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Essays in Climate and Development Economics

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Résumé

Dans ces trois chapitres, j'étudie la relation entre les chocs météorologiques, les catastrophes naturelles et des facteurs socio-démographiques dans une variété de contextes. Au chapitre 1, mes coauteurs et moi-même examinons l'impact des chocs de précipitations sur l'émigration en Turquie. Nous constatons une augmentation de l'exode rural après un choc de précipitations. Nous trouvons des preuves que ce phénomène est canalisé par le revenu, et que la spécialisation agricole d'une province joue également un rôle dans l'effet de canalisation du revenu agricole. Au chapitre 2, j'examine de quelle manière l'exposition aux sécheresses en Afrique peut augmenter le nombre d'enfants et les violences entre partenaires intimes. En utilisant une approche de pseudo-panel basée sur l'âge et le lieu de résidence, je montre que l'exposition à la sécheresse augmente la probabilité que les femmes aient un nouvel enfant et soient exposées à des violences sexuelles et physiques. Je montre également que l'utilisation de la contraception peut réduire l'augmentation du nombre de nouvel enfant, et que ces effets sont durables. Au chapitre 3, j'examine comment les chocs de catastrophes naturelles extrêmes peuvent réduire l'intensité des conflits. En utilisant des données provenant de 42 pays africains, je constate que les chocs de catastrophes naturelles extrêmes réduisent principalement l'intensité des conflits impliquant des gouvernements. De plus, je montre que les catastrophes naturelles extrêmes réduisent les violences létales et sexuelles contre les civils. Ces chapitres démontrent l'interaction entre les chocs météorologiques, les catastrophes naturelles et les facteurs socio-démographiques, soulignant la nécessité de politiques climatiques robustes et de recherches supplémentaires.

Mots-clés: émigration; changement climatique; précipitations; urbanisation; revenu par habitant; agriculture; conflits; fertilité; catastrophes naturelles; sécheresses; contraception; violence interpersonnelle; violence sexuelle; violence unilatérale; violence sexuelle liée aux conflits; méthodes de contrôle synthétique; adoption échelonnée.

JEL classification: D74; I14; I15; J12; J13; J16; O15; Q54

Summary

In these three chapters, I examine the relationship between weather shocks, natural disasters, and socio-demographic factors in different contexts. In Chapter 1, co-authors and I investigate the impact of rainfall shocks on out-migration in Türkiye. We find that rural out-migration increases after precipitation shock. We find evidence that the channel through which this operates is income. In addition, the type of agriculture practiced in a province plays a role in the agricultural income channeling effect. In Chapter 2, I examine how exposure to droughts in Africa may increase the probability of women having a child and experiencing intimate violence. Using a pseudo-panel approach based on age and living area, I show that exposure to drought increases the probability of women having a new child and being exposed to sexual and physical intimate violence. I also show that the use of contraception can mitigate the increase in new births, and that these effects persist over time. In Chapter 3, I examine how extreme natural disaster shocks can reduce the intensity of conflicts. Using data from 42 African countries, I find that extreme natural disaster shocks predominantly decrease conflict intensity in cases where governments are involved. Furthermore, I show that extreme natural disasters diminish lethal and sexual violence against civilians. These chapters demonstrate the interplay between weather shocks, natural disasters, and socio-demographic factors, highlighting the need for robust climate policies and additional research.

Keywords: out-migration; climate change; rainfall; urbanization; per capita income; agriculture; conflicts; fertility; natural disasters; droughts; contraception; interpersonal violence; sexual violence; one-sided violence; conflict-related sexual violence; synthetic control methods; staggered adoption.

JEL classification: D74; I14; I15; J12; J13; J16; O15; Q54

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Introduction

As climate change continues to alter weather patterns and increase the frequency and severity of natural disasters, it is crucial to understand the impacts on society. Adaptation is necessary at all levels, from policymakers creating resilient and sustainable policies to households finding ways to adapt. Research plays a vital role in mitigating the effects of climate change and developing effective adaptation strategies, particularly in understanding how households adapt. This knowledge can inform the development of more effective climate policies and guide us to an optimal response. The continued study of the effects of climate change on society will provide valuable insights and guide us toward a more resilient future.

The responses and adaptations of households are often considered in terms of their preferences, or utility functions. A common approach is to consider households that use natural capital and labor to produce output.[1] In this approach, shocks to natural capital can affect outcomes and expectations for future consumption, potentially leading to household adaptations. These adaptations can be voluntary or involuntary and can include factors such as migration, fertility rates, and even violence and conflict.

The analysis of household adaptation patterns can provide insight into the next decades in terms of demography, economics, and conflicts. These patterns can also be placed in a broader context by considering households' consumption patterns, which are a fundamental element in forecasting the impacts of climate change. This knowledge can be incorporated into larger bodies of research to help develop more precise models and improve our understanding of future paths.

The use of econometrics to study the impact of weather and climate variations on socio-economic outcomes, such as migration (Beine and Parsons, 2015), demographic trends (Brueckner and Schwandt, 2015), and conflicts (Burke et al., 2015), has grown in popularity in recent

[1]For a detailed analysis on the definitions of household and adaptation in the literature, see Carman and Zint (2020).

decades. One major challenge in this field is identifying the specific impact of climate. To address this, researchers have employed cross-sectional approaches that compare regions with different climates while controlling for confounding factors (for instance Mendelsohn et al., 1994). More recent studies tend to use panel data and fixed effects to identify random weather variations as a way to identify the impact of climate on adaptation.[2] However, as recently shown by Mérel et al. (2022), the theoretical framework for identifying the impact of climate through random weather variations is still evolving.

In my doctoral dissertation, I explore the impact of weather variations and natural disasters on society dynamics, such as migration, fertility rates, and violence. The findings of my research help to define sustainable policies that take into account the possible outcomes of an increase in weather variations and natural disasters in the future.

In Chapter 1, my co-authors and I examine the relationship between rainfall shocks and out-migration in rural regions of Türkiye. By using a mediated-moderator strategy, we are able to identify the direct and indirect effects of rainfall shocks on out-migration and explore how these effects are mediated through the GDP, agricultural GDP, and intensity of conflicts.

Our findings indicate that negative rainfall shocks lead to an increase in out-migration in rural Turkish provinces. This impact is partially driven by GDP per capita, and we find evidence that the effect of rainfall shocks on out-migration is channeled differently through the agricultural GDP per capita depending on the type and sensitivity of agricultural production in the province. This research shows that an increase in the frequency of weather shocks can alter population trends in even less vulnerable, middle-income countries, potentially leading to a decrease in the population density of rural areas.

In Chapter 2, my research focuses on the impact of droughts on fertility and intimate violence in African countries. Using data from 16 countries, I investigate how increased exposure to drought can affect the probability that women will have a new child and how this impact can be mitigated by using contraception. Additionally, I examine how exposure to drought can increase the risk of physical and sexual violence from intimate partners.

The findings of this chapter highlight the significant impact of drought on women's reproductive choices and experiences of violence. My analysis shows that increased drought exposure leads to a higher likelihood of women having a new child and that this can be reduced through

[2] Works such as Burke et al. (2015); Hsiang (2016); Cattaneo and Peri (2016) provide in-depth discussions on the advantages and disadvantages of these various identification strategies.

the use of contraception. The relationship between weather shocks and fertility remains a topic of ongoing debate, and this chapter offers new evidence on fertility patterns in African countries. Furthermore, increased exposure to drought also increases the risk of physical and sexual violence from intimate partners. These effects are long-lasting, with impacts lasting up to ten years. This chapter brings quantitative analysis to a previously understudied area, highlighting the connections between fertility dynamics and domestic violence in the face of weather stressors. These results provide valuable insights into the complex relationship between natural disasters and social dynamics, emphasizing the need for gender-sensitive policies in disaster management.

In Chapter 3, I explore the relationship between extreme natural disasters and the intensity of conflicts in Africa. Using partially pooled synthetic control methods, I analyze the impact of natural disaster shocks on the intensity of conflict and other conflict-related outcomes, such as one-sided fatalities and sexual violence.

The results of this chapter demonstrate that extreme natural disasters can have a mitigating effect on the intensity of conflicts, and this decrease persists for the medium-term period. Additionally, I find that extreme natural disasters can lead to a decrease in one-sided fatalities and sexual violence in conflict situations. I also show that synthetic control methods are robust in the analysis of large-scale natural disasters. This chapter combines the literature of two fields by using synthetic control methods to examine the impact of extreme weather shocks on the intensity of conflict and sheds light on the under-researched relationship between weather shocks and violence against civilians in conflict settings. These findings enhance our understanding of the complex relationship between climate change and conflict, and the potential for policy intervention to mitigate negative outcomes.

This thesis offers new insights into the impact of weather shocks on various aspects of society. The three chapters suggest that effective policies for addressing natural disasters must be tailored to the various contexts and needs of countries and regions. This requires a deep understanding of the specific challenges and vulnerabilities that different communities face, and the ways in which these are shaped. Furthermore, the chapters emphasize the significance of taking into account context, inequality and gender when designing policies to mitigate the negative impacts of natural disasters. By recognizing how disasters and weather changes disproportionately affect different groups and how these inequalities are perpetuated by broader societal and economic structures, policymakers can create more effective and sustainable policies in the short and long term. Overall, the thesis suggests that by considering context, inequality, and gender, policies can be more responsive to the needs of different communities and regions.

Chapter 1

Impacts of rainfall shocks on out-migration are moderated more by per capita income than by agricultural output in Türkiye

This chapter is mainly based on a working paper co-authored by Bruno Lanz (University of Neuchâtel, ETH Zürich and Massachusetts Institute of Technology,), Amir H. Delju (World Meteorological Organization), Etienne Piguet (University of Neuchâtel) and Martine Rebetez (University of Neuchâtel and WSL Swiss Federal Institute for Forest, Snow and Landscape Research). For helpful comments and suggestions, we thank Cristina Cattaneo, Fabien Cottier, Katti Millock, and the participants at various conferences. We also thank Nur Söğütçüklü from the Turkish State Meteorological Service for his assistance with the data. Excellent research assistance was provided by Alessio Lombini. Financial support from the Swiss National Science Foundation under grant number 100018_182122 is gratefully acknowledged.

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Abstract:

Rural populations are particularly exposed to increasing weather variability, notably through agriculture. In this paper, we exploit longitudinal data for Turkish provinces from 2008 to 2018 together with precipitation records over more than 30 years to quantify how variability in a standardized precipitation index (SPI) affects out-migration as an adaptation mechanism. Doing so, we document the role of three potential causal channels: per capita income, agricultural output, and local conflicts. Our results show that negative SPI shocks (droughts) are associated with higher out-migration in rural provinces. A mediated-moderator approach further suggests that changes in per capita income account for more than one quarter of the direct effect of droughts on out-migration, whereas agricultural output is only relevant for provinces in the upper quartile of crop production. Finally, we find evidence that local conflict fatalities increase with drought and trigger out-migration, although this channel is distinct from the direct effect of SPI shocks on out-migration.

Keywords: out-migration; climate change; rainfall; urbanization; per capita income; agriculture; conflicts.

1.1 Introduction

Ongoing changes in the climate system are responsible for an increased frequency of extreme weather events (IPCC, 2021). Because human societies fundamentally rely on climate to sustain themselves, formulating adaptation policies requires understanding how local shocks affect population dynamics. In particular, results from interdisciplinary research at the farm level suggest that extreme weather events are a key detrimental determinant of agricultural yields (e.g., Schlenker and Roberts, 2009; Burke and Lobell, 2010).[1] In turn, societies with a predominantly rural population who rely on agriculture for subsistence and income are more exposed to increasing weather variability, and understanding adaptation mechanisms in these regions is highly policy relevant.

In this paper, we focus on out-migration as an adaptation margin (Boas et al., 2019; Borderon et al., 2019; Hoffmann et al., 2020; Call and Gray, 2020), and provide novel evidence on how random deviations from long-run precipitation patterns act as a push factor in individual migration decisions. Importantly, migration patterns are known to differ across a rural to urban dimension (see S.Barrios et al., 2006), and we quantify how the impact of rainfall shocks differs among rural, transitional, and urban regions. We refer to such local conditions, encompassing economic and social factors, as having a *moderating* role in the relationship between weather realizations and out-migration decisions.

In addition, we document the relevance of alternative causal channels through which random rainfall shocks affect out-migration.[2] We refer to the factors that account for the relation between rainfall shocks and out-migration as *mediating* variables (or simply *mediators*). In this paper, we focus on the role of three mediating variables. The first is per capita GDP (Beine and Parsons, 2015; Mastrorillo et al., 2016), which captures economy-wide impacts associated with local climate-induced shocks. For example, the impact of rainfall shocks may ripple through local economic activities, not only agriculture, so that economy-wide impacts ultimately affect the broader populations living in rural regions. The second is agricultural GDP per capita (Feng et al., 2010; Cai et al., 2016), which also builds on empirical evidence at the farm level cited above. However, we note that poverty and subsistence restrictions can also imply a reduction

[1] An empirical association between extreme weather events and agricultural production is also documented in Jayachandran (2006), Iizumi and Ramankutty (2015), Fezzi and Bateman (2015), Ochieng et al. (2016), among others.

[2] While our data include international out-migration, we cannot use it to quantify separately the effects on national and international out-migration.

in migration (a poverty trap, see Cattaneo and Peri, 2016). Lastly, we consider the role of conflicts as a mediating variable, as proposed by a growing literature (e.g. Burke et al., 2009; Kelley et al., 2015; Abel et al., 2019). Based on data from the Uppsala Conflict Data Program, reporting conflict-related fatalities at a highly disaggregate level, we document how rainfall shocks affect local conflicts, which in turn may affect the extent of migration out of a given province.

Our empirical approach leverages longitudinal data for 71 Turkish provinces from 2008 to 2018. Studying these data is important for at least two reasons. First, while a large strand of research on climate-related migration is conducted in low-income countries, research on middle-income countries remains scarce (see Cattaneo et al., 2019; Hoffmann et al., 2020, for a discussion). In Türkiye, we observe almost fifty provinces that are predominantly rural, although these are surrounded by either transitional or urban regions. This setting allows us to contribute to an understanding of how rural communities adapt to changes in weather shocks in a context of urbanization and structural change. Second, the Turkish Statistical Institute (TSI) provides high-quality provincial-level data, including out-migration, GDP per capita, and agricultural GDP per capita, as well as a host of other socio-demographic characteristics for each province (see also Delju et al., 2019). Similarly, the Turkish State Meteorological Service (TSMS) offers long-term precipitation records, with station-level measurement available for more than thirty years. We use these data to construct a set of standardized precipitation indices (SPI), enabling us to characterize the extent to which yearly rainfall deviates from a long-run local distribution of precipitation.[3]

Using an SPI allows us to control for differences in climatic conditions across provinces and estimate the direct effect of random rainfall shocks measured relative to a long-run distribution of rainfall for each province. In addition, we exploit the longitudinal dimension of the data to introduce fixed effects in the analysis. More specifically, we use province-level fixed effects to control for any time-invariant provincial characteristics that could affect out-migration. This would, for example, include the existence of urban agglomerations in neighboring districts or the fact that rural regions tend to experience higher out-migration on average (we come back to this below). Our analysis also controls for year fixed effects to factor out the passage of time, capturing any temporal trends in rural to urban migration (see Auffhammer and Vincent, 2012).

[3]Note that our baseline analysis uses a 12-month SPI so as to capture yearly rainfall shocks over the growing season. However, we recognize that Türkiye includes several climatic regions with different agricultural systems, and below we discuss the robustness of our results to alternative measures of rainfall shocks.

Taken together, our empirical strategy allows us to isolate the direct impact of random deviations from the local regime of rainfall measured over the previous thirty years, and quantify how these shocks act as a push factor in decisions to migrate out of each respective provinces. [4]

Building on this baseline specification, which is a well-established workhorse in the empirical literature, our contribution is twofold. First, we employ the multi-criteria analysis of Oğdül (2010) to classify each province as predominantly rural, transitional, or urban. We then use this classification as a moderating factor to estimate the impact of SPI shocks across provinces of different types.[5] Second, we discriminate across three potential channels in how SPI shocks affect out-migration: GDP per capita, agricultural GDP per capita, and conflict fatalities. Intuitively, the resulting mediated-moderator analysis enables us to document how the channels of causality differ across provinces classified as rural, transitional, or urban. [6]

The mediated-moderator approach enables us to document direct and indirect effects of randomly occurring rainfall shocks, which is novel in the context of environmental migration. Figure 1.1 illustrates the paths of direct and indirect causality from rainfall shocks to out-migration. The procedure for this analysis is as follows. In a first step, we identify how random shocks to long-run precipitation impact each potential mediating variable (per capita agricultural output, per capita GDP, and conflict casualties). In each case, the relationship is allowed to be different across a rural-urban typology, our moderating factor. In a second step, we estimate the direct effect of rainfall shocks on out-migration when we control for the impact of each mediating variable. This second step allows us to quantify the indirect effect of rainfall shocks on out-migration that goes through each mediator. Overall, this procedure decomposes the direct effect of rainfall shocks on out-migration across different channels.

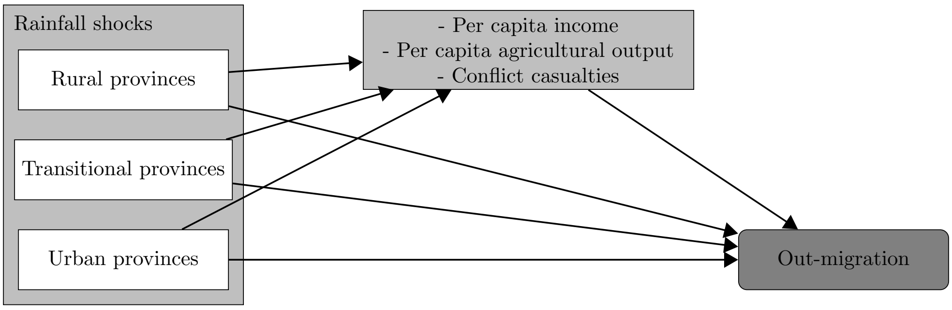
Empirical evidence derived from our data shows that years subject to below-average SPI imply higher out-migration in rural areas. Quantitatively, a negative SPI shock of one standard deviation in the long-run distribution of rural provinces is associated with an additional 0.62

[4]Note that we focus on the total effect of the SPI on out-migration and we do not include control variable into our main estimations (Berlemann and Steinhardt, 2017; Cattaneo and Peri, 2016; Angrist and Pischke, 2009). Nevertheless, in our robustness section, we show that our results remain largely unaffected by the inclusion of key socio-demographic factors variables (education, sex ratio, and population density). We come back to this below.

[5]As a robustness check, we also consider alternative measures to identify rural areas (e.g., the share of population working in agriculture). We come back to this below.

[6]More formally, the mediated-moderator approach combines a mediation analysis, which identifies a causal sequence between two variables and an outcome, and a moderator analysis, which identifies the effect of a variable on the relationship between another variable and an outcome. See, for example, Muller et al. (2005), Morgan-Lopez and MacKinnon (2006), MacKinnon et al. (2007) for a comprehensive coverage.

Figure 1.1: Causality paths from rainfall shocks to out-migration



thousand emigrants on average, corresponding to a 3 percent increase in yearly migration out of rural provinces. We then show that this effect is mediated by GDP per capita, meaning that negative SPI shocks imply a reduction of economy-wide income in rural areas, which in turn acts as a push factor triggering out-migration. This corresponds to around 26% of the direct effect of SPI shocks on out-migration in rural province. By contrast, we do not find evidence that per capita agricultural GDP is a significant mediator at the average of the sample. In fact, our data suggest that the agricultural GDP channel is only relevant for provinces that are in the upper quartile of crop production. These results suggest that, while the agricultural channel plays a role through crop production, it is only relevant for a small share of provinces that rely heavily on these crops. Lastly, we also show that the number of conflict fatalities in rural regions tends to increase with droughts, and that conflicts act as a push factor. In rural provinces, around 8% of the total effect of SPI shocks on out-migration can be attributed to conflicts. This suggests that the conflict channel operates in parallel to direct effects and depends on contextual and institutional factors (Abel et al., 2019).

These results contribute to a growing literature on the linkages between climate change and migration. While empirical evidence on this issue remains controversial (see Boas et al., 2019, for a discussion), a number of empirical studies for low-income countries provide evidence of rural-urban migration in relation to temperature shocks. This includes Marchiori et al. (2012) and Weinreb et al. (2020) for sub-Saharan Africa, Viswanathan and Kumar (2015) for India, and De Longueville et al. (2019) for Burkina Faso. Using cross-country data, Maurel and Tuccio (2016) document an impact of increasing temperature trends on urbanization, whereas Cattaneo and Peri (2016) show that poverty may prevent population movements in low-income countries, but increases them in middle-income countries.[7] This is supported by evidence reported in Nawrotzki et al. (2016) for Mexico, and Thiede et al. (2016) and Baez et al. (2017b) for South America, although further evidence on middle-income countries is needed (see Cattaneo et al., 2019, for a discussion).

We also contribute to a literature that attempts to identify the mechanisms linking climate shocks and migration. Using SPI to measure climate variability, Dallmann and Millock (2017) find that drought induces rural-rural interstate migration in India through impacts on both agricultural and total income. Similarly, Bertoli et al. (2020) report that drought increases the probability of intending to migrate, especially for low-skilled workers of rural areas, in Senegal, Niger, and Ivory Coast (for Nepal, see also Epstein et al., 2022). Another important mechanism in relation to climate shocks is conflict (Burke et al., 2015; Mach et al., 2019). Kelley et al. (2015) argue that a severe drought contributed to trigger social unrest in 2011 Syria, and being ultimately associated with mass migration, although this remains a contentious interpretation (see Selby et al., 2017; Selby, 2019). Missirian and Schlenker (2017) estimate that temperature deviations that affect agricultural yields are associated with increased asylum applications in the European Union (see also Abel et al., 2019; Cottier and Salehyan, 2021). Relative to these studies, we document the role of alternative channels in a consistent framework, showing that the mediating role differs across a rural to urban dimension.

The remaining of this paper is structured as follows. Section 1.2 provides a short discussion of the context in Türkiye and describes our empirical strategy, including our rural-urban classification and our mediated-moderator approach. Section 1.3 shows a summary of our data and reports estimation results. Finally, Section 1.4 briefly discusses the results and concludes.

1.2 Methods: Empirical strategy

This section first discusses the socio-demographic context of Türkiye. We then focus on our main specification to identify the impact of SPI shocks on out-migration, and how this relationship is moderated by the type of provinces (rural vs. urban). Next, we present how we quantify the role of three alternative channels (or mediators) to explain the relationship between SPI shocks and out-migration: (i) GDP per capita, (ii) agricultural GDP per capita, and (iii) local conflict

[7] Benonnier et al. (2019) provide evidence that access to irrigation moderates the temperature-migration relationship, as it shelters yields from weather shocks. We consider this possibility in our set of robustness checks.

fatalities. Lastly, we describe how we document the robustness of our results.

1.2.1 Context

This section provides a short overview of Türkiye's socio-economic context from 2008 to 2018. During this period, Türkiye's population grew from 70 million to 82 million, although the age structure of the population remained fairly constant, with the share of people aged 15-64 years increasing from 65% to 67%.

Ethnic composition of the country features a relatively large Kurdish population located in the eastern provinces of Türkiye. These areas have a history of tension and conflict. In line with this, we observe that violent incidents occur in eastern provinces during the 2008-2018 period, with Ankara, Hakkari, and Diyarbakir being the most affected. See Appendix A. However, these conflicts do not appear to follow a specific regional trend with time.

Over the same decade, the Turkish economy also experienced fluctuations, with GDP per capita growth ranging from 0.4 to 1.8. Official unemployment increased slightly from 9.6% in 2008 to 10.9% in 2018. Importantly, the share of agriculture in GDP fell from 7.4% to 5.8%, and the share of agricultural workforce declined from 23% to 18% of the total workforce. This indicates a decrease in the role of agriculture in the economy, although the share of the workforce in this sector remains significant.

1.2.2 Estimation of the main effects

Our empirical strategy is guided by the meta-analysis of Beine and Jeusette (2021) on climate change and migration. We quantify how random realization of rainfall in province i and year t, measured by variability in the SPI (denoted $\mathit{SPI}_{i,t}$), affects provincial out-migration ($\mathit{out\text{-}migration}_{i,t}$ in thousands of emigrants).[8] Formally, our main regression specification is given by:

$$\mathit{out\text{-}migration}_{i,t} = \alpha_i + \delta_t + \beta \cdot \mathit{SPI}_{i,t} + \epsilon_{i,t}\,, \tag{1.1}$$

[8]Bilateral migration data is also used in the literature, see for example Beine and Parsons (2015), Dallmann and Millock (2017) and Abel et al. (2019). However, such data are not available at the provincial level in Türkiye, so we rely on out-migration to identify the role of climate as a push factor in migrations decisions. For other analysis of out-migration data, see for example Feng et al. (2010), Neumann et al. (2015), Nawrotzki et al. (2016), or Debnath and Nayak (2020).

where α_i is a set of province fixed effects controlling for any time-invariant factors that involve differences in out-migration, geological conditions, and infrastructures across provinces, δ_t is a set of year fixed effects absorbing macro trends common across provinces, such as political conditions. $\epsilon_{i,t}$ is an error term.

The coefficient of interest is β and the variable $SPI_{i,t}$ is derived from station-level records of monthly rainfall from 1970 to 2020.[9] Importantly, we do not include alternative control variables in the analysis as our objective is to measure the total effect of the SPI on out-migration (Berlemann and Steinhardt, 2017; Cattaneo and Peri, 2016; Beine and Parsons, 2015; Angrist and Pischke, 2009).[10]

To meaningfully compare rainfall shocks across provinces with potentially very different climates, our main specification employs a 12-month SPI. Intuitively, this normalizes total precipitation during 12 consecutive months in year t with the empirical distribution for the same 12 consecutive months observed over a period of thirty years.[11] Therefore, observed shocks to yearly precipitation are measured relative to the long-run historical distribution of precipitation observed locally. We emphasize that the choice of a 12-month period to measure yearly precipitation allows us to focus on medium-term drought shocks (Svoboda et al., 2012).[12]

To document how the impact of the SPI on out-migration differs across rural, transitional, and urban provinces, we estimate separate coefficients β for each type of province by defining three moderating variables: $Rural_i$ equals one if the province i is predominantly rural, zero otherwise; $Urban_i$ is one if i predominantly urban, zero otherwise; and $Transitional_i$ equals one if i is neither predominantly urban nor rural, zero otherwise. We refer to these variables as moderators because they can potentially change the estimated value of β.

The moderating variables are based on a detailed multi-criteria classification by Oğdül (2010),

[9] We use a total of 130 stations in Türkiye. B reports their location.

[10] In line with this, we measure out-migration in absolute value rather than as a share of total population, since population itself may be affected by climate shocks. In addition our analysis focuses on a 10-year period, so that population size is largely accounted for by the use of province fixed effects, whereas country-wide population growth is captured by time fixed effects. Nevertheless, in the robustness analysis we check whether our estimates remain similar when we scale the outcome variable by provincial population and we add data on the composition of population in each province.

[11] We restrict the analysis to 71 provinces with complete precipitation data for at least 30 years. To calculate the provincial SPI, we employ a standardized procedure using the Standard Precipitation Index Generator software of the National Drought Mitigation Center at the University of Nebraska.

[12] A 12-month SPI broadly captures agricultural growing seasons (see also Unal et al., 2003; Deniz et al., 2011), although we do not account for potential differences in growing seasons. We discuss alternative periods to define the SPI in the robustness checks.

Figure 1.2: Classification of Turkish provinces (source: Oğdül, 2010)

which defines a province as rural if at least 50% of its constituting districts are classified as rural, urban if 50% or more of its districts are urban, and transitional if it is neither rural nor urban. District-level classification is then based on six categories of socio-demographic characteristics: agricultural production, non-agricultural production, employment structure, demography, educational level, and trade opportunities (see C for a comprehensive list of factors). The resulting classification comprises 32 rural provinces, 34 transitional provinces, and 5 urban provinces, as illustrated in Figure 1.2. In the robustness checks section below, we discuss alternative approaches to distinguish between rural and urban provinces.

We employ these data to augment the equation 1.1 as follows:

$$\begin{aligned} \textit{out-migration}_{i,t} = \alpha_i + \delta_t + \beta_1 \cdot \textit{SPI}_{i,t} \times \textit{Rural}_i + \beta_2 \cdot \textit{SPI}_{i,t} \\ \times \textit{Transitional}_i + \beta_3 \cdot \textit{SPI}_{i,t} \times \textit{Urban}_i + \epsilon_{i,t} \, . \end{aligned} \tag{1.2}$$

By interacting the variable $SPI_{i,t}$ with each moderating variable, we quantify how local SPI shocks affect out-migration for each type of province.[13] Importantly, out-migration is expected to differ in rural versus urban provinces for reasons that are not related to rainfall shocks, such as economic opportunities (Beine and Jeusette, 2021; Marchiori et al., 2012). However, we emphasize that these drivers of emigration are controlled by including province-level fixed effects (α_i) that capture time-invariant structural characteristics of each province.

[13]Note that this specification does not include $SPI_{i,t}$ as a stand alone variable and we also do not omit one of the three categories. The implication is that we directly estimate a marginal effect for each type of province rather than estimating differences as compared to the omitted category.

1.2.3 Estimation of potential channels: mediated-moderator analysis

We now present how we quantify the mediating role of economic activities and conflicts. Specifically, we use data on per capita GDP and agricultural GDP from TSI as well as the number of conflict fatalities from the Uppsala Conflict Data Program.[14] To quantify the relevance of each potential channel, we employ the mediated-moderator specification of Morgan-Lopez and MacKinnon (2006). This requires two steps. First, we estimate the impact of SPI shocks on each mediating variable, denoted $Y_{i,t}$, across each type of province:

$$Y_{i,t} = \eta_i + \phi_1\, \mathit{SPI}_{i,t} \times \mathit{Rural}_i + \phi_2\, \mathit{SPI}_{i,t} \times \mathit{Transitional}_i + \phi_3\, \mathit{SPI}_{i,t} \times \mathit{Urban}_i + \chi_t + \varepsilon_{i,t}\,, \quad (1.3)$$

where the notation follows the same logic as in equation 1.2. This equation allows us to document whether provincial-level SPI shocks have an impact on each mediator variable $Y_{i,t}$ across our rural to urban classification, a necessary condition for mediated-moderator analysis (MacKinnon et al., 2007).

The second step of our mediated-moderator analysis quantifies the impact of SPI shocks on out-migration, akin to 1.2, but we also include each respective mediating variable $Y_{i,t}$ in the regression. Formally:

$$\begin{aligned} \textit{out-migration}_{i,t} = {} & \alpha_i + \delta_t + \beta_1 \cdot \mathit{SPI}_{i,t} \times \mathit{Rural}_i + \beta_2 \cdot \mathit{SPI}_{i,t} \\ & \times \mathit{Transitional}_i + \beta_3 \cdot \mathit{SPI}_{i,t} \times \mathit{Urban}_i + \psi\, Y_{i,t} + \epsilon_{i,t}\,. \end{aligned} \quad (1.4)$$

Therefore, while the first step in equation 1.3 quantifies the impact of SPI shocks on each mediating variable, equation 1.4 controls for indirect effects linking each mediating variable and out-migration in our main specification (see also Figure 1.1). Indirect effects of SPI shocks are accounted for by changes in the mediating variable (MacKinnon et al., 2007). This allows us to assess whether SPI shocks have a stand-alone effect on out-migration once we control for contemporaneous changes in the mediating variable. In particular, evidence that the effect of SPI shocks on out-migration vanishes in equation 1.4 would indicate that the mediating variable acts as a channel for the direct relationship.

[14] We exploit geo-coded data about the location of conflict fatalities to determine the yearly number of fatalities per province. See A.

1.2.4 Robustness checks

We document the robustness of our results along four key dimensions: (i) the definition of SPI shocks; (ii) socio-demographic factors; (iii) our rural-urban classification of provinces; (iv) our measure of the migration response. In the following, we briefly explain how we implement each robustness check in turn.

We start by documenting the role of alternative definitions of SPI shocks, and consider the possibility of more long-term impacts based on 24-month and 36-month SPIs. This allows us to evaluate how longer deviations from the historical distribution of precipitation records affect out-migration flows. Related to that, we further test for the presence of year-on-year spillover effects by re-estimating equation 1.1 with a 1-year lag for the 12-month SPI (instead of contemporaneous impacts). Next, we focus on the effect of drought events, and construct an indicator variable that counts the number of successive years with a SPI smaller or equal to -1. This identifies years in which the amount of precipitation is less than one standard deviation below the long-term average, while also taking into account the possible drought that occurred in previous years. Lastly, we control for the role of temperature by using a Standardized Precipitation-Evapotranspiration Index (SPEI). This allows us to take into account evapotranspiration, which plays an important role in agricultural yields (see Proctor et al., 2022).

Next, we study how socio-demographic factors affect the total effect of SPI shocks on out-migration and consider a specification in which we control for three key factors. The first is education (Findley, 1994; Baez et al., 2017a; Kabir et al., 2018), and we include the share of population with primary education, the share of population with higher education, and the share of young adults (aged 15 to 24 years) in the population. Second, we control for the share of men per women (the sex ratio, see Gray and Mueller, 2012; Nawrotzki et al., 2016; Debnath and Nayak, 2020). As noted in Berlemann and Steinhardt (2017); Cattaneo and Peri (2016), however, these variables are potentially affected by SPI shocks, so these results should be interpreted with caution. Third, we control for population density, and use its value for $t-1$ in order to mitigate potential endogeneity concerns associated with this variable (Burke et al., 2009; Couttenier and Soubeyran, 2014).

Turning to the role of rural provinces, we start by employing two alternative approaches to our rural-urban classification. First, we use the share of provincial population living in cities with more than 300'000 inhabitants. Second, we consider the share of provincial population working in agriculture. Therefore, we re-estimate equation 1.1 interacting the 12-month SPI with each variable to document whether β varies continuously along with these two dimensions. Next, we consider the possibility that access to irrigation may be different in rural, transitional and urban areas, which in turn may buffer local shocks (as in Benonnier et al., 2019). For this purpose, we employ TSI data on the share of irrigated agricultural land in each province (available for 2003) and interact them with our SPI measure.[15] Fourth, we document the importance of crop production in rural provinces. To do so, we divide rural provinces into 3 different categories based on quartiles for 2007 crop production (in tons). Specifically, "high crop production" provinces are those above the 75th percentile, "medium crop production" are those provinces between the 75th and 25th percentiles, and "low crop production" are provinces below the 25th percentile. Based on this, we quantify how rainfall shocks differently impact GDP per capita and agricultural GDP per capita across rural districts with varying levels of crop production.

Our final robustness checks employ three alternative measures for migration. In a first step, we scale our measure of out-migration by province-level population data, and re-estimate our main equation 1.2. Second, we transform the outcome variable with a natural logarithm, allowing us to estimate proportional (percentage) results across provinces. Lastly, we exploit data on net migration rates at the province level, which is defined as the difference between in-migration and out-migration, scaled by provincial population. As our study focuses on push factors, using net migration as an outcome variable can obscure some of our results by accounting for pull factors as well. Nevertheless, considering net migration provide further confidence in the validity of our main estimates.

1.3 Data and results

This section reports our empirical results. First, we provide summary statistics for our sample. Second, we present the results from our main specification, documenting the impact of SPI shocks on out-migration across our rural-urban classification. Third, we discuss the results of our mediated-moderator analysis. Fourth, we present the results of the robustness checks.

[15] Due to the limitation of TSI data on irrigated areas, we cannot observe the evolution of irrigation installations over time.

Table 1.1: Descriptive statistics across Turkish provinces

	Mean	Std. Dev.	SD:mean ratio	Min	Max
Rural provinces (N=32)					
Out-migration	19.07	10.41	0.55	4.09	62.21
12-month SPI	0.17	1.05	6.18	-2.98	2.82
GDP per capita	6.72	1.63	0.24	2.73	11.64
Agricultural GDP per capita	2.76	1.65	0.6	0.4	11.61
Conflict fatalities	0.07	0.31	4.43	0	3.77
Transitional provinces (N=34)					
Out-migration	30.93	27.23	0.88	4.61	221.75
12-month SPI	0.19	1.06	5.58	-2.57	3.23
GDP per capita	8.99	2.67	0.3	3.31	17.59
Agricultural GDP per capita	2.84	1.36	0.48	0.4	7.61
Conflict fatalities	0.03	0.23	7.7	0	3.56
Urban provinces (N=5)					
Out-migration	118.91	147.72	1.24	6.54	595.8
12-month SPI	0.2	1.12	5.6	-2.09	2.9
GDP per capita	14.77	3.08	0.21	8.96	20.73
Agricultural GDP per capita	0.92	0.68	0.74	0.04	2.27
Conflict fatalities	0.03	0.17	5.67	0	1.16

Notes: Data sources are TSI, TSMS and Uppsala Conflict Data Program, from 2008 to 2018. *Out-migration* is in thousand of emigrants. *Conflict fatalities* is the number of fatalities reported for local conflicts (in hundred).

1.3.1 Descriptive statistics

Table 1.1 provides summary statistics for rural, transitional and urban provinces. Out-migration tends to be larger and more volatile in provinces classified as urban, although as a percentage to total population it is larger in rural provinces (4.82%) relative to both transitional (3.85%) and urban (3.34%) provinces. On average across all provinces out-migration is 31.78 thousands of emigrants each year, or around 4 percent of the provincial population.

The mean and variability for the 12-month SPI are very similar across provinces, which is implied by the way it is constructed. In particular, the SPI measures deviation in rainfall relative to a province-specific distribution measured over thirty years. Crucially, however, our identification strategy takes advantage of the random timing and magnitude of shocks to our 12-month SPI. We note that, for each type of province, the average SPI is slightly higher than zero, which indicates that precipitations are on average slightly higher compared to historical records.[16]

[16]Figure D1 in D displays the SPI per province during the observation period. The graphs does not suggest that province-level SPI features a time trend.

Other variables follow an expected pattern, with GDP per capita being significantly higher in urban provinces, followed by transitional and rural provinces, whereas agricultural GDP per capita is similar among rural and transitional provinces but substantially lower in urban provinces. We also note that the number of conflict fatalities is, on average, around two times larger in rural provinces, although the maximum is relatively close for rural and transitional provinces, and significantly lower in urban provinces.

1.3.2 The impact of SPI shocks on out-migration in rural, transitional and urban provinces

Table 1.2 reports regression results quantifying the impact of SPI shocks on out-migration (equations 1.1 and 1.2). Column 1 is a simple bivariate regression of SPI on out-migration (no fixed effects). In column 2 we add province and year fixed effects to control for, respectively, all time-invariant provincial characteristics and common macro shocks. In column 3 we estimate separately impacts of SPI shocks for rural, transitional and urban provinces (equation 1.2). In all columns we report standard errors clustered at the province level in parentheses.

Results in column 1 indicate that a one standard deviation increase in SPI, which represents a year with relatively abundant precipitation, is associated with a decrease of out-migration by 1.97 thousand emigrants on average. However, this coefficient is not accurately estimated. Furthermore, the introduction of fixed effects substantially reduces the magnitude of the coefficient (see Auffhammer and Vincent, 2012, for a similar result).

More interestingly, column 3 shows that decomposing the total effect of SPI across rural, transitional and urban provinces implies very different results. For rural provinces, there is a negative and statistically significant effect of SPI shocks in rural provinces (p-val.$<$0.05). This indicates that a drought, which represents a negative SPI shock, is associated with an increase of out-migration in rural provinces on average. Quantitatively, a one-standard deviation *decrease* in SPI *increases* out-migration in rural provinces by 0.62 thousand emigrants on average, which is around 3% of the average annual out-migration in rural provinces.

We also estimate that droughts tend to increase out-migration in transitional provinces, as the coefficient is negative and borderline statistically insignificant (p-val. $<$0.1). Lastly, the point estimate for urban provinces is positive and large relative to other provinces, although it does not reach statistical significance at conventional levels. One potential interpretation of this result is that urbanized provinces are less vulnerable to climate shocks than other provinces

Table 1.2: Baseline results for the impact of SPI shocks on out-migration

	Outcome: Out-migration in thousand of emigrants		
	Bivariate	FE	FE
	(1)	(2)	(3)
SPI	-1.97 (1.66)	0.01 (0.20)	–
SPI × Rural	–	–	-0.62** (0.27)
SPI × Transitional	–	–	-0.38* (0.20)
SPI × Urban	–	–	2.25 (1.60)
Constant	32.05*** (1.85)	28.53*** (0.77)	28.32*** (0.87)
Fixed effects	No	Yes	Yes
Number of observations	776	776	776
Number of provinces	71	71	71
Adjusted R^2	0.01	0.09	0.10

Notes: Results from linear regressions reported. *SPI* is the 12-month SPI per year and province. *Rural*, *Transitional* and *Urban* are indicator variables for rural, transitional and urban provinces, respectively. The period of observation is from 2008 to 2018. In columns 2 and 3 we include province and year fixed effects. In all columns we report standard errors clustered at the province level in parentheses. *,** and *** respectively denote statistical significance at 10%, 5% and 1%.

when exposed to precipitation shocks. In addition, a higher degree of diversification in economic activities may help to retain population in the presence of shocks. However, given the relatively small number of urban provinces in our sample, these estimates should be interpreted with caution.

Overall, our data suggest that a drought increases out-migration in rural provinces, that it has a smaller and less precisely estimated impact for transitional provinces, whereas the effect for urban provinces is large but statistically insignificantly different from zero. Taken together, these effects cancel out on average so that we find no direct effect of SPI shocks on migration when we do not consider the urban-rural classification of provinces (columns 1 and 2).

1.3.3 Channels: GDP per capita, agricultural GDP per capita and conflict fatalities

We now turn to our mediated-moderator analysis to discriminate across three potential channels linking SPI shocks and out-migration. In Table 1.3 we report the results for each mediating variable: GDP per capita in columns 1 and 2, agricultural GDP per capita in columns 3 and 4, and conflict fatalities in columns 5 and 6. Furthermore, results for the first step estimation

Table 1.3: Analysis of channels for the impact of SPI shocks on out-migration

Specification:	Channel: GDP		Channel: Ag. GDP		Channel: Conflicts	
Outcome:	GDP p.c. (1)	Out-migration (2)	Ag. GDP p.c. (3)	Out-migration (4)	Conflicts (5)	Out-migration (6)
SPI × Rural	0.05**	-0.46*	-0.01	-0.65**	-0.02**	-0.57**
	(0.02)	(0.24)	(0.03)	(0.29)	(0.01)	(0.26)
SPI × Transitional	-0.05	-0.53**	0.08*	-0.05	0.01	-0.41**
	(0.03)	(0.24)	(0.04)	(0.22)	(0.01)	(0.20)
SPI × Urban	-0.33***	1.22	-0.04	2.10	0.01	2.22
	(0.11)	(1.31)	(0.04)	(1.62)	(0.01)	(1.58)
Mediating variables						
GDP per capita	–	-3.08**	–	–	–	–
		(1.40)				
Agricultural GDP per capita	–	–	–	-4.20*	–	–
				(2.27)		
Conflict fatalities	–	–	–	–	–	3.63**
						(1.77)
Constant	8.21***	53.63***	1.53***	34.73***	0.04***	28.19***
	(0.06)	(11.11)	(0.08)	(2.92)	(0.01)	(0.88)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	776	776	776	776	776	776
Number of provinces	0.77	0.12	0.68	0.15	0.05	0.11
Adjusted R^2	71	71	71	71	71	71

Notes: Results from linear regressions reported. *SPI* is the 12-month SPI per year and province. *Rural*, *Transitional* and *Urban* are indicator variables for rural, transitional and urban provinces, respectively. *GDP p.c.* is per capita GDP, *Ag. GDP p.c.* is agricultural GDP per capita, and *Conflict fatalities* is the number of fatalities in conflicts (in hundreds). The period of observation is from 2008 to 2018. In all columns we include province and year fixed effects, and report standard errors clustered at the province level in parentheses. *,** and *** respectively denote statistical significance at 10%, 5% and 1%.

(equation 1.3), which quantifies the impact of SPI shocks on each mediating variable, are in columns 1, 3 and 5. Results for the second step of the channel analysis (equation 1.4), where we estimate the effect of SPI shocks on out-migration controlling for the mediating variable, are in columns 2, 4 and 6. In all columns, we include province and year fixed effects, and report standard errors clustered at the province level in parentheses.

Column 1 shows that SPI shocks in rural provinces have a positive and statistically significant effect on GDP per capita (p-val.$<$0.05). This implies that droughts (negative SPI shocks) are associated with a decrease in GDP per capita in rural provinces. Evidence for this relationship is a necessary condition for the mediated-moderator analysis (MacKinnon et al., 2007; Morgan-Lopez and MacKinnon, 2006). Furthermore, column 2 provides evidence that GDP per capita has a negative and statistically significant association with out-migration (p-val$<$0.05). One implication is that a decrease in per capita income acts as a push factor in migration decisions. More importantly, results in column 2 suggest that the coefficient for SPI shocks in rural provinces is significantly smaller compared to the results in Table 1.2, column 3, and still statistically significant at 10%. Taken together, these results imply that GDP per capita mediates around 26% ($= (-0.62 - (-0.46))/-0.62$) of the total impact of SPI shocks on out-migration in rural provinces.

In urban provinces, the results of column 1 suggest that SPI shocks have a negative and statistically significant impact on GDP per capita (p-val.$<$0.01). However, in column 2, the coefficient for SPI shocks on out-migration is significantly smaller when we control for GDP per capita in our main estimation. One potential interpretation is that the impact of SPI on out-migration in urban provinces is fully driven by per capita GDP, although the coefficient for the second stage remains imprecisely estimated and statistically insignificant (notwithstanding the relatively small number of urban provinces). In transitional provinces, we do not find precise evidence that SPI shocks affect GDP per capita.

Next, results presented in column 3 suggest that SPI shocks do not significantly affect agricultural output in rural and urban provinces, and have a positive but loosely statistically significant impact in transitional provinces (p-val$<$0.1). In line with this, column 4 shows that introducing agricultural GDP per capita in our main specification does not significantly affect point estimates quantifying the effect of SPI shocks on out-migration. In other words, adding GDP per capita in equation 1.2 does not significantly change our estimated effect relative to Table 1.2, column 3. This suggests that agricultural GDP is not a mediating variable in the relationship between SPI shocks and out-migration in rural provinces. We come back to this result in the robustness checks, and show that these are driven by the volume of crop production (see section 1.3.4).

Lastly, column 5 provides evidence about the relationship between SPI shocks and conflict casualties. Results for rural provinces indicate that an increase in SPI (rainy year) implies a decline in the number of fatalities (p-val.$<$0.05), whereas there are no effects for transitional and urban provinces. Furthermore, column 6 shows that conflicts have a positive and statistically significant association with out-migration (p-val$<$0.05). As expected, an increase in conflict fatalities acts as a push factor in migration decisions. In turn, controlling for conflicts reduces the impact of SPI shocks on out-migration in rural provinces as compared to Table 1.2, column 3. The share of the total effect of SPI on out-migration mediated by conflicts is around 8% ($= (-0.62 - (-0.57))/-0.62$). For urban and transitional provinces, we do not find evidence that conflicts act as a mediating variable.

Table 1.4: Robustness results for the impact of rainfall shocks on out-migration

	24-month SPI (1)	36-month SPI (2)	Lagged SPI (3)	Drought years (4)	12-month SPEI (5)	Control variables (6)
SPI × Rural	-0.32 (0.29)	-0.46 (0.34)	-0.65** (0.29)	–	–	-0.49** (0.21)
SPI × Transitional	-0.46 (0.34)	-0.49 (0.53)	-0.41* (0.24)	–	–	-0.03 (0.19)
SPI × Urban	0.12 (0.43)	-0.3 (0.9)	2.1 (1.5)	–	–	3.41 (3.02)
Lagged SPI × Rural	–	–	0.08 (0.26)	–	–	–
Lagged SPI × Transitional	–	–	-0.23 (0.17)	–	–	–
Lagged SPI × Urban	–	–	-1.39 (1.48)	–	–	–
Drought × Rural	–	–	–	1.34* (0.69)	–	–
Drought × Transitional	–	–	–	1.16** (0.54)	–	–
Drought × Urban	–	–	–	-15.94 (11.51)	–	–
SPEI × Rural	–	–	–	–	-0.65** (0.3)	–
SPEI × Transitional	–	–	–	–	-0.17 (0.19)	–
SPEI × Urban	–	–	–	–	1.4 (1.42)	–
Constant	28.35*** (0.92)	28.31*** (0.99)	28.17*** (0.93)	28.60*** (0.82)	28.387*** (0.88)	21.07 (20.19)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	No	No	No	Yes	Yes
Number of observations	776	776	771	776	776	776
Adjusted R^2	0.09	0.1	0.1	0.12	0.1	0.61
Number of provinces	71	71	71	71	71	71

Notes: Results from linear regressions reported. In column 1 and 2 the variable *SPI* is a 24-month and 36-month SPI respectively, in column 3 we include a 12-month SPI together with its lagged value. In column 4 the variable *Drought* is the count of successive years in which the 12-month SPI is equal or below -1. In column 5 the variable *SPEI* is a 12-month SPEI. In column 6, we control for the share of population above 15 years with primary and higher education, the ratio of male per female, the share of population between 15 to 24 years, and lagged population density. All columns include province and year fixed effects. Standard errors clustered at the province level reported in parentheses. *,** and ***respectively denote statistical significance at 10%, 5% and 1%.

1.3.4 Robustness checks

Table 1.4 reports the results of robustness checks using alternative measures of rainfall shocks in our main specification (equation 1.2). In columns 1 we use a 24-month SPI, in column 2 a 36-month SPI, and in column 3, we use a 12-month SPI together with its lagged value. In column 4 we use a count for the number of successive years in which a 12-month SPI is equal or below -1. In column 5 we use a 12-month SPEI. Lastly, in column 6 we include a vector of socio-demographic control variables. In all columns we include province and year fixed effects, and report standard errors clustered at the province level in parentheses.

Estimates in columns 1 and 2 show that using a 24- and 36-month SPI implies relatively similar patterns for rural provinces compared to a 12-month SPI, although point estimates are smaller and statistically insignificant. Similarly, introducing a lagged 12-month SPI (column 3) does not affect the magnitude of contemporaneous effects, whereas the coefficients for the lagged variables are small and statistically insignificant. Overall, these results suggest that the impact of rainfall shocks on out-migration is larger in the short term. However, these effects remain persistent for at least 3 years. Column 4 suggests that an additional year of drought tends to increase out-migration in both rural and transitional districts, with a pattern that is close to our baseline specification. This suggest that a long-lasting drought has a larger effect on out-migration.

Estimates in column 5 of table 1.4 show that adding temperature to our index does not significantly change our results. This is important because the potential for evapotranspiration occurring during drought periods does not significantly affect our results. Column 6 suggests that our results are robust to the addition of socio-demographic control variables, as we estimate a statistically significant and negative effect for rural provinces (p-val.$<$0.05). This shows that our main results are robust to omitted socio-demographic factors, such as the age structure of the population.

We now turn to our second set of robustness checks and focus on rural provinces. Results are reported in Table 1.5. In columns 1 and 2, we interact the 12-month SPI with, respectively, the share of population living in cities of more than 300,000 inhabitants and the share of population working in agriculture. In column 3, we include an interaction between the share of irrigated land (measured in 2003) and the 12-month SPI. Columns 4 to 5 consider the impact of SPI shocks on GDP per capita and agricultural GDP per capita (equation 1.3) accounting for crop production in rural provinces.

Results in column 1 suggest that provinces with a higher proportion of urban residents tend to experience greater positive impacts of SPI shocks (p-val.$<$0.01). Similarly, column 2 shows that an increase in the share of agricultural labor implies more negative impacts associated with SPI shocks (p-val.$<$0.05). These results are consistent with the analysis above. Column 3 suggests that larger irrigated area implies a smaller out-migration response to SPI shocks, so that irrigation acts as a buffer, although the effect is not statistically significant at conventional levels.

Estimates from column 4, Table 1.5, show that SPI shocks have a positive impact on GDP per capita in rural provinces with high and medium crop production, which is consistent with

Table 1.5: Robustness results for rural provinces

Outcome:	*Out-migration in thousand of emigrants*			*GDP p.c.*	*Ag. GDP p.c.*
	Urban population	Ag. labor	Irrigation	Crops production intensity	
	(1)	(2)	(3)	(4)	(5)
SPI	-0.36**	1.76*	–	–	–
	(0.16)	(0.91)			
SPI × % urban	0.60***	–	–	–	–
	(0.08)				
SPI × ag. labor share	–	-5.58**	–	–	–
		(2.51)			
SPI × Rural	–	–	-0.66**	–	–
			(0.29)		
SPI × Transitional	–	–	-0.62	-0.05	0.08**
			(0.5)	(0.03)	(0.04)
SPI × Urban	–	–	3.70*	-0.33***	-0.03
			(2.16)	(0.11)	(0.04)
SPI × Rural × Irrigation	–	–	0.11	–	–
			(0.17)		
SPI × Transitional × Irrigation	–	–	0.61	–	–
			(0.95)		
SPI × Urban × Irrigation	–	–	-2.60*	–	–
			(1.55)		
SPI × Rural × High crops production	–	–	–	0.07*	0.14**
				(0.03)	(0.06)
SPI × Rural × Medium crops production	–	–	–	0.06**	-0.02
				(0.03)	(0.03)
SPI × Rural × Low crops production	–	–	–	0.02	-0.10***
				(0.04)	(0.02)
Constant	28.49***	28.68***	28.32***	8.21***	1.53***
	(0.84)	(0.75)	(0.89)	(0.06)	(0.08)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	776	776	776	776	776
Number of provinces	0.11	0.1	0.1	0.77	0.68
Adjusted R^2	71	71	71	71	71

Notes: Results from linear regressions reported. In columns 1 to 3 the dependent variable is out-migration in thousand of emigrants and the dependant variable is a 12-month SPI. In column 1 and 2 we interact a 12-month SPI with *% urban* (share of population in cities) and *Ag. labor share* (share of labor force in agriculture), respectively. In column 3, we measure the effect of interact *Irrigation* (share of irrigated hectares in 2003) for *Rural*, *Transitional* and *Urban* provinces. In columns 4 the outcome variable is GDP per capita and in column 5 it is agricultural GDP per capita, and we quantify how SPI shocks in rural provinces in relation to 2007 crop production (*high crop production*, *medium crop production* and *low crop production*). In all columns we include province and year fixed effects and report standard errors clustered at the province level in parentheses. *,** and ***respectively denote statistical significance at 10%, 5% and 1%.

results in Table 1.3, column 1. More interestingly, column 5 shows that the impact of SPI shocks on agricultural GDP per capita is of opposite sign for rural provinces with high and low intensity in crop production. For provinces with high crop production, the impact is positive, whereas the effect is negative in provinces with low crop production. For provinces with medium crop production, the impact of SPI shocks is small and not statistically significant. These results suggest that SPI shocks have heterogeneous impacts across rural provinces and explain the lack

Table 1.6: Robustness results for alternative measures of migration

	% out-migration (1)	ln(out-migration) (2)	Net-migration (3)
SPI × Rural	-0.07*	-0.11	1.31*
	(0.03)	(0.07)	(0.71)
SPI × Transitional	-0.01	0.01	0.89***
	(0.03)	(0.08)	(0.29)
SPI × Urban	0.12***	0.31***	-2.04**
	(0.02)	(0.07)	(0.85)
Constant	4.00***	29.61***	-2.49**
	(0.07)	(0.15)	(0.95)
Fixed effects	Yes	Yes	Yes
Number of observations	776	776	776
Number of provinces	0.14	0.38	0.14
Adjusted R^2	71	71	71

Notes: Results from linear regressions reported. In column 1 the dependent variable is out-migration divided by provincial population, in column 2 it is the natural log of out-migration, and in column 3 it is net migration measured as the difference between in-migration and out-migration divided by provincial population. *SPI* is the 12-month SPI per year and province. *Rural*, *Transitional* and *Urban* are indicator variables for rural, transitional and urban provinces, respectively. The period of observation is from 2008 to 2018. In all columns we include province and year fixed effects and report standard errors clustered at the province level in parentheses. *,** and ***respectively denote statistical significance at 10%, 5% and 1%.

of evidence when considering agricultural GDP as a channel (column 3 of table 1.3). Our hypothesis regarding these results is that provinces that are less dependent on crops experience less damage to their agricultural production during droughts. This can lead to a substitution effect, where goods that can be produced under high climate stress experience increased demand and/or higher prices. In turn, impacts on agricultural GDP per capita would be mitigated.

The last set of robustness checks focus on alternative migration measures. Results reported in Table 1.6 largely confirm previous findings. First, when the outcome is measured as fraction of provincial population (column 1), the coefficient for rural provinces implies that a negative one-standard deviation SPI shock increases out-migration in rural provinces by around seven percentage points (p-val.<0.1).[17] When we consider a logarithmic-transformed measure of out-migration (column 2). The impact of SPI shocks in rural provinces remains negative, although

[17] The decrease in precision in the estimates is likely due to the presence of small provinces in which a relatively small increase in out-migration can result in large proportional effects. This adds noise and reduces the precision of our estimates.

it is not statistically significantly different from zero. We note, moreover, that columns 1 and 2 suggest positive and statistically significant effects of SPI shocks on out-migration in urban districts. Finally, results for net-migration in column 6 suggest that SPI shocks have a positive impact in rural provinces (p-val.$<$0.1). An implication is that drought years are associated with an overall decline in net migration.

1.4 Discussion and conclusion

This study has contributed to an understanding of the relationship between variability in rainfall and migration, providing novel empirical evidence on how SPI shocks affect out-migration across Turkish provinces. We have shown that the relationship is moderated by whether a province is rural, transitional or urban, with evidence that drought events imply increased migration out of rural provinces. We have also quantified the mediating role of per capita GDP as a channel to explain higher out-migration as a response to negative SPI shocks in rural provinces. One implication of our data is that droughts induce a decrease in per capita GDP, which in turn acts as a push factor in out-migration decisions. Evidence further suggests that agricultural GDP is also a push factor in the case of a drought, but only for rural provinces with relatively important crop production. This result complements studies by Feng et al. (2010) and Cai et al. (2016), which also emphasize the importance of shocks to agricultural output in migration decisions.

One interpretation of our results is that provinces with low level of urbanization are more exposed to climate variability, making it more likely that precipitation shocks will act as a push factor in migration decisions. This is similar to previous research that has shown that countries with a high dependence on agriculture and a lack of capacity and infrastructure to cope with climate shocks are particularly vulnerable (see Cattaneo et al., 2019). However, the mechanism linking droughts and migration in rural areas is more complex than a simple impact on the agricultural sector. One possible explanation is that price fluctuations for crops can impact the entire economy, so that for provinces with relatively large crop production where the agricultural sector makes up a larger share of the local economy, agricultural GDP is more directly affected by fluctuations in the SPI. Further research is needed to confirm this interpretation.

Furthermore, our analysis shows that conflicts also increase with droughts and play a role as a push factor in out-migration decisions, which is consistent with evidence from other contexts (Kelley et al., 2015; Missirian and Schlenker, 2017). Our data further suggest that conflict fatalities mediate the impact of SPI shocks on out-migration, although the extent of this mediation is relatively small. This result highlights the role of contextual and institutional factors affecting climate-migration linkages (see also Abel et al., 2019), and suggests that droughts give rise to separate channels through per capita GDP and conflicts. One possible explanation for this is that climate shocks can increase social risks within affected populations. This is consistent with other research on the relationship between climate shocks and conflicts (see Xu et al., 2016; Mach et al., 2019). Additional research is necessary to validate this interpretation.

Taken together, our results suggest that more frequent droughts can be expected to increase out-migration in rural areas, both by affecting economy-wide activities and through conflicts. Making local economies more resilient to rainfall shocks, through adaptation strategies or economic transfers, might help reduce the impact of the increased variability in rainfall expected with future climate change. The design of such policies could further benefit from a better understanding of destination choices in relation to rural out-migration, which could potentially hasten urbanization, lead to rural-rural displacements, or induce international displacements. In our context the data was limited with respect to the number of urban provinces, point of destination for migrants and time-varying irrigation data. This suggests that more evidence on these issues is warranted, and developing our understanding of these migration patterns remains an important research endeavor.

Chapter 2

Exposure to droughts increases birth rate and intimate violence in Africa

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Abstract:

In this paper, I study the role of droughts as a determinant of fertility and intimate violence in 16 African countries from 2004 to 2018. I exploit a measure of droughts at the sub-national level derived from the Emergency Events Database and match this to micro-level data from the Data Health Survey. This allows me to document how the occurrence of droughts affects birth rates among women, and therefore indirectly population growth, and how access to modern contraception can reduce this impact. I use the same sample of women to analyze the impact of droughts on intimate sexual and physical violence. My results provide evidence that droughts increase the probability of having a new child and show that this relationship is reduced for women who use contraception. Meanwhile, droughts also increase the probability that a woman will be exposed to sexual and physical violence from her intimate partner. These results provide novel quantitative evidence on the impact of natural disasters on women and how it can have important implications for women's health, contraception, and protection against gender violence.

Keywords: fertility; natural disasters; droughts; contraception; interpersonal violence; sexual violence.

2.1 Introduction

The occurrence, duration, and magnitude of natural disasters and natural hazards are increasing worldwide (IPCC, 2021). Understanding the impact of natural disasters on social and economic outcomes is an important asset in the formation of adaptation strategies. Evidence suggests that natural disasters are detrimental to economic growth (Strobl, 2011; Klomp, 2016), and therefore can indirectly affect demographic trends (Brueckner and Schwandt, 2015; Casey et al., 2019), inequalities within countries (Cappelli et al., 2021) and between countries (Casey et al., 2019; Noy, 2009). Countries with greater vulnerability to natural disasters are more likely to be affected, such as countries with small or developing economies and low institutional stability (Loayza et al., 2012; Noy, 2009; Kahn, 2005). The vulnerabilities to natural disasters are also important at the individual level, where different socio-economic factors, such as gender, poverty, age, and race, play an important role in the increase in negative economic, health, and well-being outcomes (Masson, 2022; Rao, 2020; First et al., 2017). In turn, the sum of these negative impacts at individual levels is an additional factor that hinders countries in their development (Fisher, 2010; Carrillo, 2005).

Due to gender discrimination, women are more vulnerable than men to the impacts of natural disasters (Masson, 2022; Rao, 2020; First et al., 2017). Although natural disasters are not the cause of gendered violence, they induce an increase in risk factors for exposure to gendered violence.[1] With the increase in the occurrence of natural disasters, understanding how they impact women's lives is necessary to develop gender-sensitive disaster management policies. A better understanding of how natural disasters impact fertility preferences or exposure to intimate violence is part of the essential information necessary for governments to develop more efficient policies and programs ensuring sustainable evolution of fertility, women's health, social inequalities, and economic development (World Health Organization, 2021; García-Moreno et al., 2015; Carrillo, 2005).

In this paper, I quantify both the evolution of births after exposure to droughts, and provide evidence on how contraception can impact this evolution, and the exposure to intimate partner sexual and physical violence. I exploit data from 16 African countries during the period 2004

[1] It is important to stress that risk factors are triggers, but not causes of gendered interpersonal violence. Natural disasters, through risk factors, increase the exposure to gendered interpersonal violence that is present in all societies, especially domestic violence (Thurston et al., 2021; Masson, 2022; Fisher, 2010).

to 2018. I use the Data Health Survey (DHS) to obtain micro-level evidence on new births and reports of intimate violence. I define birth as a dummy variable, equal to 1 when a childbirth occurs during the year prior to the survey.[2] I define dummy variables equal to 1 if a woman reports being exposed to intimate sexual violence or intimate sexual and/or physical violence.[3] The DHS also provides information on the use of contraception at the respondent level.[4] These data are merged with the Emergency Events Database (EM-DAT) to quantify exposure to droughts at the sub-national level.[5] Specifically, I use the number of people affected by droughts in the five years preceding the surveys, scaled by the population of the region.[6] This variable is used as a proxy for the various drivers of fertility and intimate violence induced by exposure to droughts. Here, droughts are periods in which water shortages due to low precipitation are reported.[7]

Based on anonymized survey data from the DHS, I exploit the recurrence of surveys to build a pseudo-panel model, comparing women of the same generation, to quantify the impact of droughts on childbirths.[8] I use time-fixed effects to factor out any temporal fertility, such as potential decline in the fertility rate in developing countries (Lerch, 2019). Then, I consider the role of contraceptive use in interaction with my measure of droughts. My results suggest that an additional drought affecting the entire population of a survey region induces a statistically significant increase in the probability that women have a new child of 15% on average, with a long-lasting effect up to 10 years. With the interaction of drought with contraception use, I show that the use of contraception significantly reduces the increase in the probability of new births induced by droughts.

Next, I exploit data on the same sample of women and I use the pseudo-panel specification to estimate the change in probability that a woman reports intimate violence induced by droughts.

[2] I define birth as a dummy variable to avoid any variations induced by twins or more children at the term of a pregnancy.

[3] The sexual and/or physical violence variable is a variable that reports both types of violence without distinction.

[4] The modern contraceptives are defined in ICF (2018) and include sterilization, contraceptive pill, intrauterine contraceptive device, injectables, implants, condom, diaphragm, contraceptive foam and contraceptive jelly, lactational amenorrhea method, standard days method, and country-specific modern methods.

[5] The EM-DAT contains over 22000 disasters in the world and covers the period from 1900 to the present day (Guha-Sapir, 2021).

[6] The exposure to droughts is measured in millions people affected by droughts in a sub-national region.

[7] This definition follows the EM-DAT definition of drought events based on Integrated Research on Disaster Risk (2014).

[8] As the data are repetitions of anonymized surveys, I have cross-sectional data repeated approximately every 5 years. I define the pseudo-panel approach by cells of 20-year regional cohorts of women between 15 and 49 years of age. I create an interaction term of the cohort cells with the survey regions in order to observe a stable group of women over time.

I use time-fixed effects to factor out any temporal trends in reporting violence, such as improvement in reporting mechanisms (Thurston et al., 2021). My results suggest a positive impact of exposure to droughts on the probability that women report being exposed to sexual and physical violence committed by an intimate partner. Quantitatively, I estimate that an additional drought affecting the entire population of a survey region induces a statistically significant increase in probability of 11% on average for women to report being exposed to intimate sexual violence, as well as an average increase in probability of 10% for women to report being exposed to intimate sexual and/or physical violence. Furthermore, these effects are stronger shortly after exposure to droughts, but persist up to 10 years after exposure to the shock.

Taken together, my results provide novel evidence on the impact of exposure to droughts on the evolution of birth rates and intimate violence on the same population of women; yet, due to the survey structure of the data, I cannot estimate whether variations in intimate sexual violence due to natural disasters are part of the variations in new births or the other way around. These results indicate that to minimize the negative effects of natural disasters on women and the entire population, it is important to design gender-sensitive policies for the management of natural disasters that reinforce and maintain, in the long run, adequate security and health care for women.

My paper contributes to the literature showing how natural disasters have an impact on fertility. This literature shows significant results, but the impact remains controversial (Frankenberg and Thomas, 2014). On the one hand, Davis (2017) analyzes the impact of Hurricane Mitch in Nicaragua and finds that birth rate increased during 4 to 6 years after this event. This effect is explained by the opportunity cost of investing in additional children or in children's education, which is influenced by the increase in the return on low- and high-education work.[9] Similarly, Nobles et al. (2015) also found an increase in child demand after the deaths induced by the 2004 tsunami in Indonesia and reported that this increase is explained by an increase in the local mortality rate.[10,11] My results suggest that natural disasters have a positive impact on childbirth, which supports the preceding findings. On the other hand, Sellers and Gray (2019)

[9] Considering the opportunity cost, an increase in returns, such as prices and income, of work requiring low education act as push factors for individuals to have more children. An increase in prices of products from the agricultural sector implies a decrease in the opportunity cost of having more children.

[10] Casey et al. (2019) explain that an increase in child mortality can induce an increase in birth rate, in order for individuals to reach their minimum goal in terms of fertility rate.

[11] Note that climate and natural disaters can play a role in early childhood health, see Blom et al. (2022).

analyze the fertility behaviors of an Indonesian community during the years 1993 to 2015 and find that climate shocks, measured as moonsoon onset delays and medium-term positive temperature shocks, might restrict the fertility rate.[12,13] In relation to fertility, access to contraception can be hampered by natural disasters (Behrman and Weitzman, 2016). This reduction in the access to contraception can be explained by increased pressure that diminishes the effectiveness of health services. Furthermore, medical services can face a change in priorities from reproductive health to emergency relief.[14] By selecting the use of contraceptive methods as a proxy, my research helps to understand how the guarantee of access to contraception can reduce birth rate increases from natural disasters.

My paper also adds to the literature on natural disasters and gendered interpersonal violence. Research related to this topic has been conducted primarily with qualitative analysis, to contextualize interpersonal violence (Enarson and Philips, 2008), but also due to the lack of systematic data collection (Masson, 2022).[15,16] My research helps to understand these dynamics and fills the gap of quantitative studies of the link between natural disasters and gendered interpersonal violence. The literature explains this link through three main types of risk factors (Thurston et al., 2021). The first is an increased pressure on housing and income, which can lead to tension within households. The second is a degradation of the social environment and increased isolation within households due to a rupture of the social network. The third is an increase in drivers of intimate violence, such as higher drug and alcohol consumption.[17] The survey-based literature shows that natural disasters increase interpersonal violence in a wide variety of contexts. For example, Rao (2020) perform logistic regression analysis to assess the

[12]Hamamatsu et al. (2014) estimate a similar trend while analyzing the impact of psychological stress induced by the 2011 Japan earthquake on birth rates.

[13]As a middle point, Evans et al. (2010) estimate that hurricanes of low severity, in the United States, induce an increase in the birth rate, while hurricanes of high severity induce a decrease in birth rates.

[14]This reduction in reproductive health services is reinforced by racial inequalities (Seltzer and Nobles, 2017; Leyser-Whalen et al., 2011) and socioeconomic status (Nandi et al., 2018).

[15]The first meta-analysis on natural disasters and gendered interpersonal violence (Thurston et al., 2021) shows that the majority report significant increases in at least some types of interpersonal violence.

[16]Until now, economic research has mainly studied how interpersonal violence against women is affected by unemployment, marriage, and measures against the offender (Anderberg et al., 2016; Bulte and Lensink, 2019; Menon, 2020; Chin and Cunningham, 2019; Iyengar, 2019).

[17]The increase in triggering stressors (i) considers economic and housing insecurity and physiological factors, such as trauma and the use of alcohol and drugs (Thurston et al., 2021; Fisher, 2010; Briere and Elliott, 2000). The enabling environment (ii) refers mainly to a decrease in the quality of health and safety services and a rupture with community and social networks (Thurston et al., 2021; Gearhart et al., 2018; Parkinson and Zara, 2013). Finally, the exacerbating drivers (iii) are based on gender and socioeconomic inequalities, family structure, and unbalanced power (Thurston et al., 2021; Masson, 2022; Fisher, 2010).

impact of flood exposure on intimate violence, using data from the National Family Health Surveys, a sub-sample of the DHS, in four Indian states during the period 1999 to 2015.[18] She shows that increased exposure to natural disasters increases physical violence by 61% and sexual violence by 232%. Similar evidence is provided for hurricanes in the US (Anastario et al., 2009) and bushfires in Australia (Schumacher et al., 2010). Rao (2020) also shows that these increases are long lasting, which is consistent with the evidence from Fisher (2010), Gearhart et al. (2018) and the results of my paper.

To my knowledge, Epstein et al. (2020) is the only quantitative study that analyzes drought impact on intimate violence. They use cross-sectional DHS data for sub-Saharan Africa to analyze the impact of a one-year deviation from long-term precipitation trends on intimate violence in the same year. They show that women living in such areas face a higher probability of intimate violence than women of the same country, living in areas not exposed to extremely low precipitation.[19] Alongside these findings, my research helps to complement the evidence on the impact of exposure to droughts on intimate violence by using a pseudo-panel approach and identifying the impact in the short- to long-term. Therefore, my research is the first to identify a lasting impact of droughts on intimate violence.

The remainder of this paper is structured as follows. Section 2.2 describes the databases I use, the procedure I use to construct my unique database for this research, and describes the empirical strategy, including my pseudo-panel approach. Section 2.3 shows a summary of the data and reports the estimation results. Finally, Section 2.4 briefly discusses the results and concludes.

2.2 Data and empirical strategy

In this section, I discuss how I use my unique database to build a fine-grained analysis of the impact of droughts on births and intimate violence. First, I describe how I build the variables of

[18]In her analysis, Rao (2020) use the state of residence of the survey respondent as a proxy for flood exposure.

[19]The analysis of Epstein et al. (2020) define droughts as variations from long-term precipitation patterns and considers cross-section variations within countries in the survey-year. They use an ordered logistic regression model with country-fixed effects to control for country-level differences and define their treatment following a bin approach, where extreme deviations in precipitation are a categorical variable for areas with high exposure to droughts. In addition to the increasing effect of low precipitation on intimate violence, they also show that young or unemployed women face a higher probability of intimate violence. Due to limitation of precipitations data, their analysis covers the years 2011 to 2018 and they are not able to assess the impact of multiple droughts or the lasting impact after a drought.

Figure 2.1: DHS survey regions observed

interest, especially new births, exposure to drought, and report of intimate violence. Second, I describe the main specification I have used to examine the impact of drought on new births and intimate violence variables. Third, I discuss the four key dimensions of the robustness checks I have employed to ensure the stability of my findings. To conduct this analysis, I have drawn on data from two sources: the Individual Recode DHS database, which contains information on women's characteristics such as their number of children, contraceptive use, and reports of intimate violence, and the EM-DAT database, which provides data on the severity and occurrence of droughts. In the following subsection, I will provide further details on these two datasets.

2.2.1 Data

My database is a survey sample covering 16 countries during the years 2004 to 2018.[20] To analyze the impact of droughts on births and intimate violence, I have focused on survey regions, defined by the DHS, that have undergone more than one survey within the same borders during this time period. These regions are located at the sub-national level and are comprised of states

[20]The 16 countries in the survey database are: Benin, Burkina Faso, Cameroon, Congo Democratic Republic, Ethiopia, Ghana, Kenya, Liberia, Malawi, Mali, Mozambique, Namibia, Senegal, Sierra Leone, Tanzania, Togo

or aggregations of states within a country.[21] Figure 2.1 illustrates the survey regions included in my analysis (see also table E1).

From my survey sample, I use observations of women who have been randomly selected to participate in the domestic violence subpart of the DHS surveys. To be eligible for the survey, a woman must be of reproductive age (between 15 and 49 years) and her privacy must be insured during the interview. The DHS provides sampling weights to adjust for variations in the probability of selection, allowing for the production of estimates that are representative of the population of women in each region (ICF, 2018). I have limited my sample to women who have completed the domestic violence subsurvey to have a consistent database for both my analyses on fertility and interpersonal violence. Although I cannot establish causality between the outcome variables using these survey data, this setup allows me to examine how childbirth and intimate violence evolve simultaneously within the same population exposed to natural disasters.

To represent births, I have created a binary variable that takes the value of 1 if the respondent has given birth in the 12 months prior to taking the survey. To focus on the control and adaptation of fertility, I use a dummy variable to reduce the potential noise caused by multiple births at the term of a pregnancy.

I also identify two dependent variables to represent exposure to intimate violence. These are binary variables that take a value of 1 if the respondent reports experiencing either sexual violence or both sexual and physical violence by their intimate partner. Data on intimate violence are chronically under-reported (Thurston et al., 2021; Gearhart et al., 2018; Rao, 2020), which can result in conservative estimates. However, the DHS domestic violence subsurvey takes measures to ensure privacy during the interview, which may reduce under-reporting.

To measure exposure to droughts, I have created a treatment variable that sums the number of people affected by droughts in the survey region during the five years preceding the survey, normalized by the population of the survey region (Noy, 2009; Klomp, 2016; Cavallo et al., 2013; Panwar and Sen, 2019).[22] This population variable is based on census data conducted prior to the survey and used by the DHS to determine the population of each survey region. This timing allows me to reduce the potential endogenity of this variable. From this specification, I

[21]For additional details, see ICF (2018).

[22]The count of people affected is based on the EM-DAT numbers reported, which report affected people as people that needed direct assistance during an emergency situation. For additional details, see Guha-Sapir (2021)

measure the exposure of women within statistical regions to drought. The higher the value, the greater the probability that the women taking the survey have been exposed to several or very large droughts. Using a five-year sum of affected people allows me to estimate the impact of drought on medium-term exposure.

In addition to this medium-term impact, I have also defined a short-term impact and a long-term impact of drought exposure by reducing the sum of affected people to two years and increasing it to ten years prior to the survey, respectively. These two additional treatment variables allow me to investigate how the impact of drought exposure may vary in magnitude depending on the time elapsed since the first exposure to drought. Using a five, two and ten year sum enables me to examine how the impact of drought exposure changes over time, as the increasing duration of drought exposure may have different effects on fertility and intimate violence. Therefore, from these variations, the short-term impact of exposure is tested, as well as the short- and long-term impact of exposure. This treatment variable is reported for each DHS region.

In order to compare women of the same generation over time, I have used the date of birth of each respondent to construct 20-year cohorts for my pseudo-panel estimation. This cohort definition, in conjunction with the individual weights provided by the DHS, allows me to ensure that data from women in the same statistical area and of the same generation are compared, while also improving the representativeness of the survey sample in my estimates.

To control for the use of modern contraception, I have used information on contraceptive usage from the DHS to determine whether the respondent has been using contraception for a period of time long enough to prevent birth during my measurement period (two years). This helps to avoid the potential simultaneity bias that could arise from controlling for women who use contraception immediately after giving birth, which could lead to a downward bias in my estimates of contraception use.

Finally, my novel database allows me to identify birth behaviors and intimate violence exposure at the respondent level for each year of the surveys, while also taking into account the occurrence of drought events of each respondent's living DHS region during the five years preceding the surveys.

2.2.2 Estimation of the main effects

To explain the impact of the exposure to droughts on childbirths and intimate violence, I estimate the following baseline equations broadly similar to the specification adopted in the literature analyzing the full impact of natural disasters. The objective of my empirical strategy is to quantify how random variations in droughts that occurred in the five years prior to a survey in year t, within a given region r, affect fertility or the report of intimate violence in year t by a woman i in that region r. Formally, I denote the magnitude of droughts by $\mathit{Droughts}_{r,t}$ which is measured as the relative number of people affected by droughts.

I compute separate regressions for fertility and intimate violence measures, I can write the main specifications in general terms as follows:

$$\mathit{outcome}_{i,t} = \alpha + \rho \mathit{Droughts}_{r,t} + \delta_t + \theta_i \times \eta_r + \epsilon_{i,t}\,. \tag{2.1}$$

where $\mathit{outcome}_{i,t}$ is a dummy variable that takes the value 1 if at least one birth occurred during the year leading to the survey, or a dummy variable that takes the value 1 if the respondent reported intimate sexual violence or intimate physical and/or sexual violence. $\mathit{Droughts}_{r,t}$ is the sum of people directly affected by droughts that occurred in a survey region in the 5 years prior to the survey divided by the population of the survey region. The goal of these estimates is to understand the full effect of natural disasters on the outcome variables, following the literature on the full impact of natural disasters and potential endogeneity (Berlemann and Steinhardt, 2017; Cattaneo and Peri, 2016; Beine and Parsons, 2015; Angrist and Pischke, 2009). As such, no control variables are included in the specification. Furthermore, year-fixed effects (δ_t), fixed effects for the 20-year cohort of each woman (θ_i), and fixed effects for the survey region (η_r) are controlled for in the model. The interaction between cohort categorization and region-fixed effects ($\theta_i \times \eta_r$) is also included, allowing for the creation of a pseudo-panel identification. This pseudo-panel specification, together with the weights provided by the DHS used to compute my estimations, allows me to use this interaction between cohorts and survey regions as a substitute for individual fixed effects. As surveys are anonymized, comparing respondents of the same generation living in the same survey region allows comparing respondents with similar characteristics. Lastly, $\epsilon_{i,t}$ is an error term.

The inclusion of cohort effects and time-fixed effects in the specification helps to isolate the impact of random variations in droughts, as time-invariant regional characteristics and time trends are controlled for. This allows for the identification of variations in reported intimate violence that can be attributed to droughts. The procedure for collecting information on intimate violence remains the same over time, and the cohort and time-fixed effects control for regional and time trends. Therefore, a statistically significant impact of droughts on the reporting of intimate violence can be considered a good proxy for estimating the intimate violence that is generated by natural disasters.

Next, I document how the impact of droughts can interact with the use of contraception. Therefore, with this specification, I look at the means for women to control and adapt their fertility in relation to drought shocks. This is shown in the following specification:

$$\begin{aligned} newbirth_{i,t} = \alpha + \rho_1 Droughts_{r,t} + \rho_2 Contraception_{i,t} + \rho_3 Droughts_{r,t} \\ \times Contraception_{i,t} + \delta_t + \theta_i \times \eta_r + \epsilon_{i,t} \,. \end{aligned} \tag{2.2}$$

were $newbirth_{i,t}$ is my birth dummy variable, $Contraception_{i,t}$ is a dummy variable equal to 1 if the survey respondents used contraception methods in a sufficient time to prevent any new birth during the 12 months prior to the surveys, and $Drought_{r,t} \times Contraception_{i,t}$ is the centered interaction between the relative sum of people affected by droughts and the contraception binary.

In summary, I use the strategy defined in Equation 2.1 to test the hypothesis that exposure to droughts increases the probability of births and intimate violence and I use Equation 2.2 to examine whether the link between exposure to droughts and births is moderated by the use of contraception.

2.2.3 Robustness checks

I document the robustness of my results along four dimensions: (i) the definition and the scale of droughts, (ii) variations in my sample, (iii) variation in the definition of my cohort identification, and (iv) non-linear and unweighted models.

To control for the definition of my treatment, exposure to droughts, I consider four variations. Fitst, I use the sum of drought events in the five years preceding the survey. This allows for an understanding of how the occurrence of a drought, regardless of magnitude, affects the results. Second, I use the sum of people affected by droughts in the five years preceding the survey, without rescaling for the population of the survey region. This allows for an understanding of how the total number of people affected by drought impacts the results and how scaling by the survey region population changes the estimates. Third, observations exposed to drought shocks greater than one standard deviation are dropped to test whether the estimations are driven by outliers. Fourth, I delay the treatment by one year to examine the temporality between the treatment and the outcome variables.

To control for the impact of variation in the size of the sample, the effect of people affected by droughts on the new birth variable is estimated without restricting the sample to women who were randomly chosen to answer the DHS sub-survey on domestic violence. This allows for a comparison of results to the main estimations in a sample with 3.5 times more observations of women.

Two strategies are used to control for the impact of the cohort on the results. First, I replace cohort effects with survey region fixed effects. This reduces the characteristics in common between women in the same entity, but allows for the estimation of a model with less specification than the cohort identification. Second, I restrict the sample to cells with a minimum of 100 observations, so that survey region cohorts have at least 100 women. This allows for an examination of whether the estimates are biased by cells that may be less representative of the population due to their low number of observations.

Finally, I account for the influence of the model by including two supplementary specifications. First, I use the Probit estimation method, as the outcomes are dummy variables. Second, I estimate my main models without weighting the population by the weights provided by the DHS. This allows for an understanding of the role these weights play in the estimates.

2.3 Results

This section reports on the empirical results. In the first part of this section, I provide summary statistics of the data. This is followed by the main results, which document the impact of droughts on new births and the impact of droughts on intimate violence. The final part of the section includes the results of the four sets of robustness checks.

2.3.1 Descriptive statistics

Table 2.1 provides summary statistics. The average of the binary variable indicating whether a woman had a child in the past 12 months before the survey is 0.63, meaning that more than half of the women in the sample had a child in the past year. This high average is consistent with the high fertility rate of 5.3 children per woman in sub-Saharan African countries (excluding

Table 2.1: Descriptive statistics

	Mean	Std. Dev.	Min	Max
New birth	0.63	0.48	0	1
Contraception	0.08	0.27	0	1
Sexual violence	0.09	0.29	0	1
Physical and / or sexual violence	0.14	0.35	0	1
Population share affected by droughts	0.38	2.02	0	17.96

Notes: *New birth* is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by intimate partner. *Physical violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and sexual violence by intimate partner. *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. The period of observation is from 2004 to 2019.

high-income) reported by the World Bank for 2010, compared to the fertility rate of 2.9 children per woman in Middle and East African countries (excluding high-income) (United Nations Population Division, 2022b).

The average use of modern contraception is 0.08, indicating that less than 10% of the women use contraception. This is lower than the average modern contraceptive prevalence of 16% in 2000 and 18% in 2010 reported by the World Bank for sub-Saharan African countries (excluding high-income). These values are particularly lower than the prevalence of 50% in 2000 and 52% in 2010 in Middle and East African countries (excluding high-income) (United Nations Population Division, 2022a). The use of contraception in my sample is relatively low, yet the area I analyze is also exposed to a relatively low level of contraception for women.

The binary variable for reported intimate sexual violence has an average of 0.09, meaning that less than 10% of the women report being already exposed to sexual violence by an intimate partner. The average of the binary variable indicating reports of sexual and/or physical violence in intimate relationships is 0.14, indicating that 14% of the women in the sample reported being exposed to such violence. These averages are lower than the estimated 33% of women aged 15-49 who have been exposed to intimate physical and/or sexual violence in Sub-Saharan African countries in 2018, according to the World Health Organization (World Health Organization, 2021). This may indicate that the variables for intimate violence are being under-reported, which could lead to a downward bias in the estimations and make them conservative(Thurston

Table 2.2: Baseline results: new birth

Treatment:	*5 years preceding survey*		*10 years preceding survey*		*2 years preceding survey*	
	Drought	Drought + contraception	Drought	Drought + contraception	Drought	Drought + contraception
	(1)	(2)	(3)	(4)	(5)	(6)
Population share affected by droughts	0.15^{***}	0.15^{***}	0.06^{**}	0.06^{**}	0.14	0.15
	(0.05)	(0.05)	(0.03)	(0.03)	(0.09)	(0.09)
Contraception	–	-0.08^{***}	–	-0.08^{***}	–	-0.076^{***}
		(0.01)		(0.01)		(0.01)
Population share affected by droughts × Contraception	–	-0.01^{**}	–	-0.01^{**}	–	-0.03^{***}
		(0.00)		(0.00)		(0.01)
Constant	0.42^{***}	0.40^{***}	0.38^{***}	0.37^{***}	0.34^{***}	0.33^{***}
	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
Observations	81,553	81,553	81,553	81,553	81,553	81,553
R-squared	0.13	0.13	0.13	0.13	0.13	0.13

Notes: In all columns, the dependent variable - *New birth* - is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. In column 1 and 2, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population. In column 3 and 4, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 10 years prior to the survey divided by the survey region population. In column 5 and 6, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 2 years prior to the survey divided by the survey region population. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. *Population share affected by droughts × Contraception* is a centered interaction term between drought measurements and contraception use. In all columns, I use time fixed effects and 270 cohort fixed effects. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

et al., 2021; Gearhart et al., 2018; Rao, 2020).

Finally, the relative share of people affected by droughts during the five years preceding the survey shows that the average of the relative affected people is 38% of the population and the minimum and maximum are 0 and 17.96. Taking into account the maximum, the value that I obtain is possible as drought can occur several times during the observation period and can affect more than one survey area. It is possible that extreme values in the data could bias the results of my analysis. To address this issue, I perform a robustness check in which I only consider exposure to drought up to a maximum of 1 standard deviation (2.02).

2.3.2 Main results

2.3.2.1 Droughts impact on new birth

Table 2.2 reports the empirical baseline results for the effect of drought exposure on new births. Each set of 2 columns has a different treatment exposure. Columns 1 and 2 show the effect of drought exposure over the five years preceding the survey, while columns 3 and 4 over the ten years and columns 5 and 6 show over the two years. The coefficients can be interpreted as showing an increase in the probability of new births due to an additional drought affecting the entire population of the survey region. Columns 1, 3, and 5 show the full impact of droughts on new births, while columns 2, 4, and 6 show the partial impact of droughts, taking into account contraception usage. The estimations in all columns use a pseudo-panel approach with 20-year cohorts and include time-fixed effects. The results are sample-weighted and include robust standard errors in parentheses.

The results in columns 1 and 2 indicate that an increase in drought exposure during the five years prior to the survey increases the probability of a woman having a new child. Results in column 1 indicate that an additional drought that has affected the entire population of the survey region during the five years preceding the survey significantly increases the probability that women have a new child by 0.15 on average (p-val.$<$0.01). The estimate in column 2 indicates that the partial effect of droughts on the probability of having a new child is similar to the full effect, with an additional drought increasing the probability of women having a new child by 0.14 on average (p-val.$<$0.01). Furthermore, the use of contraception significantly reduces the probability that a woman has a new child by 0.08 (p-val.$<$ 0.01). The interaction term between contraception use and droughts significantly decreases the probability of having a new child for a woman exposed to an additional drought by 0.01 (p-val.$<$0.01). This indicates that the use of contraception reduces the impact of droughts by approximately 7%. These results suggest that exposure to droughts has a positive impact on fertility over a medium-duration period.

Similarly, the results in columns 3 and 4 suggest that long-term exposure to droughts has a positive impact on new births. The estimate in column 3 indicates that an additional drought occurring in the 10 years preceding the survey significantly increases the probability that women have a child (0.06, p-val.$<$0.05). The estimates in column 4 indicate that the partial effect of droughts when considering contraception is not significantly different from the full effect. The interaction between contraception use and the impact of droughts shows that women using contraception significantly reduce the long-term impact of droughts by 0.01, which is a reduction of approximately 16% of the impact of droughts (p-val.$<$ 0.05). These results confirm that exposure to droughts has long-lasting effects on new births.

The results in columns 5 and 6 also show a positive effect of exposure to droughts on new birth in the two years preceding the survey, the estimates are less precise. The estimate in column 5 shows that droughts have a positive impact on new births that is similar in magnitude

(0.14, p-val.>0.1) to the estimate in column 1 (0.15), but less precise. The estimates in column 6 provide similar results, but are less precise than the estimate in column 1. Due to the lack of precision in the estimate of drought exposure, it is difficult to accurately detail the interaction between exposure to droughts and the use of contraceptives.

Overall, the estimations in table 2.2 suggest that exposure to drought has a positive impact on the probability of having a new child, and an increase in drought exposure within a country can lead to an increase in fertility rates. These findings are consistent with the evidence presented by other studies on the impact of natural disasters, such as Davis (2017) and Nobles et al. (2015).My results also suggest that the use of contraception can mitigate the impact of droughts on the probability of having a new child, although the magnitude of this effect seems to be relatively low. It is possible that this could be due to a change in women's contraceptive behavior, despite still reporting the use of contraception. The results reported in Table 2.2 also provide information on the medium- and long-term impact of exposure to drought on women's reproductive behaviors. While the difference between the estimate for exposure to droughts of 2 years and 5 years preceding the survey appears to be moderate and shows an impact in the medium term after exposure, the coefficient for the sum of people affected during the 10 years preceding the survey shows potential leads of long-term impacts of exposure to droughts on new birth.

2.3.2.2 Droughts impact on intimate violence

Table 2.3 presents my empirical baseline results for the increase in intimate violence. Each set of 2 columns uses a different treatment exposure. The treatment variables are calculated in the same manner as in Table 2.2 and the coefficients can be interpreted as showing the increase in probability of intimate violence based on an additional drought affecting the entire population of a region. Columns 1, 3 and 5 report results of the full impact of people affected by droughts on reports of sexual intimate violence. Columns 2, 4 and 6 report results of the full impact of people affected by droughts on reports of physical and/or sexual intimate violence. In all columns, I use my pseudo-panel approach with cohorts of 20 years with time-fixed effects, and I report sample-weighted estimations and robust standard errors in parentheses.

Estimates in columns 1 and 2 report that drought exposure has a medium-term positive impact on the report of sexual and physical and/or intimate violence. The results in column 1 indicate that an additional drought of the size of the population of the survey region during the

Table 2.3: Baseline results: intimate violence

Treatment:	*5 years preceding survey*		*10 years preceding survey*		*2 years preceding survey*	
Outcome:	*Sexual violence*	*Physical and/or sexual violence*	*Sexual violence*	*Physical and/or sexual violence*	*Sexual violence*	*Physical and/or sexual violence*
	(1)	(2)	(3)	(4)	(5)	(6)
Population share affected by droughts	0.11***	0.10**	0.05***	0.04*	0.15***	0.18**
	(0.03)	(0.04)	(0.02)	(0.02)	(0.51)	(0.07)
Constant	0.05**	-0.05*	0.03	-0.07**	0.01	-0.08***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.01)	(0.02)
Observations	81,553	81,553	81,553	81,553	81,553	81,553
R-squared	0.07	0.08	0.07	0.08	0.07	0.08

Notes: In column 1, 3 and 5, the dependent variable - *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by an intimate partner. In column 2, 4 and 6, the dependent variable - *Physical and/or sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and/or sexual violence by an intimate partner. In column 1 and 2, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population. In column 3 and 4, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 10 years prior to the survey divided by the survey region population. In column 5 and 6, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 2 years prior to the survey divided by the survey region population. In all columns, I use time fixed effects and 270 cohort fixed effects. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

five years preceding the survey highly significantly increases the probability that women report intimate sexual violence by 0.11 on average (p-val.<0.01). The estimates in column 2 show that the same additional drought significantly increases the probability that women report intimate sexual and/or physical violence by 0.09 on average (p-val.<0.05).

Columns 3 and 4 also report a positive impact of drought exposure on intimate violence, up to ten years after exposure. The estimate in column 3 indicates that an additional drought during the ten years preceding the survey significantly increases the probability that women report intimate sexual violence (0.05, p-val.<0.01). In addition, estimates in column 4 show that a similar drought shock increases women's report of intimate sexual and/or physical violence (0.04). However, this estimate is only loosely statistically significant (p-val.<0.1).

Columns 5 and 6 show that intimate violence reports increase by a larger share in the short term after exposure to drought. Specifically, drought shocks in the two years preceding the survey significantly increase the probability of women reporting intimate sexual violence by an average of 0.15 (p-val. < 0.01) and significantly increase the probability of women reporting intimate sexual and/or physical violence by an average of 0.1. This result suggests that, in the short term following exposure to a drought, there is a higher likelihood of intimate sexual

Table 2.4: Robustness checks: Drought definition

Treatment:	*Count of droughts*				*Droughts in level*			
Outcome:	*New birth*		*Sexual violence*	*Physical and/or sexual violence*	*New birth*		Sexual violence	*Physical and/or sexual violence*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population share affected by droughts	0.03	0.03	0.03**	0.02	0.05***	0.05**	0.02*	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Contraception	–	-0.08***	–	–	–	-0.10***	–	–
		(0.01)				(0.01)		
Population share affected by droughts × Contraception	–	-0.02**	–	–	–	-0.01*	–	–
		(0.01)				(0.00)		
Constant	0.40***	0.38***	0.06	-0.06	0.51***	0.49***	0.05	-0.06
	(0.07)	(0.07)	(0.04)	(0.05)	(0.08)	(0.08)	(0.04)	(0.06)
Observations	81,553	81,553	81,533	81,553	81,553	81,553	81,533	81,553
R-squared	0.13	0.13	0.07	0.08	0.13	0.13	0.07	0.08

Notes: In columns 1,2 5 and 6, the dependent variable - *New birth* - is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. In columns 3, and 7, the dependent variable - *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by an intimate partner. In columns 4 and 8, the dependent variable - *Physical and/or sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and/or sexual violence by an intimate partner. In columns 1 to 4, *Population share affected by droughts* is the count of drought events reported in a survey region in the 5 years prior to the survey. In columns 5 to 8, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey in levels. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. *Population share affected by droughts × Contraception* is a centered interaction term between drought measurements and contraception use. In all columns, I use time fixed effects and 270 cohort fixed effects. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

and/or physical violence compared to intimate sexual violence alone.

In general, the results in Table 2.3 show that the sum of people affected by droughts leads to significant and positive increases in intimate sexual violence and intimate sexual and/or physical violence reported by women. These findings support previous research on domestic violence, such as Khanna and Fujii (2021), Parkinson and Zara (2013), or Schumacher et al. (2010), and indirectly support the idea that droughts increase risk factors for intimate violence. Furthermore, these results show that exposure to drought has a significant impact on intimate violence in the short and long term, indicating that the increase in the level of violence against women after a drought persists for a long time. However, the magnitude of this impact is stronger in the short term after exposure, which may be due to a gradual decrease in risk factors after shock.

2.3.3 Robustness checks

2.3.3.1 Measures of droughts

Table 2.4 reports the results of the robustness checks using alternative drought measures. In columns 1 to 4, I use the count of drought occurrences in each survey region in the five years preceding each survey. In columns 5 to 8, I use the sum of people affected by drought in a survey region in the five years prior to the survey in levels. The outcome in columns 1, 2, 5 and 6 is the new birth variable. The outcome in columns 4 and 7 is the binary variable for reported intimate sexual violence, and in columns 5 and 8 is the binary variable for reported sexual and/or physical violence. In all columns, I use my pseudo-panel identification with time-fixed effects, and I report sample-weighted estimations and robust standard errors in parentheses.

Columns 1 to 4 of Table 2.4 report that the occurrence of drought leads to an increase in new births and intimate violence. In these four columns, the estimates of the drought count are of smaller magnitude compared to my main estimates and less precise for columns 1, 2 and 4 (p-val.>0.1). This may be because the magnitude of droughts is not taken into account. All of these results show a similar trend to our main results, but using a measure of drought occurrences instead of the sum of people affected. However, these estimates also suggest that using the sum of people affected by a drought allows me to better quantify the impact and magnitude of the additional shock.

Columns 5 to 8 report that measuring the people affected by drought in levels does not affect my main results. Again, the four estimates are positive, with a smaller magnitude, and the estimates of intimate violence, in columns 7 and 8, are not precisely estimated (p-val>0.5). These results, while using the sum of people affected by droughts, indicate similar effects for all my outcome variables. Furthermore, it shows that using levels, thus not scaling the impact by the population size of the survey region, can reduce the precision of my estimates.

Next, Table 2.5 reports the results of the robustness check test for extreme values of exposure to droughts and for a one-year delay measure of droughts. In columns 1 to 4, the treatment is the sum of affected people directly induced by droughts that occurred in a survey region in the five years prior to the survey divided by the population of the survey region, but I exclude observations where the measure of exposure to droughts is greater than one standard deviation (2.02). In columns 5 to 8, I impose a one-year delay on the main treatment. In columns 1, 2, 5 and 6, the outcome is the new birth dummy variable. The outcome in columns 4 and 7 is the binary variable for reported intimate sexual violence, and in columns 5 and 8 is the binary variable for reported sexual and/or physical violence. In all columns, sample-weighted estimates and robust standard errors are reported in parentheses.

Columns 1 to 4 of the table 2.5 show that the exposure to drought that I measure in my main estimates is not affected by extreme values in my treatment variable. All estimates of the impact of drought, on new births and intimate violence are of the same magnitude and precision as my

Table 2.5: Robustness checks: Droughts definition II

Treatment:	*Droughts without extreme values*				*Relative sum of droughts lagged*			
Outcome:	*New birth*		*Sexual violence*	*Physical and/or sexual violence*	*New birth*		Sexual violence	*Physical and/or sexual violence*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population share affected by droughts	0.15***	0.15***	0.11***	0.10**	0.05	0.05	0.06***	0.07**
	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)
Contraception	–	-0.08***	–	–	–	-0.08***	–	–
		(0.01)				(0.01)		
Population share affected by droughts × Contraception	–	-0.01	–	–	–	-0.01	–	–
		(0.04)				(0.01)		
Constant	0.42***	0.40***	0.05**	-0.05*	0.35***	0.34***	0.02	-0.07***
	(0.06)	(0.06)	(0.02)	(0.03)	(0.05)	(0.05)	(0.02)	(0.03)
Observations	80,506	80,506	80,506	80,506	81,553	81,553	81,533	81,553
R-squared	0.13	0.13	0.07	0.08	0.13	0.13	0.07	0.08

Notes: In columns 1,2, 5 and 6, the dependent variable - *New birth* - is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. In columns 3, and 7, the dependent variable - *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by an intimate partner. In columns 4 and 8, the dependent variable - *Physical and/or sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and/or sexual violence by an intimate partner. In columns 1 to 4, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population, but I exclude observations where the drought impact is larger than one standard deviation. In columns 5 to 8, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population, lagged by 1 year. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. *Population share affected by droughts × Contraception* is a centered interaction term between drought measurements and contraception use. In all columns, I use time fixed effects. I use 255 cohort fixed effects in columns 1 to 4, and 270 in columns 5 to 8. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

main estimates. However, the precision of the interaction between people affected by droughts and the use of contraception is limited (p-val$>$ 0.1).

Columns 5 to 8 of table 2.5 report a one-year delay in my treatment that does not change the positive impact of drought on my outcome variables. Although still positive, all estimates are smaller in magnitude and the impact of drought on new birth is not precisely estimated (p-val$>$0.1). These results show that the last year of exposure plays a role in the magnitude of my main estimates and in the precision of estimating the impact of droughts on new births. However, the results for intimate violence reinforce the hypothesis that exposure to droughts does not only affect intimate violence in the first year, but also has a lasting effect.

In summary, the results reported in tables 2.4 and 2.5 strongly support the validity of my main estimates and, in particular, my measure of droughts. These robustness checks demonstrate that my treatment variable provides precise and reproducible results using different definitions of droughts. This analysis also confirms that my main estimates are not influenced by extreme values or potential temporal issues.

Table 2.6: Robustness checks: data sets

Data set:	*Large sample set*		*Ever married set*				*Normal set*	
Outcome:	*New birth*				*Sexual violence*	*Physical and/or sexual violence*	*All violence*	*Only physical violence*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population share affected by droughts	0.01^{***}	0.01^{***}	0.10^{**}	0.09^{**}	0.12^{***}	0.10^{**}	0.01	0.02
	(0.00)	(0.00)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.03)
Contraception	–	-0.02^{***}	–	-0.11^{***}	–	–	–	–
		(0.01)		(0.01)				
Population share affected by droughts × Contraception	–	-0.01^{***}	–	-0.01^{**}	–	–	–	–
		(0.00)		(0.01)				
Constant	0.40^{***}	0.28^{***}	1.08^{***}	1.06^{***}	0.12^{***}	0.04	-0.07^{*}	-012^{***}
	(0.03)	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)
Observations	296,052	270,393	70,500	70,500	70,500	70,500	81,553	81,553
R-squared	0.10	0.11	0.13	0.14	0.07	0.09	0.10	0.08

Notes: In columns 1 to 4, the dependent variable - *New birth* - is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. In column 5, the dependent variable - *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by an intimate partner. In column 6, the dependent variable - *Physical and/or sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and/or sexual violence by an intimate partner. In column 7, the dependent variable - *All violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to any types of violence by an intimate partner. In column 8, the dependent variable - *Only physical violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed only to physical violence by intimate partner. In all columns, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. *Population share affected by droughts × Contraception* is a centered interaction term between drought measurements and contraception use. In columns 1 and 2, I use the DHS dataset without restricting it to the sample of women taking the domestic violence subsection. In columns 3 to 6, I restrict my data set to only women that have been ever married. In columns 7 and 8, I use my main data set. In all columns, I use time fixed effects. I use 320 cohort fixed effects in column 1, 316 in column 2, and 270 in columns 3 to 8. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

2.3.3.2 Sample variations

The next robustness checks, in Table 2.6, relate to the variations in the sample of women and two additional definitions of intimate violence. In columns 1 and 2, I estimate the impact of the sum of people affected by droughts on new birth while expanding the data set I use to the whole survey population, rather than just the subset of women who participated in the domestic violence sub-survey. In columns 3 to 6, I reproduce my main estimations for all my outcomes, while taking into account only women who have ever been in union. Columns 7 and 8 report the impact of the sum of people affected by droughts on the report of all types of intimate violence and only on the report of intimate physical violence. In all columns, sample-weighted estimates and robust standard errors are reported in parentheses.

The estimates of the impact of drought on new birth with a larger sample, in columns 1 and 2, are positive and highly significant (p-val<0.01). However, these results are smaller in magnitude compared to our main estimates and the interaction between contraception use and exposure to droughts completely cancels out the impact of droughts on new birth, as the magnitude of this interaction is equal to the magnitude of the impact of drought. These results

Table 2.7: Robustness checks: cohorts

Cohort:	*No cohorts*				*Only cohorts larger than 100*			
Outcome:	*New birth*		*Sexual violence*	*Physical and/or sexual violence*	*New birth*		Sexual violence	*Physical and/or sexual violence*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population share affected by droughts	0.05	0.05	0.07**	0.04	0.18***	0.18***	0.10***	0.09*
	(0.05)	(0.05)	(0.03)	(0.04)	(0.06)	(0.06)	(0.04)	(0.05)
Contraception	–	-0.06***	–	–	–	-0.08***	–	–
		(0.01)				(0.01)		
Population share affected by droughts × Contraception	–	-0.01	–	–	–	-0.01***	–	–
		(0.00)				(0.00)		
Constant	0.68***	0.68***	0.13***	0.08**	0.41***	0.39***	0.04	-0.07**
	(0.05)	(0.05)	(0.03)	(0.03)	(0.06)	(0.06)	(0.03)	(0.03)
Observations	81,553	81,553	81,533	81,553	72,143	72,143	72,143	72,143
R-squared	0.06	0.03	0.06	0.07	0.11	0.11	0.06	0.08

Notes: In columns 1,2, 5 and 6, the dependent variable - *New birth* - is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. In columns 3, and 7, the dependent variable - *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by an intimate partner. In columns 4 and 8, the dependent variable - *Physical and/or sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and/or sexual violence by an intimate partner. In all columns, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. *Population share affected by droughts* × *Contraception* is a centered interaction term between drought measurements and contraception use. In columns 1 to 4, I replace my cohorts effects with a survey region fixed effect. In columns 5 to 8, I restrict my estimations to cohorts with at least 100 observations. In all columns, I use time fixed effects. In columns 5 to 8, I use 190 cohort fixed effects. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

may indicate that my main fertility estimates (Table 2.2) represent the upper bound of the impact of droughts on new births. Nevertheless, the matter only concerns the magnitude, but does not question the positive impact of droughts on the probability of having a new child.

The results in columns 3 to 6 show positive and significant impacts of drought on my outcome variables. This indicates that using the whole group of women taking the domestic violence sub-survey and women within that group that have ever been in a union does not change my estimates on intimate violence but slightly decreases the magnitude of my estimates considering the impact of droughts on new birth.

Exposure to drought has a positive effect on all types of intimate violence, according to the results shown in columns 7 and 8. However, this increase is not precisely estimated (p-val>0.1). These findings suggest that exposure to drought seems to increase any type of intimate violence, but this increase is more significant for sexual and physical and/or sexual violence together.

2.3.3.3 Cohorts

In Table 2.7, I present robustness checks on the use of cohorts in my estimates. Columns 1 to 4 of the table show the results of replacing my cohort effects with survey region fixed effects and estimating my four main specifications. Columns 5 to 8 show the results of reproducing my main estimations but only including cohorts with at least 100 observations. For all columns, I provide sample-weighted estimates and robust standard errors in parentheses.

Estimates reported in columns 1 to 4 indicate a positive effect of droughts on all my outcome variables. Yet, the estimates in columns 1, 2 and 4 are not precisely estimated. These results indicate that my estimations are consistent with the average effect estimated by an entity-fixed effect model and that using a cohort approach, which compares women of the same generation, allows me to significantly improve the precision of my estimations.

The estimates in columns 5 to 8 show that considering only cohorts that are likely to be representative does not change my main estimations. However, not excluding observations in relatively small cells does not significantly alter the magnitude of my estimates and only slightly decreases the precision of the estimate for physical and/or sexual violence.

Taken together, the results in Table 2.7 demonstrate that using 20-year cohorts refines my specification and allows me to have more precise estimates of the impact of droughts on my outcome variables compared to a fixed entity model. Additionally, these results suggest that my estimates are not biased by cohort cells with potentially low representativeness of the population.

2.3.3.4 Specifications

Table 2.8 presents robustness checks to assess the validity of the specification used in my main estimates. In columns 1 to 4, I estimate the results using a Probit model. In columns 5 to 8, I repeat my main estimations without using the weights provided by the DHS. In all columns, robust standard errors are in parentheses.

The results reported in columns 1 to 4 show that using a Probit model leads to an increase in the magnitude of the impact of droughts on my three outcome variables. This indicates that using a linear method for my main estimation yields a pattern and precision similar to those of the Probit approach, but provides more conservative estimations.

In columns 5 to 8, the estimates suggest that the weights provided by the DHS do not significantly alter my results. Although the estimates for new births are slightly larger than my main estimates, the estimates of intimate violence are slightly lower and are not precisely estimated for physical and/or sexual violence (p-val$>$ 0.1), but both remain positive. These results without survey weights support the reliability of my main estimates, as they provide very similar

Table 2.8: Robustness checks: specification

Specification:	*Probit*				*No weights*			
Outcome:	*New birth*		*Sexual violence*	*Physical and/or sexual violence*	*New birth*		Sexual violence	*Physical and/or sexual violence*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population share affected by droughts	0.43***	0.42***	0.60***	0.44***	0.20***	0.20***	0.06***	0.03
	(0.14)	(0.14)	(0.17)	(0.15)	(0.03)	(0.03)	(0.02)	(0.02)
Contraception	–	-0.22***	–	–	–	-0.09***	–	–
		(0.03)				(0.01)		
Population share affected by droughts × Contraception	–	-0.03**	–	–	–	-0.01***	–	–
		(0.01)				(0.00)		
Constant	-0.20	-0.24	-2.00***	-2.17***	0.71***	0.54***	0.04	-0.06**
	(-0.16)	(0.16)	(0.26)	(0.18)	(0.03)	(0.04)	(0.02)	(0.03)
Observations	81,553	81,553	79,531	79,725	84,519	81,553	81,533	81,553
R-squared	–	–	–	–	0.11	0.12	0.06	0.08

Notes: In columns 1,2, 5 and 6, the dependent variable - *New birth* - is a binary variable equal to 1 if the woman taking the survey had a child in the past 12 months before the interview. In columns 3, and 7, the dependent variable - *Sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to sexual violence by an intimate partner. In columns 4 and 8, the dependent variable - *Physical and/or sexual violence* - is a binary variable equal to 1 if the women taking the survey reported being exposed to physical and/or sexual violence by an intimate partner. In all columns, *Population share affected by droughts* is the sum of affected people directly induced by droughts that happened in a survey region in the 5 years prior to the survey divided by the survey region population. *Contraception* is a binary variable equal to 1 if the respondent of the survey started taking modern contraception at least 24 months prior to taking the survey. *Population share affected by droughts × Contraception* is a centered interaction term between drought measurements and contraception use. In columns 1 to 4, I replace my OLS specification by a Probit specification. In columns 5 to 8, I do not use the population weights provided by the DHS. In all columns, I use time fixed effects. I use 270 cohort fixed effects in columns 1, 2 and 6 to 8, 248 in column 3, 252 in column 4 and 275 in column 5. The period of observation is from 2004 to 2018. Robust standard errors in parentheses. *,** and *** respectively denote significance at 10%, 5% and 1% levels.

estimates and demonstrate that my main results are not subject to error in weight calculations.

In summary, the results in Tables 2.4 to 2.8 demonstrate the robustness of my main estimations to a variety of specifications. In particular, these results indicate that my main specification provides precise estimates, enabling me to clearly evaluate the effect of increased exposure to drought on fertility and intimate violence.

2.4 Discussion and conclusion

This study investigates the relationship between droughts and births in 16 African countries, finding that natural disasters, particularly droughts, have an impact on birth decisions and therefore demographic trends. It also documents the role of contraception in reducing fertility shocks caused by natural disasters. At the same time, droughts also increase the probability of exposure to intimate violence, particularly sexual violence. These results suggest that when women are exposed to droughts, they face an increase in the probability of having a new child, while also being more likely to experience sexual and physical violence from their intimate partners. Additionally, exposure to drought has a long-lasting impact on these probabilities, up to ten years. My results are robust to a wide range of robustness checks.

The findings of this study indicate that intimate violence and new births follow a similar pattern for the same group of women affected by droughts. However, due to the limitations of the data used in this study, it is not possible to provide evidence on the relationship between these two outcomes when exposed to natural disaster stress. As the data used is collected through surveys, it is not possible to test whether increases in intimate sexual violence induced by natural disasters can explain a part of the increases in new births or vice versa. Therefore, more research would be necessary to examine the relationship between these phenomena under natural disaster-induced stress. However, the similar pattern found for intimate violence and new births suggests that more research is needed to understand the causal links between these factors and to develop more comprehensive and gender-sensitive policies.

My results suggest that ensuring access to contraception could reduce fertility shocks generated by natural disasters during and after a natural disaster. In addition, these findings highlight the importance of incorporating measures to protect women from intimate violence in disaster management policies and programs. This requires the development of national and international policies that address natural disasters in a gender-sensitive manner, including reinforcing policies during non-disaster times (Masson, 2022; Rao, 2020; Parkinson and Zara, 2013; Fisher, 2010). Such strategies can contribute to the sustainable development of societies by considering factors such as fertility, women's health, inequalities, and social and economic growth. Further research is needed to explore the relationship between natural disasters, fertility, and intimate violence in the context of additional variables such as intersectionality, types of violence, and the quality and availability of healthcare and security infrastructure.

Chapter 3

Extreme natural disasters and conflict intensity: A partially pooled synthetic control approach

This chapter is mainly based on a single-authored paper by Nathan Delacrétaz. For helpful comments and suggestions, I thank my supervisor Bruno Lanz, Fabien Cottier, Rebecca Stuart, and participants at IRENE workshop 2022. Excellent research assistance was provided by Alessio Lombini. Financial support from the Swiss National Science Foundation under grant number 100018_182122 is gratefully acknowledged.

Abstract:

In this article, I analyze how the intensity of conflict is influenced by droughts, floods or volcanic eruptions. I use longitudinal data for African countries from 1995 to 2020 to quantify the impact of extreme natural disasters on conflicts. Based on a partially pooled synthetic control approach, my results show that extreme natural disasters induce a decrease in conflict intensity in the following years and that this decline is mainly driven by conflict involving governments. I further show that extreme natural disasters decrease conflict-related civilian deaths and the frequency of conflict-related sexual violence. My results suggest important trends that have to be taken into account for governance and global stability, especially considering the projected increase in natural disasters.

Keywords: natural disasters; conflicts; one-sided violence; conflict-related sexual violence; synthetic control methods; staggered adoption.

3.1 Introduction

The intensity and frequency of natural disasters are increasing and extreme events alter the governance and stability of the world (IPCC, 2021). Evidence shows that natural disasters impact several socio-economic factors, such as growth (Strobl, 2012), productivity (Leiter et al., 2009), migration (Boustan et al., 2012), and fertility (Nobles et al., 2015). In addition, empirical evidence suggests that natural disasters of moderate intensity increase the intensity of conflicts, whereas extreme natural disasters mitigate conflicts (Xu et al., 2016).[1] However, evidence on how natural disasters affect conflict-related outcomes such as violence inflicted on civilians by army groups remains scant, with the exception of Haer and RezaeeDaryakenari (2022).

In this paper, I document the evolution of conflict fatalities in 13 African countries that have experienced an extreme natural disaster shock.[2] I identify all African countries that have been exposed to an extreme natural disaster using data from the Emergency Events Database (EM-DAT, Guha-Sapir, 2021).[3] In total, 15193 natural disasters from all over the world are considered to construct the distribution of disasters according to the population affected. In each instance, I calculate the intensity of the natural disaster as the percentage of the population in the country affected by the disaster. The people directly affected are those who needed help or were killed by a natural disaster. From this distribution, natural disasters with an intensity above the 99^{th} percentile are considered extreme.[4] Next, I use data on conflict fatalities from the Uppsala Conflict Data Program database (UCDP, 2021) to define the intensity of conflicts through the annual sum of conflict fatalities.

Then, I define a donor group of 29 African countries that have not been exposed to an extreme natural disaster during the entire observation period. I use a partially pooled synthetic control method (SCM) to fit an average treatment effect on the treated, reflecting post-treatment deviation in relative conflict fatalities of my treatment group to his counterfactual. This procedure allows me to estimate an average treatment on the treated that minimizes the

[1] Note that the literature on natural disasters and conflict relationships has never considered natural disasters as a sufficient or necessary condition to start conflicts (Sharifi et al., 2021; Theisen, 2017). Therefore, the start of a conflict is subject to additional contextual conditions.

[2] I use data for 42 African countries from 1995 to 2020 composed of a treated group of 13 countries and a donor group of 29 countries.

[3] The treated group is defined as African countries that have faced an extreme natural disaster in the period between 2000 and 2015, and that had not faced such natural disasters of this magnitude five years before and after the shock.

[4] With this identification strategy, the shocks considered are eleven droughts, one flood and one volcanic eruption. See details in Table F1.

pre-treatment bias per treated country and on the average (Ben-Michael et al., 2022). My results suggest that extreme natural disaster shocks generate a decrease in conflict fatalities. Quantitatively, an extreme natural disaster induces a decrease in conflict fatalities of 60 persons per million people in the year after the shock. This negative impact remains fairly constant for the five years following the shock. My results also show that the decreases that I estimate mainly impact conflict involving governments (state-based) and conflict-related civilian fatalities (one-sided).[5]

To investigate this in more detail, I exploit my identification strategy to estimate the impact of extreme natural disasters on two conflict-related outcomes: one-sided fatalities and conflict-related sexual violence, defined as armed groups deliberately killing civilians or committing any action that inflicts harm of sexual nature on civilians.[6] Both of these outcomes could follow different dynamics from the overall intensity of conflicts (Balcells and Stanton, 2021; Nordås and Cohen, 2021). I use data from the Armed Conflict Location & Event Data Project database (ACLED, Raleigh et al., 2010) to analyze the impact of extreme natural disaster shocks on the annual number of one-sided fatalities and conflict-related sexual violence events. My results further suggest that extreme natural disaster shocks induce a decline in one-sided fatalities of up to 24.60 fatalities per million people. In addition, I estimate that the occurrence of conflict-related sexual violence events is reduced by up to 8 events.

Next, I conduct a series of robustness checks to test the sensitivity of my results on conflict fatalities, one-sided fatalities and conflict-related sexual violence to potential concerns about inference of the shocks, group definitions, magnitude of the effect of the shock, and choice of predictors. These robustness checks are based on modifications of my extreme natural disaster shock threshold, donor, and treatment group definitions. I show that my results are robust to region and country exclusions in the treated and donor groups and that my identification strategy for these groups is also robust to variations in definitions of extreme natural disaster shocks. I also show that the magnitude of the estimates for conflict-related sexual violence events might be subject to an overestimation due to overfitting. However, when correcting for potential overfitting, the impact of extreme natural disasters on conflict-related sexual violence

[5] A state-based conflict is an armed conflict where at least one side involved is the government of a state. In contrast, non-state based conflicts never involve governments as a belligerent.

[6] Note that these outcomes will be analyzed alongside conflict fatalities that I define as any death related to any types of conflicts.

remains negative and, therefore, the negative treatment effect I estimate remains consistent.

These results contribute to the literature on the linkages between climate change and conflict. In this literature, two paths explain the decreases in conflict intensity due to natural disasters; the probabilities of negotiations and the difficulties of armed groups in finding resources and opportunities to engage in conflict.[7] For example, Kreutz (2012), working with global data covering the period 1990 to 2004, shows that natural disasters can increase the probability of talks between belligerents and ceasefires, but not peace agreements. Similarly, Nemeth and Lai (2022), studying territorial civil wars from 1980 to 2005, shows that when both sides of a conflict are hit by a natural disaster, the probability of negotiation increases. Walch (2018) also show that a decrease in conflicts in the three years after storms in the Philippines is explained by the difficulties of rebels in recruiting new soldiers and an increased presence of national and international actors. Since my results on conflict fatalities are mainly driven by state-based conflict fatalities, they suggest that the main path should prominently involve governments.

I also contribute to a second strand of the literature on natural disasters and violence against civilians. However, the literature on natural disasters and conflicts has not often considered one-sided violence as an outcome of interest, while the literature on one-sided violence has not often explored the link with natural disasters. This literature shows that conflict does not necessarily involve one-sided violence; therefore, they need to be analyzed as independent outcomes. For example, Weintraub (2016) show that antipoverty subsidies can increase territorial control of governments, while insurgents increase one-sided violence to recapture territory (see also Kalyvas, 2006).[8] Balcells and Stanton (2021) identifies three main rationales for these variations in one-sided violence: (i) induce coercive cooperation, (ii) increase the cost of conflicts to force concessions, and (iii) reduce the support of civilians for opponents.[9] Finally, Haer and RezaeeDaryakenari (2022) established a first milestone in quantitative analysis of the impact of

[7]The literature on the natural disaster-conflict nexus also shows that this relationship is subject to contextual factors, especially the ability to mitigate and manage the outcomes of natural disasters, defined as vulnerability in the literature (D'Angeli et al., 2022; Buhaug and von Uexkull, 2021; Ide et al., 2020; Uexkull et al., 2020). The vulnerabilities of a community are determined by economic, political, social, and environmental assets (Brancati, 2007; Nel and Righarts, 2008; Cohen and Werker, 2008; Brancati, 2007). Additional paths are analyzed to explain vulnerability to natural disasters while considering conflicts, such as agricultural production (von Uexkull and Buhaug, 2021), ethnic discrimination, or the low level of human development (Ide et al., 2020).

[8]This dynamic must be seen in the context of the literature on one-sided violence showing that one-sided violence is part of conflict strategies, more than collateral damage (Raleigh, 2012; Wood, 2010).

[9]Economic studies reinforce this argument while showing that heterogeneous shocks on wages and luxury goods can reinforce one-sided violence (Dube and Vargas, 2013; Rigterink, 2020).

natural disasters on one-sided violence. They use African data from 1997 to 2017 and provide evidence of a negative impact, lasting up to eight years, that is explained by the struggle of armed groups and civilians to survive in the aftermath of natural disasters. My research helps fill the research gap and complements these findings by providing quantitative evidence on the impact of extreme natural disasters reducing one-sided fatalities.

As an extension to the literature on one-sided violence, my research also provides evidence on the impact of extreme natural disasters on non-lethal one-sided violence, through the analysis of conflict-related sexual violence. Although most of the research on one-sided violence focuses on lethal forms, research on non-lethal violence helps to understand a wider range of harmful practices in the context of conflicts (see Nordås and Cohen, 2021, for a discussion lethal and non-lethal difference in the research). Cohen (2016) and Benson and Gizelis (2020), among others, stress that conflict-related sexual violence is not a constant in conflicts and that it can be subject to different dynamics than other types of one-sided violence.[10] Wood (2018) also shows that conflict-related sexual violence is not only due to opportunity or strategy, but is a practice subject to a specific context. Regarding these variations in dynamics and differences compared to one-sided fatalities, I analyze the impact of extreme natural disasters on the occurrence of conflict-related sexual violence. As natural disasters can alter these dynamics and practices, and as very little literature considers the link between natural disasters and conflict-related sexual violence, my research helps to understand the relationship between natural disasters and different types of one-sided violence.

This article is organized as follows. Section 3.2 describes my data and my empirical strategy, including the identification of extreme natural disaster shocks. Section 3.3 shows a summary of my data for the treated and donor groups, presents my estimation results for conflict fatalities, one-sided fatalities and conflict-related sexual violence, and shows the robustness of my approach. Finally, in Section 3.4 I discuss the results and conclude.

[10]It should be noted that it is important to differentiate domestic violence from conflict-related sexual violence. Domestic sexual violence remains the most common sexual violence, but conflict-related sexual violence is reported to be more brutal and organized (Wood, 2014).

3.2 Data and identification

In this section, I first provide an overview of the data used for my analysis, which includes conflict fatalities data from the UCDP database (UCDP, 2021), shocks from extreme natural disasters data from EM-Dat (Guha-Sapir, 2021), and one-sided fatalities and conflict-related sexual violence events data from ACLED (Raleigh et al., 2010). Next, I present my identification strategy, which outlines the method used to define the treated and donor groups. Third, I describe the synthetic control method (SCM) that I employed and provide background on its evolution. Fourth, I detail the robustness checks performed to ensure the validity of my main estimates.

3.2.1 Conflict fatalities and violence

The database for this study includes data from 42 African countries from 1995 to 2020 at the country and year level.[11] These countries are divided into a treated and donor group, as identified in Section 3.2.2. The focus on African countries is chosen because previous research has shown that developing countries are more vulnerable to the negative impacts of natural disasters (Loayza et al., 2012; Noy, 2009; Kahn, 2005), allowing for the documentation of the strongest outcomes that natural disasters can generate.

The main outcome variable is calculated as the annual sum of all conflict-related fatalities reported by the UCDP database, per country, divided by the lagged population of the country in millions, from the World Bank (2022b). The resulting variable is expressed as:

$$Conflicts_{c,t} = \frac{Sum\ of\ conflict\ fatalities_{c,t}}{Lagged\ country\ population\ in\ mio_{c,t-1}} \tag{3.1}$$

where *Sum of conflict fatalities* is the sum of all types of conflict fatalities for country *c* and year *t* and *Lagged country population in mio* is the population of country *c* in millions in year *t-1*. The total population is lagged by one year to account for the fact that extreme natural disasters can affect the total population and potentially introduce endogeneity bias.

[11] Countries in my treated group are the following: Burundi, Chad, Comoros, Eswatini, Ethiopia, Gambia, Malawi, Mali, Mauritania, Mozambique, Namibia, South Africa, and Zimbabwe. The countries in my donor group are the following: Algeria, Angola, Benin, Botswana, Cabo Verde, Cameroon, Central African Republic, Democratic Republic of Congo, Republic of Congo, Ivory coast, Egypt, Equatorial Guinea, Gabon, Ghana, Guinea, Guinea-Bissau, Libya, Madagascar, Mauritius, Morocco, Nigeria, Rwanda, Senegal, Sierra Leone, Sudan, Tanzania, Togo, Tunisia, Uganda. The countries in these groups are selected following the identification developed in Section 3.2.2. I do not take into account the remaining 12 African countries as they do not correspond to the conditions of my treated and donor groups.

The fatalities of one-sided violence are calculated using the same method as in Equation 3.1. This allows the estimation of the impact of extreme natural disasters on one-sided fatalities per million people. In the ACLED database, one-sided violence events are defined as any deliberate use of armed force on civilians by an armed group with no minimum number of civilian fatalities.

The last outcome variable is the sum of conflict-related sexual violence events, calculated at the country and year level. In this case, I use the number of events, rather than fatalities, to assess the occurrence of non-lethal violence, as the number of survivors is not clearly defined in the database. In the ACLED database, sexual violence events are defined as any action of an armed group that causes sexual harm to a civilian, such as rape, public stripping, sexual torture, and other sexually violent acts.[12]

3.2.2 Identification of treated and donor groups

To identify extreme natural disaster shocks, I use data on the number of people affected by natural disasters from the EM-Dat database. The affected people are those who required immediate assistance during a natural disaster, such as food, water, shelter, or medical assistance. I also include the number of deaths and people missing directly caused by a natural disaster. From this information, I create a variable representing the intensity of natural disasters at the country level, based on the share of the population affected by a natural disaster event. This variable is defined as follows:

$$\textit{Extreme natural disaster}_{c,t} = \frac{\textit{Sum of people affected by a natural disaster}_{i,c,t}}{\textit{Lagged country population}_{c,t-1}} \tag{3.2}$$

where *Sum of people affected by natural disaster* is the sum of people affected by a natural disaster for event *i*, in country *c* and in year *t*. As with the main outcome variable, a one-year lag is applied to the country population to reduce heterogeneity.

Next, I use the share of people affected by a natural disaster to calculate the 99^{th} percentile of the world distribution of natural disaster intensity. [13] I set this 99^{th} percentile as the threshold for defining extreme natural disaster shocks, that is my treatment. Specifically, my treatment is defined as a year which a shock bigger or equal to the 99^{th} percentile occured.[14] This strategy

[12] Sexual violence is classified as a subtype of one-sided violence in the ACLED database.

[13] This strategy is similar to the identifications of extreme natural disasters used by Cavallo et al. (2013); Diop et al. (2021)

[14] The 99^{th} percentile of natural disaster impact is equal to 0.13 of the affected country population

allows me to consider two elements of the contextual requirements for the use of SCM defined by Abadie (2021). First, the severity of shocks ensures that the variations induced in conflicts are large enough to surpass the volatility of conflict fatalities, allowing a distinct treatment effect to be identified. Second, extreme natural disasters are by nature unpredictable, and therefore there is a lack of anticipation before the shocks.

Using this definition of extreme natural disaster shocks, I define my treated group as African countries that experienced an extreme natural disaster in the period between 2000 and 2015, and did not experienced a natural disaster of this magnitude five years prior to and after the shock. This identification strategy allows to identify a distinct pre-treatment period occurring five years before the event, a year of shock defined by the exposure to an extreme natural disaster, and a post-treatment period of five years. This allows me to estimate the difference in conflict fatalities between the countries exposed to shocks and their counterfactuals. To define my donor pool, the countries used to build a counterfactual of the countries exposed to the shocks, I also use my extreme disaster threshold. As a result, my donor pool is defined by African countries that have not faced extreme natural disasters during the observation period of 1995 to 2020.

There may be concerns about the permissiveness of this definition of the donor pool in terms of exposure to natural disasters and its impact on inference and the availability of a clear comparison group (Abadie, 2021). To address these concerns, below I perform several robustness checks to test the effect of the condition on the selection of my donor pool.

3.2.3 Method: Partially pooled SCM

Based on the SCM approach of Abadie and Gardeazabal (2003) and Abadie et al. (2010), I define the outcome variable for each treated country j as the annual sum of conflict fatalities per million people, calculated as the sum of conflict fatalities divided by the population of the country with a one-year lag, represented as $Conflicts_{j,t}$.[15] To construct the synthetic counterfactual for each treated country j, I create a weighted sum of the annual sum of conflict fatalities per million people, $Conflicts_{i,t}$, for all countries i in the donor pool. This is represented formally as:

[15] The analysis of the impact of violence on growth in the Basque Country (Abadie and Gardeazabal, 2003) is a first important step in the use of SCM in different contexts. To observe the impact of terrorism in the Basque Country on GDP per capita, Abadie and Gardeazabal (2003) construct a synthetic counterfactual to the Basque Country by weighting other Spanish regions. This strategy allows them to identify the terrorism-induced negative shock in GDP per capita of the Basque Country by comparing the evolution of the treated GDP per capita with the synthetic one.

$$Conflicts^{SCM}_{j,t} = \sum_i \omega_{j,i} Conflicts_{i,t} \ , \tag{3.3}$$

where $\omega_{j,i}$ is the weight assigned to the donor country i in constructing the synthetic counterfactual of the treated country j. These weights are calculated by minimizing the squared sum of pre-treatment differences in the relative conflict fatalities between the treated and synthetic countries. This minimization is defined as follows:

$$\min_{\omega_{j,i}} \sum_{t=0}^{T_0} (Conflicts_{j,t} - \sum_i \omega_{j,i} Conflicts_{i,t})^2 \qquad s.t. \sum_i \omega_{j,i} = 1 \quad \omega_{j,i} \geq 0 \tag{3.4}$$

where T_0 represents the last year before treatment. Equations 3.3 and 3.4 allow me to express the reasoning behind the construction of the counterfactual of a treated country. The simplicity of this definition is cited by Abadie (2021) as a key strength of the SCM.

Building upon equations 3.3 and 3.4, it is important that the difference between the pre-treatment conflict fatalities of a country and its synthetic counterfactual be close to 0 in order to ensure a good matching (as specified in Equation 3.4). In the post-treatment periods, the difference in relative conflict fatalities between a treated country and his synthetic counterfactual can be used to measure the treatment effect of the extreme natural disaster shock. This can be defined in the following way:

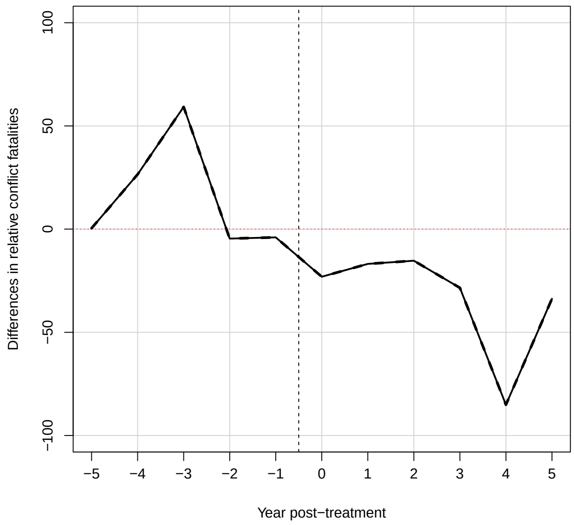
$$Conflicts_{j,t} = \phi_t D_t + Conflicts^{SCM}_{j,t} \tag{3.5}$$

where D_t is a dummy variable indicating the year of treatment. However, as noted by Abadie (2021), the application of SCM can be subject to bias in certain contexts when estimating the treatment effect. This bias arises from the fact that the probability of achieving a perfect pre-treatment match decreases with shorter duration of pre-treatment or smaller number of units in the donor group. Since a good pre-treatment fit is necessary to accurately estimate the treatment effect, this bias can limit the application of SCM. To address this issue, Ben-Michael et al. (2021) propose a bias correction term on pre-treatment outcome values. This correction is derived from a ridge regression of post-treatment outcomes for donor units, resulting in the definition of ridge-augmented SCM (RASCM) as:

$$Conflicts^{RASCM}_{j,t} = \sum_i \omega^{RASCM}_{j,i} Conflicts_{i,t} + (Y_j - \sum_i \omega^{RASCM}_{j,i} Y_i) \cdot \hat{\eta} \tag{3.6}$$

where Y_j is the vector of relative conflict fatalities before treatment and $\sum_i \omega^{RASCM}_{j,i} Y_i$ is

Figure 3.1: Separate SCM estimate differences in relative conflict fatalities



Notes: The solid black line represents the separate SCM estimated differences between the treated group and the synthetic counterpart.

the ridge-augmented estimate of the SCM bias, which corrects for the bias due to imperfect matching. Additionally, the ridge-augmented SCM does not restrict the weights to be positive. By using this bias correction, Ben-Michael et al. (2021) demonstrate that the ridge-augmented SCM produces smaller pre-treatment residuals, leading to a more accurate treatment effect. This ridge-augmented approach forms the basis of my approach at the country level, as the bias correction enables the use of SCM in a wider range of applications where good matching may not have been achievable otherwise.

In my analysis, the 13 treated countries are exposed to extreme natural disasters at different times during the observation period, which is a case of staggered adoption. As the specification in Equation 3.6 is applicable to situations where only one unit is treated at a single time, Ben-Michael et al. (2022) propose the use of the partially pooled approach to estimate an average treatment effect on the treated (ATT) under staggered adoption. This approach involves minimizing imbalances between the treated unit and the counterfactual in two ways: by estimating the treatment effect separately for each treated country using separate SCM (state-level imbalance), and by estimating the treatment effect on the average outcome of all treated units using pooled SCM (pooled imbalance).[16]

[16]More details on these imbalances are provided in Appendix G.

To obtain an unbiased ATT with the separate SCM, all country-specific estimates must be unbiased and have a good pre-treatment fits, which is not the case with my data.[17] Figure 3.1 illustrates this by displaying the difference in outcome between the treated and synthetic groups, while using separate SCM estimates. The differences shown in the pre-treatment years exhibit significant variations between the treated and synthetic averages, indicating the need to greatly reduce them. In contrast, the pooled SCM approach leads to poor country-level imbalance and is therefore more susceptible to bias due to non-linearity.

Since separate and pooled SCMs are prone to bias, I use the partially pooled SCM proposed by Ben-Michael et al. (2022) as a robust estimator under staggered adoption. The partially pooled SCM seeks to find weights that minimize a convex combination of the country-level imbalance, q^{sep}, and the pooled imbalance, q^{pool}, as defined in the following equation:

$$\min_{\omega_1,\ldots,\omega_j \in \Delta} \quad \frac{\upsilon}{2} q^{pool}(\Omega) + \frac{(1-\upsilon)}{2} q^{sep}(\Omega) + \lambda \sum_j \sum_i f(\omega_{j,i}) \tag{3.7}$$

In this equation, υ is a hyperparameter that ranges from 0 to 1 and balances the partially pooled SCM between the separate SCM ($\upsilon = 0$) and the pooled SCM ($\upsilon = 1$). Ben-Michael et al. (2022) provide a thorough analysis of the partially pooled SCM that demonstrates its robustness. As my shocks are subject to staggered adoption and the ATT from separate SCMs is prone to significant pre-treatment bias, my estimations are based on the partially pooled SCM approach.

3.2.4 Robustness checks

To evaluate the validity of my estimates, I conduct a thorough robustness analysis on my partially pooled SCM. These robustness checks, which are consistent with the SCM literature, can be divided in six key components. The first two components are the attribution of artificial shocks to donor countries, the in-space placebo, and the backdating of shocks to treated countries, the in-time placebo. These artificial shocks are used to test whether placebo shocks can produce a treatment effect of similar magnitude as to my ATT. The third component involves testing the impact of variations in the predictors used on the pre-treatment fit. The fourth component consists of leave-one-out estimations, in which units are removed one at a time to assess for

[17] See van Dijk et al. (2022) or Funke et al. (2021) for applications of staggered adoptions with good country-specific pre-treatment fits.

their effect on the magnitude of my estimations. The fifth component involves altering the definitions of extreme natural shocks to test the stability of my results. The sixth component tests for potential overfitting bias, which refers to the possibility that a model's counterfactual may have a good fit in the pre-treatment period but does not produce consistent post-treatment counterfactuals.

The in-space placebo analysis aims to determine whether the estimated ATT can be reproduced by imposing the same extreme natural disaster shock on a country that has not been exposed to a real shock. The goal of this test is to verify that the estimated effect is due to the extreme natural disaster and cannot be consistently reproduced by an artificial shock. Therefore, this test considers potential concerns about the volatility of the outcome and the size of the shock (Abadie, 2021; Abadie et al., 2015). To generate placebo treatments, I iteratively assign each year of shocks identified in the treatment group to all countries in the donor group. Using this approach, I can calculate a p-value for my main estimates, equal to the share of placebo estimates with a magnitude at least as large as my main ATT (see Abadie et al., 2015; Andersson, 2019; van Dijk et al., 2022). Following Andersson (2019) and van Dijk et al. (2022), I restrict the placebo to estimations that do not have excessively large pre-treatment bias. To do this, I impose a maximum value for the mean squared prediction errors equal to the largest individual mean squared prediction error in the separate estimations of my treated countries.

The in-time placebo focuses on the timing of the shock. This test mainly examines whether any shock anticipation occurs during the pre-treatment period (Abadie, 2021; Abadie et al., 2015). Given the nature of my shock, it seems unlikely that there would be anticipation of extreme natural disasters. However, this method can also be used to assess the credibility of the synthetic control (Abadie, 2021). In my case, I use this robustness check to test the construction of my synthetic units. To perform the in-time placebo, I shift the treatment shock two years earlier than the real shock to assess for any significant deviation in the outcome variables.

To assess the impact of the choice of predictors on my estimates, I perform SCM with auxiliary variables. These auxiliary variables include GDP per capita, the share of discriminated population, life expectancy, and the population at the country and year level.[18,19] This robust-

[18] Data on the GDP per capita are provided by the United Nations Statistics Division (2022) data, on the share of discriminated population by Bormann et al. (2017), life expectancy by World Bank (2022a) data, and on population by World Bank (2022b) data

[19] This share is all the population of all politically discriminated groups divided by the total country population. The definition of political discrimination is determined by Bormann et al. (2017).

ness check allows me to determine how using only relative conflict fatalities as predictor, for maximum sparsity and transparency, affects the result and the pre-treatement fit compared to the choice of adding additional predictors (Abadie, 2021).

I also conduct a variety of leave-one-out robustness checks (Abadie et al., 2015; Andersson, 2019). These checks are designed to control for the influence of the choice of units in the donor group and to test for potential inference in the donor group, i.e. whether any of the extreme natural disaster shocks affect the outcome of the countries in the donor group. To address these potential biases, I divide my leave-one-out robustness checks into four specifications. First, I drop all countries in the donor group that are neighbors of a treated country and are exposed to a natural disaster of the same type as the extreme natural disaster shock type and in the same year, regardless of magnitude. Second, I remove the donor countries from each African region from my sample one at a time.[20] This test verifies whether my estimates are driven by large variations in outcomes in each African region, such as a regional large-scale conflict. For the third and fourth robustness checks, I conduct leave-one-out analyses for both the treated countries and donor countries to assess the influence of each group on my estimates. To perform these tests, I remove one country at a time from each group in my sample. These two checks intuitively control for whether the exclusion of a unit has a large impact on my results, and if so, whether this large deviation is due to an extreme natural disaster shock or an idiosyncratic variation in the outcome.

I also test the impact of the threshold that defines the treated and donor groups. I divide this analysis into three parts. First, the treated group is restricted to countries that have not experienced any natural disaster in the five years before and after the shock. Second, I reduce the threshold for the donor pool, of 13%, to a limit of natural disasters that affected less than 5% of the population. Third, the threshold of the donor pool is restricted to natural disasters that affected less than 1% of the population. These three variations in my identification criteria reduce the precision of my estimates by decreasing the size of the treated and donor groups, but they allow me to test the robustness of my treatment effect under various group selections.

As a final set of robustness checks, I test for potential overfitting in my model by increasing pre-treatment periods. Models with large variation in outcome variables and short pre-treatment periods have higher propensity to be subject to overfitting (Abadie, 2021). Overfitting leads

[20]The five African regions I use are: eastern Africa, middle Africa, northern Africa, southern Africa and western Africa.

Table 3.1: Descriptive statistics of the treated and donor groups

	Mean	Standard deviation	Minimum	Maximum
Treated (N=13)				
Share of conflict fatalities	12.88	59.98	0	756.35
Population share affected by natural disaster	0.02	0.09	0	0.97
Share of one-sided fatalities	0.98	7.61	0	118.81
Count of sexual violence event	0.97	3.35	0	32
Year of shock	2005.50	4.64	2000	2015
Donor (N=29)				
Share of conflict fatalities	32.21	176.63	0	3737.51
Population share affected by natural disaster	0.01	0.01	0	0.08
Share of one-sided fatalities	4.98	35.40	0	606.35
Count of sexual violence event	2.17	10.07	0	115

Notes: *Share of conflict fatalities* is the yearly sum of fatalities due to violent conflicts divided by the one year lagged country population in millions. *Population share affected by natural disaster* is the sum of people affected by natural disasters divided by the one year lagged country population at the year and country level. *Share of one-sided fatalities* is the yearly sum of fatalities due to one-sided violence divided by the one year lagged country population in millions. *Count of sexual violence event* is the yearly sum of declared sexual violence event committed by armed groups on civilians. The period of observation is from 1995 to 2020.

to a good fit in the pre-treatment period, but results in biased replication of a credible post-treatment period. To control for potential bias arising by overfitting in my model, I extend the pre-treatment period up to 10 years. This allows me to test if a model with longer pre-treatment period can replicate similar results than the ones I observe, thus decreasing the probability of a bias due to overfitting. Note that adding predictors can also serve as a test for overfitting, and the ability to reproduce similar results with additional predictors can also help to reduce the probability that the model is biased by overfitting.

3.3 Results

This section reports my empirical results. First, I present summary statistics on the treated and donor groups. Second, I present my main estimates of the impact of extreme natural disasters on conflict fatalities, one-sided fatalites and the occurrence of conflict-related sexual violence. Finally, I present the results of my robustness check strategy to demonstrate the validity of my approach and my estimation results.

Table 3.2: Pre-treatment average of outcome variables

	Treated (1)	Synthetic (2)	Donor (3)
Share of conflict fatalities	42.44	42.22	35.67
One-sided fatalities	17.34	17.32	5.63
Occurrence of sexual violence	0.72	0.72	1.64

Notes: *Share of conflict fatalities* is the yearly sum of fatalities due to violent conflicts divided by the one year lagged country population in millions. *Share of one-sided fatalities* is the yearly sum of fatalities due to one-sided violence divided by the one year lagged country population in millions. *Count of sexual violence event* is the yearly sum of declared sexual violence event committed by armed groups on civilians. The treated and synthetic averages covers only the values of the 5 years prior of the shocks of each treated countries. The donor averages is based on the whole period where donor can be in the pre-treatment period, 1995 to 2015.

3.3.1 Descriptive statistics

Table 3.1 presents summary statistics for the treated and donor groups. n average, countries in the donor group experience higher conflict fatalities, with a rate of 30.95 per million people, compared to countries exposed to extreme natural disasters, which have a rate of 12.88 per million people. This difference is further supported by the maximum values for conflict fatalities, which are 3737.51 for the donor group and 756.35 for the treated group. This difference is consistent with previous research that has found that countries exposed to extreme natural disasters tend to have lower levels of conflict intensity (Xu et al., 2016).

In terms of the share of people affected by natural disasters, the treated group has a higher average (0.02) and maximum (0.96) than the donor group (0.01 and 0.08). The larger maximum value in the treated group can be attributed to my definition of extreme natural disaster shocks. However, it is important to note that the maximum share of people affected by natural disasters in the donor group is still relatively high, and it is necessary to ensure that the results are not driven by inference from these natural disaster shocks.

The average of share of one-sided fatalities follows a similar pattern as the values of all types of conflict fatalities, with an average of 0.98 fatalities per million people in the treated group compared to 4.98 fatalities per million people in the donor group. The maximum value of one-sided fatalities is also higher (606.35) than in the treated group (118.81). The occurrence of

Table 3.3: Differences in estimates of all type of conflict fatalities, state-based, non-state based and one-sided fatalities

Year post-treatment	0	1	2	3	4	5
All types of conflict difference	-20.06	-61.64	-63.26	-46.38	-82.55	-54.78
P-value all types	0.14	0	0	0.12	0	0.07
State based and one-sided conflict difference	-13.42	-51.29	-12.40	-49.31	-63.77	-38.04
P-value State based and one-sided	0.08	0	0.16	0.06	0	0
State based and non state based conflict difference	-73.63	-6.67	-15.90	-24.57	-26.58	-29.47
P-value state based and non state based	0	0.07	0.16	0.12	0.13	0.13
Non state based and one-sided conflict difference	-1.83	-1.57	-10.32	-7.38	-4.13	-5.04
P-value non state based and one-sided	0.17	0.17	0.13	0.23	0.32	0.32

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shock. The differences are between the partially pooled average and the counterfactual in relative conflict fatalities per million people. *P-value* is the p-value I calculate from the in-space placebo analysis.

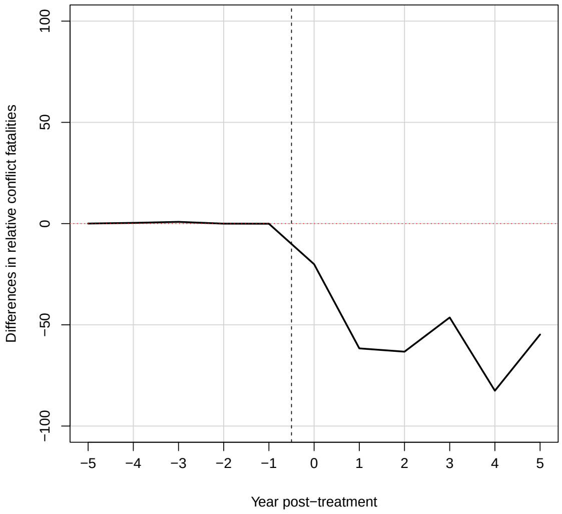
conflict-related sexual violence events also follows a similar pattern, with an average of 0.97 occurrence and a maximum of 32 in the treated group, compared to an average of 2.17 and a maximum of 115 in the donor group. These summary statistics highlight potential differences between the treated and donor groups, and this could raise concerns about the comparability of the groups. Nevertheless, through my results and my robustness checks, I show in the following that I build a credible counterfactual.

Table 3.2 presents the pre-treatment averages for the treated, synthetic, and donor groups for conflict fatalities, one-sided fatalities and occurrence of sexual violence. The treated and synthetic averages are similar, while the donor averages show larger deviations. Therefore, the comparison of averages between treated and synthetic is a comparison of countries that have followed similar trends before treatment.

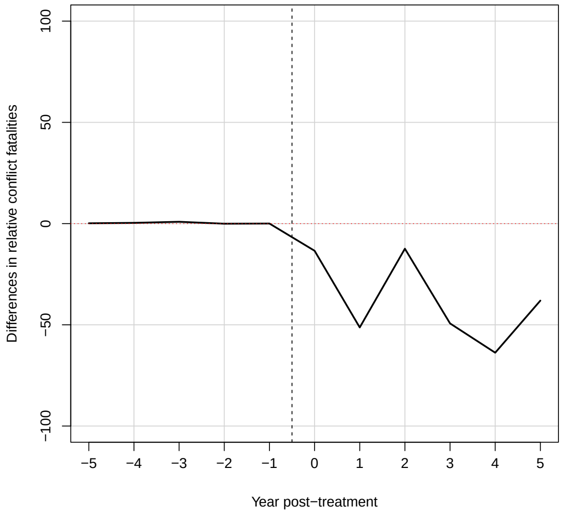
3.3.2 Impact of extreme natural disasters on conflict fatalities

The results of the estimates for all types of conflict fatalities are presented in Figure 3.2a. Figure 3.2b shows the impact of extreme natural disaster shocks on state-based and one-sided conflict fatalities, Figure 3.2c shows the impact on state-based and non-state based conflict fatalities, and Figure 3.2d shows the impact on non-state based and one-sided conflict fatalities. The corresponding estimates are also reported in Table 3.3. The panels in Figure 3.2 report differences between the average effect of treatment on conflict fatalities in million and the synthetic coun-

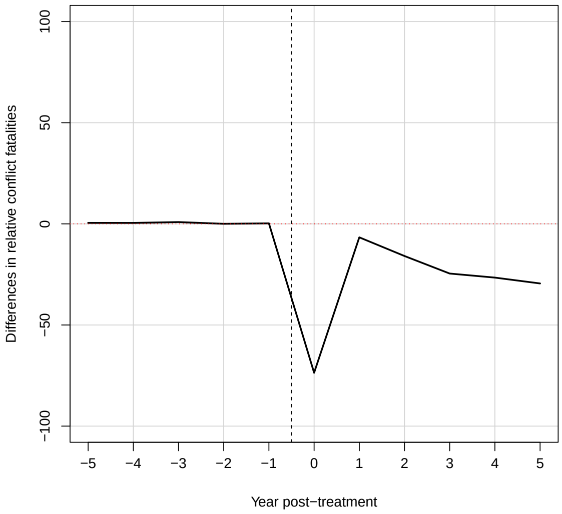
Figure 3.2: Differences in estimates of all types of conflict fatalities, state-based, non-state based and one-sided fatalities



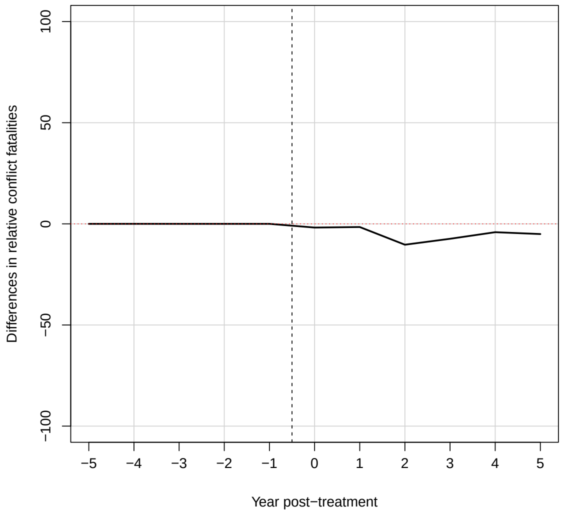
(a) All types of conflict fatalities



(b) Only state-based and one-sided conflicts



(c) Only state and non-state based conflicts



(d) Only non-state based and one-sided conflicts

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

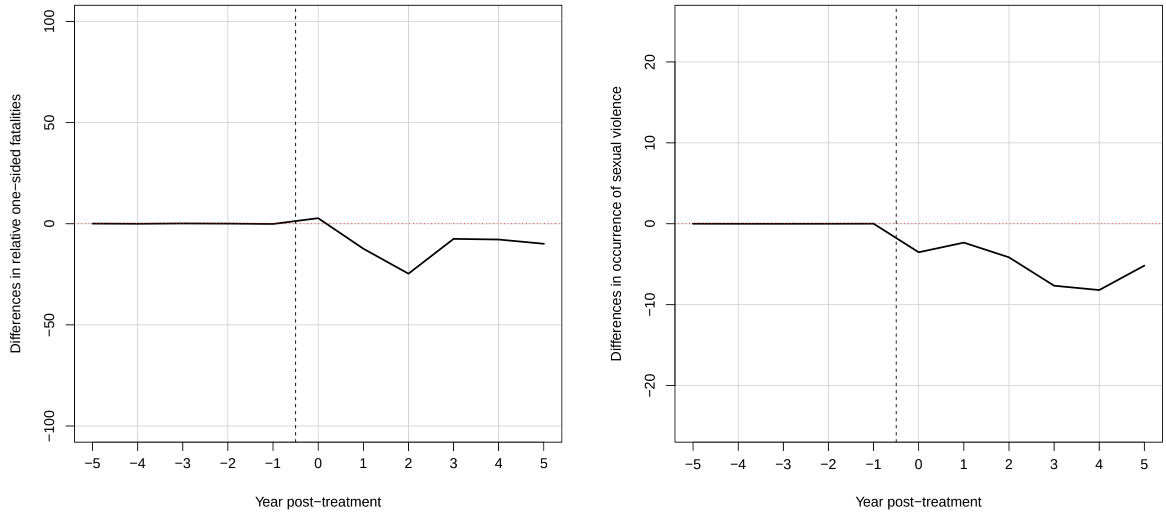
terparts. A flat line equal to zero in this figure indicates that there is no difference between the ATT and the synthetic counterpart. Therefore, a good pre-treatment period should be a flat line equal to zero, and the treatment effect is reported as the difference between the synthetic and the ATT.

The pre-treatment period in Figure 3.2a indicates that the estimated ATT fits well with the synthetic counterpart. The post-treatment estimates reveal that extreme natural disasters lead to a decrease in conflict fatalities. At the end of the year of the extreme natural disaster shocks, the estimate already shows a decline in conflict of 20.06 fatalities per million people, equivalent to a decrease of 1.56 times the average share of conflict fatalities in the treated group. In the following years, the treatment effect increases, reaching a maximum of 82.55 fatalities per million people in the fourth year after the shock, which is equivalent to approximately 6.4 times the average number of conflict fatalities in the treated countries. Taken together, these results suggest that extreme natural disaster shocks induce a decrease in conflict fatalities between 2 and 6 times the average fatalities in these countries, indicating that the magnitude of extreme natural disasters is relatively large compared to the average intensity of conflicts. Furthermore, this decrease remains relatively consistent from one year after the shock until the end of the observation period, indicating a medium-term decrease in conflict intensity.

The estimates reported in Figure 3.2b show a negative impact of extreme natural disaster shocks on both state-base and one-sided violence. While this impact is of a smaller magnitude than the estimated main impact, it exhibits a similar pattern. When considering the impact of extreme natural disasters on state-based and non-state based violence alone (Figure 3.2c), the impact remains negative, equal to 73.63 fatalities per million in the year of the shock. However, the magnitude decreases in the following years. For non-state based and one-sided violence (Figure 3.2d), the effect is negative but relatively small compared to the main estimates, as the smallest decrease being 1.57 in the year following the shock and the largest decrease is 10.31 in the second year after the shock.

The results reported in Figure 3.2 show that the negative impact of extreme natural disaster shocks on all types of conflict fatalities is mainly due to decrease in state-based conflicts, with a contribution from a decrease in one-sided fatalities. This suggests that the main path for the decrease in the conflict intensity induced by extreme natural disasters may be driven by actions of governments, such as increased negotiations and ceasefires (Kreutz, 2012; Nemeth and Lai, 2022). It is also possible that the second major path is a recovery period for armed groups of any type, in which they reduce their lethal actions against civilians.

Figure 3.3: Differences in estimates of one-sided fatalities and sexual violence



(a) One-sided conflicts fatalities (b) Annual count of sexual violence events

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

3.3.3 Impact of extreme natural disasters on one-sided fatalities and conflict-related sexual violence

The impact of extreme natural disaster shocks on one-sided fatalities and on the occurrence of conflict-related sexual violence events is shown in Figure 3.3 and reported in Table 3.4. These estimates are based on data from the ACLED database, which provides more detailed information on the type of one-sided violence and allows for a more precise assessment of the impact of extreme natural disasters on conflict-related sexual violence.[21]

The estimates in Figure 3.3a show a negative impact of extreme natural disasters on one-sided fatalities. In the year of the shock, the estimate is 2.75, indicating a potential increase in one-sided violence during the shock. However, this estimate has a relatively high p-value of 0.89, indicating that it may not be very precise. In the year following the shock, the estimate shows a decrease of 12.33 fatalities per million people, equivalent to a decrease of 10% compared with the maximum annual one-sided fatalities reported in the treated group and 12 times the mean. The impacts estimated for the three years following the extreme natural disaster are also

[21] Note that, since this database is more recent, these estimates cover only the period 1997 to 2020 and Cabo Verde, Comoros, Mauritus are not taken into account due to missing data.

Table 3.4: Differences in estimates of one-sided fatalities and sexual violence

Year post-treatment	0	1	2	3	4	5
One-sided fatalities difference	2.75	-12.33	-24.69	-7.48	-7.81	-9.91
P-value one-sided	0.89	0.07	0.01	0.05	0.13	0.13
Occurrence of sexual violence difference	-3.52	-2.33	-4.15	-7.66	-8.20	-5.17
P-value sexual violence	0.02	0.05	0.03	0.02	0.02	0.02

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shock. The differences are between the partially pooled average and the counterfactual. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence. *P-value* are the p-values I calculate from the in-space placebo analysis.

relatively large and significant, reaching a maximum of 24.60 one-sided fatalities per million people. These estimates are larger than those obtained from the UCDP data (see Figures 3.2b, 3.2d), which may be due to the fact that the ACLED database includes all one-sided fatalities, while the UCDP database only considers one-sided violence with at least 25 civilian fatalities.

The estimate of the impact of extreme natural disasters on the occurrence of conflict-related sexual violence, shown in Figure 3.3b, exhibits a strong fit, with very low deviations from zero in the pre-treatment. The results also suggest a significant decrease in the occurrence of these events in the five years following an extreme natural disaster. In the year of shock, the occurrence of conflict-related sexual violence decreases by 3.52, with a p-value of 0.03. This decrease is equivalent to three times the average of conflict-related sexual violence in the treated group and a decrease of approximately 10% compared to the maximum. The largest impact, in the fourth year after extreme natural disasters, is a decrease of more than eight times the average and a decrease of 25% of the maximum occurrence of conflict-related sexual violence in the treated group. These results indicate that extreme natural disaster shocks significantly reduce the occurrence of conflict-related sexual violence.

These results on one-sided fatalities and conflict-related sexual violence show that, while these outcomes do not necessarily follow the pattern of all types of conflict fatalities, extreme natural disasters have a negative impact on their occurrence. The negative impact on one-sided fatalities supports the novel findings of Haer and RezaeeDaryakenari (2022) and suggests that armed groups may be recovering from the shock and reducing their lethal actions against civilians. A similar hypothesis may be proposed to explain the decline in conflict-related sexual violence induced by extreme natural disasters. However, the specific path through which natural disasters may impact conflict-related sexual violence requires further investigation, as the relationship between these factors is not yet fully understood.

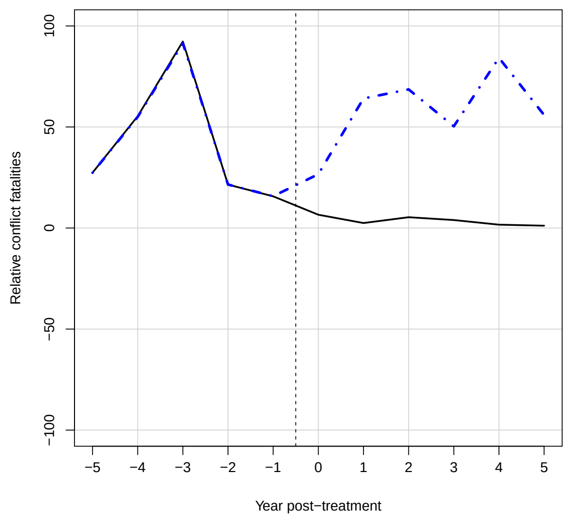
3.3.4 Trends of impacts of extreme natural disasters on conflict fatalities, one-sided fatalities and conflict-related sexual violence

The differences between treated and synthetic counterparts reported above provide a clear picture of the estimated effects. However, the trends of these variables allow one to observe the evolution of the treated and synthetic outcome variables over time, enabling to assess the credibility of the synthetic counterpart and any potential overfitting concerns. In Figure 3.4 I plot the average trend of treated countries and their counterfactuals in conflict fatalities, one-sided fatalities and occurrence of sexual violence. The results in Figure 3.4 show that my estimates are subject to large variations. This is expected given the nature of my outcome variables, which can experience significant fluctuations from year to year compared to outcomes more commonly used with the SCM (such as GDP per capita Abadie and Gardeazabal (2003)).

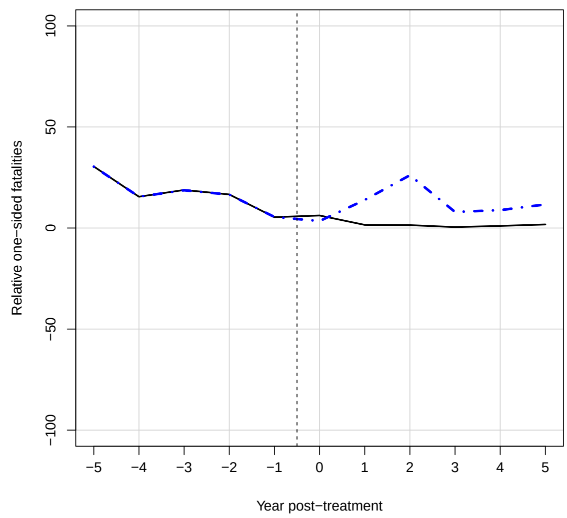
The pre-treatment periods in the three cases show a very good fit between the treated pattern and the synthetic pattern, indicating that the treatment effect remains credible in terms of how the variables I observe in my treated countries could have evolved without natural disasters shocks. The variations in post-treatment remain relatively large, while in all cases the treated averages get close to zero. For conflict fatalities and one-sided fatalities, the post-treatment counterfactual shows variations that remain in the interval of the variations observed in my treated group before the shock. This suggests that the counterfactual is credible as an alternate scenario of what could have occurred in my treated countries without extreme natural disasters. However, the counterfactual for the occurrence of conflict-related sexual events exceeds the variations observed in the pre-treatment periods, which may indicate an overestimation bias in these estimates and warrants caution when interpreting them.

The trends in Figures 3.4 can raise concerns on how my results may be influenced by large values in the treated and donor groups, as well as the potential for overfitting of my model. In the following sections, I show that decreases in my three outcome variables remain consistent to modification of my treated and donor groups composition and that I estimate similar results while increasing the pre-treatment duration, which reduce the probability of overfitting.

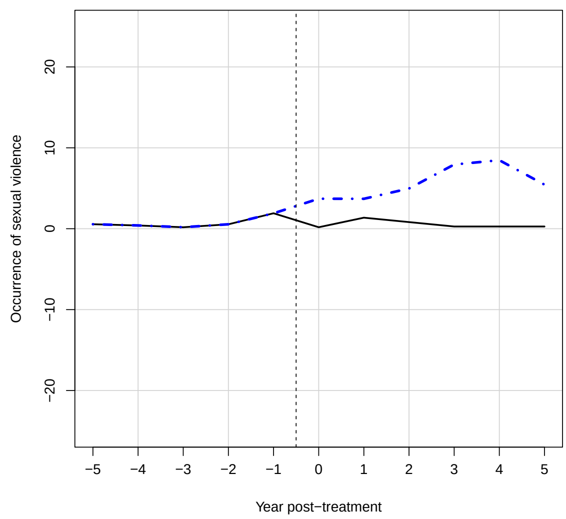
Figure 3.4: Trends of treated and synthetic outcomes



(a) Conflict fatalities



(b) One-sided fatalities



(c) Sexual violence

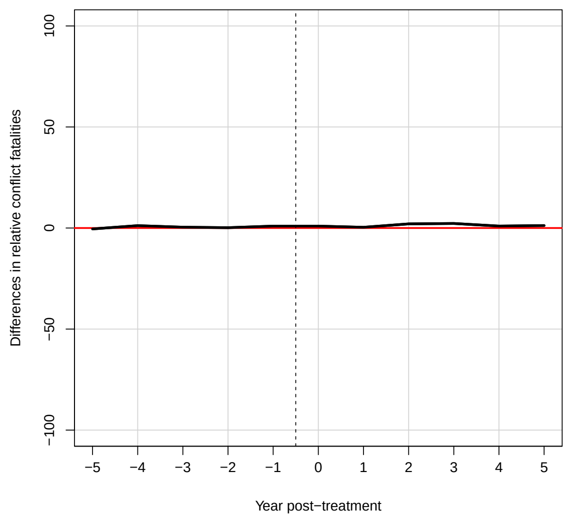
Notes: The solid black lines represent the average in outcomes of the treated countries and blue dash-dotted lines is the synthetic counterparts.

3.3.5 Robustness checks

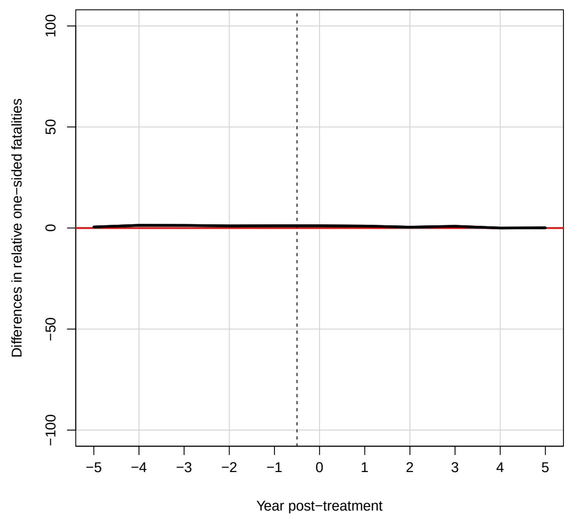
3.3.5.1 In-space placebo

The average of the estimates for the in-space placebo for conflict fatalities, one-sided fatalities and the occurrence of conflict-related sexual violence are shown in Figure 3.5. The p-values

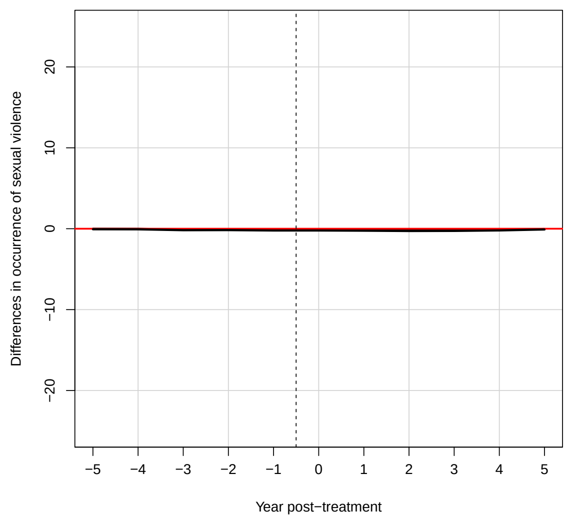
Figure 3.5: In-space placebo estimate differences



(a) All types of conflict fatalities



(b) One-sided fatalities



(c) Sexual violence

Notes: The black line present the mean of difference across in-space placebo treated units.

calculated from these estimations are reported in Table 3.3 for conflict fatalities and in Table 3.4 for one-sided fatalities and the occurrence of conflict-related sexual violence.

My in-space analysis and my p-value estimations demonstrate that my main estimations are not equivalent to placebo treatments. I use the application of iterative and artificial shocks in my donor pool to count the number of these placebo impacts that are greater than my main estimate. I report up to 162 SCM in-space placebos, and the average impact of in-space placebos remains between 0.01 and 2.25 throughout the post-treatment period (see Table I). This shows that when placebo shocks are applied to a group of countries that are not exposed to extreme natural disasters at all, the separate average treatment for conflict fatalities, one-sided fatalities, and occurrence of sexual violence is very close to zero. My p-value calculations indicate that most of my estimates have a p-value below 0.05, which means that only 5% of all the in-space placebos found for the same year have an impact of greater magnitude than my main estimate. However, three estimates for one-sided fatalities have a p-value greater than 0.1, indicating that these estimates are less precise, overall, than the estimates for conflict fatalities and the occurrence of sexual violence. This can be explained by the fact that only 78 placebos reach my pre-treatment bias threshold, so an additional placebo shock of at least the same magnitude as my estimate has a greater impact on the p-value.

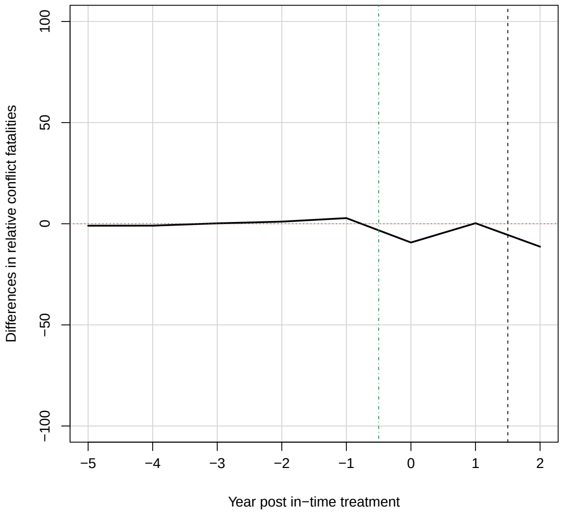
Overall, the in-space placebo results suggest that the consistent decreases observed in my main estimates for conflict fatalities, one-sided fatalities, and sexual violence over the years following an extreme natural disaster are not likely to be due to placebo shocks. This conclusion supports my hypothesis that extreme natural disaster shocks are sufficient to meet the size effect condition defined by Abadie (2021) and that my results are not driven by the volatility of outcome variables.

3.3.5.2 In-time placebo

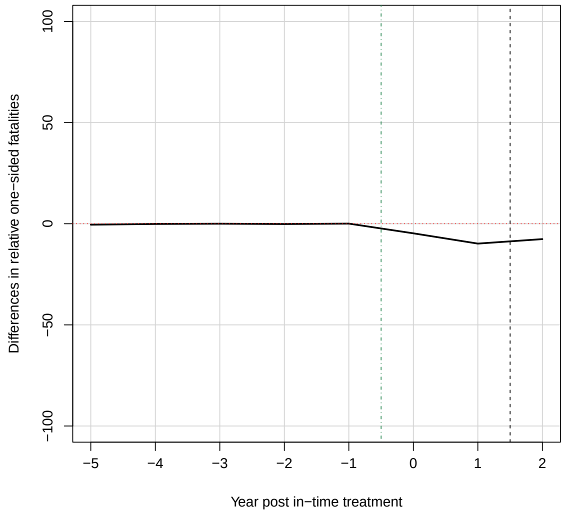
The results of the in-time placebo analysis, where the year of the extreme natural disaster shock is changed to two years earlier, are presented in Figure 3.6. The vertical green dash-dotted lines are the in-time shocks, the black dotted lines represent the actual time of the shocks, and the horizontal black lines show the estimates for the in-time placebos.

In Figure 3.6a, the in-time placebo analysis shows that my main estimates are not sensitive to placebo pre-treatment shocks. Shifting the shock for conflict fatalities by two years earlier slightly decreases the precision of the pre-treatment period, as the deviations from zero are larger. However, based on the p-value calculated unsing the in-space placebo for the main estimates, none of these variations is significant. The impact also fluctuates between positive and negative values, which appears to be more like noise than an identifiable shock, as anticipation does not seem to be a credible scenario in this study. For one-sided fatalities and the occurrence of conflict-related sexual violence, shown in Figures 3.6b and 3.6c, the estimates exhibit greater

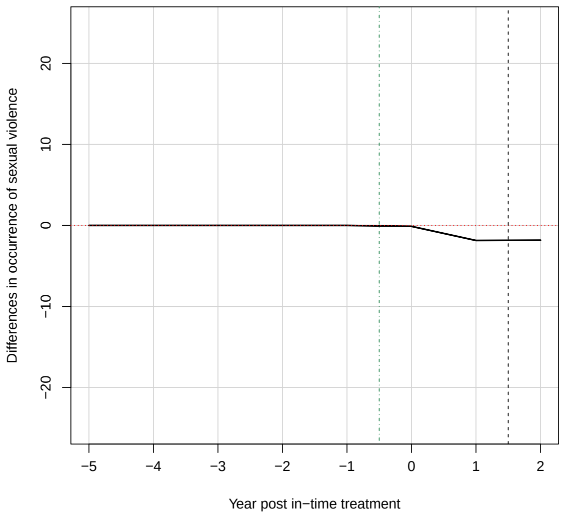
Figure 3.6: In-time placebo estimate differences



(a) All types of conflict fatalities



(b) One-sided fatalities

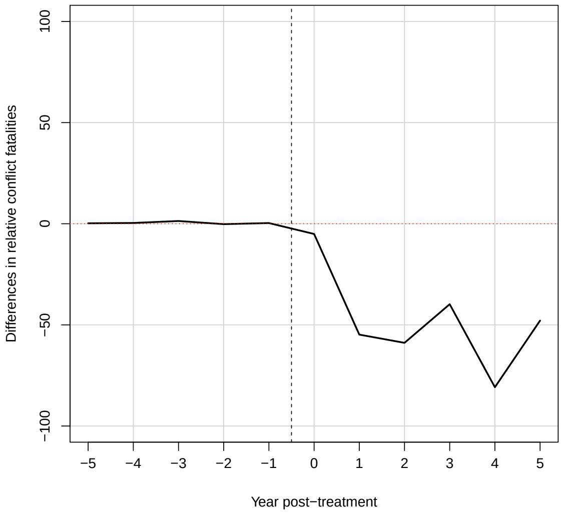


(c) Sexual violence

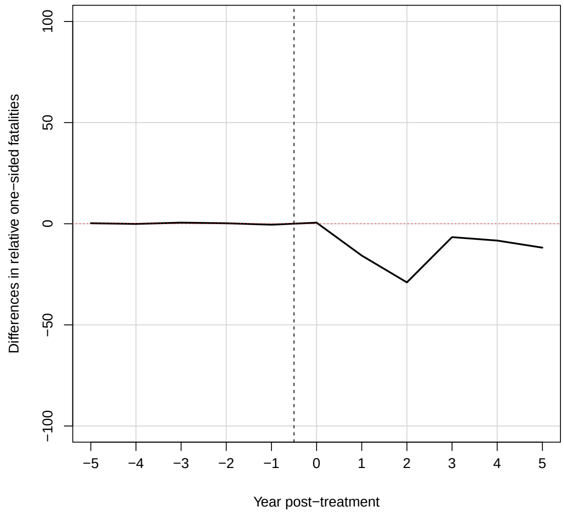
Notes: The black line present the in-time placebo partially pooled SCM estimated differences between treated group and the synthetic counterpart.

sensitivity to in-time placebo shocks. None of the variations after placebo treatments are of the same magnitude as the main estimates, but these results show negative impacts. This suggests that the imperfect construction of these two models may lead to a downward bias. However, as shown in the supplementary materials in Appendix J, this does not significantly impact the

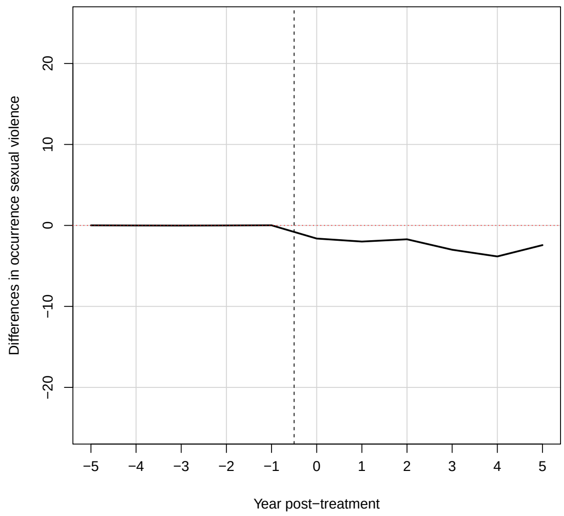
Figure 3.7: Differences in estimates with auxiliary variables



(a) All types of conflict fatalities



(b) One-sided fatalities



(c) Sexual violence

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

overall results.

3.3.5.3 Choice of predictors

The results of the addition of predictors are presented in Figure 3.7. These results suggest that the estimates of the impact of extreme natural disasters are not significantly affected by the inclusion of auxiliary variables. For conflict fatalities and one-sided fatalities, the pre-treatment period exhibits larger deviations from then the main estimates, but these variations remain small. The estimates of the post-treatment period confirm the negative effect of extreme natural disasters, with the magnitude of the estimates remaining close to the main estimates for conflict fatalities and one-sided fatalities. For the occurrence of sexual violence, the magnitude of the estimates is reduced, with the deviation declining from 0.34 to 4.66. However, this does not change the negative trend (see details in Table K1). All estimates have p-values less than 0.1, except for the estimates of conflict fatalities and one-sided fatalities in the year of shocks, which are less precise (p-val>0.1). Overall, these results indicate that the selection of predictors does not significantly impact the main estimates of the impact of extreme natural disasters, as all of the estimates are relatively similar in terms of magnitude and significance. Therefore, using only relative conflict fatalities as predictors, which allows for maximum sparsity, does not significantly alter my estimates.

3.3.5.4 Leave-one-out

The results of the first leave-one-out robustness check, which omits donor countries that have experienced natural disasters of the same type and in the same year as their treated neighbors, show that removing potentially biased countries from the donor pool does not significantly affect the estimates (the estimates are reported in Figures L1). The fit of the pre-treatment periods remains as good as in the main estimations. The estimates without neighbors that could be exposed to inferences are extremely close to my main estimates, with most deviations below or very close to 1. These results suggest that the context of the sample does not face any inference problems (Abadie, 2021) and that none of the shocks considered in the analysis impact the outcomes of relative conflict fatalities, one-sided fatalities, and the occurrence of sexual violence in the donor group.

Next, the results of my estimates following the removal of donor countries by region show similar trends to my main estimates (as shown in Figure L2 and Table L1). The pre-treatment period difference in estimates remains close to zero, indicating a good fit. On average, these leave-one-out estimations have only small deviations from my main estimates. However, when all donor countries are removed within a region, estimates of conflict fatalities and the occurrence of sexual violence show significant deviations from the main estimates. For example, removing donor countries in the Middle African region for conflict fatalities and removing donor countries in the Northern African region for the occurrence of sexual violence leads to smaller trends.[22,23] In contrast, removing donor countries in the western African region results in a negative trend of greater magnitude in both cases.[24] My estimates for one-sided fatalities do not show these large variations in terms of magnitude. These results indicate that although the estimated magnitude of the treatment effect may vary with changes in the donor group, the decreasing trend induced by extreme natural disaster shocks remains consistent.

The estimates, when treated countries are removed one at a time, also show a decreasing pattern in conflict fatalities, one-sided fatalities, and the occurrence of sexual violence (as shown in Figure L3 and Table L1). The pre-treatment periods remain close to zero for all estimations, indicating a good fit. The differences between my main estimates and the mean of all leave-one-treated estimates are small, with the largest deviation being 3.36 in the year of the shock for conflict fatalities. All estimations show a decreasing pattern in relative conflict fatalities, one-sided fatalities and occurrence of sexual violence. However, the minimum values also indicate that in some years, estimates may be influenced by a few treated countries, but these countries change overtime and do not affect the overall decreasing impact.

Finally, the results of removing one donor country at a time, the leave-one-donor, lead to conclusions similar to those of my other leave-one-out test (details in Figure L4 and Table L1). Once again, the pre-treatments show good fits, as the difference between the ATT and their counterfactuals always remains close to zero. On average, the leave-one-donor estimates do not show significant deviation from my main estimates, with the largest difference being 3.76 in the year of the shock for conflict fatalities and the smallest difference being 0.01 in the first year after the shock for the occurrence of sexual violence. However, when considered individually, the leave-one-donor estimates show that the magnitude of the impact is reduced by removing the Democratic Republic of the Congo for conflict fatalities, Cameroon for one-sided fatalities,

[22]The donor countries in the middle African region are: Angola, Cameroon, Central African Republic, Democratic Republic of Congo, Republic of Congo, Equatorial Guinea, Gabon.

[23]The donor countries in the middle African region are: Algeria, Egypt, Libya, Morocco, Sudan, Tunisia.

[24]The donor countries in the middle African region are: Benin, Cabo Verde, Cote d'Ivoire, Ghana, Guinea, Nigeria, Senegal, Sierra Leone, Togo.

and Sudan for the occurrence of sexual violence, respectively. This suggests that the magnitudes of my estimates are affected by the choice of countries in the donor pool. However, none of the three countries has been exposed to any of the extreme natural disaster shocks reported in my treatment shocks. Additionally, these results are specific to these specifications, as removing the entire region of each of these countries does not significantly reduce the magnitude of my estimates. These results suggest that the variation in magnitude may be due to idiosyncratic variation in outcomes, rather than being an inference from the identified shocks.

In summary, these different leave-one-out robustness checks show that the impact identified in my main estimations is robust to changes in the compositions of my treated and donor groups. While some variations on the magnitude of the impact are found, the main decreasing trend in relative conflict fatalities remains consistent, and my results do not suffer from any inference issues.

3.3.5.5 Identification thresholds variations

Restricting my treatment group to countries that have not been exposed to natural disasters in the five years before and after the treatment year does not significantly change the negative trends of my main estimates (for details, see Figure L4 and Table M1). However, the estimates of conflict fatalities and one-sided fatalities are of smaller magnitudes, while the estimates for sexual violence are larger than my main in the year of the shock. The following trend remains negative, but again shows that the coefficient for the first year of the shock should be interpreted with caution for one-sided fatalities. These estimates indicate that my main results, which use countries that are exposed to only one extreme natural disaster and not to any disasters in the five years prior and after this shock, are not significantly affected by natural disasters of smaller magnitude.

Reducing the threshold of my donor pool to countries that are not exposed to any natural disasters that affect more than 5% of the population during the years 1995 to 2020 yields results similar to my main estimates (estimates are reported in Figure M2 and Table M1). For conflict fatalities, the pre-treatment period estimates show a larger deviation from zero between the ATT and its counterfactual compared to my main estimate. This can be attributed to the decrease in the size of the donor pool, which reduces the probability of perfect matching. Nevertheless, the post-treatment estimates show negative trends, but with a smaller magnitude for one-sided fatalities and sexual violence.

Finally, I also estimate a negative effect of treatment while restricting the donor group threshold to countries that have never been exposed to natural disasters that affected more than 1% of the population, during the years 1995 to 2020 (as shown in Figure M3 and Table M1). This strict restriction results in a small donor pool of seven countries. The variations in pre-treatment periods show that this decrease has significantly impacted the fit quality for conflict fatalities and also reduced, to a smaller extent, the fit quality for one-sided fatalities and the occurrence of sexual violence. The effects of treatment remain negative and slightly higher than my main estimates for conflict fatalities and one-sided fatalities. The magnitude of treatment for the occurrence of sexual violence remains overall negative, but very small compared to my main estimates.

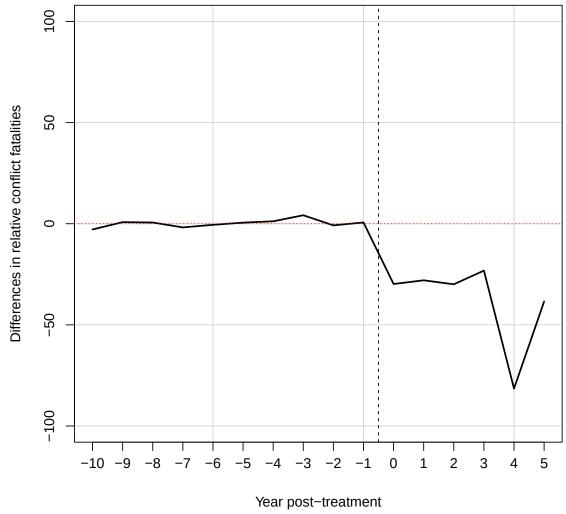
From these variations in the definitions of the groups, I conclude that my identification of the treated and donor groups is not subject to any comparison group issue. The variations induced by natural disasters of magnitudes smaller than my extreme shocks play a role on the size of my treatment effects, but do not significantly alter the negative trend of my results.

3.3.5.6 overfitting and longer pre-treatment periods

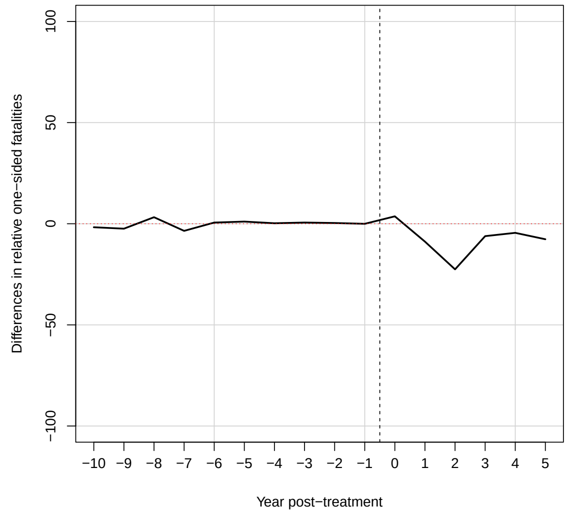
Increasing the duration of the pre-treatment period up to 10 years results in a similar negative impact of extreme natural disasters for conflict fatalities, one-sided violence, and conflict-related sexual violence. The quality of the pre-treatment period for conflict fatalities and for the first years of one-sided violence is not as good as in my main estimation, possibly due to more missing values in the data at the beginning of the pre-treatment period. The estimates in the post-treatment periods show decreasing trends for conflict fatalities, one-sided fatalities and conflict-related sexual violence, but the magnitude of treatment are smaller for conflict fatalities and sexual violence, although they remain mainly precisely estimated (see Figure 3.8 and Table N1).

These results show that my main estimates remain consistent and do not appear to be affected by an overfitting bias, as reinforced by the results obtained when adding predictors. However, the magnitude of the estimates for conflict-related sexual violence is strictly lower than my main estimates and seems more credible given the trend of this outcome variable.These results suggest that the magnitude of the effect of natural disaster shocks on conflict-related sexual violence in my main model should be interpreted with caution, but the decreasing trend is consistent.

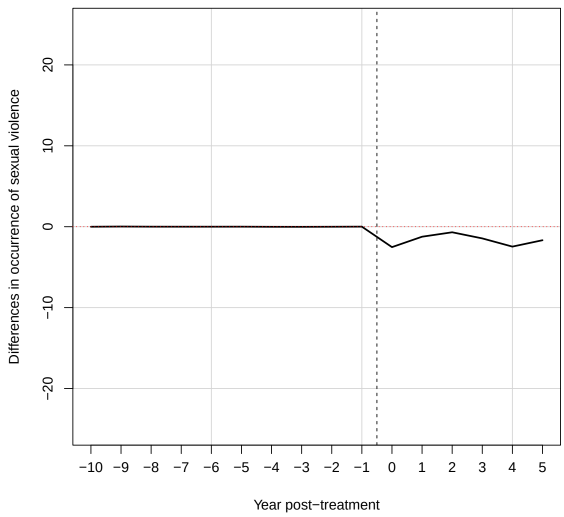
Figure 3.8: Differences in estimates with 10 years of pre-treatment



(a) Conflict fatalities



(b) One-sided fatalities



(c) Sexual violence

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

3.4 Discussion and conclusion

In this study, I demonstrate that extreme natural disaster shocks lead to a significant decrease in conflict-related fatalities, which can be as much as eight times the average conflict-related fatalities. I also find that these decreases are primarily driven by a decrease in state-based conflicts, indirectly supporting the hypothesis that states exposed to major natural disasters are more likely to engage in negotiations and ceasefire agreements.

Additionally, I use my strategy to provide new evidence on less studied conflict-related outcomes. My results show that one-sided fatalities and conflict-related sexual violence are also negatively impacted by extreme natural disasters. However, I find that the magnitude of this impact that I estimate for conflict-related sexual violence may be subject to bias. Despite this, the main decreasing trends I find remain robust. These findings align with the theory and findings that natural disasters can lead to a decrease in lethal violence against civilians and suggest that conflict-related non-lethal violence events may be subject to similar dynamics.

In addition, through a range of robustness checks, I demonstrate that my main findings are robust. I also show that using an SCM approach to estimate the impact of extreme natural disasters on countries is a valid and robust method. These results suggest that these methods, combined with a clear shock identification strategy, can be applied in a wider range of research fields than currently used.

The increase in the frequency of natural disasters calls for a deeper understanding of their effects. It is therefore important to conduct further research on the role of natural disasters in different conflict outcomes, not just fatalities. As non-lethal outcomes may have different dynamics than lethal ones, a better understanding of those specific dynamics would provide a more comprehensive understanding of conflict dynamics overall. Furthermore, it can be valuable to consider a variety of conflict-related outcomes while analyzing the vulnerability of different countries and populations to natural disasters, as this may make the definition of their vulnerability more precise. Further research is also necessary to better understand the different paths linking natural disasters and conflicts, as well as the interaction between these paths. Developing this knowledge will enable more informed predictions of the impacts of natural disasters and help stabilize and develop countries facing significant negative outcomes from natural disasters.

Conclusion

This thesis studies three topics relating to natural disasters and social dynamics in middle- and low-income countries. The first chapter details the evolution of out-migration in Türkiye, particularly examining the impact of precipitation shock on rural provinces. By using a mediated moderator approach, it shows that negative rainfall shocks can increase out-migration, and that the primary channel for this effect is through income. In particular, this negative shock works through agricultural income, but the specialization of the agricultural sector in the province is an essential factor when considering precipitation as a driver of migration in middle-income countries.

This first chapter makes a valuable contribution to the literature on migration by focusing on rural areas in a middle-income country, an understudied area in the field of climate-induced migration. Additionally, our hypothesis on the specialization of the agricultural sector brings new insights and raises further research questions on channel and adaptation in this field.

The last two chapters analyze the impact of natural disasters on low-income countries. Chapter 2 considers the impact of drought on women's health, specifically fertility and intimate violence. It shows that the same group of women exposed to drought face an increased probability of having a new child and being exposed to sexual and physical violence. This suggests that the increasing frequency of natural disasters may have negative consequences for women, which could impact population growth and development in these countries.

This second chapter presents novel quantitative evidence on the impact of exposure to drought on fertility and the ability to access sexual health services to mitigate the negative effects of drought. It also shows, through quantitative evidence, that rates of sexual and physical intimate violence mirror fluctuations in fertility. These results offer unique insights into the impact of natural disasters on women and underscore the need for additional research on the connection between intimate violence and fertility.

Finally, Chapter 3 shows that extreme natural disasters can have a negative impact on conflicts, particularly in terms of one-sided fatalities and conflict-related sexual violence. These results indicate that extreme natural disasters can have similar impacts on various conflict-related outcomes, providing insight into how climate change may affect global stability in the coming decades.

This third chapter makes three important contributions to the literature. First, it provides further clarification on the types of conflict that exhibit a decrease in intensity, which indirectly supports the hypothesis on the underlying mechanisms. Second, it demonstrates that in the case of extreme natural disasters in Africa, lethal violence against civilians also tends to decrease in intensity, supporting the novel idea of a recovery period following a major natural disaster. Finally, the chapter suggests that sexual violence may follow a similar pattern after extreme natural disasters, although more research is needed to confirm these findings.

This thesis contributes to the understanding of household preferences by examining the effects of weather shocks and natural disasters on these preferences. The three chapters demonstrate that these events can lead to both voluntary and involuntary adaptations, such as migration and changes in fertility rates, which can have significant impacts on demography. Furthermore, the thesis considers the influence of various forms of violence, including intimate violence and conflicts involving armed groups, on household preferences. These findings highlight the importance of considering the diverse effects of external shocks on household preferences when designing forecast models.

Overall, these three chapters provide novel evidence on the impacts of natural disasters and weather variations on society. They highlight the importance of considering context and inequality in the development of policies to mitigate the negative effects of natural disasters, particularly in terms of gender-sensitive policies. The findings of these chapters suggest that effective policies to address natural disasters must take into account the unique contexts and needs of each country and region. Furthermore, it is essential to consider issues of inequality, particularly with regard to gender, in the development of such policies to ensure their long-term effectiveness.

Appendix

The Appendix contains supplementary information for all chapters.

A Chapter I: Conflicts

Figure A1: Time trend of conflict fatalities for Turkish provinces

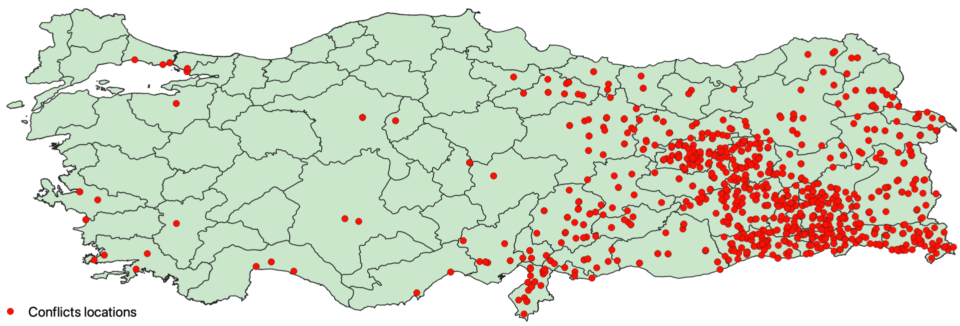
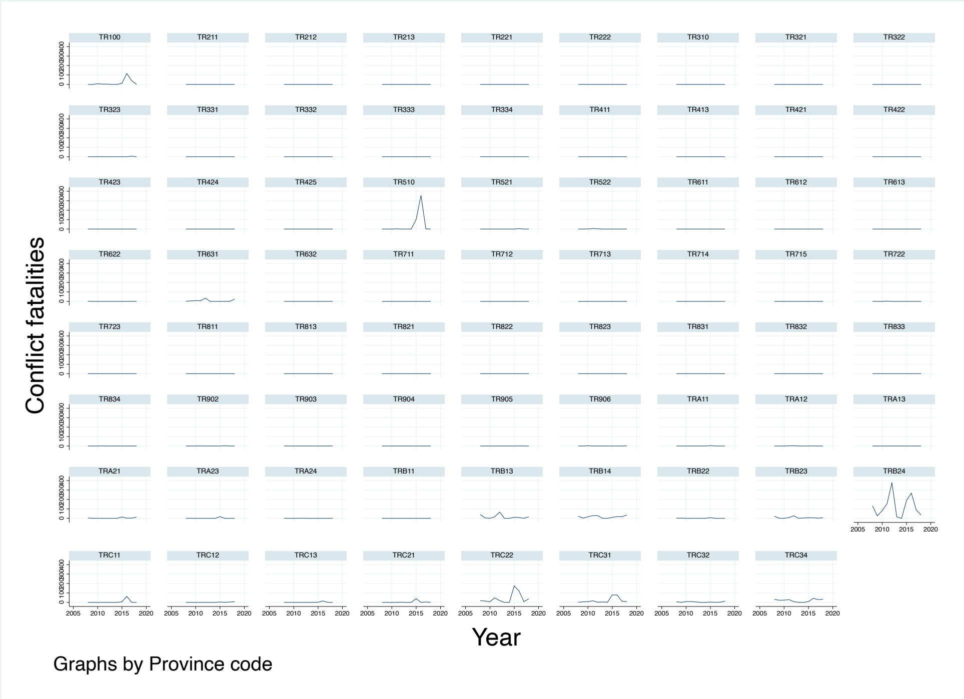
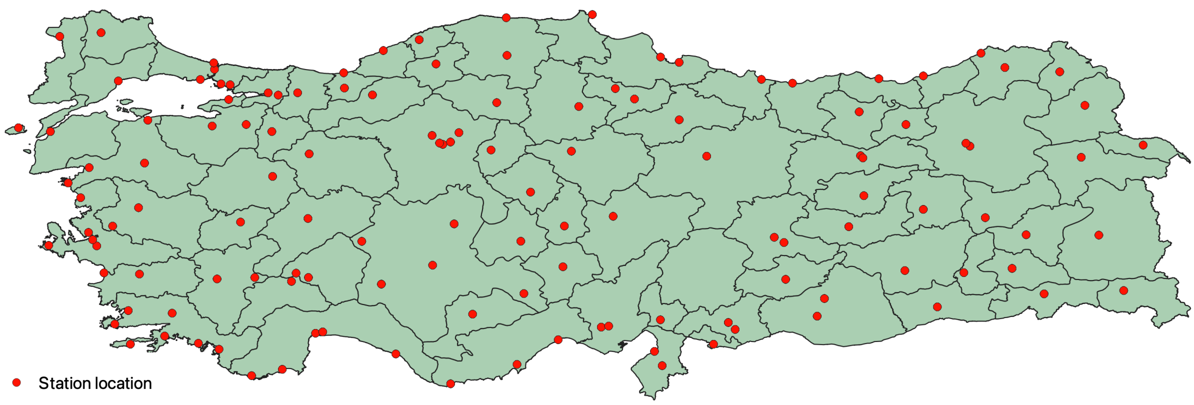


Figure A2: Location of conflict for Turkish provinces



B Chapter I: Location of measuring stations

Figure B1: Location of measuring stations for Turkish provinces



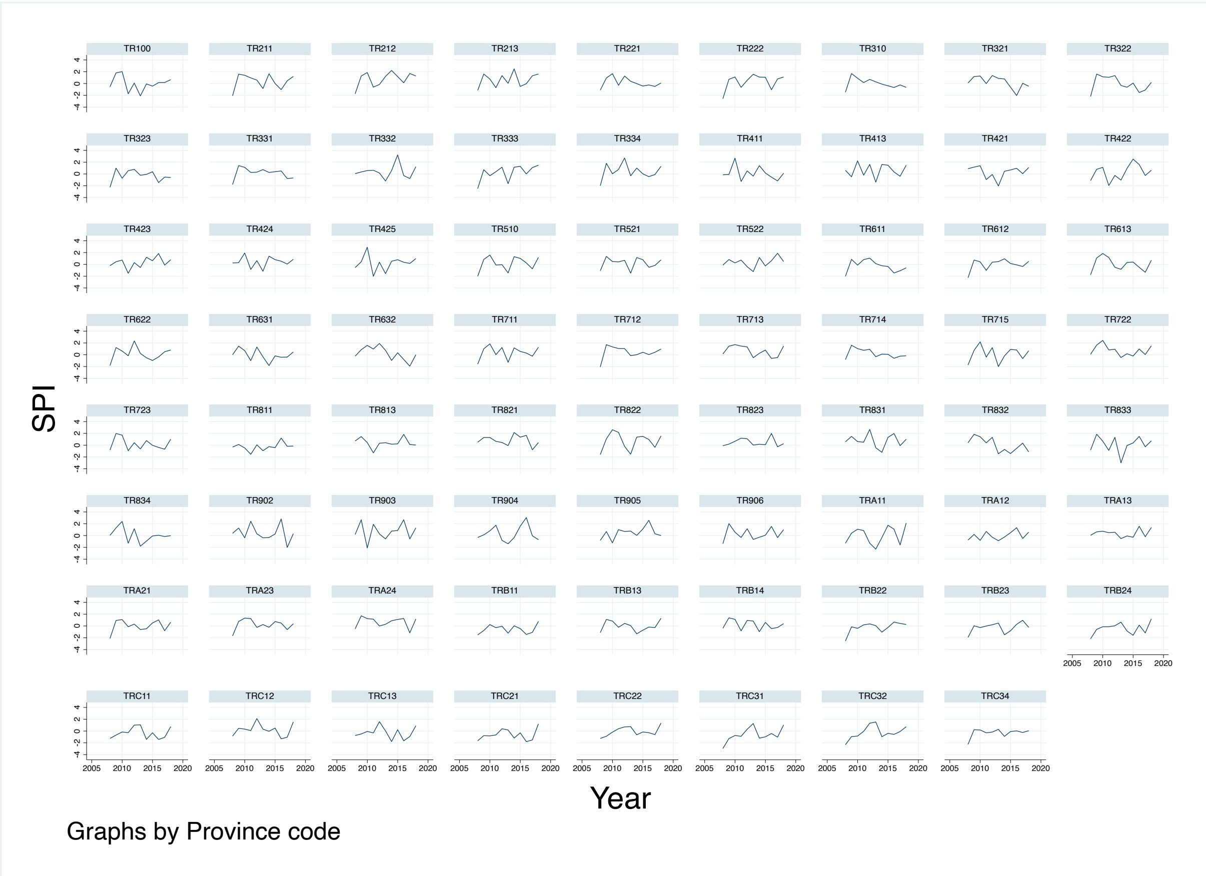
C Chapter I: Rural-urban classification

Table C1: List of variables used in Oğdül (2010) six-factors analysis

Categories	Variables
Agricultural production	Percentage of agricultural production in percentage of total agricultural production Agricultural production values per 1000 rural residents Agricultural production values per 1000 people engaged in agriculture
Non-agricultural production	Level of non-agricultural production Employment in industrial sector per total employment Employment in construction sector per total employment Employment in commercial sector per total employment Employment in transportation sector per total employment Employment in finance sector per total employment
Employment structure	Employee per total employment Women employee per total employment Employers per total employment Dependency ratio
Demography	Population size Rate of urbanization Population density
Educational level	Literate per total population Literate women per total women population Higher education graduates per total population Service zone grade for civil servants in education and academics
Trade opportunities	Accessibility (availability of airports, ports and railways) Budget income per capita Number of branch banks

D Chapter I: SPI time evolution

Figure D1: Time trend of SPI for Turkish provinces



E Chapter II: Surveys

Table E1: List of surveys

Country	Survey
Benin	2006 DHS 2011-12 DHS 2017-18 DHS
Burkina Faso	2003 DHS 2010 DHS 2014 MIS 2017-18 MIS
Cameroon	2004 DHS 2011 DHS 2018 DHS
Congo, Democratic Republic	2007 DHS 2013-14 DHS
Ethiopia	2011 DHS 2016 DHS
Ghana	2003 DHS 2008 DHS 2014 DHS 2016 MIS
Kenya	2003 DHS 2008-09 DHS 2014 DHS
Liberia	2007 DHS 2013 DHS 2016 MIS
Malawi	2004 DHS 2010 DHS 2014 MIS 2015-16 DHS
Mali	2006 DHS 2012-13 DHS 2015 MIS 2018 DHS
Mozambique	2003 DHS 2011 DHS 2015 AIS
Namibia	2006-07 DHS 2013 DHS
Senegal	2005 DHS 2010-11 DHS 2017 DHS
Sierra Leone	2008 DHS 2013 DHS
Tanzania	2004-05 DHS 2010 DHS 2015-16 DHS
Togo	2013-14 DHS 2017 MIS

F Chapter III: Composition of treated and donor groups

Table F1: List of treated countries

Country	Type of shock	Year of shock
Mozambique	Flood	2000
Mauritania	Drought	2001
Zimbabwe	Drought	2001
Namibia	Drought	2002
Ethiopia	Drought	2003
South Africa	Drought	2004
Burundi	Drought	2005
Comoros	Volcanic activity	2005
Eswatini	Drought	2007
Chad	Drought	2009
Mali	Drought	2011
Gambia	Drought	2012
Malawi	Drought	2015

G Chapter III: Imbalances in staggered adoption

This appendix develops on the two errors explained by Ben-Michael et al. (2022) in the context of staggered adoption.

The first error, known as state-level imbalance, occurs when the bias in the pre-treatment of the Average Treatment Effect (ATT) is estimated exclusively for each treated country. In order to obtain an unbiased ATT using this approach, all country-specific estimates must have perfect pre-treatment fits, which is not the case in my data. Formally, the separate SCM approach can be written as solving a single joint optimization problem on a matrix with weights $\Omega = [\omega_1, ..., \omega_J] \in \mathbb{R}^{N \times J}$:

$$\min_{\omega_1,...,\omega_j \in \Delta_j^{SCM}} \underbrace{\frac{1}{2J}\sum_j \left[\frac{1}{T_j - 1}\sum_{t=0}^{T_0}\left(Conflicts_{j,t} - \sum_i \omega_{j,i} Conflicts_{i,t}\right]}_{q^{sep}} + \lambda \sum_j \sum_i f(\omega_{j,i}) \tag{G1}$$

where q^{sep} represents the state-level imbalance, namely the average mean square error before the intervention across the J treated countries (Ben-Michael et al., 2022). To illustrate this concept, Figure 3.1 shows the difference in outcome between the treated and synthetic groups, while using separate SCM estimates with my data. The differences observed in the pre-treatment years demonstrate significant variations between the treated and synthetic averages, indicating that the level of state-level imbalance is high and needs to be significantly reduced in this case.

The second ATT error, known as the pooled imbalance, occurs when the average outcome of all treated countries is fitted with the outcomes of countries in the donor pool. This can lead to a poor balance at the state-level and is more prone to bias due to non-linearity. The pooled SCM approach can be described as follows:

$$\min_{\omega_1,...,\omega_j \in \Delta_j^{SCM}} \underbrace{\frac{1}{L}\sum_l \left[\sum_t^{T_0}\left(Conflicts_{j,t} - \sum_i \omega_{j,i} Conflicts_{i,t}\right)\right]^2}_{q^{pool}} + \lambda \sum_j \sum_i f(\omega_{j,i}) \tag{G2}$$

where L is the number of pre-treatment periods and q^{pool} is the pooled imbalance at each pre-treatment period. It is worth noting that the use of a pure pooled structural causal model (SCM) is not a common practice in the literature.

H Chapter III: Trends of treated and counterfactual

Table H1: Trend of treated and counterfactual

Year post-treatment	-5	-4	-3	-2	-1	0	1	2	3	4	5
Conflict fatalities average	27.39	55.33	92.30	21.49	15.69	6.57	2.47	5.34	3.95	1.66	1.15
Synthetic conflict fatalities	27.37	54.98	91.46	21.52	15.76	26.63	64.11	68.60	50.33	84.21	55.93
One-sided fatalities average	30.44	15.49	18.83	16.61	5.37	6.21	1.55	1.43	0.47	1.05	1.76
Synthetic one-sided fatalities	30.36	15.51	18.69	16.55	5.48	3.46	13.88	26.12	7.95	8.86	11.67
Occurrence of sexual violence average	0.56	0.40	0.18	0.55	1.91	0.18	1.36	0.82	0.27	0.28	0.27
Synthetic occurrence of sexual violence	0.55	0.40	0.19	0.55	1.90	3.71	3.70	4.97	7.93	8.47	5.45

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. *Conflict fatalities average*, *One-sided fatalities average*, and *Occurrence of sexual violence average* are the annual average of all countries in treated groups. *Synthetic conflict fatalities*, *Synthetic one-sided fatalities*, and *Synthetic occurrence of sexual violence average* are the values of the synthetic counterfactual.

I Chapter III: In-space placebos

Table I1: In-space placebo averages and count of placebos

Year post-treatment	0	1	2	3	4	5
Conflict fatalities in-space average	0.94	0.36	2.05	2.25	0.97	1.22
Observation in-space conflict fatalities	162	162	162	162	162	162
One-sided fatalities in-space average	1.12	0.97	0.42	0.85	-0.01	0.10
Observation in-space one-sided fatalities	78	74	74	74	69	63
Sexual violence in-space average	-0.19	-0.22	-0.26	-0.24	-0.18	-0.07
Observation in-space sexual violence	165	149	149	149	131	112

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence. *P-value* are the p-values I calculate from the in-space placebo analysis.

J Chapter III: In-time placebos

Table J1: Differences in in-time placebo estimates

Year post in-time placebo treatment	0	1	2
Conflict fatalities in-time difference	-9.28	0.26	-11.34
One-sided fatalities in-time difference	-4.74	-9.82	-7.6
Sexual violence in-time difference	-0.21	-1.56	-1.68

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence.

This Appendix aims to demonstrate that the results obtained in the in-time placebo are dependent on the simplicity of the model, and not on a poor specification. In the main results, I choose to use only the outcome variables as predictors in the SCM to strike a balance between model complexity, sparsity, transparency, and precision. The main results present the simplest models, since the estimates are not significantly different from those of more specific models. To illustrate this, I reestimate the models for one-sided fatalities and sexual violence by adding various auxiliary variables.[25] Figures J1a and J1c and Table J2 show estimates for the impact of extreme natural disaster shocks on one-sided fatalities and the occurrence of conflict-related sexual violence, and Figures J1b and J1d and Table J3 present the estimates for the in-time placebo with specific auxiliary variables.

The inclusion of auxiliary variables does not significantly affect the fit of the model for one-sided fatalities and conflict-related sexual violence, as the difference in estimates before treatment remains close to zero. Both variables also exhibit a decreasing trend after treatment, with the magnitude of the treatment effect being larger for one-sided fatalities and smaller for conflict-related sexual violence compared to the main results. The calculated p-values are also similar to those in the main results. When considering in-time placebos, I observe only small deviations after the placebo shocks, indicating that these models fit the outcomes well.

[25] Both models takes into account GDP per capita, life expectancy and the share of population discriminated. The model for one-sided violence also takes into account population.

This additional analysis suggests that, while the main model may not be perfect for the three variables under consideration, using a simpler and more general model for each outcome variable does not significantly alter the results compared to models that are more specific to the variables. Therefore, the credibility of the main results is not undermined by the use of a less precise model.

Table J2: Differences in in-time placebo estimates with specific auxiliary variables

Year post-treatment	0	1	2	3	4	5
One-sided fatalities difference	0.53	-15.73	-28.99	-6.62	-8.32	-11.83
P-value one-sided fatalities	0.83	0	0	0.04	0.04	0.04
Sexual violence difference	-1.62	-1.99	-1.71	-3	-3.83	-2.43
P-value sexual violence	0.08	0.08	0.07	0.06	0.06	0.03

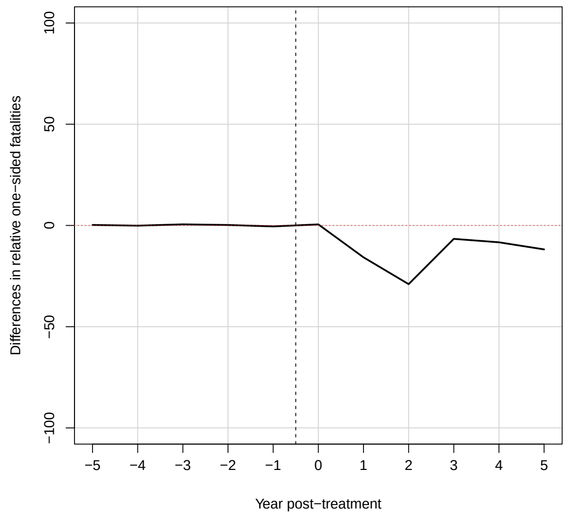
Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence. *P-value* are the p-values I calculate from the in-space placebo analysis.

Table J3: Differences in in-time placebo estimates with specific auxiliary variables

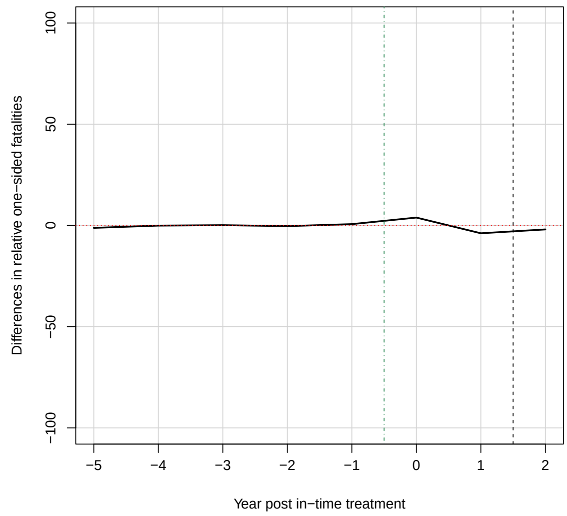
Year post in-time placebo treatment	0	1	2
One-sided fatalities in-time difference	3.90	-3.86	-1.96
Sexual violence in-time difference	0.12	-0.67	-0.25

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence.

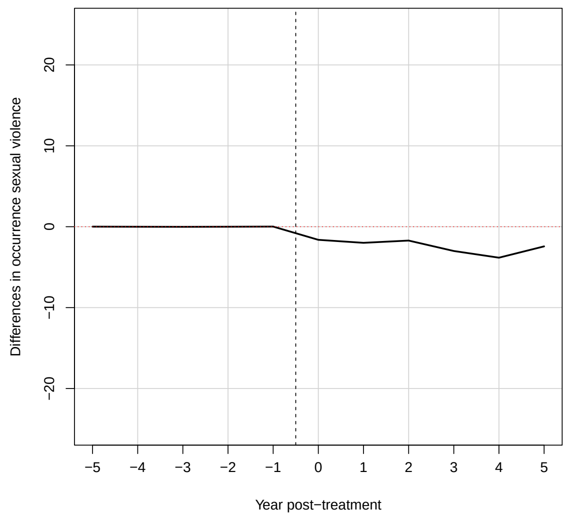
Figure J1: In-time placebo estimate differences with specific auxiliary variables



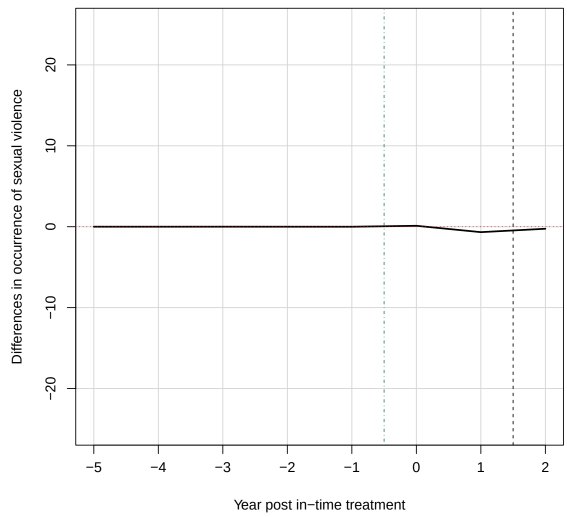
(a) One-sided fatalities



(b) One-sided fatalities – in-time



(c) Sexual violence



(d) Sexual violence – In-time

Notes: The solid black lines in Figures J1a and J1c represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart. The black lines in Figures J1b and J1d present the in-time placebo partially pooled SCM estimated differences between treated group and the synthetic counterpart.

K Chapter III: Choice of predictors

Table K1: Differences in estimates with auxiliary variables

Year post-treatment	0	1	2	3	4	5
All types of conflict difference	-20.06	-61.64	-63.26	-46.38	-82.55	-54.78
P-value all types	0.14	0	0	0.12	0	0.07
One-sided fatalities difference	-0.94	-14.12	-25.56	-8.15	-8.61	-11.48
P-value one-sided	0.17	0	0	0	0.04	0.04
Occurrence of sexual violence difference	-1.62	-1.99	-1.71	-3	-3.83	-2.43
P-value sexual violence	0.08	0.08	0.07	0.06	0.06	0.03

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence. *P-value* are the p-values I calculate from the in-space placebo analysis.

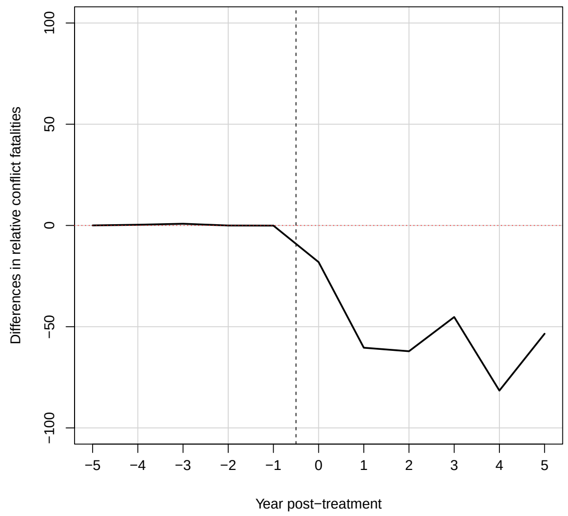
L Chapter III: Leave-one-out

Table L1: Differences in leave-one-out estimates

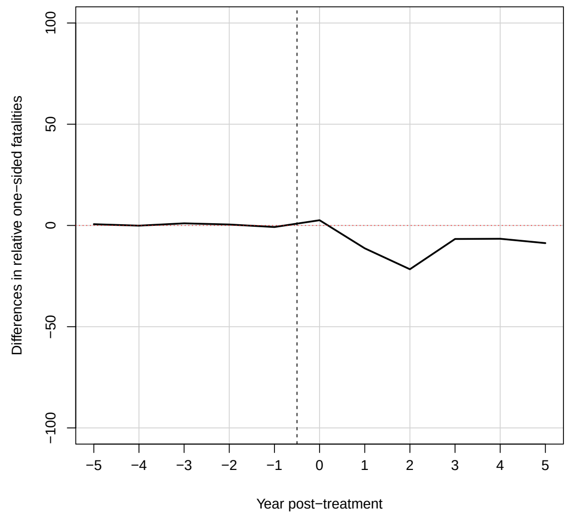
Year post-treatment	0	1	2	3	4	5
No neighbor:						
Conflict fatalities difference	-18.10	-60.39	-62.13	-45.23	-81.57	-53.48
One-sided fatalities difference	2.59	-11.29	-21.63	-6.68	-6.58	-8.72
Sexual violence difference	-3.24	-2.17	-3.17	-6.61	-7.11	-4.46
Regions:						
Conflict fatalities average difference	-27.05	-51.16	-59.42	-47.09	-71.11	-51.17
One-sided fatalities average difference	0.58	-13.11	-22.58	-11.24	-9.94	-11.26
Sexual violence average difference	-2.98	-2.32	-3.57	-6.86	-6.87	-4.52
Treated:						
Conflict fatalities average difference	-23.42	-58.75	-60.94	-44.19	-82.33	-53.92
One-sided fatalities average difference	2.90	-12.21	-24.97	-7.88	-7.42	-9.91
Sexual violence average difference	-3.29	-2.25	-4.21	-7.36	-7.87	-4.99
Donor:						
Conflict fatalities average difference	-23.82	-61.66	-63.34	-47.30	-82.19	-55.66
One-sided fatalities average difference	2.34	-12.44	-24.45	-8.02	-8.35	-10.18
Sexual violence average difference	-3.41	-2.31	-4.14	-7.52	-7.96	-5.07

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. For *no neighbor*, the differences are between the partially pooled average and the counterfactual. The differences are between the partially pooled average and the counterfactual. For *Regions*, *Treated* and *Donor*, the estimates are average of all partially pooled estiamtes. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence.

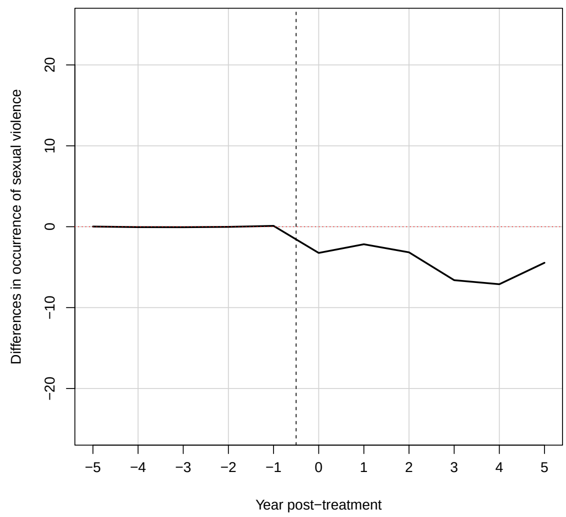
Figure L1: Leave-one-out neighbor



(a) All types of conflict fatalities



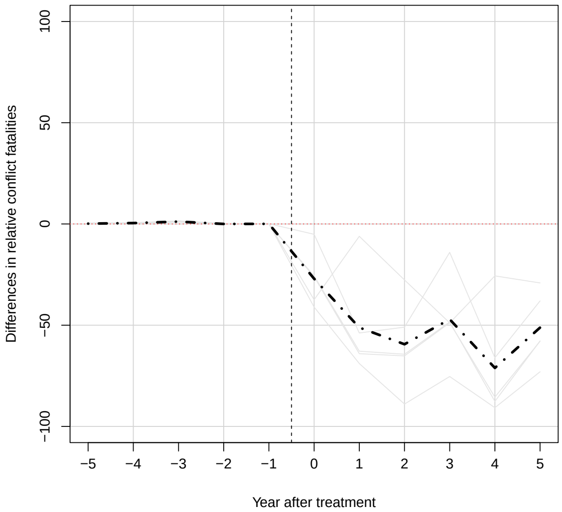
(b) One-sided fatalities



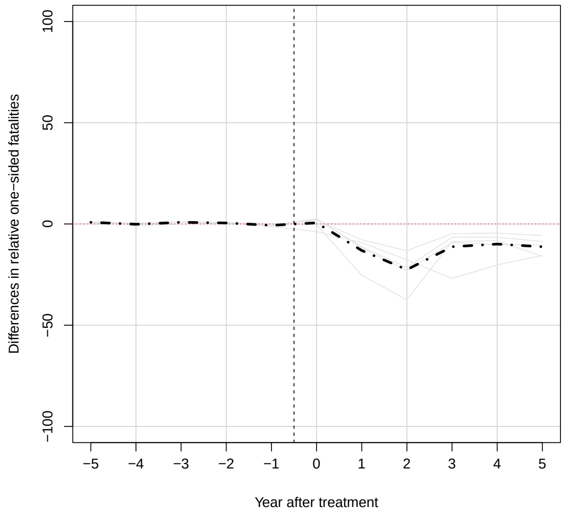
(c) Sexual violence

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

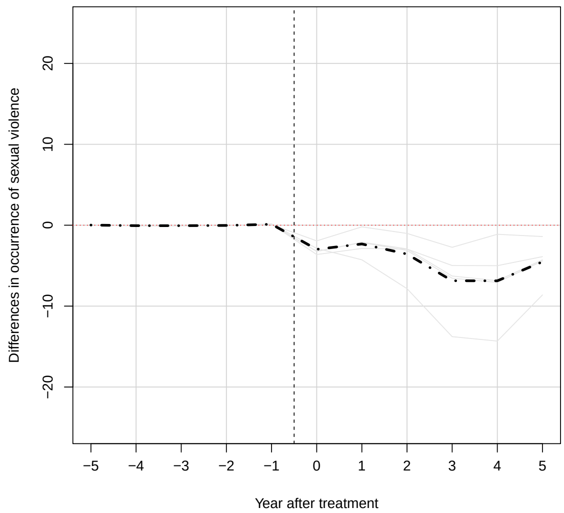
Figure L2: Leave-one-out region



(a) All types of conflict fatalities



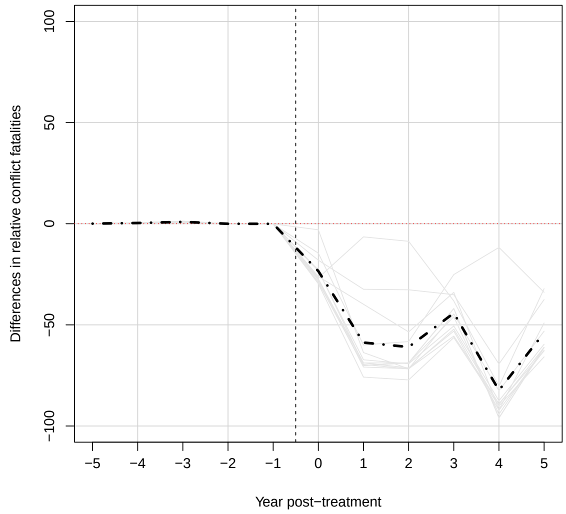
(b) One-sided fatalities



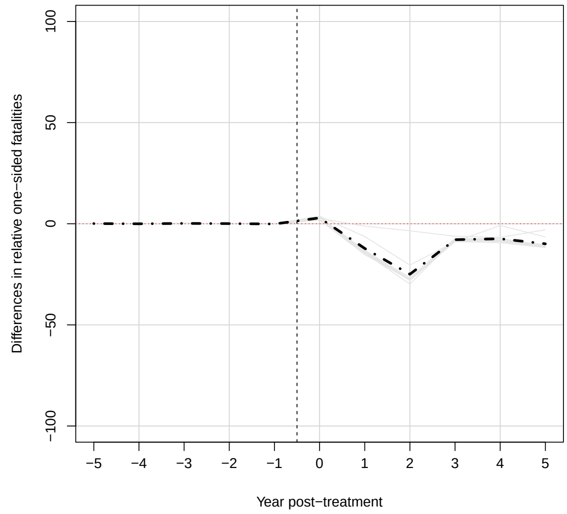
(c) Sexual violence

Notes: The black dash-dotted represent the average of estimated differences and the solid grey lines represent each partially pooled estimated differences between the treated group and the synthetic counterpart.

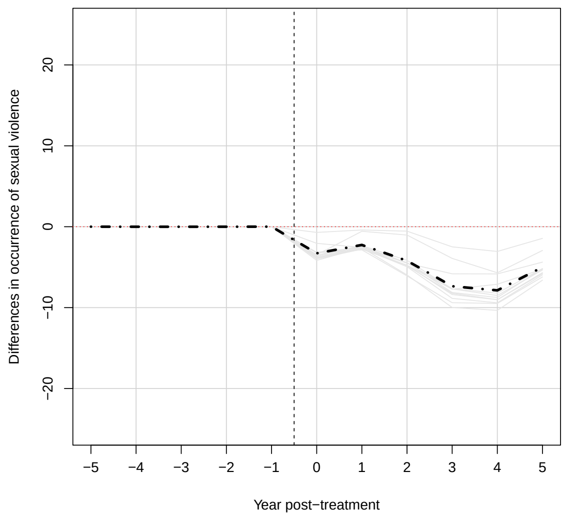
Figure L3: Leave-one-out treated



(a) All types of conflict fatalities



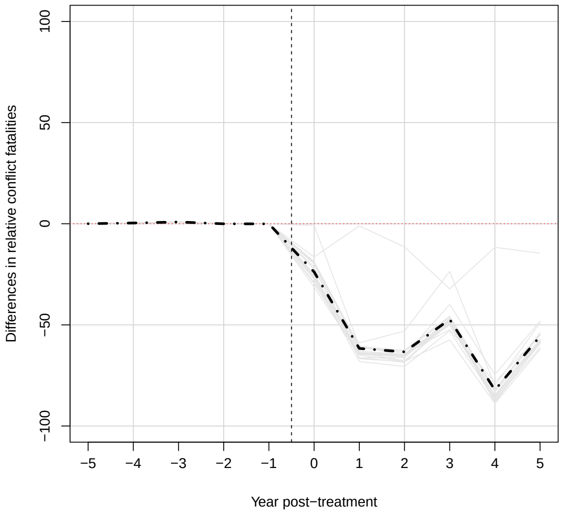
(b) One-sided fatalities



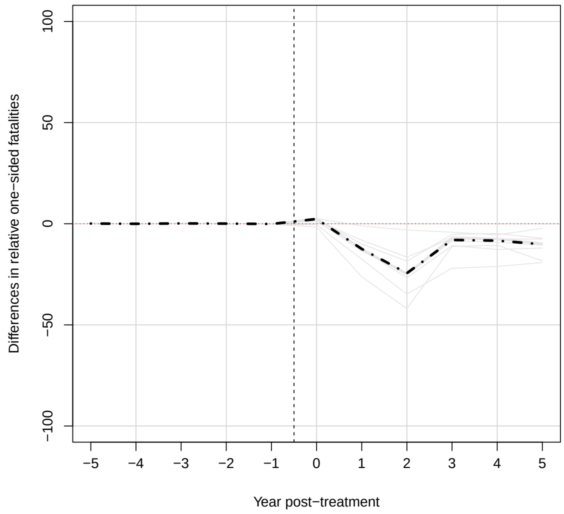
(c) Sexual violence

Notes: The black dash-dotted represent the average of estimated differences and the solid grey lines represent each partially pooled estimated differences between the treated group and the synthetic counterpart.

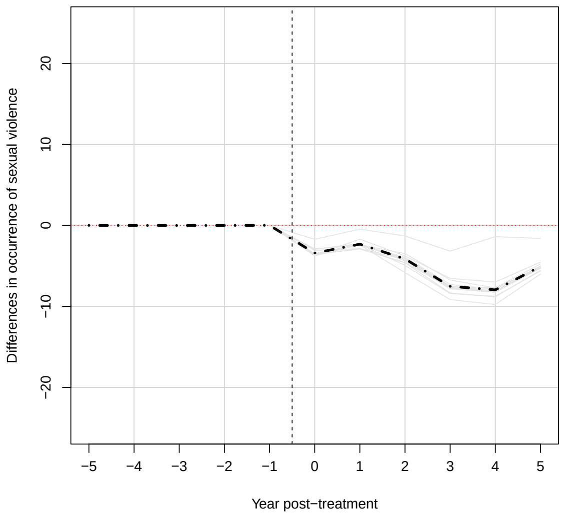
Figure L4: Leave-one-out donor



(a) All types of conflict fatalities



(b) One-sided fatalities



(c) Sexual violence

Notes: The black dash-dotted represent the average of estimated differences and the solid grey lines represent each partially pooled estimated differences between the treated group and the synthetic counterpart.

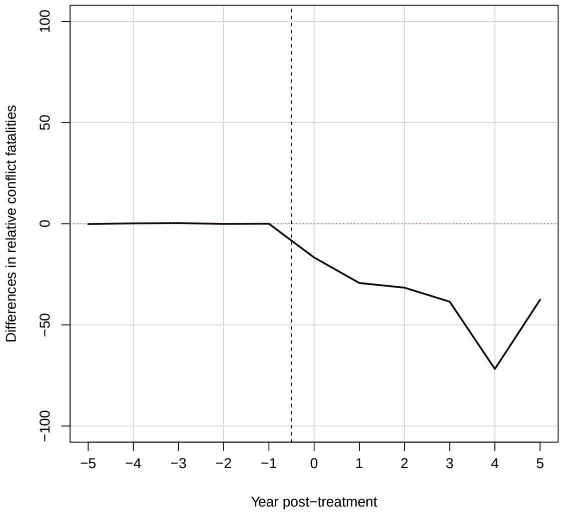
M Chapter III: Identification threshold variations

Table M1: Differences in threshold variation estimates

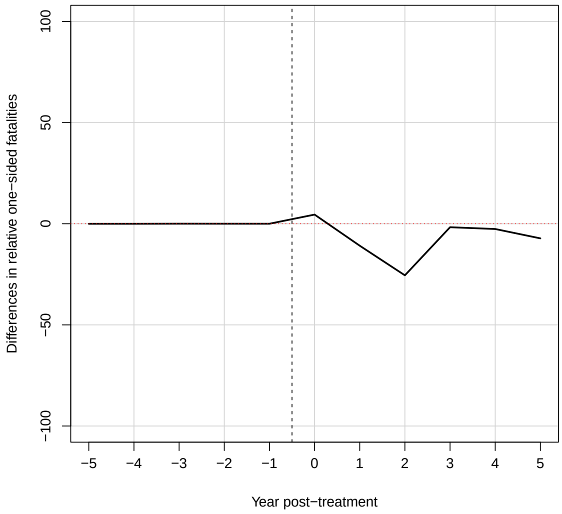
Year post-treatment	0	1	2	3	4	5
Restricted treatment group:						
Conflict fatalities	-16.68	-29.29	-31.62	-38.54	-71.80	-37.58
One-sided fatalities	4.52	-10.86	-25.50	-1.72	-2.61	-7.23
Sexual violence	-4.03	-4.17	-9.95	-10.94	-8.24	-7.47
Restricted donor group to 0.05:						
Conflict fatalities	-6.88	-34.35	-33.83	-20.05	-67.54	-32.53
One-sided fatalities	0.25	-13.93	-21.59	-6.72	-6.08	-9.62
Sexual violence	-1.71	-0.46	-1.31	-3.17	-1.40	-1.61
Restricted donor group to 0.01:						
Conflict fatalities	-17.16	-82.10	-82.89	-44.68	-106.45	-66.66
One-sided fatalities	-9.49	-32.14	-57.27	-23.27	-15.96	-27.63
Sexual violence	-1.29	-0.65	0.02	-0.86	-1.74	-0.90

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence.

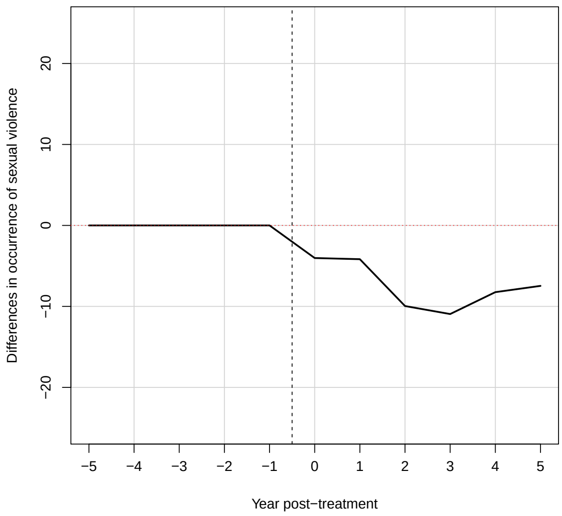
Figure M1: Restricted treated group



(a) All types of conflict fatalities



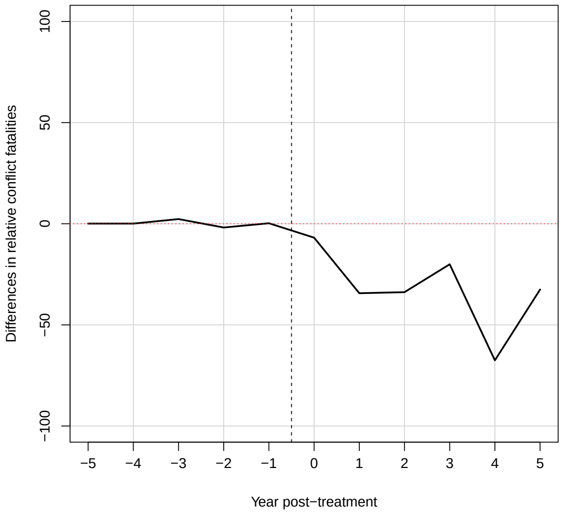
(b) One-sided fatalities



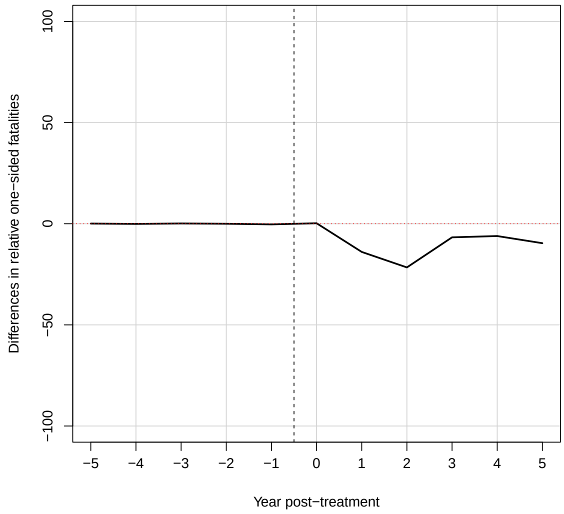
(c) Sexual violence

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

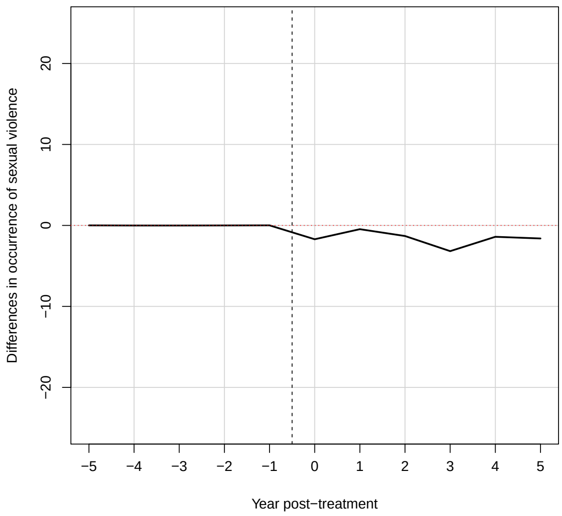
Figure M2: Donor threshold at 0.05 share of population



(a) All types of conflict fatalities



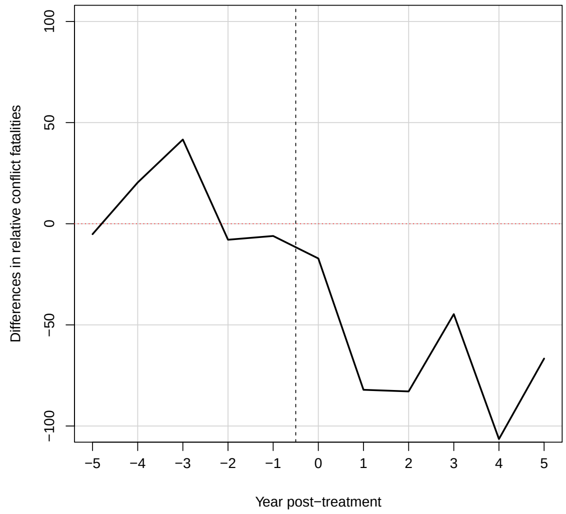
(b) One-sided fatalities



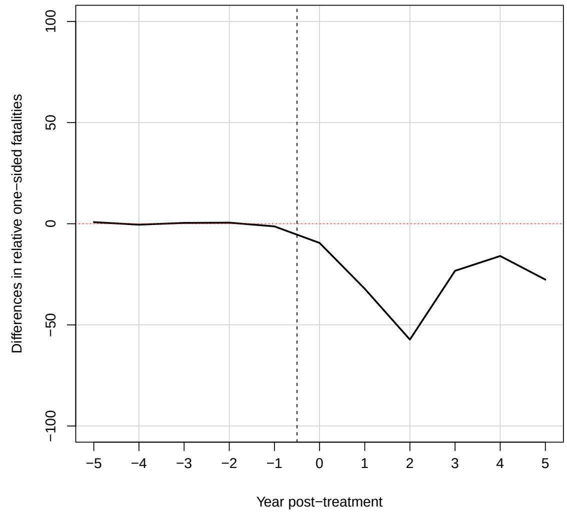
(c) Sexual violence

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

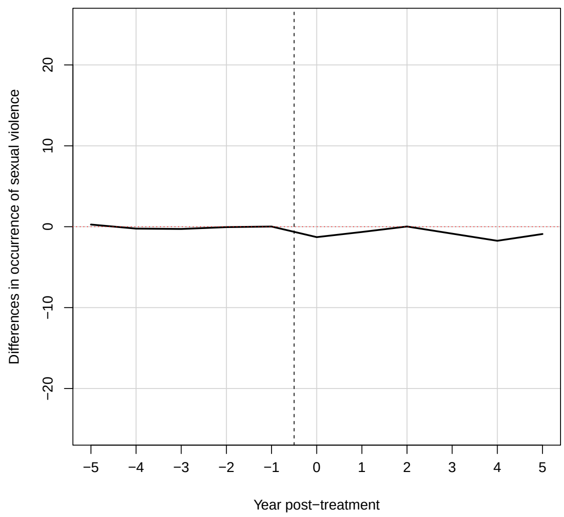
Figure M3: Donor threshold at 0.01 share of population



(a) All types of conflict fatalities



(b) One-sided fatalities



(c) Sexual violence

Notes: The solid black lines represent the partially pooled SCM estimated differences between the treated group and the synthetic counterpart.

N Chapter III: 10 years pre-treatment period

Table N1: Differences in estimates with 10 years pre-treatment

Year post-treatment	0	1	2	3	4	5
Conflict fatalities difference	-29.79	-27.97	-29.94	-23.18	-81.51	-38.47
P-value conflict fatalities	0.07	0.07	0.09	0.14	0	0.08
One-sided fatalities difference	3.66	-8.85	-22.54	-6.13	-4.50	-7.67
P-value one-sided	1	0	0	0	0	0
Occurrence of sexual violence difference	-2.52	-1.24	-0.69	-1.45	-2.46	-1.67
P-value sexual violence	0.02	0.07	0.25	0.05	0.03	0.05

Notes: *Year post-treatment* is the number of years after the extreme natural disaster shocks. The differences are between the partially pooled average and the counterfactual. *Conflict fatalities* are fatalities for all types of conflicts in million people. *One-sided fatalities difference* are fatalities in million people. *Occurrence of sexual violence difference* is the annual count of occurrence of conflict-related sexual violence. *P-value* are the p-values I calculate from the in-space placebo analysis.

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