Conflict and Child Mortality in Mali: A Synthetic Control Analysis

EDOARDO MASSET

Indirect effects of conflict on mortality of vulnerable groups are as important, or more important, than the direct effects. However, data limitations and methodological challenges hinder the estimation of excess deaths produced by conflict, and few studies explore the mechanisms by which conflict harms civilian populations. We estimate the impact of the Malian conflict on child mortality over the period 2012–2018 using Demographic Health Survey data. We use birth histories to build time series of child mortality, and we employ novel synthetic control methods to show that the Malian conflict significantly increased child mortality in northern Mali. We conduct a difference-in-difference analysis of the impact of conflict on key determinants of maternal and child health and conclude that a reduction in access to safe sanitation and to child vaccinations in conflict areas was among the most likely causes of the increase in mortality. Northern Mali is today one of the poorest and most neglected areas of the world where humanitarian assistance is urgently needed.

Introduction

It is believed that the impact of armed conflict on health and mortality of women and children far exceeds the impact on those directly involved in conflict (Bendavid et al. 2021). Understanding the extent of the impact of conflict on child mortality, and understanding the mechanisms through which this occurs, is critical for the design and delivery of maternal and child health interventions in humanitarian crises (Gaffey et al. 2021). But the existing evidence linking armed conflict to child mortality is limited.

The estimation of indirect deaths produced by secondary effects of conflict such as poverty, displacements of populations, destruction of infrastructure, and disruption of health services, is methodologically difficult. Indirect deaths are not observed and need to be estimated as "excess deaths" by a comparison to a counterfactual status quo in which conflict

Edoardo Masset, London School of Hygiene and Tropical Medicine. LIDC, 20 Bloomsbury Square, WC1A 2NS, London, UK.
Email: Edoardo.Masset@lshtm.ac.uk

did not occur. The identification of a counterfactual no-conflict group is difficult for various reasons. In some cases, conflict is widespread, and no area is left unaffected that can be used as a comparison group. In some other cases, conflict is geographically localized but its negative impacts spill over into neighboring areas thus undermining their use as a comparison group. Finally, conflict often generates a humanitarian response whose mitigating effect on mortality is difficult to disentangle.

Empirical studies of conflict and mortality tend to fall into one of three categories. Some studies use national-level data on conflict duration and mortality and compare countries with and without conflict using cross-country regression analyses (see, e.g., Chen, Loayza, and Reynal-Querol 2008; Human Security Report Project 2011). These studies fail to find a meaningful correlation between conflict and mortality mainly because modern conflicts rarely affect the entire country. Modern conflict tends to be heavily localized in small geographic area and needs to be analyzed at a much more disaggregated spatial level.

Some studies combine national health surveys data, such as the data of the Demographic Health Surveys (DHS), from multiple countries and compare mortality rates of affected and unaffected areas using spatially referenced data of the Armed Conflict Location & Event Data Project (ACLED) or of the Uppsala Conflict Data Program Georeferenced Events Dataset (see, e.g., Burke, Heft-Neal, and Bendavid 2016; Wagner et al. 2018; Bendiavid et al. 2021). These studies provide global estimates of the mortality consequences of conflict on children. For example, Wagner et al. (2018) used data from 35 African countries and estimated that during the period from 1995 to 2015, a child exposed to conflict in the first year of life had a 7.7 percent higher chance of dying before age one than a nonexposed child. These studies have two limitations. First, the areas identified by the ACLED and Uppsala databases are large, and the assignment of conflict status to a particular observation is often imprecise. Second, and more important, areas exposed to conflict tend to differ in terms of mortality determinants from other areas, both in the levels and in the trends, in such a way that unexposed areas are not always a valid comparison group.

A third group of studies estimates excess deaths by comparing populations affected and unaffected by conflict in the same country using local household surveys. These studies face the same problems described above in identifying an unbiased comparison group and obtaining results that reflect the relevance of different contextual characteristics. For example, Ouili (2017) found that in Cote d'Ivoire children under-5 living in conflict areas had a 3 percent higher probability of dying before age 5. Similarly, Lindskog (2016) estimated that postneonatal mortality in conflict areas of the Democratic Republic of Congo increased between 1996 and 2003. However, the study of Kandala et al. (2014) which also focuses on the Democratic Republic of Congo found no effects of conflict on child mortality, which is tentatively explained by the displacement of mothers from high-conflict areas to safer areas. Not all studies find an impact of conflict on mortality. In another example, Singh et al. (2005) compared mortality risk of children of refugees and host populations in north-western Uganda and in south Sudan and found no effects, which they attributed to humanitarian assistance to displaced populations.

In order to address several methodological limitations encountered by the empirical studies described above, in this paper we assess the impact of conflict on child mortality in Mali using the synthetic control method proposed by Abadie and coauthors (Abadie and Gardeazabal 2003; Abadie, Diamond, and Hainmueller 2010, 2011). To add robustness to our analysis, we also employ two extensions of the synthetic control method: the "synthetic difference-in-differences" estimator proposed by Arkhangelsky et al. (2021), and the "causal impact" method of Brodersen et al. (2015), and we compare the results.

We apply synthetic control methods to the analysis of the impact of the conflict that broke out in Mali in January 2012, when rebel forces took control of northern Mali and proclaimed the independence of the region. We compare mortality trends in northern Mali to those of other regions over the period from 1988 to 2017, and we find that mortality rates in northern Mali after the conflict were two percentage points higher than what they would have been in the absence of conflict. About 10 children out of 100 did not reach age five at the time the hostilities began, and conflict added two other children to the count. We estimated that for every 43,000 children born in northern Mali each year, about 4,700 die before reaching age five, of which between 550 and 1,100 are excess deaths produced by conflict. We highlight that these effects should be interpreted as lower bounds of the true effects because the regions used as comparators were also, although indirectly, affected by conflict.

In order to explore the mechanism of the impact of conflict on mortality, we first disaggregate the analysis by child age. Deaths of infants (up to one year of age) are normally associated with conditions at delivery, and with poor antenatal and postnatal care. Deaths of older children are normally associated with poor preventative and curative health care, and with environmental factors such as poverty and a risky disease environment. We find that conflict had no impact on infants and that the increase in mortality occurred mostly among older children.

Further, we conduct a difference-in-difference analysis to estimate the impact of conflict on key determinant of child health. The results suggest that conflict had no impact on the nutritional status of children and of their mothers, and that the impact on antenatal care was very limited. We find suggestive evidence of the impact of conflict on access to safe sanitation, and to child vaccinations.

The paper is structured in the following way. The next section sets the context, introducing the Malian conflict and baseline mortality rates. The third section describes the synthetic control methods and the method employed to calculate mortality rates. The fourth section presents the main results of the study. The sixth section uses descriptive data and a difference-in-difference analysis of mortality determinants to infer the mechanisms of impact of conflict on mortality. Finally, the last section concludes.

Civil conflict and child mortality in Mali

The Malian civil conflict broke out in 2012 with a separatist insurgency led by Tuareg-Jihadist groups. Mali had witnessed three other Tuareg rebellions since independence, but none had reached the same level of violence and of risk to the stability of the state. In January 2012, a Tuareg group (the Mouvement de Liberation de l'Azawad) and three Jihadist groups (AlQaeda in Islamic Maghreb, Ansar Dine, and MUJAO) attacked Malian security in northern Mali. The Malian army was forced to retreat, and on April 2012 the rebels declared the independence of the Azawad—the rebels' naming of northern Mali—which includes the regions of Timbuktu, Gao, and Kidal (see Figure 1).[1] A string of military defeats by the Malian army led to popular unrest, an army mutiny, and eventually to a coup d'etat. As the country plunged into chaos, the rebel groups marched southwards in the direction of the capital city (Bamako).

In early 2013, an international military coalition recaptured the occupied areas and forced the rebels to disperse among the civilian population or to retreat to inhospitable areas. However, fighting broke out again in May 2014, and northern Mali has remained insecure and contested since then. The jihadist groups have regained control of some rural and remote areas, while the coalition forces have kept control of urban centers (Benjaminsen and Ba 2019). Peace agreements were signed in 2015 with some rebel groups, but the attacks against Malian and UN forces, and against civilians have continued. Many remote areas of the north are today under rebels' control and totally inaccessible.

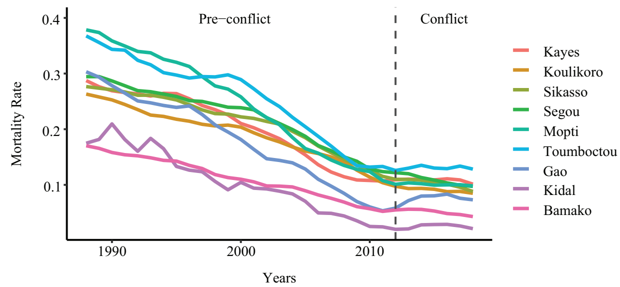
The Malian civil conflict can be portrayed as an ethnic-based rebellion for independence, but the reality is more complex and the causes are deeper. First, as of the last census of 2009, the main ethnic groups in the north were the Songhai (45 percent), the Tamasheq (32 percent), and the Pehul (7 percent). The rebellion was led by Tamasheq groups of Arab descent, sometimes with support of the Pehul, but the Tamasheq group is itself divided into factions (Chauzal et al. 2015). Second, since independence successive governments did very little to reduce divisions between north and south. On the contrary, they marginalized the north and fomented divisions between groups. Finally, population pressure led to a competition over

FIGURE 1 The area of northern Mali affected by conflict

resources between farmers and livestock herders and between ethnic groups (World Bank 2016; Benjaminsen and Ba 2019)

Field reports suggest that the conflict had a dramatic impact on living conditions. The first wave of violence in 2012 produced displacements of population, and it was estimated that at some point about a third of the population in the north had left their homes (Etang-Ndip, Hoogeveen, and Lendorfer 2015). Many internally displaced people made return to their homes but found their livestock and assets depleted (Hoogeveen, Rossi, and Sansone 2018). Conflict had a heavy impact on the agricultural economy (Kimenyi et al. 2014). Mobility and access to markets were reduced, and thefts and looting by armed groups became common. Food prices increased, damaging consumers. Public services were affected the most (World Bank 2016). Schools and health facilities were occupied, looted, and destroyed. Health staff and teachers abandoned the area and made no return, particularly in Gao. The whole region is today inaccessible to government staff, and the few available services are provided by humanitarian organizations.

FIGURE 2 Under-5 mortality by region (1988–2018)



The conflict affected a population already living in precarious conditions. Mali is ranked among the poorest countries in the world and has one of the highest child mortality rates. The chart in Figure 2 shows mortality rates of children under-5 by region over the period from 1988 to 2018. A dotted vertical line indicates the time the conflict started in January 2012. Mortality rates were obtained by pulling together data from five DHS surveys of 1995–1996, 2001, 2006, 2012–2013, and 2018, using a method described in the next section.

Several points stand out in relation to the chart in Figure 2. First, mortality rates in Mali are today above 100 per thousand, a level which is second only to those reported by war-torn countries, such as South Sudan, Somalia, and the Central African Republic. The causes of high-mortality rates in Mali are difficult to identify. The WHO Global Observatory, using extrapolations of verbal autopsies, blames malaria, pneumonia, and malnutrition as the main causes.[2] Willcox et al. (2018) using in-depth case studies in selected communities concluded that inadequate antenatal care and poor quality of services are the root causes.

Second, there was a massive improvement in child survival rates over the past 30 years. In the late 1980s, a child in Mali had only a probability between 60 and 80 percent of being alive by age five. In the early 2010s, the same probability was between 85 and 95 percent. Mortality rates decreased in all regions, but they decreased faster in regions with higher mortality, thus leading to a reduction in regional disparities.

Finally, the downward trend in mortality rates slowed down in the early 2010s in all regions, suggesting that other factors affected mortality before the beginning of conflict. Mortality rates, however, deteriorated more markedly in the northern regions of Gao, Kidal, and Timbuktu. In the rest of the paper, we will investigate to what extent conflict caused this deterioration.

Synthetic control methods

Synthetic control is a method proposed in a series of articles by Abadie and coauthors (Abadie and Gardeazabal 2003; Abadie, Diamond, and

Hainmueller 2010, 2011), which in a recent review was described as "the most important innovation in the policy evaluation literature in the last 15 years" (Athey and Imbens 2017). The method was designed to evaluate the impact of policies on large aggregates such as countries, states, or regions. Early applications assessed the impact of civil conflict on GDP in northern Spain (Abadie and Gardeazabal 2003), and the impact of tobacco legislation on cigarette consumption in California (Abadie, Diamond, and Hainmueller 2010). In public health, it has been used to assess the impact of national-level health system reforms, changes in legislation, and taxation among others (Bouttell et al. 2018). Two studies have used synthetic control to assess the impact on child mortality of democratic reforms (Pieters et al. 2016) and of trade liberalization (Barlow 2018).

In a typical synthetic control analysis, there is an intervention in a country or region (the "treatment unit") and we have observations for a long period of time before and after the intervention for the treated unit and for a set of comparison units. The idea behind synthetic control is that a weighted combination of comparison units is an appropriate counterfactual for the treated unit. Concretely, synthetic control uses preintervention information on the determinants of the outcome, including the outcome itself, and assigns weights to the comparison units giving more weight to those units that are more similar to the treatment unit. We do not describe here the statistical model and its properties, which the reader can find in Abadie (2021). For reference, the treatment effect at time t is

$$\widehat{\tau_t} = Y_t - \sum_{j=1}^{J} w_j Y_{jt}, \tag{3.1}$$

in which Y is the outcome of interest and ws are weights assigned to j comparison units. The weights are obtained by minimizing the following expression:

$$\left(\sum_{h=1}^{k} v_h \left(X_h - w_j X_{hj} - \cdots - w_J X_{hJ} \right)^2 \right)^{\frac{1}{2}} \tag{3.2}$$

subject to the restriction that the weights are nonnegative and that they add to one. The additional weights $v_1, \ldots, v_k$ reflect the relative importance of the $X_1, \ldots, X_k$ predictors.

The results of synthetic control analysis are displayed in self-explanatory charts that illustrate the difference in the trends between the treatment unit and the synthetic control. Standard errors, and corresponding p-values and confidence intervals, cannot be calculated, and inference is obtained by conducting placebo tests. These tests simulate the impact of hypothetical interventions, which separately employ the synthetic control method in each comparison region. By estimating the impact of hypothetical interventions on the comparison, the researchers verify that the method is not misinterpreting random patterns as project effects. These falsification tests do not prove the validity of the estimates but increase their plausibility against alternative explanations.

In our empirical analysis, we also employ two novel methods that can be interpreted as extensions of the synthetic control approach. The first method is the synthetic difference-in-differences approach of Arkhangelky et al. (2021). This method integrates the synthetic control method with standard difference-in-differences analysis. Difference-in-differences and synthetic control were developed to address causality under different circumstances. Difference in differences calculates the impact of the intervention as the difference between the changes in the treatment and the comparison groups under the assumption that, in the absence of the intervention, the trends in the outcomes in the two groups would have been parallel in the postintervention period. Synthetic control was designed for those cases in which a parallel trend assumption does not hold, and the method compensates for the lack of parallel trends by re-weighting the comparison observations using pretreatment period information. Synthetic difference in differences estimates treatment effects by regression analysis, as in difference in differences, but after correcting for parallel trends by weighting, as in synthetic control analysis (Arkhangelsky et al. 2021). As for the synthetic control method, we refer the reader to the article by Arkhangelsky et al. (2021) for a description of the statistical model and of its properties. For reference, we report here the expression minimized by a fixed effects regression:

$$\sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta t - W_{it}\tau)^2 \omega_i \lambda t, \tag{3.3}$$

in which ωs and λs are weights for the covariates and for the time periods, respectively, while W is an indicator of treatment status, and τ is the estimated treatment effect.

The second extension of synthetic control analysis considered is the "causal impact" method of Brodersen et al. (2015). This method employs a Bayesian structural time series model for the estimation of a synthetic control. It uses the comparison units to predict a synthetic control time series and estimates the causal effect by calculating the difference between the time series of the treated unit and the time series of the synthetic control unit. Details of the statistical model and its properties can be found in Brodersen et al. (2015).

Intuitively, the method favors units that are more similar to the treatment unit and then computes the posterior distribution of the counterfactual time series given the values of the series of the treated units in the preintervention period. Importantly, the model assumes the invariance of the relationship between the covariates and the outcomes of the treated unit over the pre- and postintervention period. This requires that the covariates used in the model should not include factors that are affected by the intervention, which often limits the covariates to just the outcome of interest itself. Finally, like the synthetic difference-in-differences approach, causal impact calculates standard errors and confidence intervals, which is an advantage in comparison to the synthetic control method.

In our analysis, we do not use model-based mortality rates, such as those provided by the UN Interagency Group for Child Mortality Estimation. Instead, we calculate mortality rates from birth history data using the synthetic cohorts probability method (Rutstein and Rojas 2006) used by the DHS.[3] This method calculates mortality rates over a five-year period for the following groups: children less than 1 month, 1–2 months, 3–5 months, 6–11 months, 12–23 months, 24–35 months, 36–47 months, and 48–59 months. Combining data of several DHS surveys, we are able to calculate monthly five-year mortality rates, effectively building a time series consisting of a five-year moving average. There are two advantages to this approach. First, the use of a five-year interval for the calculation of mortality rates increases the precision of the estimates and addresses issues of statistical power in performing comparisons between groups. Second, the estimated rates are comparable to those commonly reported by national statistical offices and based on DHS data.

The synthetic cohort probability method calculates mortality rates for hypothetical ("synthetic") cohorts over a specific time period. For example, the calculation of the mortality rate for the children under-5 in the period 2010–2015 will include all children exposed to mortality risk during the period 2010–2015. Some of these children will have been born before 2010 and will enter the exposure window 2010–2015 only when they are older than one month. Since they are not exposed to mortality risk for a full five-year period, they are left censored. Some children will have been born after 2010 and therefore also exposed to mortality risk for less than five years. They are right censored.

Children included in the calculation of the mortality rate for a specific interval, therefore, fall into one of three categories: (a) those fully exposed, (b) those exiting the interval before being fully exposed (right censored), and (c) those entering the interval later in their age (left censored). To address censoring, the method assumes that only half of children in groups (b) and (c) are exposed to mortality risk. The denominator of the mortality rate is children exposed to the risk of dying (E), while the numerator is children deaths D, both adjusted for censoring. The mortality rate (mr) for each ($j = 1,\ldots,8$) group is

$$mr_j = \frac{D_j^a + D_j^b + \frac{D_j^c}{2}}{E_j^a + \frac{E_j^b}{2} + \frac{E_j^c}{2}}. \tag{3.4}$$

TABLE 1 Child mortality rates: definitions and formulae

Mortality rate	Definition	Formula
Infant mortality	Probability of dying between birth and first birthday	$1 - \prod_{j=1}^{4}(1 - mr_j)$
Child mortality	Probability of dying between first and fifth birthday	$1 - \prod_{j=5}^{8}(1 - mr_j)$
Under-5 mortality	Probability of dying between birth and fifth birthday	$1 - \prod_{j=1}^{8}(1 - mr_j)$

Mortality rates for different age cohorts are calculated subtracting from one the product of the survival probabilities ($1 - mr_j$) of the relevant age groups using the formulae in Table 1. The rate in (3.4) refers to a rate calculated at a given month over the preceding five years. We calculated mortality rates for each month over the period from 1989 to 2018 thus building time series of five-year mortality rates.

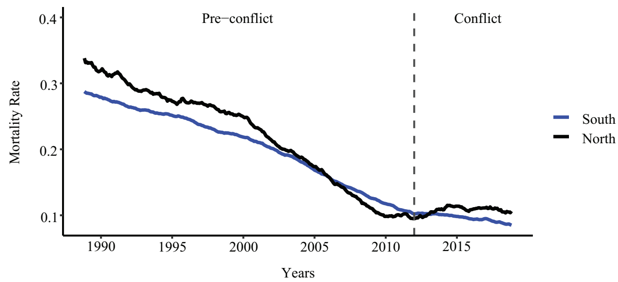
The data required for the calculation of these rates are found in birth histories. In birth histories, all women of reproductive age (15–49 years) report all births and deaths over their lifetimes. These data are reported with some error. Mothers may not recall all births and deaths or their exact dates, so that underreporting and age heaping (the preference for reporting specific ages such as, e.g., 6 months or 12 months) are common. Notwithstanding these limitations, mortality rates from birth histories are considered the best estimates of the true mortality rates (Hill 2013). We used data from the Malian DHS of 1995–1996, 2001, 2006, 2013–2013, and 2018 and calculated mortality rates for each of the nine regions in which the country is divided. We pooled the data from the five surveys and calculated monthly mortality rates for the 30-year period between 1988 and 2018.

Mortality rates calculated retrospectively are susceptible to some biases (Institute for Resource Development 1990; Pullum and Becker 2014). First, mothers tend to have a better recollection of births than deaths, particularly those occurring in the past. Omissions of deaths will cause mortality rates of the past to be artificially low. Second, infant mortality is positively correlated with mother's age, and average mothers' age increases as we use birth histories to calculate mortality rates 10 or 15 years before the interviews. However, pulling together data from overlapping surveys greatly obviates these issues. The overlap between birth histories of different surveys builds a sample that at any given time is balanced in terms of mother's age and other characteristics, and for which deaths omissions are less relevant.

Results

Northern Mali (including the regions of Timbuktu, Gao, and Kidal) is scarcely populated, comprising 65 percent of total land but only 9 percent of total population, and only 5 percent of its GDP. Nomadic and transhumant

FIGURE 3 Under-5 mortality rates of north and south compared



pastoralism are the main sources of income. People have no access to roads, electricity, or other modern facilities. Conversely, the southern regions are well connected by roads and have much better infrastructure. The south benefits from better rains, is covered by permanent vegetation, and households practice a more profitable and varied agriculture. All major Malian cities are in the south, as well as all best jobs in trade, administration, services, and manufacturing.

Differences in child mortality between the north and the south are not as large as other socioeconomic differences. Child mortality rates were higher in the north in the early 1990s but are similar today (Figure 3). Both north and south witnessed a sharp reduction in mortality rates over the past 30 years, although the reduction was faster in the north. Improvements in mortality rates are correlated with numerous programs to improve maternal and child health, such as the Expanded Program on Immunisation, the Roll Back Malaria program, the National Emergency, and Obstetric care program, and the increased use of community health workers to reach populations living in remote areas (Assaf et al. 2020). Maternal and child health interventions led to significant increases in the proportion of deliveries in health facilities, the proportion of children seeking care for respiratory diseases and fever, and to notable increases in access to antenatal and postnatal care.

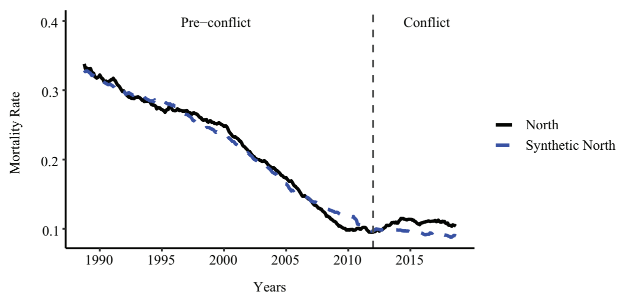
Although in recent times the Malian conflict spread from the north to southern regions of the country, such as Mopti and Segou, for the most part of the period from 2012 to 2018, the armed conflict was limited to the northern regions of Timbuktu, Gao, and Kidal. In our analysis, we jointly consider the three regions of the north as affected by conflict, and we use the remaining six regions of the south (Bamako, Mopti, Kayes, Koulikoro, Segou, and Sikasso) as comparison regions unaffected by conflict.

Trends in mortality rates in the north and south were not parallel before the conflict, and a simple difference-in-differences analysis would produce biased estimates. Synthetic control analysis on the other hand allows us to correct for these differences. We built our synthetic control group[4] by

TABLE 2 Average predictors of under-5 mortality in the preconflict period (1988–2011)

Variables	North	Synthetic north	South
Under-5 mortality	0.220	0.220	0.197
Drinking water	0.462	0.563	0.602
Safe sanitation	0.195	0.239	0.286
Mother illiteracy	0.861	0.869	0.846
Two tetanus vaccinations	0.389	0.358	0.411
Full vaccination cycle	0275	0.252	0.298

FIGURE 4 Under-5 mortality: synthetic control analysis



minimizing the differences between north and south using the following variables: the under-5 mortality rate, the proportion of mothers without education, the proportion of households with access to safe drinking water, the proportion of households with access to safe sanitation, the proportion of children who received a full cycle of vaccinations, and the proportion of mothers that received two tetanus vaccinations. To select these variables, among other potential predictors, we built an algorithm that predicted mortality trends in the preconflict period. To do so, we split the period from 1998 to 2011 in two. We then estimated a synthetic control for the first half of the period and predicted mortality rates in the second half. We repeated this exercise for all possible combinations of 12 potential predictors. Finally, we selected the set of predictors that produced the smallest mean squared error when predicting the outcomes in the second half of the sample.

The synthetic control algorithm built a comparison region using the following regions with relative weights in brackets: Mopti (62 percent), Segou (22 percent), Bamako (15 percent), and Sikasso (1 percent). Table 2 shows the average values of the predictors in the preconflict period in the north, in the synthetic north, and in the south. The algorithm improved the balance of the covariates, particularly mortality rates and vaccination rates, and the mean values of the variables in the north are more similar to those of the synthetic north than to those of the south.

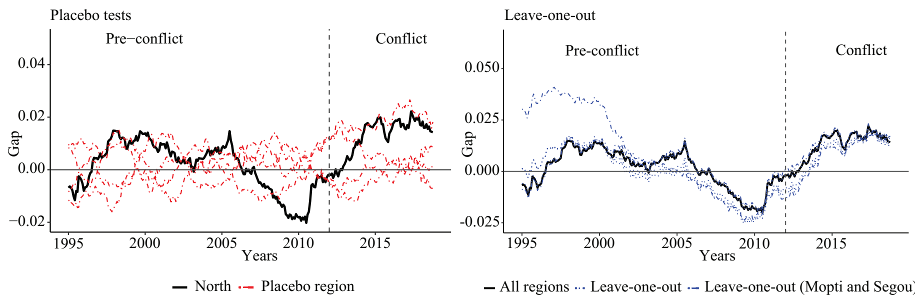
Mortality trends of the synthetic north and of the actual north are shown in Figure 4. The synthetic control tracks mortality rates of the north

TABLE 3 Impact of conflict on under-5 mortality

	Synthetic control	Synthetic difference in difference	Causal impact
Average effect	0.0127	0.0196**	0.0254***
Standard error		(0.0077)	(0.0064)
Confidence interval		[0.0044, 0.037]	[0.0128, 0.038]
Root mean square prediction error	0.009	0.007	0.003

***is statistical significance at 1%, ** is statistical significance at 5%, and *is 10%.

FIGURE 5 Inference and robustness analysis

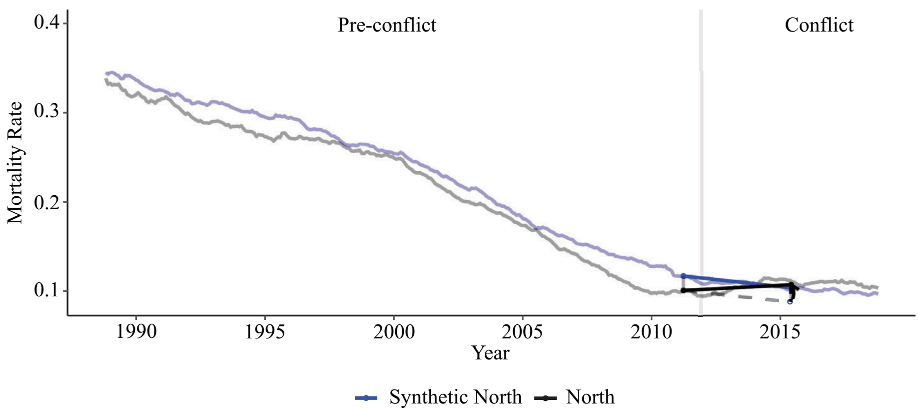


very well in the preconflict period. There is a visible difference in mortality rates after conflict, which on average is equal to 1.3 percent points (see Table 3).

Could this impact be entirely driven by chance? In order to assess the plausibility of this result, we conducted a series of placebo tests. In placebo tests, we estimate the impact of conflict in unaffected regions. We do not expect conflict to have an impact on unaffected regions. If indeed we do not find a significant impact of conflict in unaffected regions, we can be more confident that the results found in the north are not spurious. Conversely, if we detect an impact of conflict in an unaffected region (which is similar in magnitude to the impact estimated in the north) then we should consider that the impact observed in the north might be the result of random variation.

To present the results of multiple placebo tests, it is convenient to calculate the treatment effect as the difference between the mortality rate in the treated region and the mortality rate in the synthetic control. These differences are displayed for all regions in Figure 5. When mortality rates in the treatment unit and synthetic control are very similar, the differences are close to zero and run along the horizontal line. In the chart, the difference for the north is displayed in black bold, whereas differences for the other regions are in dashed red. As expected, the difference in the northern region is larger than those of any other region. However, there is one comparison region (the region of Kayes, located in the western part of the country), with a difference comparable in magnitude to the difference of the north.

FIGURE 6 Under-5 mortality: synthetic difference in differences



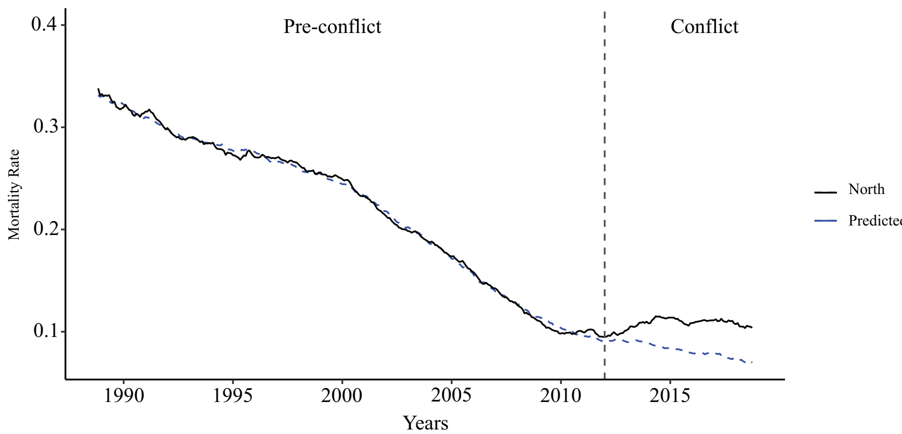
This should caution against interpreting the results as conclusive about the impact of conflict on mortality.

Abadie, Diamond, and Hainmueller (2010, 2015) use the results of the placebo tests to calculate a p-value of the hypothesis that the observed effect is the result of chance. The p-value is simply the fraction of placebo effects that are larger or equal to the estimated effect. In our case, where we run six placebo tests (one for each region), and we find one positive placebo effect, the p-value would be equal to 0.33. This p-value, however, has limited meaning with reference to a standard level of statistical significance given the small number of regions available to run placebo tests.

The right chart in Figure 5 illustrates a robustness analysis of the results. In general, we would expect the results not to change significantly with the inclusion or exclusion of one of the regions. In the chart, we illustrate the result of leaving out one region at a time when estimating the synthetic control (dotted lines). The dashed lines were obtained after removing from the sample either of the regions with the highest weight (Mopti and Segou) that were more influential in building the synthetic control. The results do not differ significantly when we leave out one of the regions, including leaving out one of the most influential regions.

The chart in Figure 6 illustrates the impact of conflict estimated using the synthetic difference-in-differences method. Recall that synthetic difference in differences employs two types of weights. The first set of weights operates in the same way as the synthetic control weights and builds a synthetic control whose preconflict mortality trend is as similar as possible to the trend observed in the treatment unit. The second set of weights defines the relative importance of the preconflict time period considered, giving more weight to time periods that are more similar and closer to the intervention period. The method built the synthetic control using just two regions: Mopti (with a weight of 59 percent) and Segou (weight of 41 percent) and

FIGURE 7 Under-5 mortality: causal impact



giving relatively high weights to months close to the start of conflict. The weights minimizing the expression in (3.3), similarly to those found by the synthetic control algorithm in (3.2), are set to be nonnegative and to sum to one, which typically results in some weights being set to zero. The sparsity of the synthetic controls is considered an advantage of both approaches that allows refining the interpretation of the estimates (Abadie 2021). For example, the heavy reliance of the synthetic control on the Mopti region, which is bordering the north, and was presumably indirectly affected by the conflict, would suggest that the obtained estimates should be considered as a lower bound of the true values.

The chart in Figure 6 illustrates how the difference in differences is obtained. The estimated average impact of conflict on mortality is reported in Table 3, and it is equal to about 2 percent points. This method is regression based and allows the estimation of standard errors. The estimated impact is statistically significant at the conventional 5 percent.

Lastly, we estimate the impact of conflict using the causal impact method. Recall that causal impact uses a Bayesian time series model to predict the mortality trend in the north in the absence of conflict using data from the unaffected regions. The predicted and the observed mortality trends of the north are shown in Figure 7. Causal impact is the algorithm that produced the best preconflict predictions. This is clearly visible from the chart of Figure 7 and from the value of the root mean square error reported in Table 3. It is also the method that finds the largest impact of conflict on mortality. It estimated an average effect of conflict on mortality of 2.5 percent points, statistically significant at 1 percent (Table 3).

The three methods concur in finding an impact of conflict on under-5 mortality in a range between 1.3 and 2.5 percent points. In the north, about 10 children out of 100 would not reach age five at the time hostilities broke out. This number increased to 12 children during the conflict period. To get an idea of what this means in terms of absolute numbers, consider that according to the 2009 census there were about 1.2 million people living in northern Mali. Of these, given the age population structure reported by the DHS data, we estimate that about 43,000 children were born in any given year. Our estimates indicate that after the conflict, of all children born in a given year, about 4,700 die before reaching age five, and that between 550 and 1,100 of these deaths can be considered as indirect deaths caused by the war.

We conducted a sensitivity analysis to assess whether the results are robust to selection bias brought about by population displacement and to errors in the measurement of mortality. Anecdotally, large displacements of population were reported at the start of the conflict (World Bank 2016). Hoogeven et al. (2018) found that agricultural households, representing the vast majority of the population, were unlikely to abandon their home, but that some small-business owners migrated to the south in some cases without making return. A movement of population from the north to the south would bias the impact of conflict on mortality downwards because the fleeing population was presumably negatively affected by conflict and originated from a disadvantaged socioeconomic background.

In order to assess the size of this potential bias we removed from the sample all mothers that at the time of the 2018 survey had been residing in the south for less than seven years. In this way, we remove from the sample all women that moved to the south after the conflict broke out in 2012. After removing these mothers (8.2 percent of the 2018 sample), we find a larger impact of conflict on child mortality although by a small magnitude (see Table A1 in the online Appendix).

Mortality rates were calculated using birth histories collected through mothers' interviews. As mothers are recalling past events, there is a risk they omit births or deaths as the recall time gets longer, although the direction of the bias on mortality rates is unclear (Institute for Resource Development 1990). Recall bias should equally affect observations in the north and in the south, and we would expect it to affect the absolute mortality levels at any time but not the comparison of mortality rates across groups. This, however, is not the case. In our sample, the removal of records based on recall periods of 20 or 15 years, leads not only to a reduction of mortality estimates in the preconflict period but also to a reduction in the estimate of the impact of conflict on mortality in the conflict period. This occurs because removing deaths based on a long recall flattens the mortality trends in the preconflict period, and the predicted counterfactual trends in the conflict period. This suggests that, after controlling for recall error, the true impact of conflict on mortality could be lower than estimated in Table 3 and closer to the estimates in Table A1 in the online Appendix.

Mechanisms of impact

Synthetic control methods are unable to analyze the mechanisms of impacts. One obvious question to ask is whether the increase in mortality in conflict areas was the result of economic factors, such as poverty and undernutrition, or of health factors, such as mothers' health and access to services. We start answering this question by separately estimating impact on mortality of infants (months 0–11) and of children (months 12–59). Infant and child mortality have different determinants. Infant mortality is associated with poor mothers' health, and with insufficient antenatal and postnatal care, whereas child mortality is normally associated with poor living standards and limited access to health care.

Conflict had a limited impact on infant mortality and a comparatively larger impact on child mortality (see Table 4; also Figures A1, A2, and A3 in the online Appendix). The impact on child mortality was larger in absolute terms as well as in relative terms since preconflict infant mortality rates were higher than child mortality rates. This suggests that the Malian conflict had a limited impact on mortality via mother's health and antenatal and postnatal care, and that a deterioration in living standards and in access to health services played a bigger role.

We further investigate the factors behind the rise in mortality rates with a difference-in-differences analysis of mortality determinants in the north and in the south. To guide our search we modify the classical Mosley–Chen framework (Mosley and Chen 2003) for understanding child mortality. The last three rows of the panel in Figure 8 simply reproduce the Mosley–Chen framework, which conceptualizes the proximate determinants of mortality as consisting of injury (accidental and intentional), undernutrition (calories and micronutrients), environmental contamination (via fingers, water, food, and insects), and maternal factors (such as age, parity, and birth interval). We include in the framework determinant factors identified in the conflict literature (top row in the chart): violence, poverty, destruction of infrastructure, displacements, and disruption of health services.

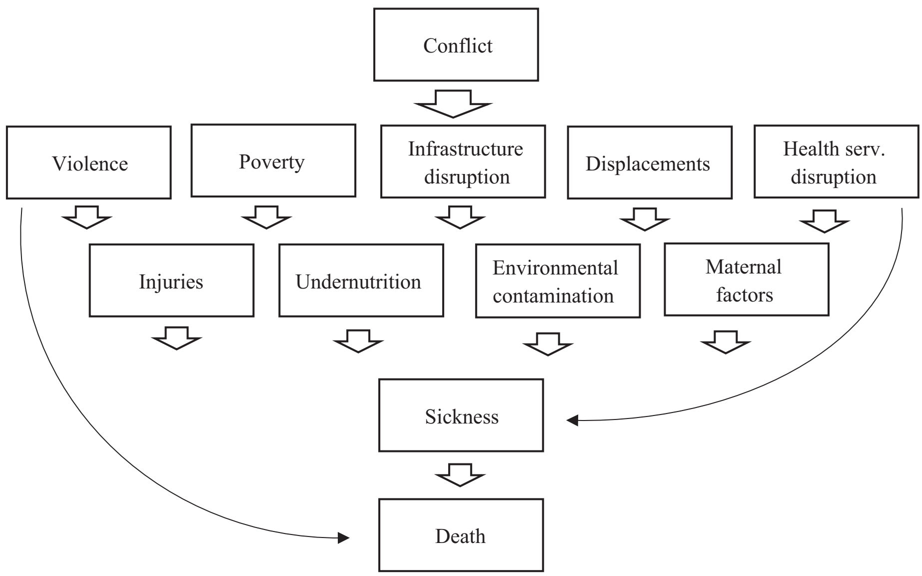
Violence is the immediate outcome of conflict, which occurs through attacks against civilians or accidental casualties. Conflict increases poverty by weakening food production systems, and by disrupting markets (Justino 2012). This in turn affects livelihoods, access to food, and leads to undernutrition. The latter increases children's vulnerability to diseases and mothers' health during pregnancy and lactation (Guha-Sapir and van Panhuis 2004). Conflict causes the destruction of infrastructure such as water facilities, sewerage systems, roads, and electricity. Damages to water and sewerage facilities contaminate the environment, and most indirect deaths during conflict are indeed attributed to diseases such as acute respiratory infections, measles, diarrhea, and malaria (Coghlan et al. 2006; Human Security Report

TABLE 4 Impact of conflict on infant and child mortality

	Preconflict (2007–2011)North	Preconflict (2007–2011)South	Synthetic control	Synthetic difference in difference	Causal impact
Infant mortality	0.0711	0.0700			
Average effect			0.0032	0.0033	0.0044
Standard error				(0.0024)	(0.0034)
Child mortality	0.0562	0.0444			
Average effect			0.0052	0.0125*	0.0157**
Standard error				(0.0069)	(0.0062)

***is statistical significance at 1%, **is statistical significance at 5%, and *is 10%.

FIGURE 8 Impact of conflict on child mortality

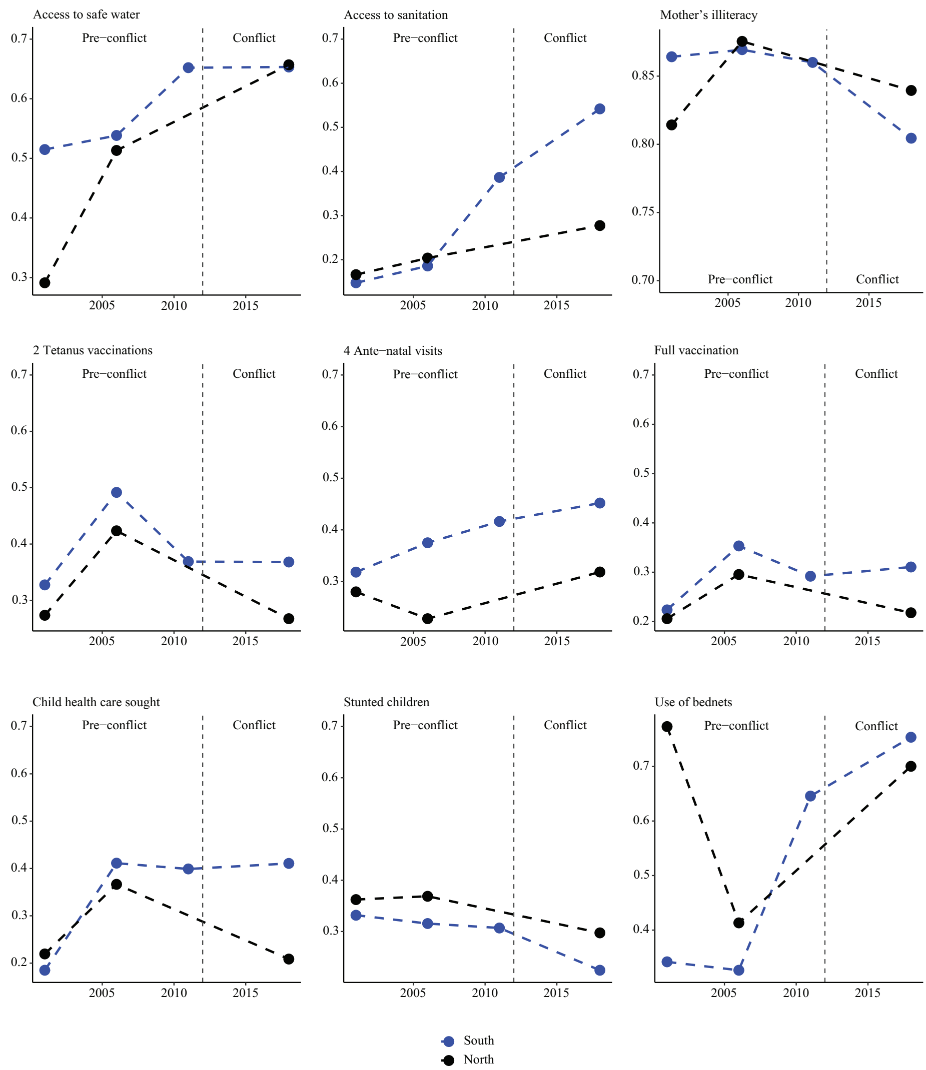


Project 2011; Herp et al. 2003). The displacement of population can spread new pathogens in host populations, and if refugee camps are overcrowded, infections easily spread (Altare and Guha-Sapir 2014). Finally, looting of facilities, destruction of health infrastructure, and staffing difficulties, disrupt health services and access to health care, see in this regard evidence from Cote d'Ivoire (Ouili 2017), Uganda and Burundi (Chi et al. 2015), and from multiple countries in systematic reviews (Keasley, Blickwedel, and Quenby 2017; Black et al. 2014).

Violence did not play a big role in Mali as relatively few civilian deaths were reported. Infrastructure was affected, but the conditions of roads and of water and sanitation facilities were already poor before the conflict. Large displacements of population were reported, but the numbers are difficult to verify. There were reports of looting of health facilities and of abandonment of health centers by staff (World Bank 2016). Finally, there were reports of damage to agricultural production and of disruption of livelihoods (Kimenyi et al. 2014). Access to health care and poverty, therefore, appear to be more likely candidates to explain the increase in mortality.

The DHS data contain information on several proximate determinants of child mortality such as the proportion of households with access to safe drinking water, the proportion of households with access to safe sanitation, the proportion of mothers who have never been to school, average mothers' body mass index, the percentage of children under-5 that are stunted, the percentage of mothers receiving two anti-tetanus vaccinations during pregnancy, the percentage of mothers attending at least four antenatal

FIGURE 9 Trends of mortality determinants



visits, the proportion of households whose all children slept under a mosquito bednet the night before the survey visit, the percentage of mothers who sought health care for children with cough or fever during the previous two weeks (excluding traditional healers and shops), and the percentage of children that had ever been vaccinated. The charts in Figure 9 offer a graphical comparative illustration of the trends in the determinants in the north and in the south. The charts suggest a deterioration in some of the indicators during the conflict period.

We employ a difference-in-differences analysis to assess the impact of conflict on mortality determinants and we use this assessment to infer the mechanisms by which conflict increased child mortality. For each mortality determinant we estimate the following model:

$$y_{irt} = \alpha + \sum_{k=1}^{6} \beta_k REGION_{kr} + \sum_{j=1}^{2} \gamma_j ROUND_{jt} + \partial D_{rt} + \sum_{m=1}^{n} \theta_m Z_{irt} + \varepsilon_{irt} \quad (5.1)$$

in which y is the mortality determinant (e.g., access to safe sanitation) for mother i in region r and at survey round t. The β_k coefficients control for time-invariant differences between regions, of which we include five in addition to the northern region (Kayes is used as the reference region). The γ_j coefficients control for time effects that are common to all regions. We include only three survey rounds (2001, 2006, and 2018) because the survey round of 1996 coded several of the outcome variables in a different way from successive surveys, and the survey round of 2012 did not collect data in the northern region (we use the 2001 round as reference). The coefficients θ_m control for the effects of individual characteristics affecting the outcomes. We include in Z the place of residence of the mother (whether urban or rural), mother's age, household size, mother's literacy, whether the mother is working or farming, and whether the mother is heading the household. Finally, D_{rt} is equal to one for all mothers in the north during the conflict period, and ∂ measures the impact of conflict on the outcome.

The difference-in-differences effects estimated by model (5.1) are valid as long as the assumption of parallel trends in the outcomes between regions is tenable. If the trends in the outcomes before the conflict period were different in the various regions, then attributing the observed difference-in-difference effects to conflict alone is not correct. We probe the plausibility of the parallel trends assumption for each estimated outcome by augmenting model (5.1) to include region-specific trends (Angrist and Pischke 2015). To do so, we include interactions between all regions and a time index, and we conduct an F-test of the hypothesis that all the coefficients of the region-specific trends are jointly equal to zero (Wing, Simon, and Bello-Gomez 2018).

Five of the 10 mortality determinant considered in our difference-in-difference analysis fail to pass the parallel trends test (see Table 5), which implies that the observed effects are likely to be affected by other factors in addition to conflict. We decide to focus our discussion on the estimates that do not violate the parallel trends assumption. We find that conflict negatively affected access to safe sanitation and child vaccination rates. It did not affect nutritional indicators, such as mothers' Body mass index (BMI) and child stunting, and antenatal tetanus vaccinations.

These results are in agreement with the findings of Ataullahjan et al. (2020) on the coverage of maternal and child health interventions during the Malian conflict. Using survey data of the Mali's National Evaluation Platform, they find that antenatal care, deliveries in health facilities, and deliveries assisted by skilled health professional increased in the conflict

TABLE 5 Changes in determinants of child mortality

	Difference-in-difference effect2001–2018[a]	Test of parallel trends[b]
Access to safe water	0.161**	12.04**
	(0.039)	(0.006)
Access to safe sanitation	–0.261**	0.24
	(0.024)	(0.632)
Mother illiteracy	–0.004**	14.43**
	(0.012)	(0.004)
Mother's BMI	–8.079	1.72
	(5.231)	(0.219)
Stunting among under-5	0.012	0.48
	(0.016)	(0.503)
Two tetanus vaccinations	–0.021	0.08
	(0.031)	(0.790)
At least four antenatal visits	0.002	14.72**
	(0.021)	(0.003)
All children sleeping under a bednet	–0.279***	45.71***
	(0.055)	(0.000)
Health seeking for child cough	–0.181**	7.52**
	(0.031)	(0.021)
Child ever vaccinated	–0.080*	0.00
	(0.041)	(0.973)

[a] Coefficient δ of model (5.1), region-specific clustered standard errors in parentheses.
[b] F-test of the null hypothesis that the region-specific time trends are jointly equal to zero, p-value in parentheses.
***is statistical significance at 1%, ** is statistical significance at 5%, and *is 10%.

regions, possibly because of an influx of displaced populations from rural and remote areas to urban centers. They also find a reduction in vaccine coverage in conflict areas and observe that although there was no change in aid funding of maternal and child health programs, large resources were made immediately available for child nutrition. Access to safe sanitation appears to be the mortality determinant most affected by the Malian conflict. Internal displacements and the ensuing overcrowding and lack of access to facilities are a major source of diarrhea, cholera, and parasitic infections (Als et al. 2020). In Mali, the incidence of diarrheal diseases reaches a peak in the age group between 6 and 35 months (INSTAT 2019), which could explain why child mortality was relatively more affected than infant mortality.

Conclusions

We estimated the impact of the Malian civil conflict on child mortality. At the start of conflict in 2012, the under-5 mortality rate in northern Mali was about 10 for every 100 children. We estimated that conflict increased this mortality rate to about 12 for every 100 children, and that out of 4,700 deaths among children born in a given year, between 550 and 1,100 were "excess deaths" caused by conflict. The impact was large on child mortality but negligible on infant mortality.

A difference-in-difference analysis showed that antenatal care did not deteriorate in the north during conflict, which is consistent with the absence of impact on infant mortality. We found no effect of conflict on nutritional indicators, which is consistent with reports of a large influx of food aid in the area (Ataullahjan et al. 2020). The deterioration in access to safe sanitation, resulting from displacements, and a reduction in vaccination rates appear to be the most likely determinants of the observed increase in under-5 mortality.

Beyond the estimation of excess deaths caused by conflict, our work makes two methodological contributions. First, we use three novel estimation methods based on synthetic control analysis and compare the results. Synthetic difference in differences and causal impact produced larger estimates than synthetic control. They also performed better in tracking mortality trends of the treated unit in the preconflict period. The root mean square error, our measure of prediction accuracy, was lower in the synthetic difference-in-differences method, and lower still in the causal impact method. These two methods have also the additional advantage of allowing the calculations of standard errors and corresponding confidence intervals and p-values, which many researchers would find appealing. On the other hand, the synthetic control method relies on conducting falsification tests for inference. These tests have the advantage of forcing researchers to inspect and analyze single cases, which may lead to further hypotheses or lines of inquiry.

Our second methodological contribution consists of developing a new approach to the calculation of time series of mortality rates. We show how to combine multiple cross-sectional survey rounds to build monthly series of mortality rates based on large and balanced samples. The time series can then be used to assess the impact of large-scale policies or events such as natural disasters, epidemics, and economic crises. Multiple rounds of DHS data are available for most countries and for long periods of time so that the opportunities to analyze the impact of policies at the national or regional level following the example of this paper are many.

Our study has some limitations. First, synthetic control methods are quasi-experimental. Their results are suggestive but offer no definitive proof of impact. Conflict does not affect countries by chance. Regions of northern Mali might have special characteristics correlated with conflict, which are also correlated with the levels and the trends in mortality rates. Synthetic controls use observed characteristics to build a valid comparison group but do not remove this potential bias entirely. Second, the analysis was conducted using only six comparator regions. A further spatial disaggregation of the data was not possible without compromising the accuracy of the mortality rates, and DHS data from neighboring countries of Burkina Faso and Niger were not available. The small pool of comparator regions does not compromise the construction of a valid synthetic control as this is frequently composed of a weighted average of only few observations within the pool, but it prevents the calculation of *p*-values when conducting placebo tests as it is customary in the literature. Third, the study does not conclusively estimate the true impact of civil conflict on mortality. Northern Mali was the beneficiary of emergency aid, which likely reduced the impact that conflict would have otherwise had on mortality. In addition, the effects of conflict likely extended, at least indirectly, to some comparison regions, for example through a reduction in health spending or service provision. The results obtained should therefore be considered as lower bounds of the true effects.

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Notes

1 The map was downloaded from Political Geography Now (www.polgeonow.com.). Modified from Wikimedia map by Orionist, incorporating images by Carport and Nord-NordWes.

2 See WHO. Global Health Observatory data depository: Child mortality database. 2015. https://apps.who.int/gho/data/node.main.ChildMort?lang=en of infant mortality (first year of life), whereas malnutrition, contaminated water, incomplete vaccinations, and parental neglect are the main causes of child mortality (years 1 to 4).

3 Mortality rates were calculated in Stata using the SYNCMRATES package (Masset 2016). See also Elkasabi (2019) for a more detailed explanation of the synthetic cohort probability method, and for an application using the R package.

4 All estimations were conducted in R. We used the "Synth" package for the synthetic control approach (https://cran.r-project.org/web/packages/Synth/Synth.pdf), the "synthdid" package for the synthetic difference in differences method (https://synth-inference.github.io/synthdid/), and the "CausalImpact" package for the causal impact method (https://google.github.io/CausalImpact/CausalImpact.html).

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