), our main estimations (black lines) have a good pre-treatment fit, similar direction, and a non-extreme magnitude of the war-induced effects. This provides confidence that our main results are not an artifact of the specific composition of the control samples. Two noteworthy exceptions are the Republic of Congo and Kenya: our baseline estimates are clearly above most of the estimates based on alternative control samples, which suggests that with our main control sample we are likely underestimating the real economic loss caused by those civil wars.[21]

[19] Note that our main results hold when we exclude the Polity variable from our model specification (Tables A.III–A.V in the Online appendix).

[20] Duration and severity of war do not seem to have significant non-linear effects on the war-induced GDP gap (see Tables A.VI–A.VII in the Online appendix).

[21] We also conducted the same exercise under more restrictive conditions. For each treated country, we re-estimated the effect of civil war using 100 alternative, randomly drawn control samples

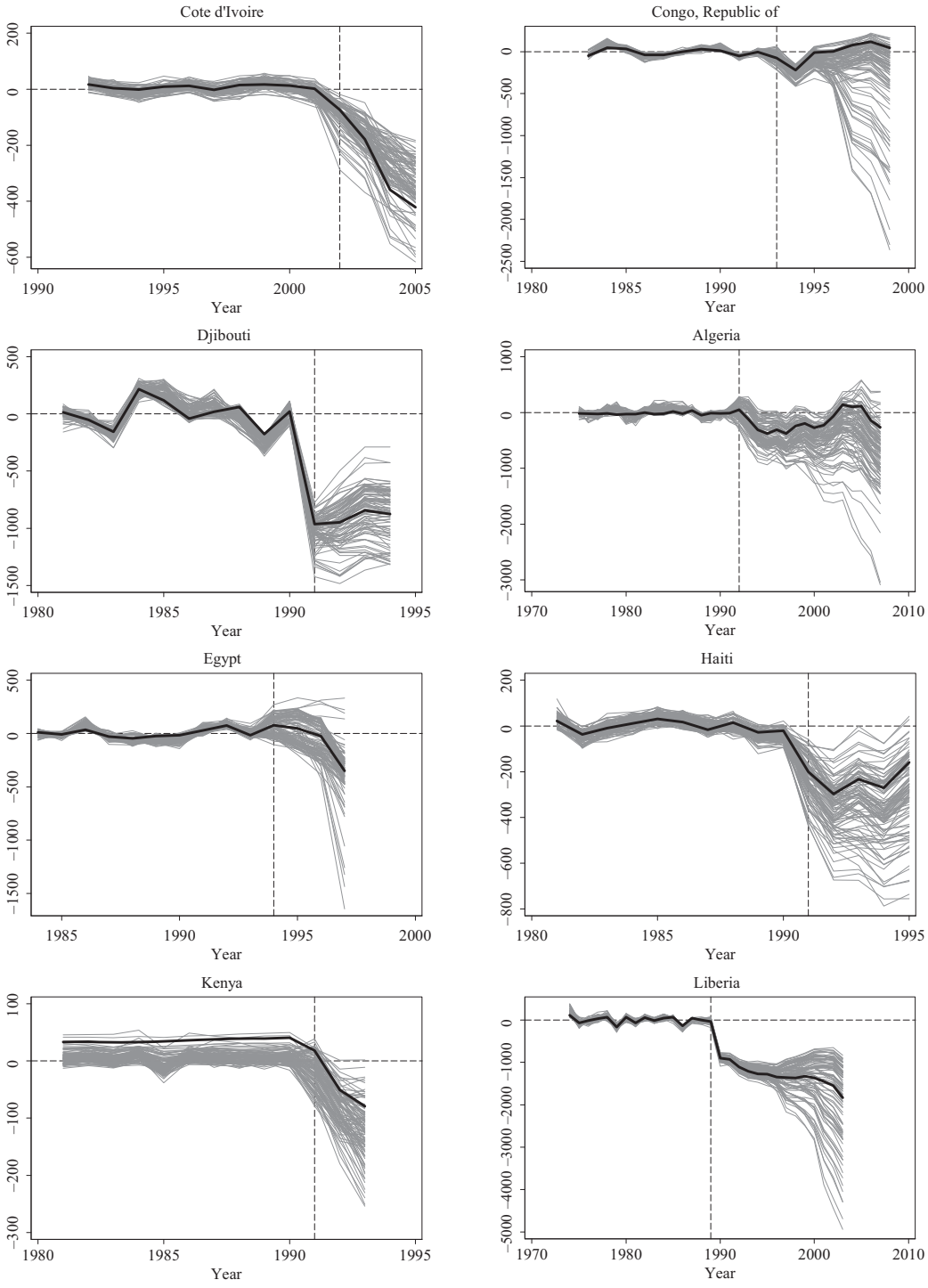

Figure 3. Real GDP per capita and its synthetic counterfactuals (using alternative control samples)

The black line represents the difference between the observed GDP per capita of the country in question and its synthetic counterfactual reported in Figure 1. The gray lines represent the difference between the observed GDP per capita and its synthetic counterfactuals obtained using alternative, randomly drawn control samples. Each control sample includes 50% of the countries belonging to the control sample in Figure 1.

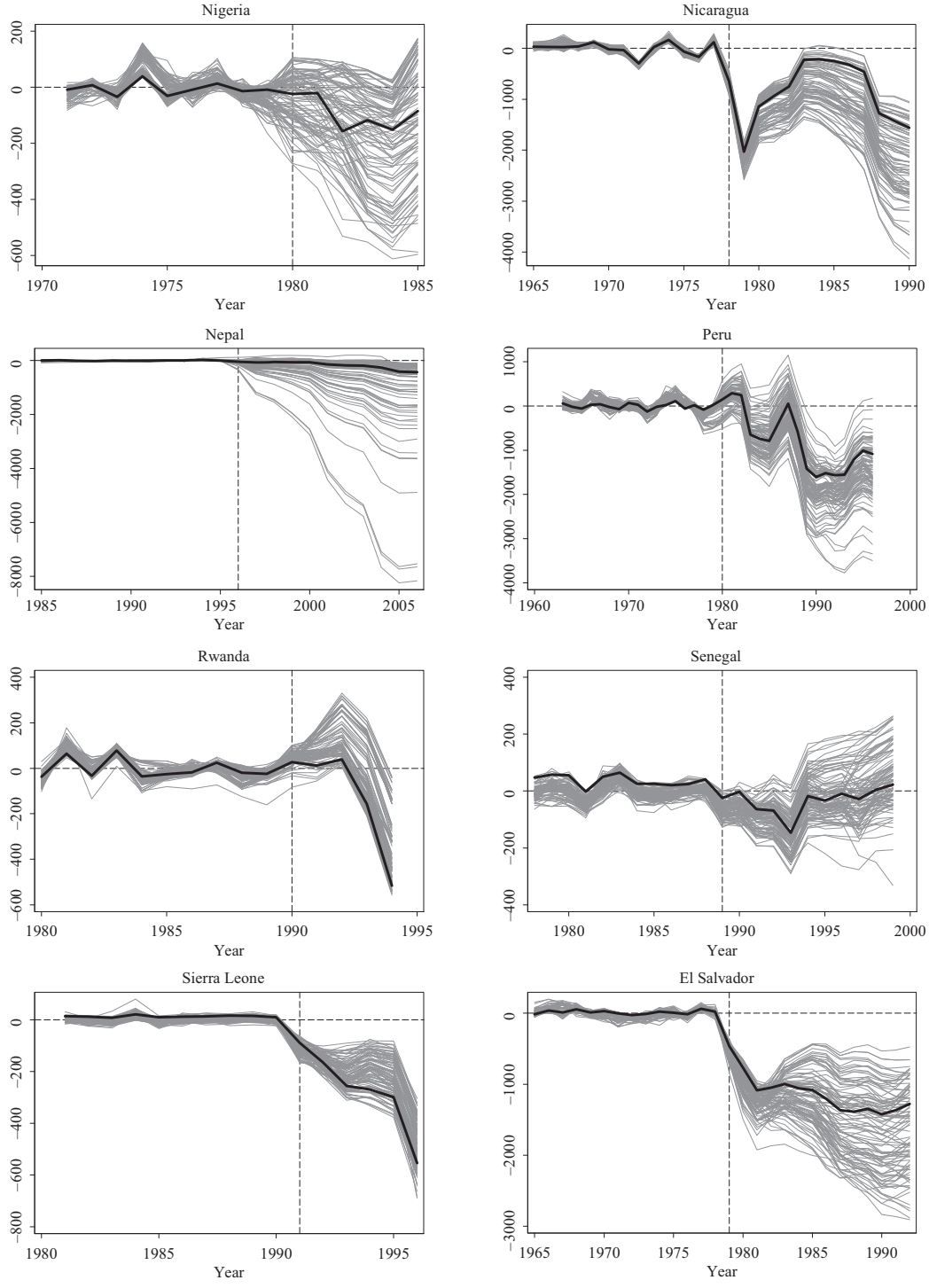

Figure 3. (continued)

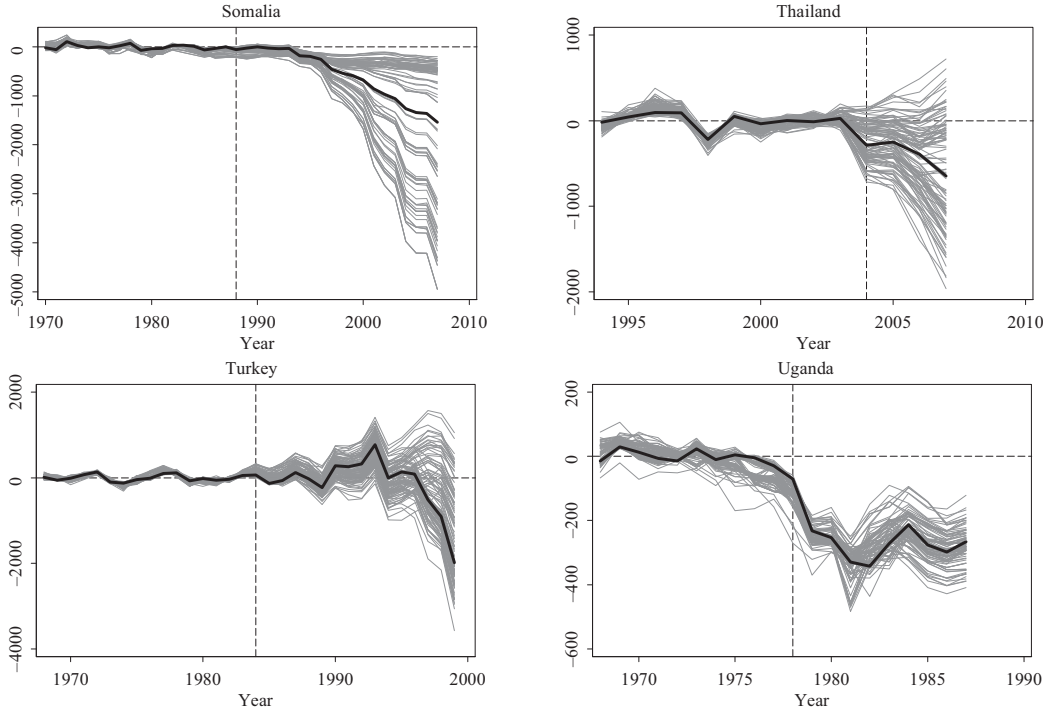

Figure 3. (continued)

The range of estimates of the war-induced GDP gap (i.e. our dependent variable) based on these alternative pools of potential control countries may prompt questions about the robustness of the estimated effects of ethnic fractionalization. To deal with this concern, we created three alternative dependent variables based on the estimated values of the per capita GDP gaps obtained with the 100 alternative control samples. In particular, for each country-year combination, we use alternatively (i) the *mean* value and (ii) the *median* value of the ranges of estimated gaps as well as (iii) the value of the gap obtained with the alternative control sample showing the *best pre-treatment fit* (i.e. the minimum pre-treatment root mean square prediction error, RMSPE). Tables A.VIII–A.X in the Online appendix report the corresponding regression results, which are overwhelmingly consistent in sign and statistical significance with our main analysis.[22]

Further evidence

In Table III we report the results of additional estimations on the relationship between a country's ethnic demography and the war-induced GDP gap. First, we explore some interaction effects associated with ethnic fractionalization. Given that it may take time for violence-induced interethnic distrust to spread, we expect ethnic fractionalization to have a stronger impact over time. Moreover, interethnic distrust should be especially high when the civil war pits ethnic groups against each other as individuals experience violence at the hands of ethnic-others; thus, we expect ethnic fractionalization to have a larger negative impact in ethnic civil wars. Results in columns 1 and 2 confirm our expectations, as the interactions between fractionalization and duration and between fractionalization and ethnic war are significant and negative.

consisting of 25% of countries from the main control sample. Estimation results (Figure A.2 in the Online appendix) indicate that these alternative estimates too are 'in line' with our main estimates.

[22] We experimented with an alternative robustness check in which we limit the control sample to countries with less dissimilar pre-treatment characteristics compared to the treated country. The limited control sample is obtained with a standard k-means cluster analysis based on the pre-treatment average values of all the covariates, which allows us to split our original set of countries in two subsamples. We then used as potential control countries those included in the subsample containing the treated country. Our cost estimates are very similar for any single country and on average (–17.3%), but in almost all cases the pre-treatment RMSPE is larger than for our main estimations reported (Figure A.3 in the Online appendix). When we estimate the relationship between ethnic fractionalization and measures of the per capita GDP gap based on this robustness check, we still find a negative and significant coefficient for the ethnic fractionalization variable (Table A.XI in the Online appendix).

Table III. Explaining the impact of civil war: Further evidence.

	Dependent variable: Percentage GDP per capita gap between observed and synthetic series during war						
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
Ethnic fract.	–0.031	0.010					
	(0.128)	(0.072)					
Ethnic fract.*Years at war	–0.034*						
	(0.015)						
Ethnic fract.*Ethnic war		–0.366**					
		(0.133)					
Ethnic polarization			0.121	0.097	–0.264	0.101	0.158
			(0.099)	(0.091)	(0.339)	(0.167)	(0.110)
Ethnic polar.*(log)Deaths					0.047		
					(0.043)		
Ethnic polar.*Years at war						0.002	
						(0.017)	
Ethnic polar.*Ethnic war							0.112
							(0.185)
Polity2	–0.045**	–0.041**	–0.045**	–0.043**	–0.043**	–0.046**	–0.044**
	(0.009)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)	(0.009)
Polity2, squared	0.002**	0.002**	0.002**	0.002**	0.002**	0.002**	0.002**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
(log)Deaths				–0.015†	–0.039†		
				(0.008)	(0.022)		
Years at war	–0.005	–0.019**	–0.021**	–0.022**	–0.022**	–0.022**	–0.023**
	(0.009)	(0.003)	(0.003)	(0.004)	(0.003)	(0.009)	(0.003)
Ethnic war	–0.030	0.179**	–0.016	–0.014	–0.021	–0.017	–0.071
	(0.043)	(0.069)	(0.017)	(0.032)	(0.031)	(0.017)	(0.089)
Guerrilla war	0.096	0.078	–0.014	–0.033	–0.028	–0.013	0.001
	(0.066)	(0.057)	(0.042)	(0.041)	(0.039)	(0.042)	(0.043)
SNC war	–0.025	–0.060	–0.160*	–0.160*	–0.159**	–0.159**	–0.163**
	(0.063)	(0.075)	(0.063)	(0.064)	(0.061)	(0.061)	(0.061)
Decade dummy	X	X	X	X	X	X	X
Observations	190	190	186	149	149	186	186
R-squared	0.455	0.427	0.440	0.455	0.473	0.442	0.450
Number of countries	20	20	19	17	17	19	19

Panel corrected standard errors in parenthesis. Inference: ** $p < 0.01$, * $p < 0.05$, † $p < 0.1$.

Second, as a sort of placebo test, we look at the effects of ethnic polarization (*Ethnic polarization*) instead of ethnic fractionalization (Reynal-Querol, 2002). In polarized countries (i.e. with an ethnic structure that approaches a bimodal distribution), we should not expect the same effects as in ethnically fractionalized countries: with only a few, large ethnic groups, conflict-induced interethnic distrust could be (more than) compensated by enhanced intra-ethnic trust, as the probability of economic exchange among co-ethnics is higher than for highly fractionalized countries. We are thus agnostic as to the direction of the effect of polarization on economic impact. We do, however, expect any detrimental economic impact of polarization to be smaller than that of fractionalization, as the ethnically bounded markets that emerge during wartime would be larger (and the corresponding loss of efficiency lower). Columns 3–7 indicate that neither the coefficients of ethnic polarization nor those of its interaction with conflict intensity, duration, and the ethnic war dummy are statistically significant. Thus, this evidence confirms that highly fractionalized countries are especially vulnerable to civil war-induced economic havoc; a different form of ethnic heterogeneity – polarization – does not seem to matter.

Conclusion

Civil wars kill, maim, and destroy on a large scale. While this basic assertion is beyond dispute, we know relatively little about why some civil wars cause more economic

damage than others. Building on recent findings on the relationships between ethnicity, trust, economic outcomes, and violent conflict, we argue that civil war erodes interethnic trust and that highly fractionalized societies experience especially serious losses, as they rely heavily on interethnic business relations.

Existing studies do not offer harmonized, country-specific, and time-varying measures of the economic costs of civil war needed to test this argument. In fact, estimating the economic costs of civil war has proven challenging due to endogeneity problems. As war-torn countries are inherently different from peaceful countries and civil war violence is both a cause and a consequence of dismal economic conditions, simple comparisons between conflict-ridden countries and peaceful countries at similar levels of development or between prewar and wartime economic performance are not necessarily useful. In this article, we tackle this concern by using SCM to construct artificial comparison units for 20 war-torn countries. We thus obtain our dependent variable by calculating the distance between each country's observed GDP per capita in the years of war and its synthetic counterfactual in the same period had the war not occurred. Our measure indicates that the countries in our sample experienced an annual average loss of 17.5% of GDP per capita in the years of war. Future studies will need to assess whether these findings travel across different samples of civil wars.

Based on this sample, panel regressions provide robust evidence of a negative association between ethnic fractionalization and the war-induced economic impact over the course of conflict; consistent with our expectation, more fractionalized countries tend to experience substantially larger economic losses. A nuanced understanding of the variation of the economic costs of civil wars and their causes, to which we make an initial contribution with this article, represents an important stepping-stone to effective conflict prevention and post-conflict development policies. In particular, our findings suggest that the returns to conflict prevention investments (e.g. economic assistance, security forces training, deployment of international peacekeepers) are likely to be especially high in very fractionalized countries and that restoring interethnic trust there should be one of the primary foci of post-conflict recovery initiatives.

Replication data

The dataset, codebook, and do-files for the empirical analysis in this article, along with the Online appendix, can be found at http://www.prio.org/jpr/datasets.

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