The Economic Consequences of Political Turmoil

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When I started studying Economics in Heidelberg in 2011, I didn't remotely plan to be working in academia 12 years later. Like all good Economics first semester students, I planned to get my degree and then make big money at some bank or so. Only towards the end of my Bachelor's degree, the first "real academic courses" talking about research designs and empirical questions in public choice and terrorism, taught by Axel Dreher and Martin Gassebner who would turn out to become the supervisors of this PhD thesis, got me hooked. I started reading and enjoying economic papers and books, and pursued my Master's degree with the goal to, one day, do research myself. I was lucky enough to pick Gerda Asmus-Bluhm as the supervisor for my seminar thesis and later Master thesis. Next to doing a great job channeling my clumsy research attempts into the right direction, she encouraged me to first learn *R* programming and spatial data analysis, and then apply for the PhD position her now-husband Richard Bluhm was offering in Hannover. Again, I was lucky enough that they decided to hire that inexperienced and clumsy Master student from Heidelberg, and teach him how to actually conduct empirical research.

In Hannover, I learned very much, very fast. The PhD students in Heidelberg warned me of the Institute of Macroeconomics where I would spend the next six years of my career. Apparently, the culture there would be direct, mean, and unfriendly. Shortly after I met Paul Schaudt, the institute's first PhD student and my office mate for the first 2.5 years, I told him about these accusations. He laughed and said "yes, that's true. We are mean, but we really love each other – that's what makes us special." Now I can say he was right, and I am very thankful for that. I am deeply grateful to my first supervisor, boss, co-author and friend Martin Gassebner who introduced me to the world of academia. He taught me how important and fun it was to network at conferences, collect and give feedback, and just don't get tired playing the game. I thank him for all the opportunities he arranged and financed, the helpful feedback for all my projects (even after watching the presentation for the X'th time), the many beers and fun at conferences and in Hannover, and above all for trusting me early on with my research decisions and teaching responsibilities, which helped me build the confidence needed in academia. Similarly, I am indebted to my second supervisor Axel Dreher who, next to so much helpful advice on my papers and publication strategies, gave me the important advice that nothing is as important in academia as always staying for the last beer. Without Axel, I never would have met Martin and probably ended up outside academia in some better paying, yet tremendously boring job.

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Chapter 1

Introduction

> "Great privation and great risks to society have become unavoidable. All that is now open to us is to redirect, so far as lies in our power, the fundamental economic tendencies which underlie the events of the hour, so that they promote the re-establishment of prosperity and order, instead of leading us deeper into misfortune." (John Maynard Keynes, 1919/2007, p. 147)

John Maynard Keynes wrote the above words only months after Europe saw its, until then, deadliest political conflict. The First World War cost tens of millions of lives in the four years from 1914 to 1918. As foreshadowed by Keynes's criticism, the Treaty of Versailles, which focused more on remedying the Allied Powers' costs of war than rebuilding Europe after the years of destruction, paved the way for the even bigger tragedy of the Second World War twenty years later. To no avail, Keynes urged the Allied Powers to allow the losing parties, despite their misdeeds during the war, to rebuild their economies. A slow economic recovery due to high reparations and a later occupation of Germany's industrial center led from hunger winters to hyperinflation and economic depressions to regime instability. All this fueled increasing grievances in the German population, who soon fell for the fascist propaganda of right-wing demagogues. In 1933, almost every second German citizen voted for the extreme right-wing NSDAP party, which finally led Europe into its darkest years in recent history.

The failure of economic reconstruction and repeated cycles of violence due to unresolved grievances is a recurring problem worldwide. Paul Collier defines four "traps" that provoke vicious cycles and stop countries from lifting their people out of poverty. The first of these traps is the "conflict trap": war and destruction make countries poor, while the social grievances and unstable political regimes in poor countries exacerbate the odds of conflict. Hence, conflict and poverty enforce each other, requiring targeted and enormous help in post-war reconstruction and economic recovery to escape this vicious cycle (Collier, 2008). The Marshall Plan helped Germany and Europe after the Second World War to escape this conflict trap, multiply their incomes, and enjoy 80 years of stable peace. At the time of writing this thesis, other nations find themselves in the midst of war, hoping to first silence the weapons and then experience the assistance they need to (re)build their economies, escape the conflict trap, and lay the foundations for their populations to live peaceful lives.

In February 2022, Russia attacked Ukraine, causing havoc and destruction and ending the longest period of peace Europe had seen so far. While the war is still waging, economists are already discussing the challenges of reconstructing Ukraine and saving global supply chains amid sanctions against Russia (Becker et al., 2022). At the same time, countries like Yemen, Syria, Libya, and Somalia, to only name a view, ran down their economies and political systems through decades of war and destruction, often fueled by the political and financial interests of other nations. In Egypt, Myanmar, Mali, Sudan, and Niger, among others, militaries and mercenary groups violently ousted their governments and ended hopes of democratization and international cooperation. The citizens of Belarus, Iran, Türkiye, and Russia are leading protests against their repressive governments, facing persecution and death penalties as punishment. The people in all these nations are waiting for the violence to stop, and many hope to receive the international attention and assistance to lead their economies (back) to a path of sustained economic growth.

The goal of this thesis is to document the many aspects via which civil violence hinders economic development, and how the international community can support post-war economic recovery. I do not aim to just quantify the negative impact of war on the economy. There is already plenty of evidence in that direction, and that war and destruction harm people and capital is probably evident in itself, being demonstrated regularly in the evening news (Fearon and Laitin, 2003, Collier et al., 2003, Collier and Hoeffler, 2004, Blattman and Miguel, 2010). Much more, the research in this thesis aims to evaluate economies' chances to recover from war, and guide reconstruction policies towards the important but latent heterogeneities that cloud the true economic impact of political turmoil. Despite the apparent destructiveness of war and violence, there remains vast disagreement about the persistent economic consequences of (civil) war. By investigating the latent heterogeneities and identifying the transmission channels of war and violence, this thesis sets out to contribute to this discussion and promote finding a general theory of war and sustained recovery.

At the heart of this thesis lies the ongoing discussion in the literature whether and how political turmoil persistently affects the economy. Various recent studies point towards long-run consequences of violence on the people that were directly affected (Verwimp et al., 2019). Among other things, even decades after the end of a conflict, households in conflict zones own less capital (Justino and Verwimp, 2013, Mercier et al., 2020), face higher inequality (Bircan et al., 2017), and show a lack of schooling (Brück et al., 2019). The devastation of war often displaces people for generations (Verwimp and Muñoz-Mora, 2018, Verwimp et al., 2020), and lets them lose trust in their government and neighbors (Tur-Prats and Valencia Caicedo, 2020). Yet, other studies brace the resilience and fast automatic recovery of countries and towns from the destruction of war (Blattman and Miguel, 2010, Blattman, 2012). Macroeconomic theories like the Solow model view economies following a balanced growth path. Economic shocks like war can let them deviate for a while, but economic automatisms lead them back to their paths in the end (Barro and Sala–i–Martin, 1992, Mankiw et al., 1992). Empirical evidence reasons that large scale destruction from bombing in Japan and Germany leave no significant imprint a mere two decades later (Davis and Weinstein, 2002, Brakman et al., 2004). Even when analyzing the same conflict, empirical studies come to different conclusions. While Miguel and Roland (2011) provide evidence that districts in Vietnam that experienced more US bombing attacks grew even richer than their peers due to successful reconstruction policies, Dell and Querubin (2018) and Singhal (2019) show that still today, harder-hit districts host weaker local institutions along with less trusting and less mentally healthy inhabitants.

The chapters in this thesis are devoted to these contrasting results. Across the four chapters summarized below, my co-authors and I analyze different mechanisms, heterogeneities, and externalities via which political turmoils in their different shapes can have long-lasting economic and social consequences. Those peculiarities of conflict often are not directly as visible as is the physical destruction of war, which makes it easy to overlook them when designing reconstruction policies. Hence, the main goal of this thesis is to point out the indirect costs of political turmoil, and to guide international policy towards successful and efficient remedies to war. To paint an extensive picture of the economic consequences of political turmoil, this thesis combines evidence from global samples at different spatial resolutions with the investigation of specific historical case studies. The next two chapters look at global samples of various types of conflicts at the subnational district (ADM2) as well as the bilateral country level. Chapter 4 then investigates the local effects of WWI casualties on the economic trajectories of German counties during the crucial period of Germany's structural transformation. Finally, Chapter 5 analyzes the persistent individual-level effects of Kenya's struggle for independence in the 1950s. Overall, this thesis therefore not only analyzes the economic effects of political turmoil at various spatial resolutions and time periods. It also provides evidence across different types of political turmoil.

Civil conflicts constitute the most common type of political turmoil. They encompass violent clashes between governments and non-governmental groups, e.g., rebel factions or guerilla groups. Civil conflicts can describe small quarrels between organized groups and militaries that restrict casualties and destruction to the parties involved. They can also escalate into full-scale civil wars that take thousands of lives each year (Bluhm et al., 2021). Commonly, international actors pursue their own interests by supporting either side of a civil conflict financially or directly in combat. Such conflicts with outside support are often termed "internationalized conflicts" (Sundberg and Melander, 2013). The next two chapters of this thesis focus on these many facets of civil conflicts.

Chapter 2 develops a new measure for local conflict exposure, and demonstrates the causal medium-run effects of conflict exposure on economic activity. The currently used measures for conflict exposure make it difficult to distinguish correctly between more or less conflict-affected counties. Commonly, researchers use geocoded conflict event data to determine where a battle took place, overlay these point data with shapefiles tracing locations' boundaries, and then count the number of conflict events or battle deaths within a location (see, among others, Bellows and Miguel, 2009, Chamarbagwala and Morán, 2011, Dube and Vargas, 2013). I propose instead to measure conflict exposure as the share of a location's population that lives in direct vicinity to a battle event in a given year. For that, I leverage gridded population as well as nighttime light data to identify the population distribution within the second administrative (ADM2) regions across 70 countries worldwide. In the main specifications, I then calculate for each ADM2 region the share of the population that inhabits the area within a 3km buffer around any of the conflict events that occurred during a year. This generates a location-year panel where I observe conflict exposure on a scale from 0%–100%.

First, Chapter 2 provides robust evidence that locations with more concentrated population patterns experience higher conflict exposure levels for a given number of battle events. I measure population concentration by the spatial Gini coefficient of nightlights, which assigns a value between 0 and 1 to each district, 0 indicating a perfectly equal distribution of the population and a value of 1 meaning that all of a district's nightlights are emitted from only one pixel (Achten and Lessman, 2020). This finding constitutes an important heterogeneity in conflict exposure, as the pre-determined spatial population distribution determines locations' resilience to conflict.

Second, Chapter 2 conducts a causal analysis of the medium-run effects of conflict exposure, holding the number of conflict events constant. I use a Shift-Share Instrumental Variable following, e.g., Dreher et al. (2021) and Nunn and Qian (2014) to predict conflict exposure by the interaction of a time-invariant share, here the interaction of the average conflict propensity and the spatial Gini coefficient of nightlights, and a space-invariant Shifter, here the global oil price. Various robustness tests emphasize that this instrument suffices the assumptions outlined in Adão et al. (2019), Borusyak et al. (2022) and Goldsmith-Pinkham et al. (2020) in the form that a) the variation in the global oil price identifies significant changes in oil countries' likelihood of battle events as oil provides both the means and motivation to fight, and b) the local Shares are plausibly exogenous conditional on covariates.

My results show that higher conflict exposure persistently decreases locations' economic activity. I do not find the same effect for the other common measures of conflict exposure, suggesting that measuring conflict exposure as the share of affected people uncovers an important heterogeneity for designing reconstruction policies. A mechanism analysis indicates that locations with higher conflict exposure according to my novel measure face higher out-migration and flight of capital, which constitute important levers for post-war economic policies.

In Chapter 3, my co-author Henry Stemmler and I take a closer look at conflict-induced changes of international financial flows hinted at in Chapter 2. Looking at bilateral, sector-level trade flows for more than 150 countries from 1995–2014, we analyze the trade relocation effects of civil conflicts. To do so, we first replicate prior findings that civil conflicts hurt countries' export propensity (Martin et al., 2008a,b, Novta and Pugacheva, 2021). Building on this observation, we then develop a novel estimation approach to empirically analyze whether the import partners of conflict countries turn to alternative exporters to compensate their loss in imports, and crucially, whether these import partners return to their original export providers once these resolve their civil conflicts.

Our estimation approach translates the triadic relationship of conflict country–import partner–alternative exporter into a dyadic relationship that is observable in common bilateral trade statistics. For this, we define across all exporter–importer dyads in our sample *i*) a *relevance condition*, indicating that some conflict country used to provide a significant amount of traded goods to the dyad's importer before the conflict, and *ii*) a *similarity condition* which indicates that the dyad's exporter exhibits similar production capabilities as that same conflict country and hence constitutes a valid exporter alternative.

We find significant evidence for sizeable trade relocation in the agricultural and manufacturing sectors. Importantly, we find this effect to persist even years after the resolution of the conflict, especially in the manufacturing sector. This is, once countries substitute their export providers due to civil conflict, they are likely to not return to their original trade partners. As a potential mechanism behind this effect, we find that dyads that increased bilateral trade due to trade relocation from civil conflict have an increased likelihood to sign Preferential Trade Agreements (PTAs). These, in turn, persistently decrease countries' bilateral trade costs, who find themselves in a new general equilibrium. Our findings extend prior case-study evidence on trade relocation by Carvalho et al. (2020) and Freund et al. (2021), and outline international trade policies as important tools to help countries recover from political conflicts. If civil conflicts alter the general equilibrium on international markets, they lead to persistently worse economic outlooks for conflict countries, even if they successfully solve the roots of conflict. Hence, trade-promoting policies like temporary tariff reductions for post-conflict countries provide an easy yet effective tool to support their economic recovery.

In Chapter 4, this thesis turns to another type of political turmoil: international conflicts, or wars. International conflicts describe violent clashes between the militaries of two or more nation states that are fought on the soil of one or various belligerents and include the movement of troops across borders. Chapter 4 investigates the local economic consequences of one of the largest and most infamous international conflicts: The First World War.

Together with my co-authors Matthias Quinckhardt and Lukas Wellner, we analyze how war casualties from WWI affected local economies during Germany's late phase of structural transformation from an agricultural country to modern production in the early 20th century. Most importantly, we investigate whether differences in labor substitutability due to available mechanization options and required on-the-job skills impacts sectoral employment growth differently. The First World War in Germany provides an interesting setting to isolate the effect of a war-related human capital shock on the labor markets in industrializing economies, as almost no fighting took place on German soil. Our results are therefore informative for current developing countries, where war but also disease or out-migration can hinder their economies' structural transformation.

Working at the German county level, which constitutes the fourth administrative district, we geocoded 8.5 million casualty entries from the official frontline loss reports. While these front reports were already used in other papers (see, e.g., Koenig, 2023, De Juan et al., 2023), our new geocoding improves a lot on the initially available geocodes. The lists contain soldiers' names along with their birthplaces and casualty status, i.e., whether they were wounded or killed. We geocoded the soldiers' birthplaces linking the hierarchical location information of the loss lists to a full list of Germany's administrative areas in 1910. This lets us calculate the share of pre-war male inhabitants for each of the approximately 1000 counties that were wounded or killed during the war. In addition, we collected rich county-level census data from before and after the war, which give us detailed sector-level information on the local labor markets, agricultural land holdings, and low-skilled wages.

With these data, we run Continuous Difference-in-Differences regressions following Callaway et al. (2021), where we estimate the local marginal response of our outcome variables to increases in the casualty share. Our results demonstrate that the human capital shock had different impacts across industries. First, we find that employment in the industrial sector followed a similar direction in counties with lower and higher casualties. Hence, vacant positions in this sector were rapidly filled after the war. In the agricultural and the emerging tertiary trade-and-service sectors, employment grew significantly less in counties with higher casualty shares. We find two different explanations for this.

In the agricultural sector, especially small landholdings that were mostly used for subsistence farming, disappeared. These farmlands were taken over by medium sized and larger farms, who worked their now bigger lands with the same number of employees, but a higher number of machines. According to anecdotal evidence, these small scale farmers left their farms in favor of better-paying jobs in the manufacturing sector, where firms were keen to fill their open vacancies. In the tertiary sector, vacancies were left unfilled. This was due to this sector's reliance on an emerging White Collar workers' class in Germany, who set themselves apart from Blue Collar workers by being highly educated and having acquired specific skills for their jobs in the tertiary sector. These workers could not be replaced by (subsistence) farmers or the unemployed labor reserve as could the jobs in the manufacturing sector, letting counties with higher casualties fall behind in the transformation towards a strong tertiary sector, Germany's main employment sector today.

Our results hence point towards an important heterogeneity in how local labor markets can cope with human capital shocks. The agricultural sector can compensate a loss in workers by employing labor-saving technologies via costly one-time investments. Manufacturing firms that do not require employees with hard-to-acquire skills easily find replacement from other sectors or the labor reserve. Finally, firms that rely on well trained personnel that cannot be replaced via mechanization find no easy way to cope with a sudden reduction in the labor force. These findings contribute to the growing literature on human capital shocks (see, e.g., Hornbeck and Naidu, 2014, Andersson et al., 2022, Brainerd and Siegler, 2003, Voigtländer and Voth, 2013a, Karlsson et al., 2014, Vigdor, 2008, Boustan et al., 2012, Voigtländer and Voth, 2013b), and inform labor market policy for developing countries. Here, especially out-migration due to Brain Drain, but also general human capital shocks from conflict, disease, or natural disasters, can substantially undermine the economic transformation towards production with high-skilled employees. Hence, it is important to watch out for skill-specific labor replacement, and provide firms with help finding qualified employees, e.g., by promoting migration or financing schooling and qualification measures.

A final type of political turmoil constitute the various forms of one-sided conflict. Coups d'état describe the non-electoral ousting of a nation's government by the use or threat of violence, commonly by the same nation's military (Gassebner et al., 2016). These conflicts are one-sided because the target of violence is specifically the government, and the perpetrators specifically are members of the government and/or the country's military. Other types of one-sided conflicts describe the indiscriminate use of violence from one actor against various types of a population. Terrorism encompasses the use of force by organized non-governmental groups against government entities or citizens to extort political or financial demands (see, e.g., Gassebner and Luechinger, 2011, for an overview). Finally, one-sided violence includes the political violence of governments against their own citizens. Political violence often targets specific parts of the population, but selects the victims within such a group indiscriminately. The long-run political, economic, and social consequences of such political violence are the topic of Chapter 5.

In Chapter 5, Gerda Asmus-Bluhm, Richard Bluhm, and I investigate the individual long-run consequences of Kenya's struggle for independence from the British Empire in the 1950s. Early in the 1950s, the so-called Mau Mau formed a rebel group to oppose British colonial rule and give property rights, especially over land holdings, back to the Kenyan people. The Mau Mau consisted of independence fighters that mainly drew from three out of Kenya's almost 50 ethnic groups. These were the Kikuyu, Embu, and Meru who, before the arrival of the British, represented the most influential ethnic groups in the fertile area around Nairobi. The British colonizers reacted to early attacks of the Mau Mau by rounding up almost everyone from the Kikuyu, Embu, and Meru ethnic groups they could find, subject them to harsh and violent interrogations, and establish a dense system of holding and punishment camps where suspects were further interrogated, tortured, and "re-educated." This system of detention camps lasted until 1959, shortly before Kenyan independence (Elkins, 2005, Anderson, 2005). Until today, survivors of "The Emergency" fight for reparations and acknowledgement from the British government.

Our study investigates how people that underwent the interrogations and torture in the British camp system fared across the rest of their lifetimes. With this, we provide helpful evidence on how to overcome periods of political violence, as it occurred manifold in former colonies worldwide and is still occurring in countries like Afghanistan, China, Russia, or Venezuela, and how to help people and societies to revert back to normal lives afterwards. To investigate the effect of the British camp system on people that were incarcerated, we geocoded camps to the exact places according to official British information from colonial newspapers, digitized and geocoded Kenyan census data from 1979 and 1989, and leveraged additional data on voting intentions, trust and preferences, and individual and household characteristics from current surveys.

We estimate Triple-Difference-in-Differences regressions to identify people that were likely incarcerated in the British detention camps if they show the combination of three characteristics: (*i*) they live close to the former site of a detention camp, (*ii*) they belong to one of the three tribes targeted by the British colonizers, and (*iii*) they were born before the end of the detention system. We find, among other things, that affected individuals are more likely to vote based on candidates' ethnicity instead of performance, and that they are less trusting in their neighbors as well as in people in general. This is the case even though the Kikuyu, who formed the majority of the Mau Mau rebellion, were in political power for most of Kenyan post-independence history. As potential channels, we find that affected people are less educated and less wealthy until today, both characteristics that correlate highly with lower trust and ethnic voting (Knack and Keefer, 1997). The loss of trust causes ethnic tensions in Kenya until today, which just recently in 2007 ignited large-scale electoral violence. Our results hence suggest that periods of political violence require careful and intensive social work as well as financial assistance afterwards to restore trust in people and institutions to allow a peaceful co-habitation.

Chapter 6 finally revisits the main chapters of this thesis for final summaries and draws a conclusion. This final chapter gathers the main findings of this thesis and states the main take-away for policy makers from the results presented below: To promote economic recovery from political turmoil, it is not enough to tackle the immediate and visible scars left by war and destruction. Almost as important is tackling the general equilibrium changes that occurred due to war and civil conflict, and to seal the social scars ripped open by political violence.

Chapter 2

Subnational Conflict Exposure and the Heterogeneous Recovery from Civil Violence

2.1 Introduction

Economies that are prone to civil conflict are also more likely to experience high levels of poverty (Fearon and Laitin, 2003, Collier and Hoeffler, 2004), unstable institutions (Besley and Persson, 2010), disrupted trade flows (Bayer and Rupert, 2004), and out-migration (Salehyan, 2014).[1] But how long do the consequences of conflict plague a society? The evidence on this question is mixed. In macroeconomic theory, the Solow model postulates that economies quickly converge back to their balanced growth paths after conflict if the structural parameters remain unaffected. Empirically, results based on (sub)national entities as the unit of observation back up this claim, while evidence from individual-level survey data points to persistent negative effects on conflict victims, bystanders, and their relatives. This chapter argues that using a proper measure for local conflict exposure is key to solving this puzzle. The convention to aggregate distinct battle locations to higher spatial resolutions by counting the number of conflict events inside (sub)national borders ignores the spatial variation of battle locations *inside* these borders. To address this spatial variation in the aggregation process, we must account for where conflict locations lie with respect to an area's economic activity. This further emphasizes an important heterogeneity that is otherwise lost in aggregation: violent clashes that occupy an area's economic center are more damaging than battles taking place in the periphery.

Figure 2.1 illustrates the key idea of this chapter. The figure shows the distribution of visible lights at night in Ukraine and Yemen from the year 2000. Red crosses indicate all conflict locations registered in either country between 2010 and 2018.[2] While the Ukrainian conflict mainly took place in the Eastern Donbas region, in Yemen almost all light-emitting locations are subject to conflict. This difference in intra-regional conflict exposure translates into heterogeneous effects of conflict on economic activity. Indeed, between 2013 and 2018, both countries experienced a similar number of battle events per year. But whereas Ukraine recovered rapidly after conflict onset, Yemen has suffered under negative growth rates ever since its civil war broke out.[3] This relationship is not restricted to the national level. Figure 2-A2 shows a similar comparison for two districts in Uganda. While the town of Jinja recorded only one violent event between

[1] This chapter is based on Korn (2023)

[2] Conflict events in Ukraine refer to the Russian-backed insurgency in the Donbas region that erupted in 2014, not the events of the Russian invasion in 2022.

[3] See Appendix, Figure 2-A1, for conflict and growth trends in the two countries.

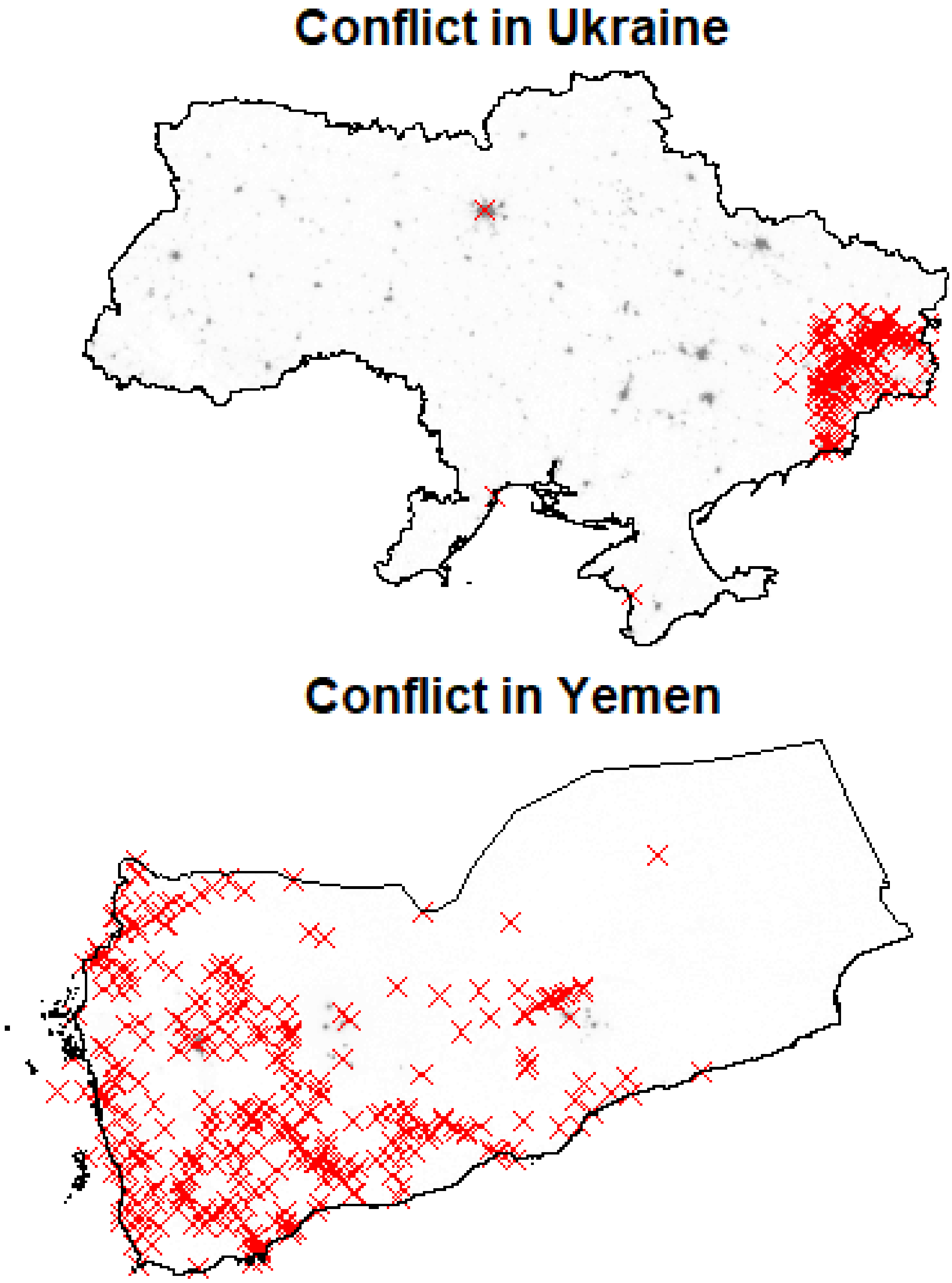
1995 and 2010, the spatially vast district of Chua recorded 72 events during the same period. The one conflict event in Jinja however occurred close to the town's economic center, whereas in Chua several populated places are located outside the battle zone. Again, even though both locations followed parallel trends in economic activity up to 2002 when Jinja recorded its one battle event, Jinja shifted to a lower path for several years afterwards.

This chapter tests whether this relationship holds for a sample of 21,259 subnational areas at the second administrative layer (ADM2, or district level) across 70 countries for the 1992–2013 period.[4] I propose a new way to measure conflict exposure with aggregated data, which takes account of the spatial variation of conflict events inside a district. Using either the pre-sample population distribution or visible lights at night as proxies for economic activity (Henderson et al., 2012, 2017, Bluhm and Krause, 2022), I compute the share of each district's economic activity that is in close proximity to a conflict event in any given year. I then investigate whether differences in settlement patterns lead to heterogeneity in conflict exposure. Theoretically, an area whose economic activity concentrates in one location faces a higher risk that a large proportion of its population is affected by violent clashes than an area whose settlements are geographically dispersed. I provide evidence that areas with a higher spatial concentration of economic activity, measured by the spatial Gini of nightlights following Achten and Lessman (2020), indeed exhibit higher values of conflict exposure for a given number of conflict events. Several other geographic determinants, e.g., mountainous terrain or gemstone deposits, affect conflict exposure only marginally and often insignificantly.

To test for a causal effect of conflict exposure on medium-run economic development, I estimate 2SLS regressions in a Bartik-like IV (or shift-share) framework for a subsample of oil-producing countries following Autor et al. (2013), Nunn and Qian (2014), and Dreher et al. (2021). This 2SLS design relies on the interaction between a time-invariant local average (or "share"), which proxies a location's likelihood to be affected by some external shock, and an exogenous time-series (or "shifter"), which each year takes the same value for all locations. As a local share, I use a location's average economic concentration, and weight it by the average number of conflict events in that location. Robustness checks following Christian and Barrett (2017), Adão et al. (2019), and Goldsmith-Pinkham et al. (2020) support the validity of this share. As the global shifter, I use the international price of crude oil. For countries with access to on-shore oil production, oil revenues constitute one of rebel groups' most important income sources, as well as the main motivation to fight (Andersen et al., 2022). At the same time, no single country in my sample, let alone one of their districts, is important enough an exporter to affect the global oil price. I find that exposure to conflict significantly decreases a location's overall economic activity, both in the same year and when lagging conflict exposure for up to twelve years. This

[4] The availability of the DMSP nightlight dataset limits the yearly range of the panel dataset.

Figure 2.1 – Two Similar but Different Civil Conflicts



Notes: This Figure plots the distribution of nightlights and conflict events in Ukraine and Yemen, respectively. Nightlight intensity is indicated by an inverted grey-scale and based on average visible lights during the year 2000. The locations of all conflict events in both countries since 2010 are indicated by red crosses.

effect stems from the new measure for conflict exposure, as robustness checks rule out the identification method and sample selection as alternative explanations.

What is more, I find evidence that conflict exposure displaces economic activity to neighboring districts. OLS results that allow for spatial spillovers by interacting conflict exposure with a spatial weighting matrix demonstrate that high conflict exposure in close-by locations *increases* a location's economic activity. This effect likely accrues due to people and capital moving away from conflict-locations towards less exposed areas. Finally, a panel analysis at the country level demonstrates that the subnational results based on nightlights as a proxy for economic activity also hold at the national level looking at GDP per capita as the outcome variable. The country-level analysis further allows testing potential diversion channels. My results suggest that high conflict exposure reduces Foreign Direct Investments (FDIs) in the medium run and causes higher rates of external instead of internal migration. In addition, the fact that the negative results aggregate to the country level suggests that internal migration does not fully cushion the shock of local conflict episodes. Instead, migration frictions lead to productivity losses if people and businesses move to alternative, peaceful locations within their country's borders.

These findings shed new light on the persistent effects of conflict exposure. Recent studies using micro-level survey data show that the effects of civil conflict can last decades (see Verwimp et al., 2019, for an excellent review). Studies found persistent effects on capital ownership (Justino and Verwimp, 2013, Mercier et al., 2020), inequality (Bircan et al., 2017), education (Brück et al., 2019), displacement (Verwimp and Muñoz-Mora, 2018, Verwimp et al., 2020), trust (Tur-Prats and Valencia Caicedo, 2020), and health (Bundervoet et al., 2009, Akresh et al., 2011, 2012). Results at the aggregated level, e.g., towns, districts, or countries, oppose this view (Blattman and Miguel, 2010, Blattman, 2012). In line with macroeconomic models, which assume that short term shocks do not affect the economic growth rate in the long run (Barro and Sala–i–Martin, 1992, Mankiw et al., 1992), they find that locations rapidly overcome the destruction from war (Davis and Weinstein, 2002, Brakman et al., 2004). Some findings go one step further, arguing that in the very long run, conflict may even have positive effects for economic growth, at least in Europe (Dincecco and Onorato, 2016, Dincecco et al., 2019).[5] Even when analyzing the same conflict, studies disagree on the persistence of conflict's detrimental effects (Miguel and Roland, 2011, Singhal, 2019). By accounting for the variation in conflict locations inside geographic areas, my results support the evidence from individual-level data that conflict has lasting negative effects.

To bridge the gap between individual-level data and aggregated empirical questions, this chapter suggests a simple but coherent way to measure conflict exposure. Researchers often use binary variables indicating the presence of civil conflict in some geographic area (Braithwaite et al., 2016, Berman et al., 2017), or count variables based on the

[5] According to Dincecco and Onorato (2016), regular conflict episodes induced urbanization and hence economic growth in Europe as towns fortified and people gathered behind city walls to escape violent raids.

number of conflict events or battle-related deaths inside that area's boundaries (Bellows and Miguel, 2009, Chamarbagwala and Morán, 2011, Dube and Vargas, 2013). Such measures disregard where exactly civil conflict takes place inside an area, and how many people are affected by it. Weighting conflict events by the economic activity close to them generates a more accurate image. This way of aggregating local shocks to higher geographic entities further has relevant implications for measuring other types of shocks, e.g., floods (Kocornik-Mina et al., 2020).

Finally, my findings contribute to the growing literature on locational fundamentals and how they affect long-run economic growth. Recent findings suggest that much of the allocation of today's economic activity is predetermined by fixed geographic fundamentals (Henderson et al., 2012, Alix-Garcia and Sellars, 2020). Still, sudden shocks may, under some circumstances, lead to diverging paths of development (Allen and Donaldson, 2020). The finding that the degree of geographic concentration of economic activity affects conflict vulnerability sheds new light on this discussion. As this geographic concentration is largely pre-determined by locational fundamentals (Achten and Lessman, 2020), it constitutes an important heterogeneity that might explain why civil conflict changes the path of development for some locations, but not for others.

The rest of this chapter is structured as follows: Section 2.2 outlines the theoretical considerations and main hypotheses. Section 2.3 introduces a new way to measure conflict exposure and presents OLS results that investigate its heterogeneity towards the spatial concentration of the economy. Section 2.4 then presents the results from OLS and IV regressions that point to a causal medium-run effect of conflict exposure on economic activity. Section 2.5 tests these findings for robustness and Section 2.6 investigates various extensions to the main estimations, before Section 2.7 concludes.

2.2 Theory

The literature disagrees whether civil conflict harms economies in the long run. While various papers provide evidence of long-lasting effects on individuals affected by civil conflict, this persistent effect seems not to pass through to the aggregated level. Similarly, a number of theoretical contributions support the idea that economies recover rapidly from short-term shocks like civil conflict. This disagreement, I argue, accrues for two reasons. On the empirical side, we need to re-think how to measure conflict exposure at the geographically aggregated level, and whether geographic characteristics provoke a heterogeneous response to civil conflict. Regarding theory, we need to reconsider whether the assumption holds that short-run shocks like civil conflict do not affect locational fundamentals.

Most of the conflict literature assumes that short run economic disruptions due to violence have no lasting effect on the economy (Blattman and Miguel, 2010). Even though economies move away from their structural growth path in the short run (Barro and Sala–i–Martin, 1992, Mankiw et al., 1992), they rapidly converge back to pre-conflict levels afterwards (Blattman, 2012). Hence, civil conflicts should have no effect on economic activity in the long run as long as conflicts do not affect locations' structural determinants of growth (Davis and Weinstein, 2002, Gupta et al., 2004, Gates et al., 2012). A nascent literature investigating these structural determinants argues that locational fundamentals are pre-determined by nature (Henderson et al., 2017, Alix-Garcia and Sellars, 2020). Still, relative agglomeration forces across locations play a significant role for the distribution of economic activity (Redding, 2022). This is, locations attract people and capital away from other places dependent on their *current* economic standing relative to other close locations. Hence, a civil conflict can persistently affect a location's agglomeration forces and hence lessen its attractiveness compared to other locations (Allen and Donaldson, 2020). Evidence abounds that people escape conflict by migrating to peaceful locations (Czaika and Kis-Katos, 2009). This can shift economic activity towards close and similar, but peaceful regions (Poot, 1995, Lewer and Van den Berg, 2008), hence reducing a location's capacity to recover from conflict. It therefore matters to what extent a location is exposed to a civil conflict to evaluate its chances of economic recovery. If, for example, all settlements in a sub-national region directly experience violence, people likely leave this region entirely and therefore shift economic activity persistently out of the region. If, on the other hand, only some part of that region is affected by conflict, economic activity can shift *within* that region. In this case, economic centers that are not subject to violence can cushion the economic downturn and preserve a region's overall production capacity. It is therefore essential to empirically account for heterogeneous conflict exposure.

Recent findings based on disaggregated individual-level data, often collected in surveys among the affected population, provide evidence for a persistent effect of conflict on various outcomes (Verwimp et al., 2019). Among other things, civil conflict has been shown to cause endured poverty (Bundervoet et al., 2009, Akresh et al., 2011, 2012, Justino and Verwimp, 2013, Mercier et al., 2020), mental health problems (Derluyn et al., 2004), deteriorating trust (Tur-Prats and Valencia Caicedo, 2020), and a lack of education (Brück et al., 2019). Furthermore, conflict-related crises like food insecurity, forced displacements and reductions in production and trade leave permanent imprints on the economy (Verwimp and Muñoz-Mora, 2018, Verwimp et al., 2020). These findings are however contrasted by studies that use geographic aggregates, e.g., towns, districts or countries, as the unit of observation. Empirical evidence from Japan (Davis and Weinstein, 2002), Vietnam (Miguel and Roland, 2011), or Germany (Brakman et al., 2004) emphasize that, despite large destruction and economic interruptions, affected places recover rapidly. Why do the persistent individual effects not translate to the aggregated level?

I argue that one essential reason for this disagreement is that the common ways to aggregate conflict data to higher geographic units neglect an important heterogeneity in how locations cope with civil conflict. Geographic conflict data usually come in the form of point data - i.e., information on perpetrators and victims associated with a specific location in space defined by geographic coordinates. To aggregate this information to a higher geographic level like districts or countries, researchers overlay these point data with (sub)national borders and then count the number of conflict events or fatalities that fall inside each spatial entity. While this concept follows the main idea of measuring conflict exposure by indicating how active a civil conflict was in a given area, it does not yet take full advantage of the spatial component of conflict data. All conflict events receive the same weight. It makes no difference whether a conflict takes place in a highly populated and economically active location, or in the unpopulated hinterland. In both cases, a spatial entity would receive the same coding of conflict exposure. Therefore, these unweighted spatial aggregates ignore how many people were directly exposed to fighting.

Weighting conflict events by the population they affect leads to the question whether differences in settlement patterns induce heterogeneity in conflict exposure. Take the extreme case of a district where the economic activity is highly concentrated to one city and the hinterlands are sparsely populated. Such an area is more likely to see a large share of its economic activity affected by violence as (*i*) civil conflict tends to cluster in space (Buhaug and Gleditsch, 2008, Aas Rustad et al., 2011, Schutte and Weidmann, 2011), preferentially in places where the potential loot is high (Berman et al., 2017), and (*ii*) the (peaceful) rest of the area shows only low economic activity. Alternatively, one may also want to assume that more densely populated areas are better fortified and therefore make less attractive targets for insurgent groups with limited fighting capacity. Either way, geographically concentrated areas may respond differently to civil conflicts than less concentrated ones. Note that this chapter does not equate spatial concentration of economic activity with income inequality (see Alesina et al., 2016, for a discussion of this issue). Instead, I focus on the mechanical relationship between settlement patterns and conflict locations to develop a way to aggregate spatial conflict data that takes account of this heterogeneity. This way of aggregation helps to connect findings from spatial conflict analysis to evidence from individual-level survey data, which themselves suffer from severe restrictions. First, despite recent efforts to encourage and facilitate survey data collection in conflict zones (Brück et al., 2013), such survey data remain as of yet scarce. Second, surveys induce an inherited survivor bias to the analysis. Enumerators can only interview people who were neither killed during the conflict nor fled to safer locations. Inference then relies on the assumption that those people available for interviews are not structurally different from people who cannot be interviewed anymore in a conflict location. We hence require a measure of conflict exposure that combines the availability of conflict event data with the accuracy of individual-level survey data.

2.3 Measuring Local Conflict Exposure

I use subnational data for 70 economies during the 1992–2013[6] period to develop a new measure for local conflict exposure based on the UCDP Georeferenced Event Dataset (GED) Version 19.1 provided by Sundberg and Melander (2013). Countries enter the sample if the UCDP GED dataset documents violent clashes that occur predominantly between organized rebel groups and state governments, or between two or more organized non-governmental groups during the sample period.[7] The benchmark unit of observation is the second administrative area (ADM2 level), which is similar to districts in the United States.

To aggregate the conflict event data to administrative areas, I calculate the share of an area's population or economic activity close to a conflict event. My preferred conflict exposure measure uses the pre-sample population from 1990 to relate conflict locations to local settlement patterns. As an alternative, I use the lagged amount of light emitted at night. Using pre-sample or lagged measures ensures that an ongoing conflict does not affect the underlying population or light distribution. I prefer the 1990 population over the lagged nightlights as my main measure for conflict exposure. While the latter allow for a more detailed and contemporary picture of subnational settlement patterns, they might themselves be affected by civil conflict and hence lead to endogeneity in my main analysis.[8] I use data on local population counts in 1990 from the Gridded Population of the World (GPW) dataset. GPW aggregates information from local housing censuses to grid cells of 2.5 arc-minutes size, i.e., about 5km at the equator. The National Oceanic and Atmospheric Administration (NOAA) provides yearly data on nightlight emissions. Nightlights were introduced by Henderson et al. (2011, 2012) as a consistent proxy for economic activity in places where subnational economic accounts are not available. For information on conflict locations, I use the UCDP Georeferenced Event Dataset (GED), version 19.1, provided by Sundberg and Melander (2013).[9] Conflict events included in the UCDP dataset constitute "an incident where armed force was used by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date." An organized actor can be a national government or an (in)formally organized group of non-governmental actors. I restrict

[6] My sample stops in 2013 due to the availability of consistent nightlights data. New satellites introduced after 2013 make the younger, high-resolution data uncomparable to the earlier nightlights data.

[7] For example, Afghanistan and Iraq are excluded because for most of the sample period, violence occurred between organized groups and international actors. Such conflicts differ in how they affect the local population due to the nature of targets (i.e., military bases or personnel of international actors). Further, e.g., Somalia is excluded because accurate information on conflict locations is hardly available.

[8] All results are robust to using nightlights instead of population data.

[9] UCDP conflict data can be downloaded free of charge at https://ucdp.uu.se.

the sample to conflict events for which the exact geographic coordinates are available (these are around 41% of the overall dataset).[10] While UCDP conflict events constitute the benchmark for my regression analysis, I provide comparisons and robustness checks for African countries using conflict data provided by the Armed Conflict Location & Event Dataset (ACLED) as introduced by Raleigh et al. (2010).[11] I restrict the ACLED data to conflict events with the highest precision, i.e., events that can be associated with a particular town (64.3% of observations). The benchmark unit of observation for my analysis constitutes the second administrative (ADM2) area based on GADM version 3.6. With either UCDP or ACLED, I define conflict exposure in ADM2 area i and year t as a weighted sum of the area's grid cells $j = 1, ..., n$ as follows:

$$ConflictExposure_{i,t} = \frac{\sum_{j=1}^{n} Pop_{ij,t} \times \mathbb{1}(Conflict_{ij,t})}{\sum_{j=1}^{n} Pop_{ij,t}} \tag{1}$$

Figure 2.2 illustrates the construction of three different conflict exposure measures for the Abidjan district in Ivory Coast. The UCDP dataset registered eleven conflict locations in Abidjan in 2011. Whereas most of these conflict locations lie close to the population center, some events occurred rather outside in the periphery. My conflict exposure measure will give relatively little weight to the remote events, but high weight to events close to the center. Panel (a) illustrates the computation of the population-based exposure measure. Here, I identify all grid cells as "treated" if they touch a buffer around a conflict event. As a baseline, I use buffers of 3km radius, while all results presented below are robust to smaller and larger buffer sizes. I then sum the population over all "treated" cells and divide this number by the total population recorded for Abidjan in 1990.[13]

This yields a conflict exposure of 33.7%. Panels (b) and (c) demonstrate two possibilities to construct the measure based on nightlights. To compute conflict exposure for the year 2011, I use nightlights recorded in 2010. For the buffer-method illustrated in Panel (b), I combine all conflict-buffers to a closed area and compute the share of nightlights emitted in this buffer-area vs. the total light emitted in Abidjan in 2010. This yields an exposure measure of 35.5% that is slightly higher than the population-based measure. Finally, Panel (c) illustrates the gridded lights approach. Here, I first aggregate nightlights to $3km \times 3km$ grid cells and then record which grid cells also hosted at least one conflict event. Again taking the share of light in "treated" cells vs. the overall light, we receive an exposure measure of 58.8%.

Note three things here. First, the nightlight-based measure on average produces higher

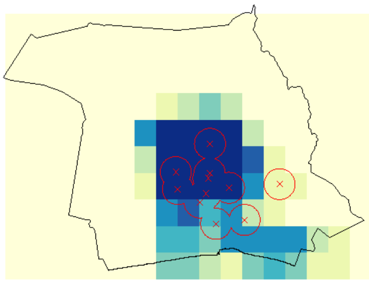
[10]My results are robust to including events of lower accuracy (available upon request).

[11]ACLED started by collecting data on conflicts in Africa and the Middle East, so data availability is best for these regions. The data are freely available online at https://acleddata.com.

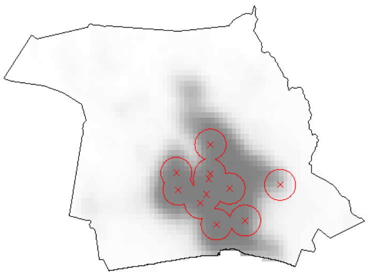
[13]Figures 2-A4 and 2-A5 in the Appendix provide two additional illustrations of how the conflict exposure share was calculated.

Figure 2.2 – Construction of Conflict Exposure Measure

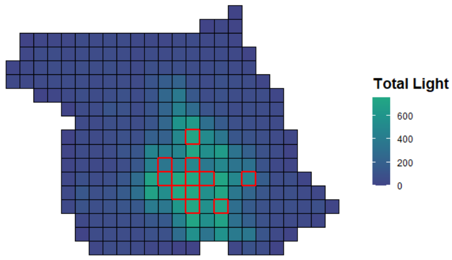
(a) Population 1990, 3km Buffers (Benchmark)



(b) Light 2010, 3km Buffers



(c) Light 2010, $3 \times 3km$ Grid Cells



Notes: This Figure demonstrates the construction of the conflict exposure measure based on the Abidjan district, Ivory Coast, in 2011. Figure (a) demonstrates the construction of the population-based exposure measure, plotting conflict events (indicated by red crosses) with 3km Buffers over the population distribution in 1990 based on data from GPW. Figure (b) uses the same buffer sizes, but plots conflict over nightlights (in inverse grey-scale). Buffers were combined to represent the total area close to a conflict event. Figure (c) plots these data aggregated to $3 \times 3km$ grid cells, where the color scheme indicates the total DN[12] *of nightlights. Red boxes indicate grid cells that include at least one conflict event. Conflict exposure estimated in these three panels is 33.7%, 35.5%, and 58.8%, respectively.*

values than the population-based measure. This likely reflects agglomeration effects that occurred since 1990 and are hence not detectable in the stable settlement patterns we can identify in the GPW dataset. Second, the gridded light-approach here produces larger exposure shares. This goes back on the (random) allocation of grid cells. In the case here, we have a number of central, high-population cells which are almost all “treated,” while small, bright pixels that end-up slightly outside a buffer in Panel (b) are blurred away by the aggregation to grid cells. This is however no general observation – the gridded light-approach might produce higher or lower exposure shares than the buffer light-approach, depending on how the grid in a given location is arranged. Finally, my measure assigns the same weight to treated cells irrespective of whether they experience one or multiple conflict events during a given year. The exposure measures hence constitute conservative, lower-bound measures of conflict exposure which would bias estimates towards zero assuming that multiple conflict events in one region mean a higher treatment intensity. I abstain from additionally weighting cells by the number of conflict events in order to bound the exposure measure at a maximum of one, and to focus on the extensive margin of identifying insecure conflict locations. That being said, I assign to each district a grid that is stable over time such that within-district comparisons are not susceptible to grid differences. The three exposure measures are highly correlated with each other, and different buffer sizes lead to qualitatively similar measures (see Table 2.1 for a comparison).[14]

Table 2.1 – Correlation across Exposure Measures

	Exp	Exp_Light	Exp_Grid	Exp_5km	Exp_1km
Exposure	1.00	0.90	0.68	0.92	0.86
Exposure_Light	0.90	1.00	0.70	0.88	0.79
Exposure_Grid	0.68	0.70	1.00	0.68	0.54
Exposure_5km	0.92	0.88	0.68	1.00	0.79
Exposure_1km	0.86	0.79	0.54	0.79	1.00

Notes: This table displays the correlations among different conflict exposure measures. The first three measures ("*Exposure*, *Exposure_Light*, and *Exposure_Grid*") refer to the three main measures using 3km buffers (Columns 1 & 2) and grid cells (Column 3). Columns (1) and (3) use population, Column (2) uses nightlights to identify settlement patterns. Columns (4) and (5) repeat the population-buffer measure from Column (1), but instead use 5km and 1km buffers, respectively.

Using the ACLED conflict data allows to compare the exposure measure over different types of civil unrest. I follow ACLED's categorization and construct separate exposure measures for (*i*) battles between rebels and government forces, (*ii*) remote violence via explosives, (*iii*) violence against civilians, (*iv*) riots, and (*v*) protests. Figure 2-A6 shows that on average,"low-cost" types of unrest like protests and riots exhibit a higher conflict exposure. This likely stems from the fact that protests and riots are more likely to occur in cities rather than in the periphery, and that it is easier to bring protesters to the streets across towns than projecting violent force across larger parts of an area. Additionally, violence against civilians is slightly more likely to spread across populated places inside an area than are battles between rebels and government troops. This observation is in line with, e.g., Kalyvas (2006), who argues that it is costly for armed forces to spread their reach geographically away from their center of control.

Centralization and Conflict Exposure. Differences in economic geography can induce heterogeneity in how districts cope with civil conflicts. I expect a location's degree of economic concentration to be an additional determinant of how conflict exposure affects

[14]See also Figure 2-A3 in the Appendix, which illustrates the relationship between the "common" conflict measure and my exposure measure. It also shows that my measure picks up conflict exposure from events that occur close to the border in neighboring districts, which would be omitted in the usual count-based measures.

economic growth. Hence, I investigate whether my conflict exposure measure takes higher values for locations with higher spatial economic concentration, holding the number of conflict events fixed. This will also provide the first stage for this chapter's subsequent analysis.

I estimate OLS regressions of the form:

$$Exposure_{ic,t} = \sum_k \beta_1^k G_{ic,t}^k + \beta_2 ln(C_{ic,t}) + \sum_k \beta_3^k G_{ic,t}^k \times ln(C_{ic,t}) + X_{ic,t}'\phi + \delta_i + \lambda_{ct} + \epsilon_{ic,t}, \quad (2)$$

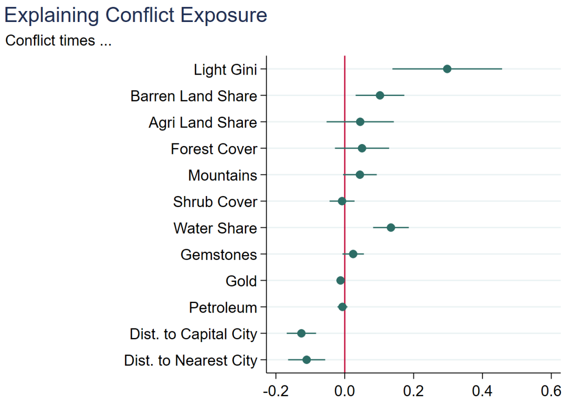
where conflict exposure of spatial entity i in country c at year t constitutes the dependent variable. The main explanatory variables are the natural logarithm of conflict events, $ln(1 + C_{ic,t})$, and k different geospatial covariates, indicated by $G_{ic,t}^k$. My main spatial variable of interest is economic concentration. For this, I follow Achten and Lessman (2020) and calculate the yearly spatial Gini coefficient of nighttime lights for each unit of observation. Just as a regular Gini coefficient, the spatial Gini coefficient takes on values between 0 and 1, where a value of 0 would indicate a perfectly equal distribution of light, and a value of 1 would indicate a perfectly concentrated distribution in only one pixel. Note that the number of conflict events is obviously endogenous to conflict exposure, such that it comes at no surprise that the correlation between conflict exposure and the number of conflict events, β_2, should be highly significant. However, the main purpose of this analysis is to determine whether the *marginal effect* of an additional conflict event varies with the degree of spatial concentration. Therefore, I am mainly interested in β_3, i.e., the coefficient from the interaction term of the number of conflict events and the Gini coefficient (or other geographic variables, respectively). A significantly positive value for β_3 would indicate that for a given number of conflict events, a location's conflict exposure increases with the value of this geographic variable. Note further that the purpose of this first-stage estimation is not to establish causality, but to test whether spatial concentration in fact significantly moderates the marginal effect of an additional conflict event on conflict exposure. All specifications control for population density, the total amount of nightlights, and climate indicators ($X_{ic,t}'$), along with location (δ_i) and country-year (λ_{ct}) fixed effects.

Figure 2.3, Panel (a), summarizes the standardized estimates for this β_3 coefficient for various geographic variables.[15] All variables are constant over time (the Gini coefficient is based on the nightlight distribution in 1992), and hence controlled for by location fixed-effects. Most coefficients are either statistically insignificant or exhibit a small gradient. Only the Gini coefficient moderates the effect of conflict events on conflict exposure signif-

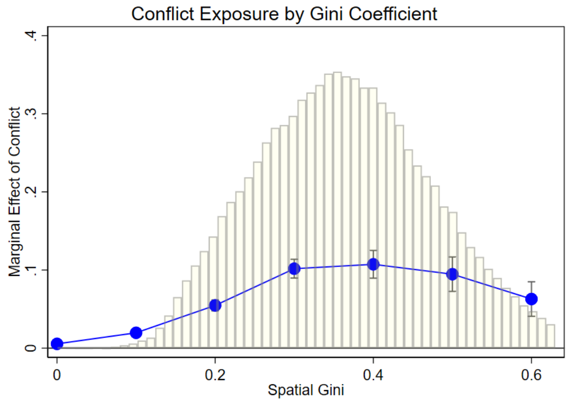
[15]Table 2-B2 in the Appendix displays detailled results for the Gini Coefficient as the explanatory variable.

Figure 2.3 – Geographic Determinants of Conflict Exposure

(a) Geographic Determinants



(b) Gini, Marginal Effects



Notes: Figure (a) plots the coefficients from regressing conflict exposure on the interaction terms of the number of conflict events and various geographical variables. All variables are centered and standardized to account for distributional differences. The regression controls for the number of conflict events as well as climate shocks, and includes country-year and district fixed effects, the latter of which account for the interaction terms' base variables. Lines depict 95% Confidence Intervals. Figure (b) plots the marginal effects of conflict events on conflict exposure conditional on the spatial Gini coefficient. The Gini coefficient is here categorized into 0.1 bins, with Gini < 0.1 *as the reference category. The histogram in the background displays the distribution of the continuous Gini variable.*

icantly, with a 1 standard deviation (SD) increase in the Gini coefficient being associated with a 0.7SD increase in conflict exposure for each additional conflict event. Panel (b) of Figure 2.3 plots the marginal effects of conflict events on conflict exposure, moderated by the Gini coefficient. Here, I repeat the main specification according to Equation 1 but using a categorical variable instead of the continuous Gini coefficient. For this, I assign observations to different bins of the Gini coefficient, each of size 0.1. The category of the least centralized locations with a Gini coefficient between 0 and 0.1 constitutes the reference category. This figure illustrates again the relevance of economic geography as a moderator for conflict exposure: A high level of economic concentration makes areas more vulnerable to conflict. This result is robust across different specifications. In the Appendix, I provide estimates from the same regression specifications, but based on nightlights (Tables 2-B3 and 2-B4), lights from the year 1992 (Table 2-B5), or for lights averaged across the whole sample period (Table 2-B6). All these alternatives deliver almost exactly the same picture. Using the contemporaneous Gini coefficient or fixing it to the year 1992 does not change the results (Table 2-B7). The results are further robust to the usage of smaller (1km) or bigger (5km) buffer sizes (see Tables 2-B8 & 2-B9), as well as for omitting the bottom ten percent and the top ten percent brightest observations from the sample (see Table 2-B10).

2.4 Conflict Exposure and Economic Activity

To investigate the medium-run effect of conflict exposure on local economic activity, I regress the natural logarithm of a location's sum of nightlights as a proxy for local economic activity on my measure for conflict exposure. I follow the general set-up of Kocornik-Mina et al. (2020), who test for a medium-run effect of floods on economic activity, and lag my explanatory variable by one additional year in each column moving to the right. All regressions include ADM2- and country-year fixed effects, and control for population density and climate indicators. What is more, I include the spatial Gini of lights and the yearly number of conflict events as additional control variables to estimate the effect of conflict exposure on economic activity *holding the number of conflict events constant.*

Various papers raise concerns of endogeneity between conflict and economic activity. For example, conflict traps impose a feedback-loop between conflict and deteriorating economic activity (Braithwaite et al., 2016), and sudden economic windfalls may cushion reasons for war (Miguel and Satyanath, 2011, Dube and Vargas, 2013, Bazzi and Blattman, 2014, Berman and Couttenier, 2015) or spur insurgency via higher expected gains (Angrist and Kugler, 2008, Nunn and Qian, 2014, Berman et al., 2017, Bluhm et al., 2021). Also international donors take civil conflict into consideration when they allocate development assistance to certain locations (Dreher and Lohmann, 2015, Bluhm et al., 2020). Therefore, I employ a 2SLS Bartik-IV (or "shift-share") strategy to test for a causal effect of conflict exposure on economic activity.[16] This 2SLS strategy was recently employed across various fields, see, e.g., Autor et al. (2013), Nunn and Qian (2014) and Dreher et al. (2021). The idea is to interact a local average that is constant over time (a so-called "share"), with an arguably exogenous time series that is the same for all locations (a so-called "shifter"). The local shares are then meant to identify how much a local entity is affected by a (global) shock.

I use the interaction of two related variables as the local share. These are (*i*) a location's average number of conflict events, and (*ii*) a location's average Gini coefficient of lights at night.[17] The decision to interact two local shares relies on the results presented in Figure 2.3 above, which show that the *interaction* of conflict and economic concentration has the highest predictive power for conflict exposure. This interacted share hence proxies for the propensity that large parts of a location's economic activity may be exposed to conflict in any given year. Note that this share, while possibly endogenous, is controlled for by ADM2 fixed effects. As a shifter, I use the one-period lagged value of the international crude oil price, an important determinant of conflict intensity (see, e.g. Andersen et al., 2022, Basedau and Lay, 2009, Dube and Vargas, 2013).

[16] Table 2-B11 in the Appendix provides OLS results for comparison.

[17] In the Appendix, I provide results using the Gini coefficent based on 1992 lights instead of the average coefficient. The results are almost identical.

Table 2.2 – 2SLS: Conflict Exposure and Economic Activity, 1992-2013

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
Exposure	-8.089**	0.303	-0.469**	-0.171	-0.442***
	(3.441)	(0.302)	(0.220)	(0.173)	(0.166)
$Exposure_{t-1}$		-8.077***	0.104	-0.600**	-0.246
		(2.626)	(0.161)	(0.264)	(0.163)
$Exposure_{t-2}$			-4.638***	0.00677	-0.762**
			(1.253)	(0.159)	(0.330)
$Exposure_{t-3}$				-4.747***	-0.116
				(1.164)	(0.180)
$Exposure_{t-4}$					-5.155***
					(1.191)
First Stage:			*Exposure*		
IV	0.00116***	0.00019***	0.000276***	0.000308***	0.000294***
	(0.000418)	(0.0000377)	(0.0000664)	(0.0000737)	(0.000116)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Obs.	190,624	180,902	171,178	161,458	151,738
$F_{1st\ stage}$	15.82	21.85	21.13	18.63	36.20
Nbr. Countries	27	27	27	27	27

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The variable of interest is the share of lights at night within 3km to a conflict event and is instrumented by a triple-interaction of *i*) the regional average of conflict events *ii*) the regional average of the Gini of lights and *iii*) the yearly world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t - s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Of course, the yearly oil price itself is not unconditionally independent of local growth trajectories. It is for example straightforward to assume that a higher oil price would increase the output of oil-producing districts.[18] However, I argue that the interaction term between both local shares and the global oil price is conditionally independent of local

[18] This however would bias my results towards zero.

economic activity as long as one controls for the two constituting two-way interactions of the oil price with either of the local shares.

I therefore estimate 2SLS regressions of the following form:

$$Exp_{ic,t} = \beta_1 \overline{G_{ic}} \times \overline{C_{ic}} \times \$Oil_{t-1} + \gamma_1 \overline{G_{ic}} \times \$Oil_{t-1} + \gamma_2 \overline{C_{ic}} \times \$Oil_{t-1} + \delta X'_{ic,t} + \pi_i + \lambda_{ct} + \epsilon_{ict} \quad (3)$$

$$ln(light)_{ict} = \beta_2 \widehat{Exp_{ict-s}} + \gamma_1 \overline{G_{ic}} \times \$Oil_{t-s-1} + \gamma_2 \overline{C_{ic}} \times \$Oil_{t-s-1} + \delta X'_{ict} + \pi_i + \lambda_{ct} + \mu_{ict} \quad (4)$$

where $Exp_{ic,t}$ denotes the exposure to conflict of ADM2 area i in country c in year t. The subscript $(t-s)$ in the second stage indicates the various lags across specifications. $\overline{G_{ic}}$ is the local average of the Gini coefficient, and $\overline{C_{ic}}$ denotes the average number of conflict events in area i. The international price for crude oil, $\$Oil_{t-1}$, is the yearly average of international oil prices in the prior year and is constant across locations. In countries with abundant oil resources, oil constitutes both a motivation and financing resource for rebel groups (Ross, 2008, Basedau and Lay, 2009, Dube and Vargas, 2013, Koubi et al., 2014). This is especially the case for countries with on-shore oil production, whereas the effect might run in the opposite direction in countries with mainly off-shore oil production, where governments exert the main control over the oil deposits and hence enjoy a financial advantage over insurgents (Andersen et al., 2022). For this instrument to have enough power to predict national conflict trends, I therefore subset my sample to countries with abundant on-shore oil resources that rebels could potentially attempt to capture. These are a total of 27 countries, which I illustrate in Figure 2-A7 in the Appendix.[19]

For countries that began oil production during the sample period, I exclude the pre-production years. Note that, while Bazzi and Blattman (2014) show that natural resources do not significantly predict conflict onset at the national level, I control for social disputes at the national level with country-year fixed effects. I use the lagged oil price to avoid concerns that conflict in an oil-producing location may affect the global oil-price, and to allow violent actors to observe changes in the oil price and react to it. However, as shown in Appendix Table 2-B12, my results are robust to using contemporary oil prices, whereas the first stage loses predictive power when leading the oil price or lagging it by more than one year. What is more, my results are further robust to subsetting the sample to countries in Sub-Saharan Africa, none of which has the market power to significantly affect the global oil price, let alone anyone of these countries' ADM2 districts. The control variables $X'_{ic,t}$ include local population density and a number of climate variables, all of which have been shown to be associated with civil conflict (see notes to Table 2.2).

[19]Even though this limits the external validity of my findings (albeit only marginally as there is no significant difference in the OLS estimates between oil and non-oil countries as discussed below), I prefer this subsample for my main analysis to clearly establish a causal relationship between conflict exposure and medium-run economic activity. The advantage of limiting the analysis to the oil-producing subsample is that this allows for valuable falsification tests of the IV, as discussed in the robustness section below.

The ADM2 fixed effects π_i account for time-invariant characteristics, while country-year fixed effects λ_{ct} control for common shocks at the country-year level. Note again that, since my instrumental variable consists of a triple interaction, I control for the interactions of the oil price with the average Gini coefficient and the average number of conflict events, respectively. The base levels of these variables as well as the interaction term of the average Gini coefficient and average conflict are absorbed by the location fixed effects. The results are robust to not including these baseline interactions (see Appendix, Table 2-B13). In addition, the triple-interaction has much higher explanatory power than interacting the international oil price with either of the local shares alone. Neither the number of conflict events nor the Gini coefficient alone constitute a strong instrument as shown in Tables 2-B14 and 2-B15 in the Appendix.

Table 2.2 presents the 2SLS results for up to four lags of conflict exposure, while Figure 2.4 summarizes the causal estimates for longer lag structures of up to 12 years. Across all specifications, the coefficients for the lagged conflict exposure variables are statistically as well as economically highly significant. The first-stage F-statistics and second-stage t-values for all twelve lagged regressions are large enough to interpret the estimates as significant at the 5 percent level according to Lee et al. (2022).[20] On average, a one-standard deviation increase in the share of the local population affected by conflict decreases local nightlights by around 17.3 percent in the same year.[21] Even though the point estimates slightly decrease when lagging conflict exposure by one or more years, the negative effects on economic activity remain significant. A one standard-deviation increase of conflict exposure is associated with a decrease of about 11.6 percent of nightlights still four years later. Even twelve years later, local nightlights remain 5.8 percent lower. These results are robust to computing conflict exposure based on 1km or 5km buffers (Tables 2-B16 & 2-B17), or based on nightlights instead of population (Tables 2-B18 and 2-B19). These results are hence in line with the recent micro-level findings and suggest that conflict exposure significantly harms economic activity, both in the short- and the medium run.

Causal Interpretation. The causal interpretation of estimates recovered from shift-share 2SLS regressions is subject to several caveats. As I discuss in more detail in Section 2.5 below, the identifying assumption I require states that the local share, i.e., the interaction between a location's average conflict propensity and its pre-sample economic concentration, is conditionally exogenous to the outcome variable of interest, i.e., contemporary lights at night (Goldsmith-Pinkham et al., 2020). Note that all specifications control for either level component of the local share, interacted with the global oil price.

[20] Lee et al. (2022) provide a table with minimal F- and t-values needed for 5-percent significance. For example, the F-Statistic of 36.2 in Column (5) requires a t-value of at least 2.27. Hence, the t-value of my IV estimate of 4.33 allows to interpret the coefficient as significant at least at the five percent level.

[21] According to $(e^{\beta \times \sigma_x} - 1) \times 100\%$, for a standard deviation of conflict exposure of $\sigma_x = 0.024$.

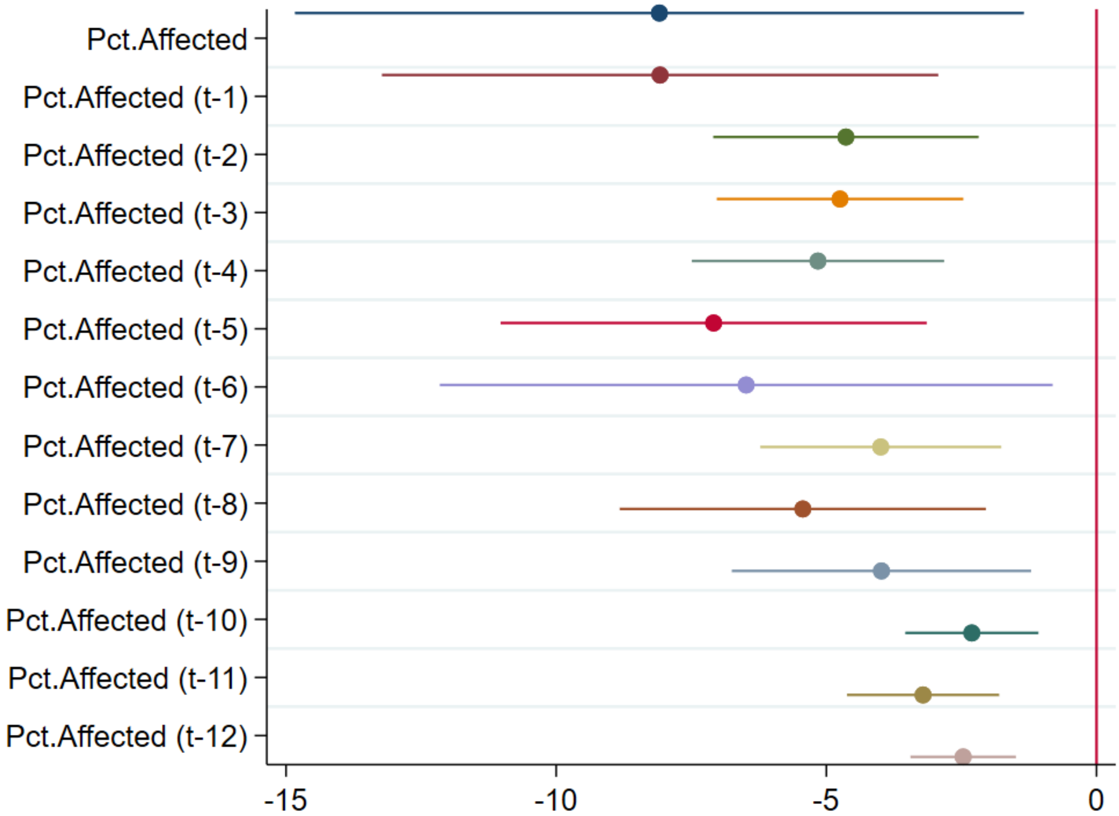
Hence, all specifications acknowledge the endogeneity of locations' conflict propensity and economic concentration by directly controlling for it. Below, I provide a number of tests that suggest that the conditional independence assumption for the interaction term between economic concentration and conflict propensity is plausible.

Note further the difference in interpretation to Staggered Difference-in-Differences designs as discussed, among others, in Callaway and Sant'Anna (2021). Staggered Difference-in-Differences or event study designs compare entities that receive a treatment at some time period and *stay treated* for the rest of the sample to other entities that are not (yet) treated. Causal inference then requires the standard parallel trends assumption as well as avoiding negative weighting of observations by, e.g., comparing newly treated to already treated entities. Shift-share 2SLS estimations also require the parallel trends assumption to hold, following the idea of a two-period Difference-in-Differences setting (Christian and Barrett, 2017). In the case of this chapter, this assumption implies that locations with high values of the interaction term between conflict propensity and economic concentration follow parallel trends in nightlight emissions as locations with lower values of the interaction term in years without a change in the oil price, conditional on the levels of conflict propensity and economic concentration. I test these assumptions for plausibility in Section 2.5 below. However, differently to Staggered Difference-in-Differences designs, observations cannot receive negative weights because the weights are explicitly assigned by the shifter in the specification, here the global oil price. Hence, the identifying variation stems from estimating the slope of my interaction between conflict propensity and economic concentration on lights at night, while observations in years with higher oil prices receive higher weights (Borusyak et al., 2022).

The interpretation therefore comes close to an Average Causal Response of the outcome on the treatment intensity (i.e., the value of the interacted shares) as recently discussed for Continuous Difference-in-Differences settings in Callaway et al. (2021). Finally, one last concern could be that oil price increases have persistent effects on conflict likelihood, hence inducing a bias from prior periods in the first stage estimation. However, I find the global oil price to have a rather short-run effect on conflict occurrence. As shown in Table 2-B12, lagging and leading the oil price by several years demonstrates that the oil price only has significant predictive power when lagging it one year or using contemporaneous oil prices. Leads or longer lags of the oil price do not significantly correlate with conflict exposure.

There remains the question whether the evidence in favor of the micro-level studies indeed accrues due to the new measure introduced here, or whether the findings are driven by my identification approach. To investigate this possibility, I repeat the 2SLS regressions, but use the number of conflict events as the endogenous variable. The results in Table 2.3 indeed match the macro-level evidence. Conflict has strong detrimental effects in the short run, but the effect turns around for later years. While insignificant

Figure 2.4 – 2SLS Regression Outcomes, Further Lags



Notes: This Figure plots the 2SLS coefficients from regressing nights at light on conflict exposure. The regression follows the specifications in Table 2.2. Each plotted coefficient is derived from a separate 2SLS regression including all smaller lags as controls. The number of observations ranges from 190,624 in specification 1 to 74,969 in specification 13.

for the second lag (although here a weak instrument problem is an issue), regions that were exposed to conflict three or four years before show higher economic activity today. Hence, the main results in Table 2.2 accrue due to the new way to measure conflict exposure. Therefore, it is essential to take intra-regional variation in conflict locations into account.[22]

2.5 Instrument Validity & Robustness

Shift-share instruments are subject to several caveats. Two recent discussions from Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022) focus on the canonical sector-level shift-share approach and argue that either the conditional exogeneity of the

[22] The results in Table 2.3 focus on countries in Sub-Saharan Africa due to weak IV issues. In Appendix Table 2-B20, I provide results for the whole sample. Whereas the results are similar, the F-Statistics for the whole sample are not large enough to rule out a weak IV problem. Note further that the main 2SLS results reported in Table 2.2 are robust to subsetting the sample to Sub-Saharan Africa (see Appendix, Table 2-B21).

Table 2.3 – 2SLS with Conflict Events as Instrumented Variable, 1992-2013

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
$ln(conflict)$	-0.517***	0.299**	-0.431	0.0608	0.0214
	(0.149)	(0.116)	(0.994)	(0.0374)	(0.0259)
$ln(conflict)_{t-1}$		-1.086***	-2.662	-0.0594*	0.0676
		(0.401)	(6.169)	(0.0306)	(0.0438)
$ln(conflict)_{t-2}$			11.16	-0.316**	-0.0415
			(26.01)	(0.136)	(0.0285)
$ln(conflict)_{t-3}$				1.496**	-0.326**
				(0.659)	(0.153)
$ln(conflict)_{t-4}$					1.581**
					(0.755)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Obs	27,210	25,772	24,331	22,896	21,461
$F_{1st\ stage}$	34.04	13.87	0.187	9.963	7.699
Nbr. Countries	12	12	12	12	12

Notes: This table reproduces the main results from Table 2.2 with the number of conflict events as the instrumented variable of interest. Only African countries are used due to a weak IV problem with the worldwide sample – results for the complete sample are reported in the Appendix for comparability. The instrument is constructed by interacting a time-invariant indicator variable for positive conflict propensity with the international crude oil price. The controls include the Gini of light, population, precipitation, temperature, precipitation squared, and temperature squared. Standard errors clustered at the ADM2 level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

local shares, or a large enough number of exogenous temporal shocks is a sufficient condition for Bartik-IV estimates to be unbiased. Goldsmith-Pinkham et al. (2020) suggest to focus on one of those assumptions, because the chances that both are fulfilled is very low. Therefore, I will follow their assumption of conditionally exogenous shares, especially because the alternative assumption from Borusyak et al. (2022) is unlikely to hold in my setting with a rather short sample period of only 20 years. Another relevant critique with specific focus on the non-industry setting comes from Christian and Barrett (2017). Their point is that a spurious correlation between the exogenous time-series and the endogenous variable can falsely create significant results, especially if the parallel-trends assumption fails. Similar to a Difference-in-Differences analysis, a valid shift-share estimation requires that the potential outcomes of locations with zero or very low propensity of being "treated" follow a trend parallel to the potential outcomes of locations with a high treatment propensity. Finally, Adão et al. (2019) raise caution for inference in the shift-share framework. They show that locations with similar shares likely underlie similar unobserved shocks, independent of their geographic proximity to each other. Hence, standard errors should be clustered in a way that accounts for correlated residuals across similar locations.

Table 2.4 – 2SLS: Oil vs. Non-Oil Countries, 1992-2013

	First Stage					
	Contemporaneous			*Lagged*		
Sample:	All	Oil	Non-Oil	All	Oil	Non-Oil
	(1)	(2)	(3)	(4)	(5)	(6)
	Exposure	Exposure	Exposure	Exposure	Exposure	Exposure
IV	0.00531	0.0166***	0.00404	-0.00303	0.0197***	-0.00503
	(0.00537)	(0.00418)	(0.00512)	(0.00409)	(0.00423)	(0.00401)
	Second Stage					
	Contemporaneous			*Lagged*		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
Exp	-3.342	-8.089**	0.458	-14.32	-5.155***	92.49
	(4.030)	(3.441)	(5.302)	(16.43)	(1.191)	(1138.8)
Sample	All	Oil	Non-Oil	All	Oil	Non-Oil
FE	✓	✓	✓	✓	✓	✓
Obs	370,943	190,624	178,408	297,311	151,738	142,614
F_{1st}	0.978	15.82	0.622	0.537	36.20	0.00689
Nbr. Countries	70	27	43	70	27	43

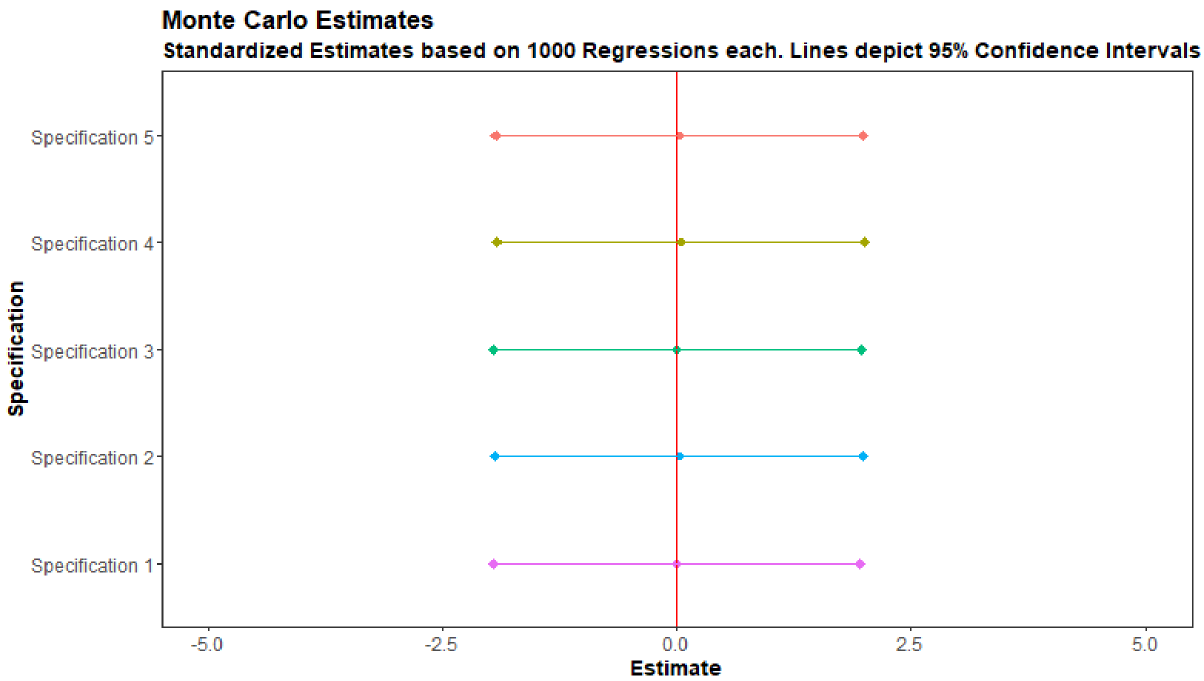
Notes: This table repeats the 2SLS regressions presented in Table 2.2, but for different samples. Columns (1)-(3) reproduce the results from 2.2, column (1), using contemporaneous conflict exposure, Columns (4)-(6) reproduce the results from 2.2, column (5), lagging conflict exposure 4 years. Columns (2) and (5) mirror the original estimates using the same oil-country sample as Table 2.2. Columns (1) and (4) use the full sample of African countries, columns (3) and (6) include non-oil producing countries only. The unit of observations is the second administrative area (ADM2) for countries with active on-shore oil production. The variable of interest is the share of lights at night within 3km to a conflict event and is instrumented by a triple-interaction of *i*) the regional average of conflict events *ii*) the regional average of the Gini of lights and *iii*) the yearly world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the variable of interest.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Christian and Barrett (2017) and Goldsmith-Pinkham et al. (2020) suggest to run a placebo-test to check whether the time series generates weak instruments in constellations where we would not expect it to affect the endogenous variable. As my 2SLS sample focuses on oil-producing countries for identification, I repeat my main regressions for non-oil producing countries. As the international oil prices should only affect the capacity and motivation to fight for rebels in oil-producing countries, we would expect no effect in this alternative sample. Table 2.4 presents results from the same regressions as in Table 2.2, Columns (1) and (5), but based on samples of either (*i*) all countries, (*ii*) only oil-producing countries as for my main 2SLS regressions, or (*iii*) only non-oil producing countries. Indeed, the IV only works if I restrict the sample to oil-producing countries. The Kleibergen-Paap F-Statistic is exceptionally low for the complete sample as well as the non-oil sample. It is hence unlikely that a spurious correlation between the oil price and local conflict waves drives my results. Another important assumption in Goldsmith-Pinkham et al. (2020) and Christian and Barrett (2017) is that locations with higher odds of conflict and locations with lower odds of conflict should follow parallel trends in their potential outcomes. In essence, shift-share IVs rely on a Difference-in-Differences estimation in the first stage. Instead of comparing the two groups across two periods, my shift-share instrument assumes that the treatment effect shows up in years when the oil price is high and vice-versa. This is, the identifying variation must come from short-term fluctuations in "treated" locations compared to "control" locations, while there should be no difference in the long-term trends between the two groups. In Figure 2-A9, I compare the trends of never treated and sometimes treated, as well as the trends of the non-zero propensity locations below median to those above median conflict propensity.[23] These locations follow an astonishingly similar trend. I interpret this as support that the parallel trends assumption holds.

Next, Goldsmith-Pinkham et al. (2020) argue that a central assumption is the exogeneity of local shares conditional on covariates. In my case, this means that the *interaction* of the average Gini and the average number of conflict events must be uncorrelated with the compound error term when controlling for both the average Gini and the average number of conflict events along with the other control variables. As a formal check for the plausibility of this assumption, the authors suggest to regress the shares on a number of potential confounders. Figure 2-A8 in the Appendix reports the estimates from regressing the local shares on various potential confounders, conditional on the control variables used in the 2SLS main regressions. This demonstrates that a number of variables that may co-determine local economic activity and conflict exposure, e.g., drug cultivation or petrol production, are not significantly related to the local shares. While some geographic variables like mountainous area or the share of agricultural land are significantly related to the local shares, it is unlikely that these temporally constant variables are correlated with the error term in my 2SLS panel regressions once ADM2 fixed effects are accounted for.

[23]Never treated, or zero-propensity, locations would be those ADM2 locations for which no conflict event was recorded over the whole sample period.

Figure 2.5 – Monte Carlo Estimates



Notes: This Figures plots the confidence intervals based on 1,000 estimations following the main specifications from Table 2.2. Specification 1 follows Column 1 and regresses lights on contemporary conflict, while Specification 5 follows Column 5 and lags conflict exposure 4 years. For each estimation, the endogenous variable was re-allocated among all observations with non-zero conflict exposure in a given year. All estimates are standardized to ease display.

As a final check, Christian and Barrett (2017) recommend randomly re-allocating the non-zero values of the endogenous variable within each year, and then re-estimating the 2SLS regressions. If the identification indeed relies on a non-spurious correlation with the time-series, randomly re-allocated values of the endogenous variable should on average produce results around zero. I produce 1,000 randomly re-arranged conflict exposure variables and re-estimate the main regressions presented in Table 2.2. The confidence intervals for the second-stage estimates based on any of the five specifications are all centered around zero (see Figure 2.5). This further encourages a causal interpretation of my main results.[24] To test for the concern of correlated residuals according to Adão et al. (2019), I repeat my main regressions with standard errors clustered at the (i) conflict-propensity level, (ii) average Gini coefficient-level, or (iii) with two-way clustered errors in both aforementioned dimensions. The results depicted in Figure 2-A11 show that my results are unaffected by different levels of clustering.

My 2SLS results are further robust to various alternative specifications. The results remain similar when varying the lag of the global oil price within a short period of time

[24]For further inspection, the time series of the oil price and the global number of conflict events is plotted in the Appendix, Figure 2-A10.

(Table 2-B12), controlling for province (ADM1)-year fixed effects (Table 2-B22), holding the sample constant over the lag structures (Table 2-B23), or using the Gini-coefficient based on lights in 1992 instead of the mean Gini for the local shares (Table 2-B24). Moreover, restricting my sample to conflict events with at least five casualties and hence more accurate media coverage (see Weidmann, 2015, for a discussion) leaves the results mostly unaffected (see Table 2-B25).

2.6 Extensions

I conduct a couple of extensions to the main findings. Below, I present alternative analyses at different spatial resolutions, test for potential channels as well as spatial spillover effects, and relate the conflict exposure measure developed in this chapter to findings based on alternative measures in the prior literature.

Country level analysis. First, to extrapolate my main findings to a higher spatial aggregation and to explore potential mechanisms, I construct the conflict exposure measure at the country level using 5km, 10km, and 15km buffers. At the country level, more moderating variables are available, and I can extend the sample period to the years 1989–2019, as I am no more constrained by the availability of nightlights data. Table 2.5 provides the results from regressing various dependent variables on (lagged) conflict exposure. Note that all regressions control for the (lagged) number of conflict events, so the conflict exposure coefficients provide the effect of conflict exposure *conditional on* the number of conflict events inside a country. Note further that I abstain from using nightlights as an outcome variable as they were only validated as good proxies at the local level and are therefore barely used at the country level. Column (1) constitutes a robustness check to the main regressions discussed above. Here, I regress the natural logarithm of GDP per capita on lagged conflict exposure. Across all four specifications, the results suggest a negative relationship between GDP per capita and conflict exposure. This relationship is statistically significant for the first, second, and third lag, hinting at a medium-run, persistent effect of conflict exposure on economic activity. The next three columns test for potential mechanisms. Two reasons for persistent effects of conflict exposure can be the diversion of investments and/or people away from the affected location. In Columns (2) and (3), I test for these potential channels by regressing the ratio of Foreign Direct Investments (FDI) to GDP and the share of the externally vs. internally displaced people on conflict exposure. Column (2) suggests a slightly delayed but persistent negative effect of conflict exposure on FDI. While all specifications report negative coefficients, these coefficients are only statistically significant for the second, third, and fourth lags. These results suggest that international investors react (slowly) to high conflict exposure and divert their investments away from the conflict country. These findings are further in line with the results in Chapter 3 of this thesis, which finds that civil conflict has a persistent effect on international economic cooperation.

Table 2.5 – Country-Level OLS Regressions, 1989-2019

	(1) ln(GDP p.c.)	(2) FDI/GDP	(3) Ext.Displaced	(4) ln(US Aid)
Exposure	-0.0650	-1.375	0.184**	0.290
	(0.0650)	(1.982)	(0.0799)	(0.503)
$Exposure_{t-1}$	-0.111**	-1.805	0.256***	0.248
	(0.0498)	(1.891)	(0.0951)	(0.469)
$Exposure_{t-2}$	-0.125**	-2.368**	-0.0295	0.421
	(0.0561)	(1.080)	(0.0910)	(0.277)
$Exposure_{t-3}$	-0.0972**	-2.430*	0.0988*	0.185
	(0.0469)	(1.249)	(0.0572)	(0.248)
$Exposure_{t-4}$	-0.0297	-2.428*	0.0970	0.248
	(0.0581)	(1.294)	(0.0737)	(0.269)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
N	4,335	4,329	2,360	3,500
R^2_{adj}	0.981	0.217	0.588	0.839
Nbr. Countries	178	178	160	154

Notes: This table displays OLS results with conflict exposure as the main explanatory variable. The unit of observation are countries. All regressions control for log(Population), the number of conflict events, IO membership, and political rights, along with country- and year-fixed effects. Standard errors clustered at the country-level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Column (3), the dependent variable is the share of externally displaced people over the total number of externally *and* internally displaced people. I chose this dependent variable because in theory, high conflict intensity with a low spatial radius of conflict (i.e., low conflict exposure) on the one hand displaces people from their homes, but allows them to stay inside the borders of their home country. Only if conflict exposure increases, i.e., if more parts of the country are subject to conflict, then people must exit the country entirely to escape from violence. The results in Column (3) support this argument. Holding the number of conflict events constant, a higher conflict exposure is associated with a higher rate of external displacement. Contrary to the FDI-results, this effect appears immediately. Conflict exposure in the same year as well as the year before is significantly associated with a higher share of external displacement, whereas later lags do not lead to estimates that are significant at the five percent level or below.

Finally, Column (4) evaluates whether the US as a donor considers conflict exposure as a relevant variable to allocate development aid. The throughout-insignificant coefficients suggest that this is not the case. Whereas the number of conflict events during the same year is slightly related to a higher amount of development aid (not shown), the actual *exposure* to conflict by the local population seems not to be taken into account in the aid allocation decision.

Spatial Spillovers. A second extension concerns spatial spillovers. Table 2-B26 reports OLS estimates which account for two different spatial dimensions. First, Column (2) introduces a spatial lag of conflict exposure as an additional explanatory variable. This spatial lag interacts conflict exposure in close locations with a spatial decay function and allows conflict exposure in proximate locations to affect nightlight activity in the observed location, weighted by the inverse distance to the observed location. I find that the direct effect of conflict exposure on nightlights remains significantly negative, while spatial spillovers of conflict exposure have a significantly *positive* effect on local economic activity. This likely hints at displacement effects: when conflict exposure forces people and capital to leave one location, close-by locations absorb this displaced economic activity. Yet, despite economic activity spilling over to neighboring locations as people avoid civil conflict, the results in Table 2.5 above suggest that at the aggregate, countries still experience a decrease in economic output. Hence, movement frictions seem to create a loss in productivity. As people and businesses switch to neighboring locations they increase output in these new locations, but not enough to compensate the loss in their location of origin.

As a final test for spatial spillovers, I introduce lagged spatial residuals to account for clustered shocks across close locations in Column (3). Whereas the introduction of lagged residuals noticeably decreases the point estimate of conflict exposure, hinting at spatially correlated shocks, the coefficients of both the direct and lagged effects of conflict exposure remain statistically significant.

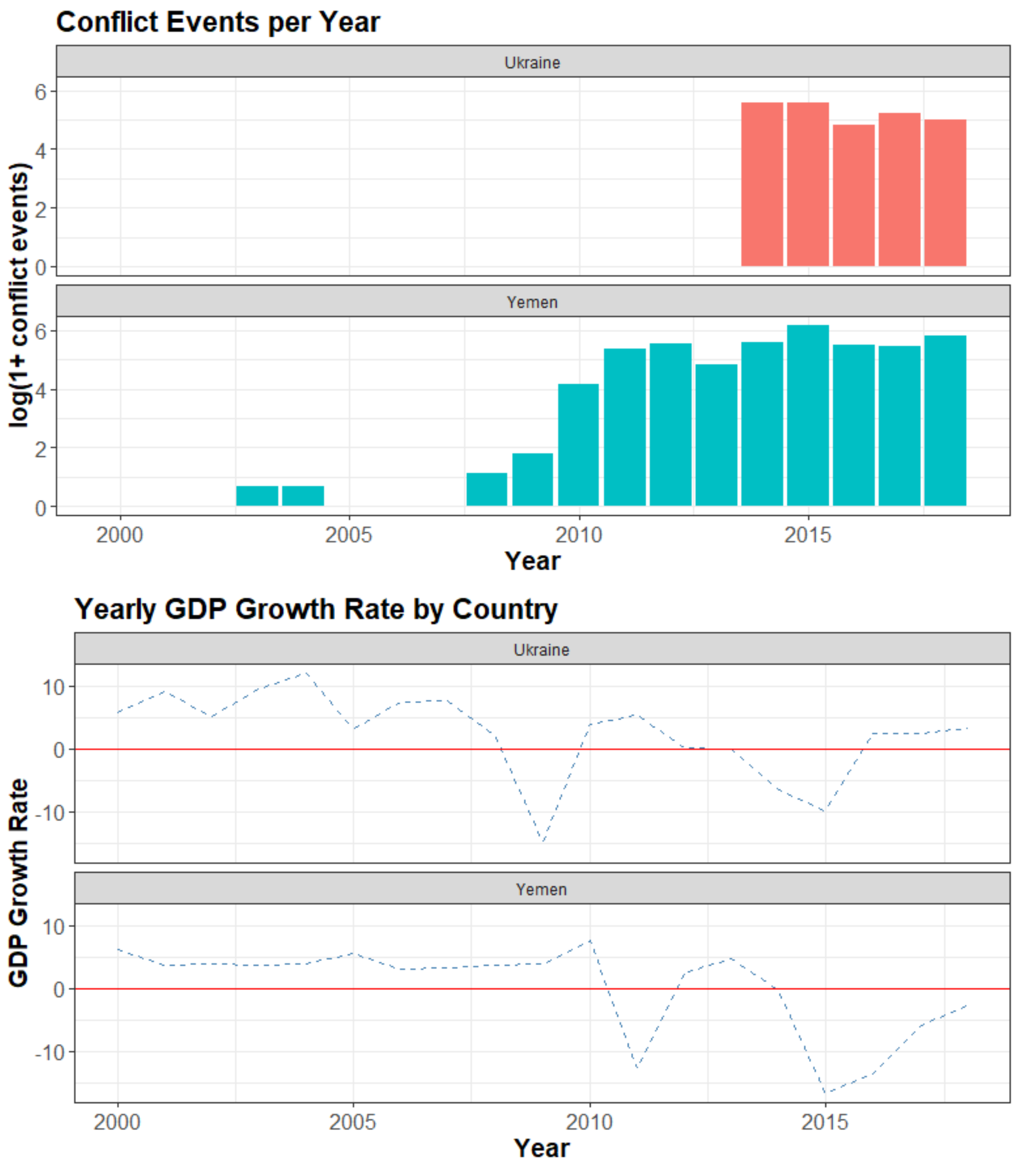
2.7 Conclusion

Civil conflict leaves a persistent imprint on (local) economies. Whereas prior evidence on this question is mixed, this chapter confirms results from mostly individual-level data by leveraging a new measure for conflict exposure that takes account of intra-regional variation in conflict locations. I find that spatially concentrated economies are more vulnerable to civil conflict, and that accounting for this heterogeneity uncovers a robust negative relationship between economic activity and conflict exposure in the medium run. There remain open questions about what role economic centralization plays for conflict vulnerability. We are still far from understanding what determines the capacity of post-conflict reconstruction, and how economic centralization plays a role in that. First and foremost, the results presented here do only test the medium run effects for up to twelve years after conflict. However, there remains the question whether the detrimental effect of conflict extends to the (very) long run. For this, it is essential to understand better how conflict exposure affects local reconstruction capabilities. More work is hence needed to discover the actual channels driving the results found here, e.g., by investigating whether migration, trade, or FDI respond differently to conflict in centralized locations than in decentralized ones. What is more, the natural question arises how regions can improve their post-war reconstruction capabilities, and how the international community can assist them in this task. Incentives to migrate or invest back in the affected locations seem a helpful approach, but need more evaluation to find out what helps and what does not.

2.8 Appendix

Appendix 2-A: Additional Figures

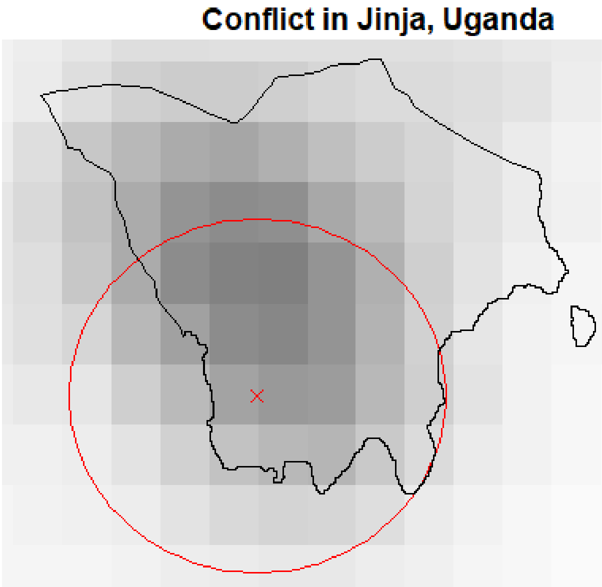
Figure 2-A1 – Conflict and Growth in Ukraine and Yemen



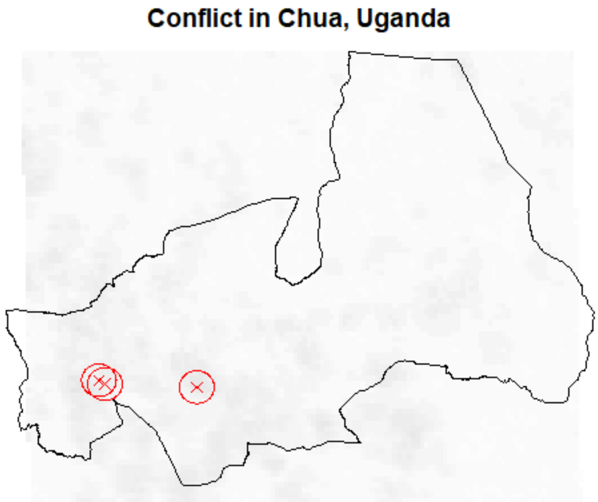
Notes: This figure compares the trajectories of the civil conflicts and economic growth in Ukraine and Yemen, respectively. The upper panel plots the logged number of conflict events that occurred in either country per year. The lower panel plots the yearly GDP growth rates for each country. The plots are based on data from UCDP and the World Bank, respectively.

Figure 2-A2 – Conflict and Growth in Uganda

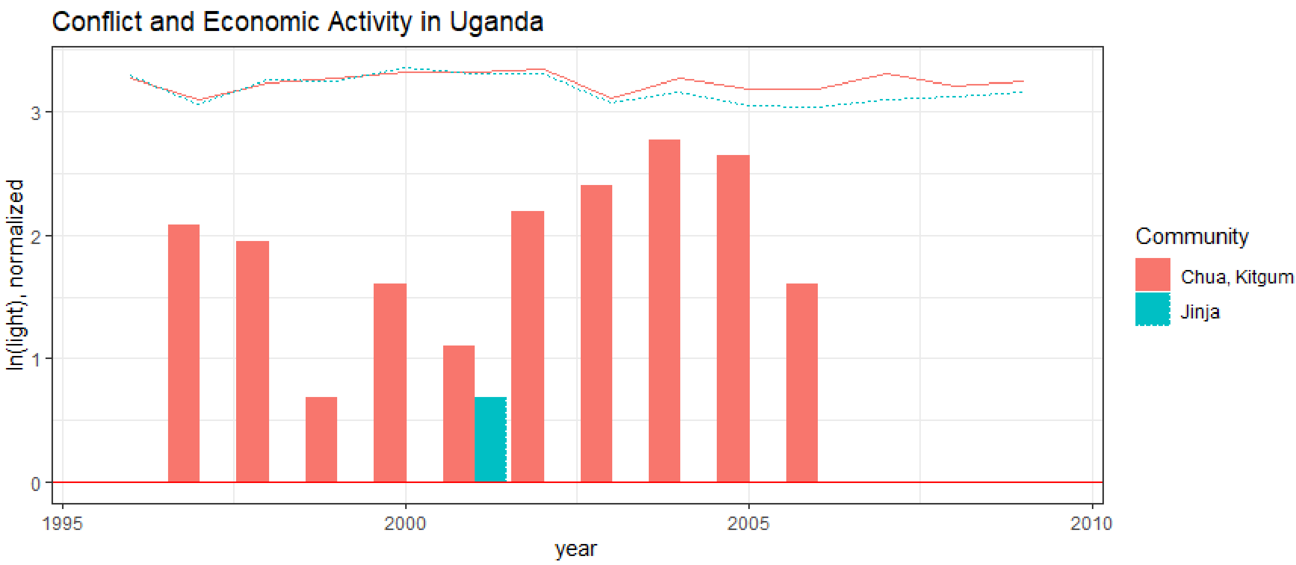
(a) Jinja



(b) Chua



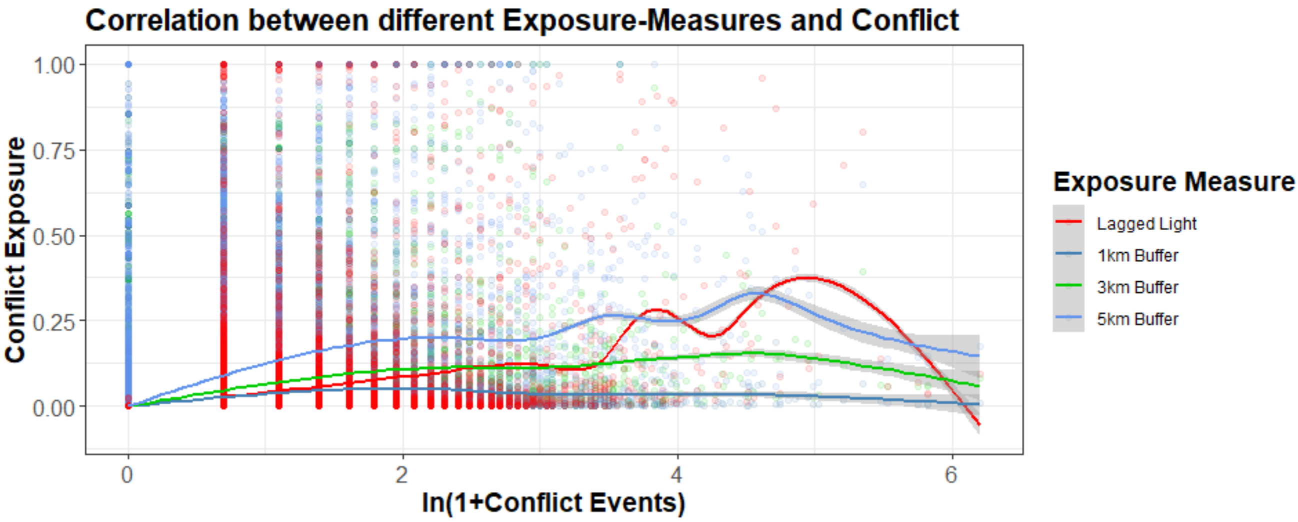
(c) Nightlights and Conflict



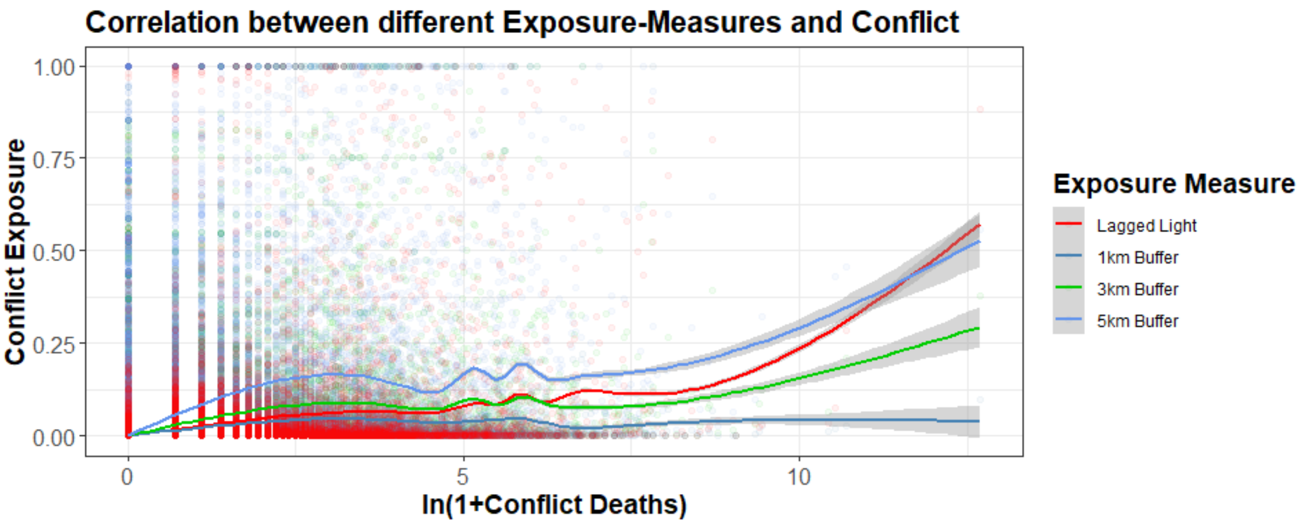
Notes: This figure provides a subnational example for the intuition of this paper. Subfigure (a) displays the distribution of nightlights (in inverted grey-scale) in the South-Ugandan district Jinja, which includes Uganda's fourth-biggest city of the same name. In Jinja, only one conflict event occurred during the sample period. This conflict event is indicated by a red cross and surrounded by a 3km-Buffer, also in red. However, this conflict event was located at a relatively central place in the rather small region and hence affected a large share (around 51%) of the region's economic activity. Subfigure (b) shows the same relationship for Chua, a rather large but remote district in Northern Uganda. Here, a total of 72 conflict events occurred during the sample period, but the conflict locations cluster in only small parts of the region. On average, less than 1% of Chua's economic activity was close to a conflict event in any given year. The chart in subfigure (c) compares the trends in economic activity (proxied by the normalized natural log of nightlights) and the natural log of civil conflict in both regions. Up to 2002, both regions followed a similar trend in economic activity. However, in the aftermath of the one conflict event in Jinja (dotted line), its total nightlight emissions drop noticeably below the output of Chua (solid line) and remains below for more than five years.

Figure 2-A3 – Exposure Measures vs. Common Conflict Measures

(a) Conflict Events



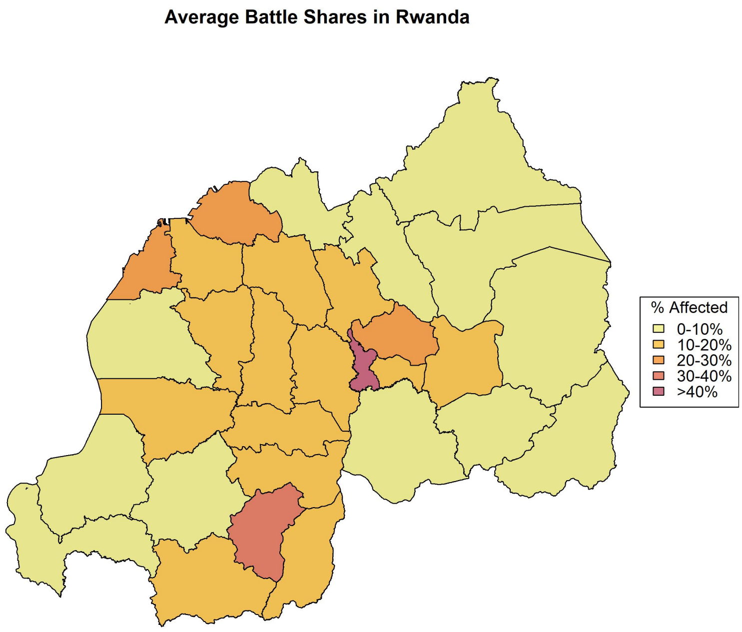
(b) Fatalities



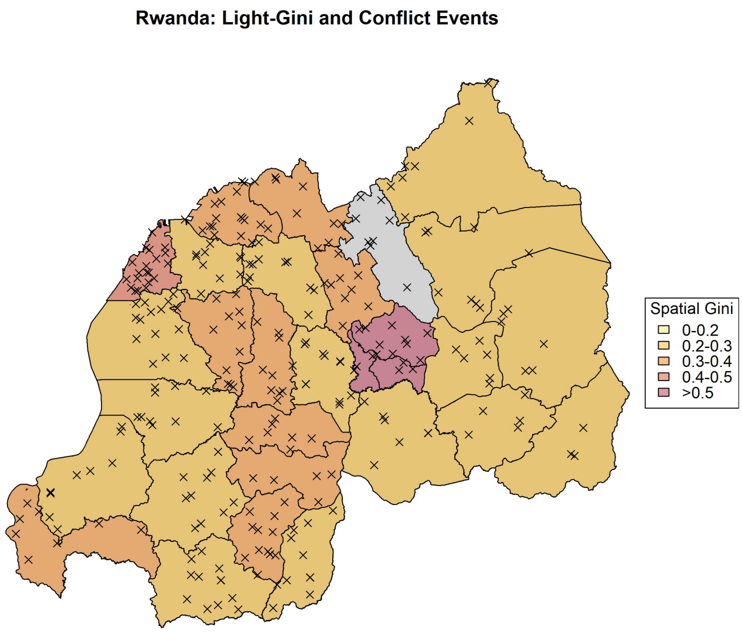
Notes: These figure illustrate the differences between the novel conflict exposure measures and common conflict measures, i.e., the (log) number of conflict events in Panel (a), and the (log) number of battle deaths in Panel (b). The lines of best fit suggest that the correlation between any of the conflict exposure measures, constructed for different buffer sizes, and the common conflict measures is low.

Figure 2-A4 – Example: Rwanda

(a) Battle Shares

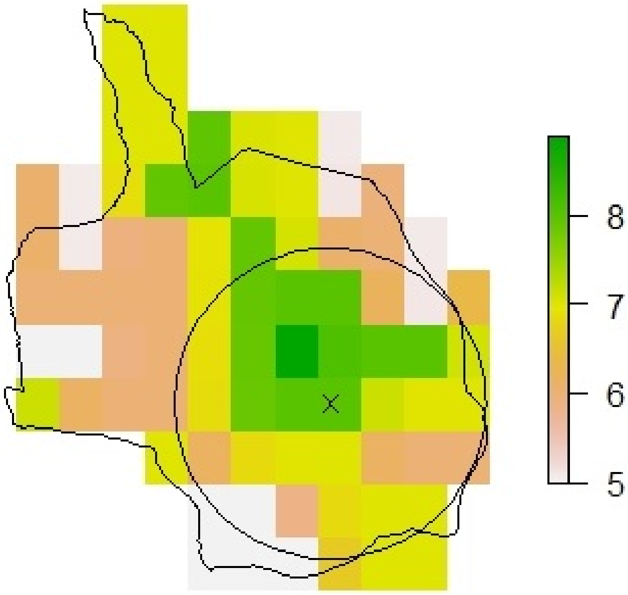


(b) Light-Gini and Conflict Events



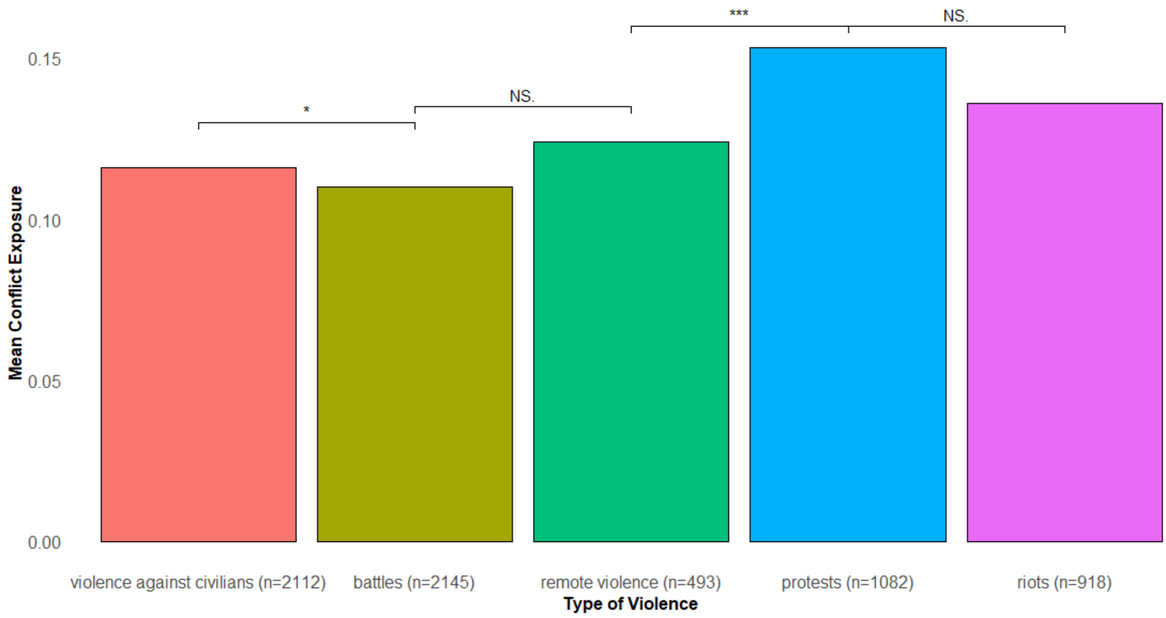
Notes: This figure illustrates the relationship between the number of conflict events, conflict exposure, and the spatial Gini at the example of Rwanda. Panel (a) indicates by darker colors higher conflict exposure in Rwandan regions, while Panel (b) plots conflict event locations of regions' spatial Gini coefficient.

Figure 2-A5 – Example: Conflict Exposure in Gulu, Uganda 2001



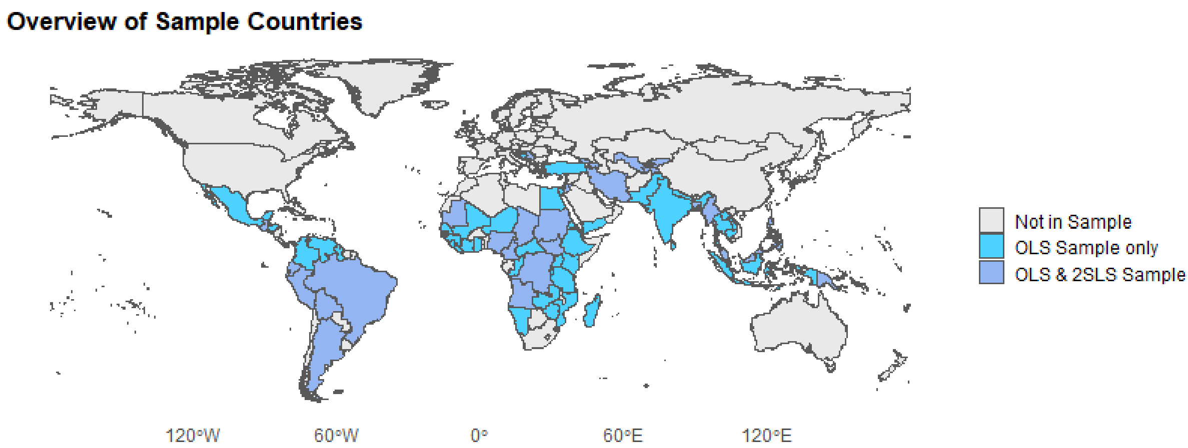
Notes: This figure provides another intuition for the construction of conflict exposure based on Gulu, Uganda in 2001. In this example, a total of six conflict events were associated with the same location inside Gulu, indicated by the cross. The buffer around these conflict events indicates which raster cells are located three kilometers or less away from these conflict events. Nightlight emissions are recorded in "Digital Numbers" (DN), where a higher DN refers to a brighter spot. In the example here, a total of 280DN fall inside the buffer around the conflict events. In Gulu region in total, nightlight emissions summed up to 444DN. Hence, the measure of conflict exposure in Gulu in 2001 amounts to $\frac{280}{444} = 63\%$.

Figure 2-A6 – Conflict Exposure Disaggregated by Type of Unrest



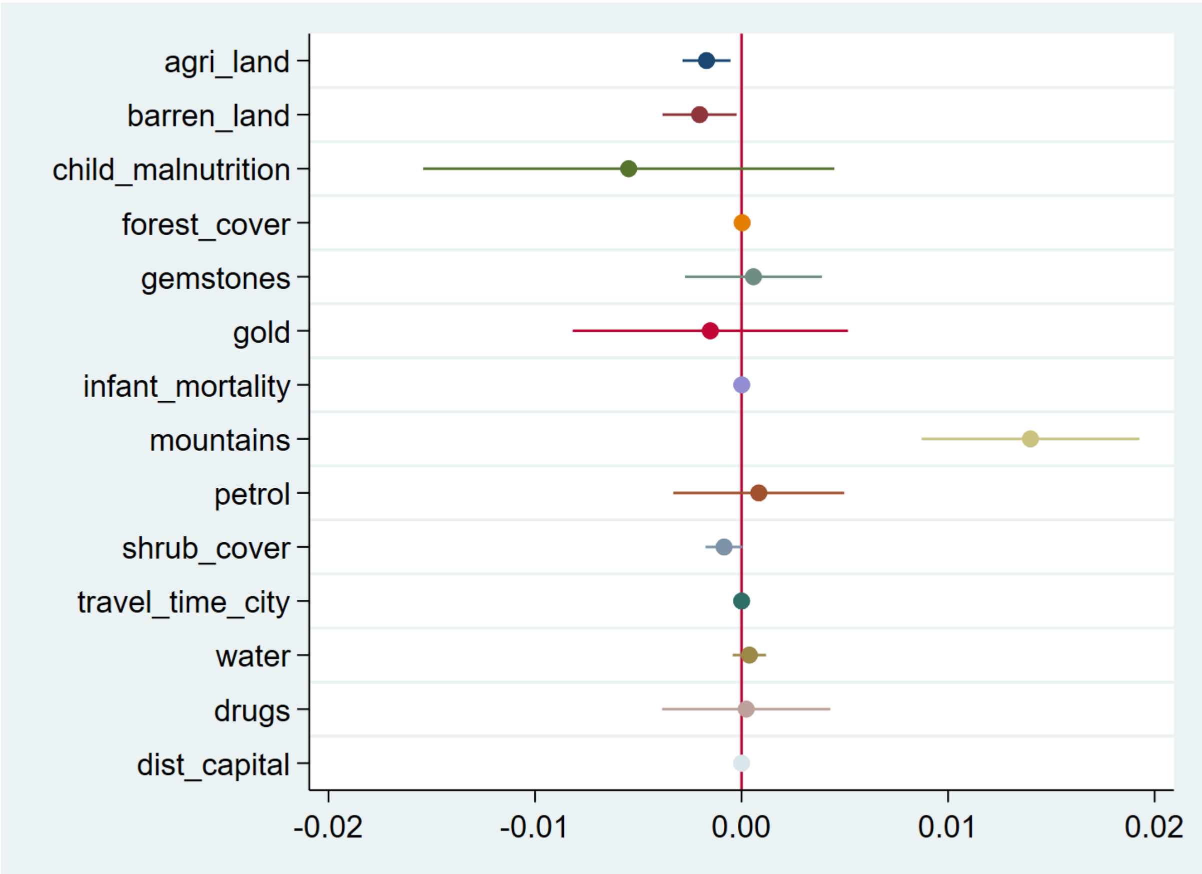
*Notes: This figure plots the average conflict exposure across types of civil unrest for all observations with positive exposure measures. All conflict exposure measures are based on ACLED event data and calculated for subnational ADM2 regions in 33 countries in Sub-Saharan Africa, 1997–2013. Bars indicate the significance of inter-group differences, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, NS. = "Not Significant."*

Figure 2-A7 – Overview of Sample Countries



Notes: This figure illustrates the sample compositions for the different specifications. Colored in dark-blue are conflict-countries with on-shore oil-resources, which are included in both the OLS and 2SLS estimates. Countries colored light-blue are part of the OLS sample, but excluded from the 2SLS sample as they did not produce on-shore oil during the sample period. Countries colored grey are not part of any sample.

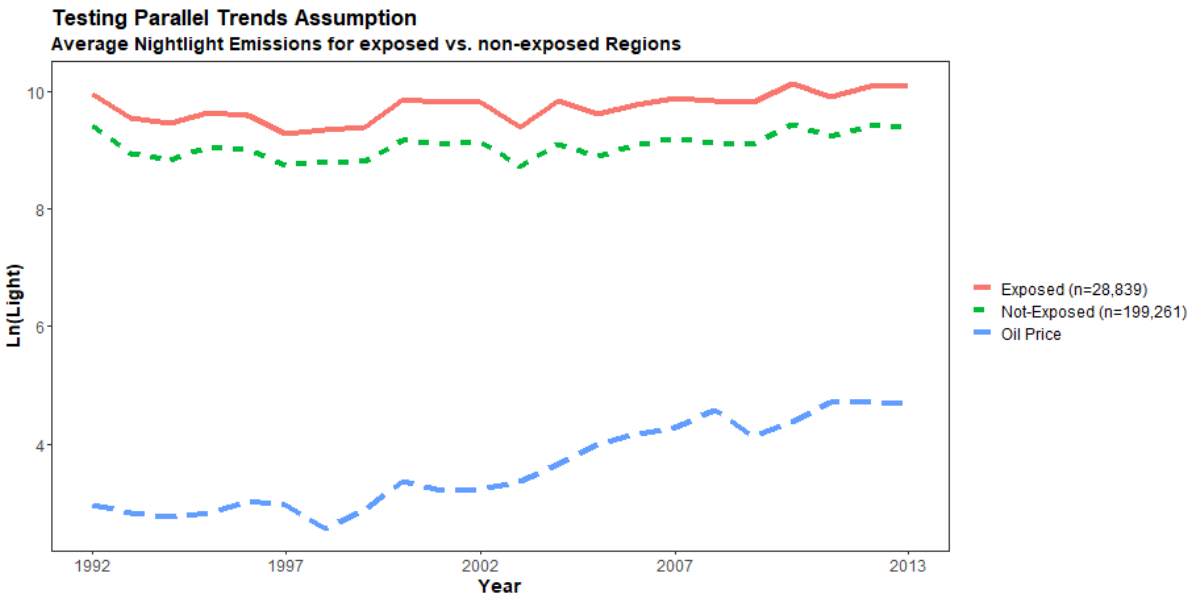
Figure 2-A8 – Correlation of Local Shares



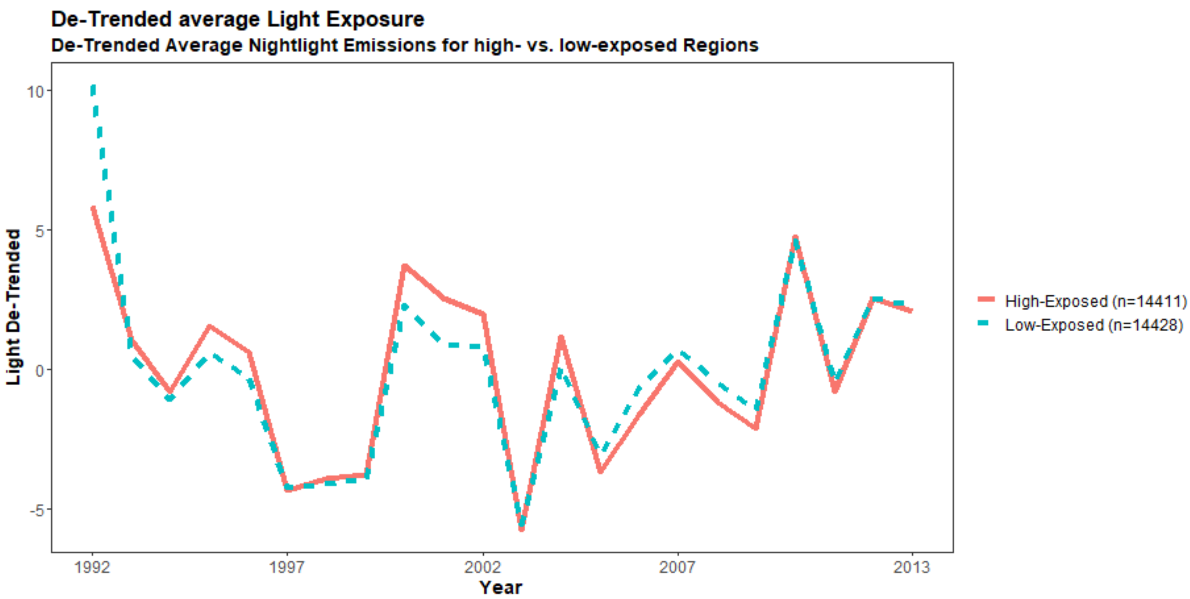
Notes: This figure reports the point estimates and the 90%-confidence intervals from regressing the local shares (i.e., the interaction of average conflict and the average Gini coefficient) on various potential confounders. All regressions control for average conflict, average Gini, average population density, average precipitation (squared), average temperature (squared), and province fixed effects.

Figure 2-A9 – Parallel Trends of Dependent Variable by Regional Share

(a) Some conflict vs. never conflict

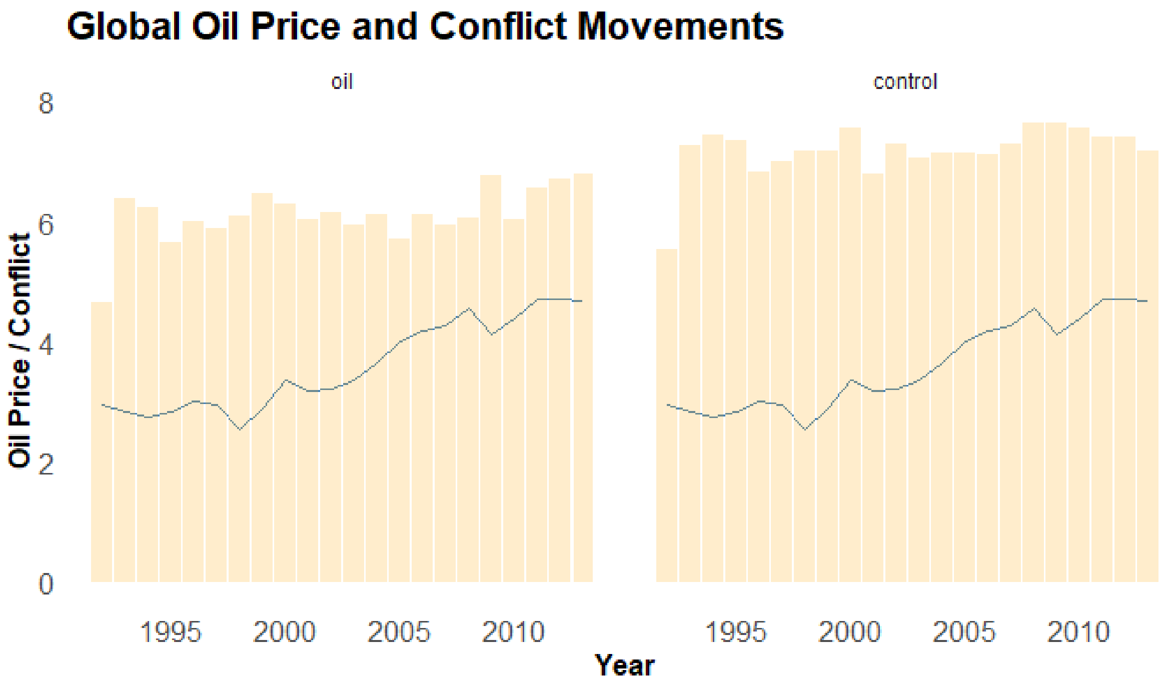


(b) Above-Median Conflict vs. Below-Median Conflict, de-trended



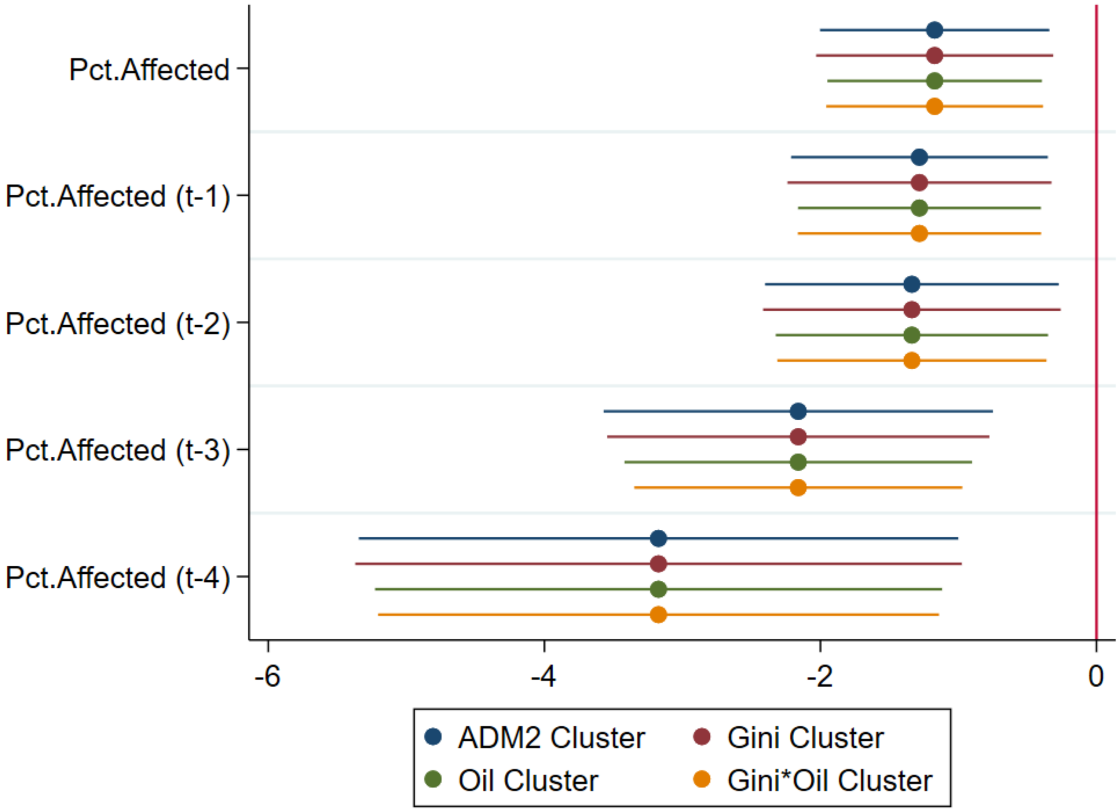
Notes: This figure plots the yearly group-level average nightlights omissions of ADM2-regions in the 2SLS sample. Panel (a) plots the yearly means of nightlights for locations with a Bartik-IV share bigger than zero (i.e., with more than one conflict event in the sample period) together with the yearly means of locations with zero share. The oil-price is included to allow for a comparison of overall trends. Panel (b) plots de-trended versions of the yearly mean-values, and further only considers locations with non-zero Bartik IV shares but splits the sample into above-median and below-median shares. Both graphs point to similar overall-trends across locations with different treatment-propensity and therefore suggest that the similar-trends assumption is likely to hold.

Figure 2-A10 – Yearly Oil Price and Conflict Events in Logs.



Notes: This figure plots the time series for the global oil price (line) and total yearly conflict events (bars), separated for oil-producing and non-oil-producing countries.

Figure 2-A11 – Different Cluster Dimensions Following Adão et al. (2019)



Notes: This figure plots the estimates and confidence intervals of the main 2SLS specifications with standard errors clustered at either the location-level, the conflict-propensity, the average Gini coefficient, or with two-way clusters at the conflict-propensity and Gini coefficient level. Lines depict 95% Confidence Intervals.

Appendix 2-B: Additional Tables

Table 2-B1 – Descriptive Statistics

Full Sample

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
exposure	459,067	0.002	0.035	0	0	0	1
ln_light	461,487	8.160	1.527	0	7.101	9.188	14.221
light_gini	426,060	0.363	0.112	0.001	0.280	0.438	0.883
conflict_events	461,487	0.091	1.790	0	0	0	493
pop_dens	457,575	445.936	2,369.469	0	20.748	187.149	76,856
precipitation	435,175	920.646	3,180.088	0	90.732	776.561	181,425
temperature	444,090	22.243	5.173	−7.102	19.578	26.287	32.310
agri_land	450,597	44.465	29.797	0	17.231	69.901	99.985
barren_land	450,597	3.783	15.472	0	0	0.068	100
forest_cover	450,597	30.371	26.038	0	7.606	48.503	99.834
shrub_cover	450,597	9.266	13.820	0	0.438	12.552	98.577
water_share	450,597	5.440	12.694	0	0.015	2.515	95.599
gemstones	450,597	0.060	0.237	0	0	0	1
gold	450,597	0.016	0.124	0	0	0	1
petrol	450,597	0.042	0.200	0	0	0	1
dist_capital	450,597	599.089	499.601	4.387	202.507	860.737	3,776
dist_city	450,597	263.903	337.510	14.829	113.478	290.778	6,585

2SLS Sample

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
exposure	232,388	0.001	0.024	0	0	0	1
ln_light	232,410	8.000	1.553	0	6.914	9.032	13.972
light_gini	214,893	0.371	0.111	0.001	0.289	0.447	0.794
conflict_events	232,410	0.044	0.741	0	0	0	114
pop_dens	230,970	292.703	1,450.118	0	15.837	152.337	61,225
precipitation	217,226	963.377	3,980.622	0	96.971	626.525	181,425
temperature	221,665	22.288	4.936	−7.102	19.743	26.083	32.201
agri_land	227,130	49.997	28.359	0	28.290	73.670	99.917
barren_land	227,130	2.847	12.811	0	0	0.052	100
forest_cover	227,130	27.065	23.699	0	8.194	40.071	99.834
shrub_cover	227,130	10.080	14.437	0	0.775	13.157	96.079
water_share	227,130	5.676	13.019	0	0.015	2.552	92.056
gemstones	227,130	0.034	0.181	0	0	0	1
gold	227,130	0.012	0.107	0	0	0	1
petrol	227,130	0.047	0.212	0	0	0	1
dist_capital	227,130	759.277	505.502	4.387	340.144	1,110.527	2,873
dist_city	227,130	250.118	322.690	14.829	104.396	270.996	5,244

Table 2-B2 – OLS: Economic Activity affected by Conflict

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	-0.00864**	-0.0171***	-0.00571	-0.00741***
	(0.00386)	(0.00353)	(0.00399)	(0.00256)
$Conflict$	0.0549***	-0.0157	-0.0151	-0.0249***
	(0.00328)	(0.0113)	(0.0107)	(0.00558)
$G_{Light} \times Conflict$		0.216***	0.214***	0.211***
		(0.0372)	(0.0332)	(0.0215)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	410,495	410,495	410,495	387,403
F	77.34	62.78	83.73	60.27
R^2_{adj}	0.158	0.184	0.399	0.403

Notes: This table shows OLS results with conflict exposure as dependent variable. The unit of observation is the second administrative area (ADM2) for the sample of 70 countries. The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "Conflict," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}." Control variables are the sum of lights, population, (squared) precipitation and (squared) temperature.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B3 – OLS: 3km Buffers Exposure

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	-0.00639	-0.0176***	-0.000692	-0.00350
	(0.00431)	(0.00397)	(0.00407)	(0.00288)
$Conflict$	0.0605***	-0.0343***	-0.0347***	-0.0465***
	(0.00337)	(0.0116)	(0.0109)	(0.00647)
$G_{Light} \times Conflict$		0.291***	0.293***	0.299***
		(0.0381)	(0.0339)	(0.0248)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	412,596	412,596	412,596	389,364
F	88.44	76.40	107.2	72.03
R^2_{adj}	0.182	0.227	0.461	0.477

Notes: This table shows OLS results with conflict exposure as dependent variable following the specifications from Table 2-B2, but using 3km buffers around each conflict event to construct the conflict exposure measure as dependent variable. The unit of observation is the second administrative area (ADM2). The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "$Conflict$," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}."
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B4 – OLS: 3km Buffers Exposure, Grid Cells

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	0.00479**	-0.00517***	-0.00454	-0.00716***
	(0.00201)	(0.00119)	(0.00304)	(0.00221)
$Conflict$	0.0475***	-0.0355***	-0.0353***	-0.0477***
	(0.00328)	(0.0109)	(0.0107)	(0.00427)
$G_{Light} \times Conflict$		0.254***	0.255***	0.264***
		(0.0335)	(0.0316)	(0.0169)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	408,565	408,565	408,565	385,507
F	55.06	73.26	97.91	55.43
R^2_{adj}	0.196	0.288	0.465	0.528

Notes: This table shows OLS results with conflict exposure as dependent variable. The unit of observation is the second administrative area (ADM2) for the sample of 70 countries. The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "Conflict," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}." Control variables are the sum of lights, population, (squared) precipitation and (squared) temperature.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B5 – OLS: 1992 Lights Exposure, Grid Cells

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	0.00500**	-0.00502***	-0.00838***	-0.0109***
	(0.00204)	(0.00119)	(0.00293)	(0.00201)
$Conflict$	0.0468***	-0.0367***	-0.0365***	-0.0496***
	(0.00328)	(0.0109)	(0.0107)	(0.00417)
$G_{Light} \times Conflict$		0.256***	0.257***	0.269***
		(0.0337)	(0.0314)	(0.0165)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	408,496	408,496	408,496	385,431
F	53.42	70.67	96.00	54.21
R^2_{adj}	0.190	0.283	0.464	0.526

Notes: This table shows OLS results with conflict exposure as dependent variable following the specifications from Table 2-B2, but using lights from the year 1992 to construct the conflict exposure measure as dependent variable. The unit of observation is the second administrative area (ADM2). The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "$Conflict$," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}."
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B6 – OLS: Average Light Exposure

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	0.00455**	-0.00552***	-0.00999***	-0.0127***
	(0.00217)	(0.00130)	(0.00341)	(0.00275)
$Conflict$	0.0484***	-0.0333***	-0.0333***	-0.0457***
	(0.00333)	(0.0111)	(0.0108)	(0.00435)
$G_{Light} \times Conflict$		0.252***	0.253***	0.263***
		(0.0343)	(0.0319)	(0.0172)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	377,384	377,384	377,384	356,232
F	55.69	72.71	95.49	54.10
R^2_{adj}	0.201	0.289	0.471	0.530

Notes: This table shows OLS results with conflict exposure as dependent variable following the specifications from Table 2-B2, but using average lights per grid cell to construct the conflict exposure measure as dependent variable. The unit of observation is the second administrative area (ADM2). The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "$Conflict$," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}."
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B7 – OLS: 1992 Lights Exposure

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	-0.00861**	-0.0176***	0.00135	-0.000295
	(0.00386)	(0.00354)	(0.00361)	(0.00234)
$Conflict$	0.0549***	-0.0229*	-0.0217*	-0.0320***
	(0.00328)	(0.0119)	(0.0113)	(0.00667)
$G_{Light} \times Conflict$		0.244***	0.239***	0.241***
		(0.0403)	(0.0362)	(0.0257)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	410,489	410,336	410,336	388,929
F	77.30	65.05	83.08	67.82
R^2_{adj}	0.158	0.190	0.402	0.401

Notes: This table shows OLS results with conflict exposure as dependent variable following the specifications from Table 2-B2, but using a stable Gini-coefficient based on lights from the year 1992. The unit of observations is the second administrative area (ADM2). The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted "$Conflict$", and (b) the Gini coefficient of nightlights, denoted "G_{Light}".
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B8 – OLS: 5km Buffers Exposure

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	-0.00145	-0.0174***	-0.0128**	-0.0160***
	(0.00389)	(0.00356)	(0.00534)	(0.00437)
$Conflict$	0.103***	-0.0305**	-0.0327**	-0.0443***
	(0.00432)	(0.0139)	(0.0132)	(0.0101)
$G_{Light} \times Conflict$		0.408***	0.415***	0.417***
		(0.0480)	(0.0438)	(0.0383)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
N	410,489	410,489	410,489	389,072
F	151.1	125.0	161.2	120.5
R^2_{adj}	0.233	0.283	0.443	0.451

Notes: This table shows OLS results with conflict exposure as dependent variable following the specifications from Table 2-B2, but using 5km Buffers to construct the conflict exposure measure as dependent variable. The unit of observation is the second administrative area (ADM2). The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "$Conflict$," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}."
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B9 – OLS: 1km Buffers Exposure

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	-0.0111***	-0.0152***	0.000677	-0.00224
	(0.00353)	(0.00320)	(0.00319)	(0.00167)
$Conflict$	0.0256***	-0.00828	-0.00653	-0.0183***
	(0.00243)	(0.00943)	(0.00935)	(0.00344)
$G_{Light} \times Conflict$		0.104***	0.0996***	0.112***
		(0.0291)	(0.0277)	(0.0135)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
N	410,489	410,489	410,489	389,072
F	38.92	33.01	35.40	34.07
R^2_{adj}	0.0901	0.0997	0.329	0.283

Notes: This table shows OLS results with conflict exposure as dependent variable following the specifications from Table 2-B2, but using 1km Buffers to construct the conflict exposure measure as dependent variable. The unit of observation is the second administrative area (ADM2). The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "$Conflict$," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}."
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B10 – OLS: Dropping Outliers

	(1) Exposure	(2) Exposure	(3) Exposure	(4) Exposure
G_{Light}	0.00823***	-0.00374***	-0.00639***	-0.00674***
	(0.000950)	(0.000542)	(0.00228)	(0.00232)
$Conflict$	0.0511***	-0.0871***	-0.0853***	-0.0703***
	(0.00384)	(0.00711)	(0.00695)	(0.00633)
$G_{Light} \times Conflict$		0.398***	0.392***	0.334***
		(0.0257)	(0.0242)	(0.0220)
Country-Year FE	✓	✓	✓	✓
ADM2 FE			✓	✓
Geo Controls				✓
N	327,379	327,379	327,208	317,543
F	47.52	99.94	110.0	60.25
R^2_{adj}	0.274	0.484	0.602	0.613

Notes: This table shows OLS results with conflict exposure as dependent variable. For this table, I exclude all districts within the bottom ten percent and the top ten percent of the lights distribution from the sample. The unit of observation is the second administrative area (ADM2) for the sample of 70 countries. The main explanatory variables constitute (a) the natural logarithm of the number of conflict events, denoted by "Conflict," and (b) the Gini coefficient of nightlights, denoted by "G_{Light}." Control variables are the sum of lights, population, (squared) precipitation and (squared) temperature.

Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B11 – OLS: Conflict Exposure and Economic Activity

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
$Exposure$	-0.0322	-0.0351*	-0.0418**	-0.0298	-0.0306
	(0.0216)	(0.0200)	(0.0200)	(0.0198)	(0.0205)
$Exposure_{t-1}$		-0.0236	-0.0381**	-0.0409**	-0.0325**
		(0.0178)	(0.0168)	(0.0178)	(0.0165)
$Exposure_{t-2}$			-0.0205	-0.0346**	-0.0415**
			(0.0163)	(0.0154)	(0.0167)
$Exposure_{t-3}$				0.00401	-0.00785
				(0.0157)	(0.0153)
$Exposure_{t-4}$					0.0257
					(0.0162)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	387,407	369,250	351,092	332,931	314,716
R^2_{adj}	0.996	0.996	0.997	0.997	0.997

Notes: This table shows OLS results with the natural logarithm of the sum of nightlights as dependent variable. The unit of observation is the second administrative area (ADM2) for the sample of 33 countries in Sub-Saharan Africa. The main explanatory variable constitutes conflict exposure, measured as the share of economic activity in close proximity to a conflict event. As controls variables, I include the number of conflict events, the spatial Gini of lights, population density, (squared) precipitation and (squared) temperature.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B12 – 2SLS: Varying the Oil-Price Lag

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)	(6) ln(light)
$Exposure$	-3.808	-7.395*	-8.089**	-6.825**	-5.864**	-4.248**
	(2.393)	(4.444)	(3.441)	(2.692)	(2.908)	(2.112)
Oil Price	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$
ADM2 FE	✓	✓	✓	✓	✓	✓
Ctry-Year FE	✓	✓	✓	✓	✓	✓
N	171,178	180,902	190,624	190,624	181,112	171,600
F_{1st}	5.583	3.947	15.82	10.37	8.243	8.488

Notes: This table provides the second stage results of 2SLS estimates following Table 2.2. Column (3) repeats the baseline estimate, while the columns to the left and right each lag and lead the oil price instrument by one additional year, respectively.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B13 – 2SLS: No Baseline Interactions

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
$Exposure$	-3.792***	0.116	-0.238**	-0.115	-0.262**
	(1.114)	(0.119)	(0.121)	(0.120)	(0.125)
$Exposure_{t-1}$		-3.571***	0.0438	-0.386**	-0.144
		(1.125)	(0.0757)	(0.184)	(0.110)
$Exposure_{t-2}$			-2.239**	-0.0101	-0.464**
			(0.979)	(0.0975)	(0.233)
$Exposure_{t-3}$				-3.047***	-0.0841
				(1.118)	(0.108)
$Exposure_{t-4}$					-3.102***
					(1.204)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,514	180,822	171,128	161,438	151,748
F_{1st}	20.07	25.03	28.62	30.33	27.92

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of lights at night within 3km to a conflict event and is instrumented by a triple-interaction of *i*) the regional average of conflict events *ii*) the regional average of the Gini of lights and *iii*) the yearly world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B14 – 2SLS: Only Conflict Propensity Share

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
Exposure	-5.771**	0.444	-0.437*	-0.148	-0.704*
	(2.646)	(0.331)	(0.242)	(0.188)	(0.417)
$Exposure_{t-1}$		-6.118**	0.360	-0.556*	-0.263
		(2.485)	(0.324)	(0.321)	(0.237)
$Exposure_{t-2}$			-5.605**	0.281	-0.956
			(2.642)	(0.311)	(0.664)
$Exposure_{t-3}$				-5.691*	0.253
				(3.000)	(0.426)
$Exposure_{t-4}$					-8.308
					(5.272)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	216,385	205,881	195,377	184,870	174,369
F_{1st}	4.218	5.655	4.510	4.228	2.373

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the local conflict propensity and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B15 – 2SLS: Only Gini Propensity Share

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
Exposure	185.1	-9.442	10.05	5.707	79.73
	(149.3)	(8.877)	(7.047)	(8.897)	(383.2)
$Exposure_{t-1}$		158.8	-6.424	21.17	32.56
		(116.4)	(6.279)	(19.44)	(153.9)
$Exposure_{t-2}$			127.1	-6.215	110.7
			(89.98)	(9.482)	(528.6)
$Exposure_{t-3}$				210.2	-5.570
				(192.5)	(44.32)
$Exposure_{t-4}$					899.6
					(4289.2)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	200,346	190,624	180,902	171,178	161,458
F_{1st}	1.537	1.867	2.007	1.196	0.0440

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B16 – 2SLS: Conflict Exposure based on 1km Buffers

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
Exposure	-4.491**	0.104	-0.608**	0.103	-0.317
	(1.811)	(0.211)	(0.253)	(0.164)	(0.195)
$Exposure_{t-1}$		-4.678***	0.0144	-0.826***	0.0592
		(1.733)	(0.178)	(0.316)	(0.156)
$Exposure_{t-2}$			-3.328***	-0.0199	-1.006***
			(1.290)	(0.201)	(0.390)
$Exposure_{t-3}$				-3.781***	-0.102
				(1.237)	(0.252)
$Exposure_{t-4}$					-4.118***
					(1.291)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,624	180,902	171,178	161,458	151,738
F_{1st}	29.57	39.78	38.82	42.76	36.79

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B17 – 2SLS: Conflict Exposure based on 5km Buffers

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
Exposure	-2.905**	0.238*	-0.114**	-0.110*	-0.195***
	(1.342)	(0.136)	(0.0546)	(0.0561)	(0.0667)
$Exposure_{t-1}$		-3.150***	0.107	-0.171**	-0.132**
		(1.197)	(0.0691)	(0.0691)	(0.0597)
$Exposure_{t-2}$			-1.975***	0.0665	-0.205**
			(0.582)	(0.0626)	(0.0939)
$Exposure_{t-3}$				-2.048***	0.0437
				(0.512)	(0.0725)
$Exposure_{t-4}$					-2.298***
					(0.645)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,624	180,902	171,178	161,458	151,738
F_{1st}	13.89	23.11	60.52	54.49	50.73

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t - s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B18 – 2SLS: Conflict Exposure based on Buffers

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
$Exposure$	-2.573**	0.0777	-0.150***	-0.110**	-0.255***
	(1.011)	(0.0968)	(0.0472)	(0.0549)	(0.0693)
$Exposure_{t-1}$		-2.584***	-0.00136	-0.204***	-0.128**
		(0.931)	(0.0574)	(0.0571)	(0.0567)
$Exposure_{t-2}$			-1.765***	-0.0184	-0.225***
			(0.584)	(0.0515)	(0.0750)
$Exposure_{t-3}$				-1.839***	-0.0641
				(0.501)	(0.0527)
$Exposure_{t-4}$					-1.987***
					(0.555)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,642	180,919	171,194	161,473	151,752
F_{1st}	31.15	46.67	134.4	125.5	82.86

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B19 – 2SLS: Conflict Exposure based on Gridded Light

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
Exposure	-1.172***	-0.0278	-0.105**	-0.160***	-0.205***
	(0.424)	(0.0647)	(0.0451)	(0.0527)	(0.0572)
$Exposure_{t-1}$		-1.283***	-0.0563	-0.133**	-0.190***
		(0.474)	(0.0624)	(0.0539)	(0.0558)
$Exposure_{t-2}$			-1.261**	-0.0612	-0.137***
			(0.579)	(0.0639)	(0.0502)
$Exposure_{t-3}$				-1.718**	-0.0911
				(0.698)	(0.0581)
$Exposure_{t-4}$					-1.842**
					(0.801)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,622	180,899	171,174	161,453	151,732
F_{1st}	51.32	82.02	92.48	34.85	21.43

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t - s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B20 – 2SLS with Conflict Events as Endogenous Variable

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
$ln(conflict)$	-0.581***	0.239***	0.0753	22.26	-0.615
	(0.147)	(0.0786)	(0.0559)	(3143.0)	(2.949)
$ln(conflict)_{t-1}$		-1.060***	0.490*	-12.13	-0.686
		(0.329)	(0.290)	(1710.0)	(3.316)
$ln(conflict)_{t-2}$			-2.519*	-102.4	0.154
			(1.431)	(14453.0)	(0.855)
$ln(conflict)_{t-3}$				595.5	3.204
				(84052.7)	(15.74)
$ln(conflict)_{t-4}$					-19.25
					(94.02)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	205,899	195,394	184,886	174,384	163,882
F_{1st}	38.33	18.65	3.643	0.0000502	0.0419

Notes: This table reproduces the main results from Table 2.2 with the number of conflict events as the endogenous variable. The instrument is constructed by interacting a time-invariant indicator variable for positive conflict propensity with the international crude oil price. The controls include the Gini of light, population, climate indicators, and climate squared.
Standard errors clustered at the ADM2 level in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B21 – 2SLS: Subset of SSA Countries

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
Exposure	-5.354***	0.445	-0.241*	-0.296	-0.665***
	(1.421)	(0.382)	(0.136)	(0.192)	(0.139)
$Exposure_{t-1}$		-5.988***	0.241	-0.372*	-0.321
		(2.076)	(0.229)	(0.198)	(0.199)
$Exposure_{t-2}$			-4.028***	0.260	-0.419*
			(1.424)	(0.252)	(0.235)
$Exposure_{t-3}$				-4.882***	0.130
				(1.517)	(0.271)
$Exposure_{t-4}$					-5.354***
					(1.029)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	24,826	23,516	22,204	20,896	19,588
F_{1st}	26.53	10.79	12.12	16.90	57.64

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B22 – 2SLS: Controlling for Province-times-Year Fixed Effects

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
Exposure	-5.252***	0.139	-0.290	-0.120	-0.375**
	(2.017)	(0.137)	(0.180)	(0.115)	(0.189)
$Exposure_{t-1}$		-4.264***	0.0525	-0.401	-0.208
		(1.547)	(0.0887)	(0.249)	(0.136)
$Exposure_{t-2}$			-2.735**	-0.0116	-0.673*
			(1.380)	(0.101)	(0.400)
$Exposure_{t-3}$				-3.154**	-0.122
				(1.555)	(0.164)
$Exposure_{t-4}$					-4.514**
					(1.994)
ADM2 FE	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,113	180,421	170,727	161,037	151,347
F_{1st}	8.685	11.43	8.862	9.132	8.605

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Province (ADM1)-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t - s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B23 – 2SLS: Constant Sample Across Specifications

	(1) ln(light)	(2) ln(light)	(3) ln(light)	(4) ln(light)	(5) ln(light)
Exposure	-8.773	0.223	-0.445**	-0.127	-0.442***
	(5.756)	(0.349)	(0.195)	(0.200)	(0.166)
$Exposure_{t-1}$		-8.420**	0.0337	-0.641**	-0.246
		(4.087)	(0.159)	(0.257)	(0.163)
$Exposure_{t-2}$			-4.371***	-0.0749	-0.762**
			(1.266)	(0.194)	(0.330)
$Exposure_{t-3}$				-4.880***	-0.116
				(1.205)	(0.180)
$Exposure_{t-4}$					-5.155***
					(1.191)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	151,738	151,738	151,738	151,738	151,738
F_{1st}	3.963	8.742	19.76	20.57	36.20

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t - s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B24 – 2SLS: 1992 Gini Coefficient in Local Share

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
Exposure	-8.722**	0.336	-0.512**	-0.189	-0.496**
	(3.910)	(0.335)	(0.247)	(0.193)	(0.195)
$Exposure_{t-1}$		-8.885***	0.117	-0.668**	-0.274
		(3.167)	(0.177)	(0.303)	(0.186)
$Exposure_{t-2}$			-5.084***	0.0102	-0.857**
			(1.457)	(0.178)	(0.390)
$Exposure_{t-3}$				-5.317***	-0.128
				(1.389)	(0.203)
$Exposure_{t-4}$					-5.813***
					(1.568)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	190,624	180,902	171,178	161,458	151,738
F_{1st}	14.91	18.64	18.64	16.99	30.73

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 3km to a conflict event and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t - s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B25 – 2SLS: Conflict Exposure based on Events with more than 5BD

	(1)	(2)	(3)	(4)	(5)
	ln(light)	ln(light)	ln(light)	ln(light)	ln(light)
$Exposure$	-2.320**	0.0422	-0.128**	-0.0994	-0.182**
	(1.138)	(0.0973)	(0.0548)	(0.0631)	(0.0837)
$Exposure_{t-1}$		-2.449**	-0.0127	-0.173**	-0.134**
		(1.132)	(0.0781)	(0.0692)	(0.0665)
$Exposure_{t-2}$			-1.877**	-0.0126	-0.162*
			(0.851)	(0.0858)	(0.0824)
$Exposure_{t-3}$				-2.116**	-0.0880
				(0.980)	(0.0904)
$Exposure_{t-4}$					-2.182**
					(0.997)
ADM2 FE	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
N	187,064	177,520	167,974	158,432	148,890
F_{1st}	12.23	15.02	23.23	13.07	11.92

Notes: This table provides the second stage results for the 2SLS estimates. The unit of observation is the second administrative area (ADM2) for countries with active on-shore oil production. The endogenous variable is the share of the 1990 population within 5km to a conflict event that was associated with at least 5 battle deaths, and is instrumented by a double-interaction of the average local Gini coefficient and the global world price for crude oil. All regressions include as control variables population density, precipitation, precipitation squared, temperature, temperature squared, as well as ADM2 and Country-Year Fixed Effects. All regressions also control for the endogenous exposure variable in year t (column 1 aside) as well as for the yearly number of conflict events and the yearly Gini of lights in year $t-s$, where s indicates the lag of the endogenous variable.
Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2-B26 – Spatial Spillovers of Conflict Exposure

	(1)	(2)	(3)
	ln(light)	ln(light)	ln(light)
Pct. Affected	-0.212***	-0.251***	-0.0940**
	(0.0288)	(0.0695)	(0.0375)
Conflict	-0.00261	0.00972*	-0.00859***
	(0.00224)	(0.00524)	(0.00285)
W×Pct. Affected		37.43***	20.25***
		(2.113)	(1.639)
Country-Year FE	✓	✓	✓
ADM2 FE	✓	✓	✓
W×ϵ			✓
N	191,163	191,163	191,163

Notes: This table provides OLS Regressions including interactions of the explanatory variable with a spatial weighting matrix to control for spatial spillovers. The coefficients for $\mathbf{W} \times Pct.Affected$ describe spatial spillovers of conflict exposure on close-by locations. The matrix **W** describes a spatial weighting matrix using the inverse distance between any two ADM2 regions in the dataset. In addition, Column (3) includes spatial spillovers of the error term as an additional control variable. Standard errors clustered at the ADM2 region in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 2-C: Spatial Gini of Lights at Night

To construct the Spatial Gini on lights, I aggregate the raster-level nightlights data, which are measured at a spatial resolution of 30 arc-seconds (ca. 1.8 km^2 at the equator), to $3 \times 3km$ grid cells and then calculate the spatial Gini over all such grid cells within an ADM2 area. Similar to the common Gini coefficient of wealth or income, the spatial Gini coefficient is a widely used concept to measure the concentration of various outcomes in space (Clark et al., 2018). Just as a regular Gini coefficient, the spatial Gini coefficient takes on values between 0 and 1, where a value of 0 would indicate a perfectly equal distribution of light and a value of 1 would indicate a perfectly concentrated distribution in only one point or pixel of a region. Other than as an indicator for the concentration of economic activity, the spatial Gini has already been used to measure the spatial concentration of, e.g., access to public transport (Jang et al., 2017) or CO_2 emissions (Zhou et al., 2018).

Chapter 3

The Persistence of Trade Relocation from Civil Conflict. Evidence from the Structural Gravity Model

3.1 Introduction

Arguably, episodes of political violence are among the main obstacles to long-run economic growth (Czaika and Kis-Katos, 2009, Blattman and Miguel, 2010, Gates et al., 2012).[25] Plenty of findings demonstrate that civil conflicts hinder economic, political, and social development in the conflict country, and how the back-and-forth between economic development and violence can entangle developing countries in a conflict trap (Fearon and Laitin, 2003, Collier and Hoeffler, 2004, Verwimp et al., 2019). Similarly, evidence abounds that civil conflicts significantly hurt countries' exports (see, e.g., Martin et al., 2008a, Novta and Pugacheva, 2021). Yet, we still lack profound knowledge of what determines countries' chances of economic recovery after conflict. Whereas various findings suggest that even large-scale destruction does not leave a persistent imprint on the economy (see, e.g., Davis and Weinstein, 2002, Miguel and Roland, 2011), others find evidence that individuals retain life-long social and economic scars from conflict (Akresh et al., 2012, Brück et al., 2019, Mercier et al., 2020, Tur-Prats and Valencia Caicedo, 2020, among others). This chapter focuses on one aspect of post-conflict economic recovery: How well can conflict countries recover the international trade relationships that they lost during the political turmoil?

As developing countries become more and more integrated into global value chains, their national conflicts cease being isolated phenomena. Instead, national economic disruptions can affect the global economy, and face international responses. This became specifically apparent during one recent incidence of international warfare. After Russia declared war on Ukraine in February 2022, the world witnessed a large-scale restructuring of international trade networks. Ukraine's destroyed capacity to export as well as severe economic sanctions against Russia forced countries world-wide to restructure their supply chains and promote new diplomatic relationships with alternative trading partners. The question remains whether these shifted supply chains will be resolved once peace is established, Ukraine rebuilt, and the sanctions against Russia lifted, or whether Ukraine and Russia face persistently worse growth outlooks as other countries replaced them as their main trading partners. In this chapter, we investigate the interdependencies between civil conflict, international trade, and economic development to provide an answer to this question.

We argue that domestic shocks can cause a restructuring of international markets that

[25] This chapter is based on Korn and Stemmler (2022).

will outlast the duration of the shock itself. For a global sample, we provide empirical evidence that importers respond to civil conflicts of their export partners by shifting supply chains to alternative export providers. What is more, we find this trade relocation to persist even after the end of the civil conflict that provoked it. For this, we develop a novel strategy to estimate how domestic shocks affect bilateral trade between *two other* countries by isolating the change in countries' relative attractiveness as export providers as they move up the ladder due to civil conflicts in ex ante preferred countries. In effect, this provides another aspect for firms' global sourcing decisions (Antràs et al., 2017). While this chapter looks at civil conflicts as major domestic shocks provoking trade relocation, our empirical approach can be applied to alternative unilateral shocks.

We estimate the trade relocation effects of unilateral economic disruptions by investigating how civil conflicts shift global trade networks.[26] In essence, we analyze whether and under which circumstances importers divert their demand from a conflict country to another, peaceful country. Our main results are based on Partial Equilibrium (PE) structural gravity estimations using bilateral trade data for over 150 countries during the period from 1995 to 2014.[27] To augment the typical dyadic gravity specification by variation from a third country, we define a "relocation propensity" variable that indicates whether a dyad is likely to be subject to trade relocation from conflict in another country. This indicator variable combines yearly information on the relationship between any conflict country and the two countries in a given dyad. Our framework hence considers dyads likely to be affected by trade relocation if (1) a conflict country used to be a relevant exporter for the dyad's importer, and (2) that dyad's exporter offers a variety of goods similar to that of the conflict country.

On average, we find bilateral trade to increase by around 6% in response to civil conflict in another country.[28] We only find evidence of trade relocation for national civil wars, while our results for international wars remain inconclusive, likely due to the rare occurrence of such events during our sample period. Our analysis further reveals a significant heterogeneity with respect to the traded sector. We find that trade in agricultural and manufacturing goods exhibit a trade relocation effect of up to 11%, whereas fuel exports do not respond at all. The fact that the international fuel trade does not react to civil conflict reflects the dependence of importers on specific suppliers of fuel exports. This was very well demonstrated recently by the European Union's hesitation to stop oil and gas imports from Russia in the light of Russia's invasion of Ukraine.[29] Similarly, oil and gas exports are of special financial importance for belligerents

[26] We follow the established definition of a civil conflict episode as a year in which violent battles between a national government and a non-governmental group demanded at least 25 battle deaths within the country's own borders (Sundberg and Melander, 2013).

[27] The availability of internal trade data restricts our sample to this time period.

[28] In Appendix 3-E, we confirm the significant negative direct effect of civil conflict on countries' exports that have already been found by, e.g., Martin et al. (2008a) and Novta and Pugacheva (2021).

[29] https://www.economist.com/the-economist-explains/2022/02/26/if-the-supply-of-russian-gas-to-

on either side of a conflict, who have an interest in maintaining fuel shipments.[30] Second, in the agricultural and manufacturing sectors, trade relocation only occurs if the prior value chain integration via Foreign Direct Investment (FDI) was negligible. However, the effect is the opposite in the minerals sector, where large amounts of FDI are associated with higher trade relocation. The difference may be driven by foreign owned mining production's vulnerability to civil conflict (Berman et al., 2017). A final heterogeneity lies in the timing of the relocation decision. We find that in the manufacturing sector, relocation is stronger after long conflict spells. This finding is in line with recent research in the business literature. Especially Multinational Enterprises (MNEs), who incorporate the threat of political tensions in their location decision of FDI, must weigh the costs from staying versus the costs of relocating. Depending on their vulnerability to conflict and local advantages for production, resuming production in a conflict zone can be the better option (Dai et al., 2017). For some, the possibility to stay is even worth investments to promote peace (Oetzel and Miklian, 2017).

Once a firm relocates its production sites or finds new providers of (intermediary) goods in another country, it has economic incentives to lobby for better and cheaper market access. Hence, trade relocation may persist after the end of a civil conflict if trading costs remain decreased. In a recent study, Freund et al. (2021) provide case study evidence for this argument for the automotive sector in response to Japan's 2011 earthquake. Similarly, Carvalho et al. (2020) find that supply-chain linkages from that same earthquake in Japan exacerbated the overall shock to the economy. In our generalized setting, we find that trade flows remain relocated for up to nine years after the end of a civil conflict, further complicating conflict countries' recovery from war. This effect is mostly driven by the manufacturing sector. As a possible channel to explain this result, we find that a civil conflict in one country makes its main importers more likely to form Preferential Trade Agreements (PTAs) with alternative exporters. This supports the intuition that the persistent relocation is fostered by deeper market integration, and follows the idea of endogenous PTA formation (see, e.g Egger et al., 2008). In the end, international markets find themselves in a new equilibrium (Allen and Donaldson, 2020). Our findings suggest that civil conflicts can harm economic development in the long-run as trade flows remain diverted away from the conflict country, underlining the view of civil conflict as "development in reverse" (Collier et al., 2003).

We finish our analysis by conducting a General Equilibrium (GE) analysis based on the recent civil war episodes in Colombia, Ukraine, and Türkiye as case studies.[31] These

europe-were-cut-off-could-lng-plug-the-gap (last accessed March 07, 2023).

[30] See https://www.economist.com/middle-east-and-africa/2014/11/01/a-sticky-problem as an example (last accessed March 07, 2023).

[31] We selected these case studies as they constitute the most significant spikes in violence according to UCDP data which have clear start and/or end points during our period of observation. For Ukraine, we analyze the conflict in the eastern Donbas region that erupted in 2014 and was officially considered a domestic war, but exclude the recent Russian invasion of Ukraine from our analysis.

case studies confirm our PE findings and indicate that importers who used to rely heavily on shipments from the conflict countries switch to shipments from alternative exporters. What is more, we estimate changes in overall national welfare measured by total consumption expenditures in response to these conflicts. Here, we find that national welfare decreases for almost all countries involved, even for those exporters on the receiving end of the relocated trade flows. This suggests that trade relocation cannot fully offset the global loss in economic activity caused by conflict.

Our paper contributes to several strands of the literature. First, we add to the evidence of how civil wars affect the international economy. Recent findings emphasize that civil wars depress the quantity and prices of exported goods (Ksoll et al., 2018, Ahsan and Iqbal, 2020). These effects are not bound to the conflict country but often spill over to neighboring countries (Qureshi, 2013, De Sousa et al., 2018). Especially in the case of transnational terrorism, protective countermeasures persistently complicate the exchange of goods, multiplying the direct effects of violence (Mirza and Verdier, 2014). Similarly, international wars as well as non-violent disputes between countries reduce bilateral trade (Fuchs and Klann, 2013, Garfinkel et al., 2020a). These effects further persist when conflict erodes trust between parties (Rohner et al., 2013). However, improved trade relationships can decrease the likelihood that international wars break out as gains from trade increase the opportunity-costs of starting a war (Martin et al., 2008a,b, 2012, Garfinkel et al., 2020b). Trade restrictions and competition can even foster political violence (Amodio et al., 2020). We extend this line of the literature by considering the general equilibrium effects of civil conflict. As international markets are tightly linked, civil conflicts are hardly a unilateral or bilateral phenomenon. By providing evidence that civil wars can affect trade flows between other, peaceful countries and provoke shifts in the international equilibrium, we consider new economic consequences from political violence.

Second, our findings add to the discussion about the persistence of the economic consequences of civil violence. According to economic theory, an economic shock should affect nations only in the short-run, while their economy rapidly recovers after the conflict is resolved (Barro and Sala–i–Martin, 1992, Mankiw et al., 1992, Blattman, 2012). These theoretical considerations receive support from several empirical findings (see, e.g., Davis and Weinstein, 2002, Brakman et al., 2004, Miguel and Roland, 2011). However, recent micro-level evidence points toward a persistent effect of civil conflict on affected individuals (Akresh et al., 2012, Brück et al., 2019, Tur-Prats and Valencia Caicedo, 2020). We contribute to this literature by pointing out that general equilibrium effects can cause the effects of civil conflict to persist. Our findings suggest that temporary trade relocation fosters market integration via PTAs, which in turn leads to persistent trade diversion away from the (former) conflict country.

Finally, we contribute to the literature investigating trade relocation effects. Recently, Korovkin and Makarin (2022) investigated how domestic firm-level trade within Ukraine responded to the civil conflict in the Crimea and Donbas region. Opposing our findings, they find that trade between unaffected firms decreases if one of the firms had a trading partner in the conflict region due to second-order effects. This suggests that domestic trade relocation occurs differently than across countries. At the global level, the literature so far mainly analyzed the trade relocation effects of PTAs. Several papers provide evidence that PTAs increase trade flows between signees ("trade creation") while decreasing trade between any signee and non-signees ("trade diversion") (Dai et al., 2014, Cheong et al., 2015, Conconi et al., 2018). We advance this literature by measuring global trade relocation in a *triadic* relationship, which allows to include unilateral shocks that occur outside an observed dyad. While we apply this strategy to civil conflict as a shock and bilateral trade as an outcome variable, the same specification can be applied to alternative bilateral dependent variables like migration or financial flows, as well as to different unilateral shocks such as climate shocks (Jones and Olken, 2010), resource windfalls (Bahar and Santos, 2018), taxes and regulations (Grubert and Mutti, 1991, Emran, 2005), or currency devaluations (Krugman and Taylor, 1978, Rose, 2021).

In the next section, we derive our empirical specifications from the structural gravity model of international trade and introduce our dataset. Afterwards, Section 3.3 discusses our main results. Section 3.4 presents several extensions to our main estimations. Finally, we will discuss a number of robustness checks in Section 3.5, before Section 3.6 concludes.

3.2 Estimation and Data

The goal of our main analysis is to estimate the persistent effect of civil conflict in one country on bilateral trade between two other countries. To do so, we augment the bilateral structural gravity model used in the international trade literature that was derived in Anderson (1979) and Anderson and van Wincoop (2003). We exclude international wars from our main analysis, as these constitute rare events during our sample period and complicate the analysis as international wars usually occur in tandem with sanctions by allied countries that additionally distort trade relationships.

The occurrence of a (civil) war has been shown to significantly impact national production capacities (see, e.g., Blattman and Miguel, 2010, for an overview). We follow the common definition of a civil conflict episode as a year in which violent combat between a national government and a non-governmental group demanded at least 25 battle-related deaths, but ignore lower conflict categories to ease the interpretation of our results (Sundberg and Melander, 2013, Bluhm et al., 2021). Among other things, open battles between governments and rebel groups are often associated with a large-scale absence of workers, (internal) refugee flows and migration, road or port blockages, as well as a general uncertainty that reduces (foreign) capital investments. In sum, all these factors reduce countries' production and hence their export capacity. We investigate whether this production shock provokes a substitution effect to alternative exporters, i.e., whether countries that depend on imports from the conflict country increase trade with other countries to compensate the loss of imports from the conflict country.[32]

Estimation. We derive our main estimating equation from the structural gravity model of international trade as detailed in Appendix 3-C. We estimate all regressions using the Pseudo-Poisson Maximum Likelihood (PPML) estimator to account for the large number of zero bilateral trade flows as well as heteroskedasticity following Santos Silva and Tenreyro (2006). Our main estimation equation takes the following form:

$$X_{ijst} = exp\left[\pi_{ist} + \lambda_{jst} + \mu_{ijs} + \beta \cdot \sum_{k} (R_{jks,t-2} \times S_{ik,t-2} \times C_{k,t-1}) + \gamma \cdot Z_{ijt}\right] + \eta_{ijst} \quad (5)$$

In Equation 5, the dependent variable X_{ijst} denotes exports from country i to country j in sector s and year t. Following the standards of structural gravity estimations, we include fixed effects for the exporter-sector-year (π_{ist}), the importer-sector-year (λ_{jst}), and exporter-importer-sector (μ_{ijs}) to account for (un)observable time-varying and country-specific variation, e.g., productivity shocks or multilateral resistances, and time-invariant, dyad-specific variation as distance or colonial ties.

Especially the multilateral resistance terms have been widely discussed in the trade literature. They resemble the theoretical concept that trade between two countries always depends on each country's average market access to all other countries globally. For example, two countries that are geographically or politically isolated would trade much more with each other than if they were well connected with other countries, everything else equal. Since these multilateral resistances are however impossible to quantify and include in estimations, the trade literature mandates the use of country-year fixed effects to control for each country's multilateral resistances when estimating bilateral trade flows. As each country's multilateral resistances are the same for each trade partner, country-year fixed effects fully account for the cross-country variation in multilateral resistances (Head and Mayer, 2014). Overall, the combination of fixed effects in gravity regressions leaves the dyad-sector-year level as the only remaining variation, where we control for bilateral preferential trade agreements (PTAs) as well as sanctions, both denoted by the vector Z_{ijt}. The error term η_{ijst} accounts for the remaining variation in X_{ijst} that is not explained by the fixed effects and independent variables.

Our coefficient of interest β indicates how bilateral trade between countries i and j in sector s reacts to conflict in another country k. It hence estimates how bilateral trade flows react to third-party conflict shocks which we approximate by an interaction

[32] In Appendix 3-E, we confirm prior evidence that civil conflict has a direct negative impact on conflict countries' export capacities.

of three variables: (i) conflict incidence in any country $k \neq i, j$, denoted by $C_{k,t-1}$, (ii) the relevance of country k as an exporter for country j in sector s, $R_{jks,t-2}$, and (iii) the similarity of production and export portfolios between exporters i and k, $S_{ik,t-2}$. Note that we lag conflict by one year and the relevance and similarity conditions by two years to (i) leave time for trade relocation effects to materialize and (ii) use country characteristics before the conflict in country k.[33]

Relevance $R_{jks,t-2}$ indicates whether country j used to import relatively large amounts from country k in sector s prior to the conflict. For our main estimations, we define $R_{jks,t-2}$ as an indicator variable by ranking j's import providers by the amount of shipments, and assigning the value of one to the countries from the top of the list that together amount to 50% of j's imports. These amount to on average 3.8 exporters and varies between 1 and 10 exporters per importer and year. For robustness, we show results using alternative cut-offs below. This relevance characteristic fulfills the first necessary condition for productivity shocks in some third country k to impact trade between countries i and j. We mainly would expect trade relocation in favor of exporter i to take place if a productivity shock hit a country k with a higher comparative advantage than i in producing goods for consumption in importer j. Hence, it is necessary that the importer j ranked the conflict country k high as an exporter before the productivity shock such that the alternative exporter i has a chance to move up in the ranks and start to export more to country j. However, this stepping-up is conditional on i exhibiting a similar (albeit lower) comparative advantage as k. This motivates our second necessary condition for trade relocation: similarity.

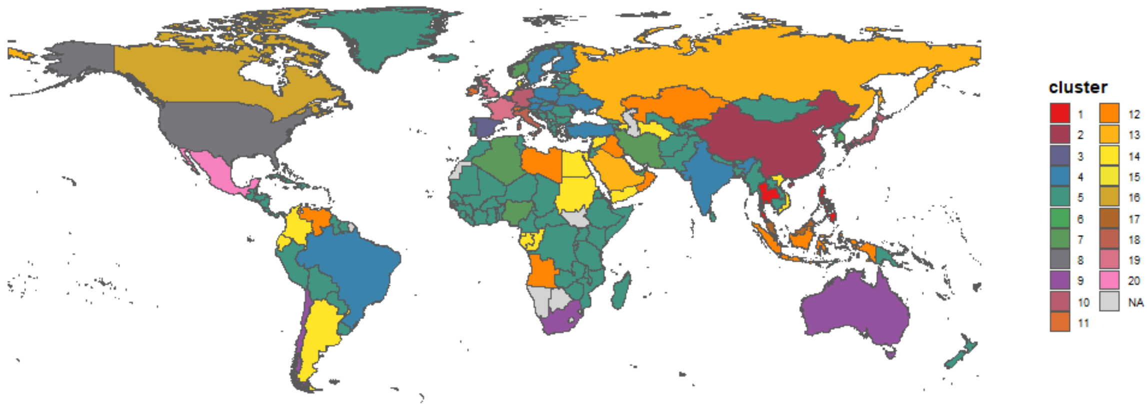
To proxy similarity $S_{ik,t-2}$, we leverage different variables to identify whether two countries i and k were overall net exporters of similar goods before the conflict broke out in country k.[34] We use disaggregated export data for 145 product lines according to the 4-digit ISIC Rev. 3.1 classification. First, we assign countries into groups with similar export structures. We apply a K-Means clustering algorithm as developed by Hartigan and Wong (1979), which allocates countries according to their similarity in production to a pre-defined number of groups. For our preferred specifications, we divide all exporting countries in a given year into 20 different groups. Our method is similar to the process applied by Kim et al. (2020), who assign trade-dyads to groups according to the similarity in the sectoral composition of their trade flows. As this method of assigning exporter groups inherits some degree of randomness, and there is no clear candidate for a "perfect" number of groups, we test for robustness across different group sizes. Figure 3.1 below illustrates the allocation of clusters for the year 2005. To test whether our similarity measure is robust to other specifications, we additionally construct an export similarity

[33]In the robustness section, we also examine trade flows in the same year as the conflict takes place, which does not alter our results.

[34]This is, we abstain from focusing on pre-shock *bilateral* exports to j in order to not mis-interpret low exports as a low latent relative comparative advantage.

index following Benedictis and Tajoli (2007a,b), which mirrors the correlation between sectoral exports across countries.[35]

Figure 3.1 – Export Similarity Clusters



Notes: This graph shows the distribution of exporter groups among 179 countries based on a K-Means clustering algorithm with 20 random centers in the year 2005. The algorithm randomly selects 20 countries as centers and then assigns all remaining countries to the center with the most similar export structure. Similarity is computed based on export-data for 145 product lines based on the ISIC Rev. 3.1 classification.

Our variable of interest, the trade relocation propensity between countries i and j, constitutes the triple interaction of the indicators for relevance $R_{jks,t-2}$, similarity $S_{ik,t-2}$, and conflict $C_{k,t-1}$. In other words, whenever *all* three indicator variables take the value of one for *any* country k, our relocation propensity variable takes the value of one for the ijs dyad.[36] We hence identify a positive relocation propensity as the specific case that conflict country k was a significant trading partner of importing country j in sector s in the past, *and* this same country k used to offer a similar variety of goods as exporter i. Note that the single components of this triple interaction are all controlled for by the fixed effects in our regressions. This is, only the composite variation across all three indicators affects bilateral trade X_{ijst}, while the occurrence of conflict in country k, the production portfolio of exporter i, and the ranking of trade partners by importer j are controlled for by the respective country-year fixed effects.

A causal interpretation of our results requires the unexplained variation captured by the error term η_{ijst} to be uncorrelated with our relocation propensity variable, conditional

[35]More precisely, the similarity index measures the inverse distance in sectoral exports between two countries x and y via the formula $1 - \frac{\sum_i |x_i - y_i|}{\sum_i (x_i + y_i)}$. This, hence, compares the absolute distance in sectoral export volumes across countries to both countries' total exports in this sector. If both export exactly the same in all sectors (i.e. $\sum_i |x_i - y_i| = 0$), the similarity index takes the value of 1. Does only one country export the total combined value of the two countries in each sector, the index takes the value of zero.

[36]Note that this constitutes the extensive margin, coding a dyad as subject to trade relocation if they are affected by at least one conflict. In the Appendix, we provide results for the intensive margin, using the number of identified relocation possibilities as the explanatory variable. While the results are very similar, we prefer the extensive margin due to the easier interpretation of the results.

on our control variables and fixed effects. We hence must rule out that unobserved, non-random characteristics captured by η_{ijst} are associated with a higher likelihood that our relocation propensity variable takes the value of one. For this, it is important to note again that none of the three ingredients to our relocation propensity variable is dyad-year-specific. First, the incidence of civil conflict in country k, $C_{k,t-1}$, is an event observed by all dyads in a given year t and hence is controlled for by year fixed effects. Second, the relevance characteristic $R_{jks,t-2}$ is specific to a dyad's importer only and hence does not vary across an importer's export partners in a given year. Hence, importer-sector-year fixed effects account for all characteristics that make an importer more likely to experience trade relocation from a given conflict country k. The same argumentation holds for the similarity condition $S_{ik,t-2}$ at the exporter side, which is controlled for by exporter-sector-year fixed effects. Finally, characteristics that are specific to a given dyad and might increase its average propensity that both $R_{jks,t-2}$ *and* $S_{ik,t-2}$ are one is accounted for by dyad fixed effects.

A potential bias in our estimates hence requires the presence of unobserved characteristics that vary at the dyad-sector-year level, correlate with the interaction of our relevance and similarity conditions, and mainly occurs in years when country k observes a civil conflict. One potential caveat could be, for example, that our results are mainly driven by one of the two variables, while the other only generates minimal identifying variation. In Appendix 3-F, we therefore provide an in-depth discussion of the determinants and variation of both the relevance and similarity conditions and demonstrate that both variables exhibit sufficient variation. Furthermore, we provide several robustness checks below which demonstrate that both conditions are required together to estimate a significant relocation effect.

Another caveat could be that during years when a conflict is active in a country that is relevant to a dyad's importer and similar to its exporter, the dyad's preferences for trading with each other systematically increase for reasons other than the civil conflict in the third country. One such possibility could be that importers apply bilateral sanctions to countries that are linked to the conflict country k, but strategically spare countries they identified as potential export substitutions for k. Here, it is reassuring that controlling for various types of sanctions leaves our results qualitatively unchanged. Our results are further not sensitive to controlling for pre-existing observable trade preferences in the form of PTAs. The non-sensitivity of our results to the inclusion of these bilateral, time-varying control variables makes us confident that the likelihood that unobserved characteristics are correlated with our trade relocation variable is low.

We view reverse causation as an unlikely threat to our identification. Reverse causation would require that bilateral trade flows between two countries are significantly linked to the likelihood that a civil conflict emerged in another country that is relevant to the dyad's importer and similar to its exporter two years prior. While there is evidence that the US staged coups to increase trade with targeted countries (Berger et al., 2013), we are not aware of any evidence or anecdotes that governments stage civil wars in third countries to increase exports to or imports from a specific other, non-conflict country. In addition, we demonstrate in welfare analyses below that typically, all countries are worse off from a civil conflict and the resulting restructuring of international trade flows. This is, even though some countries can increase their exports to others by substituting for the conflict country, they still experience an overall net welfare reduction due to decreased trade opportunities with the conflict country and therefore should have no incentive to stage a civil war abroad.

Note, finally, that our identifying assumptions are distinct from those in the current Difference-in-Differences (DiD) or Two-Way-Fixed-Effects (TWFE) literature. Several papers caution against the use of simple TWFE regressions with more than two time periods and staggered treatments (see, e.g., Callaway and Sant'Anna, 2021, de Chaisemartin and D'Haultfoeuille, 2022, Roth et al., 2022, for recent surveys). The complication in staggered DiD or TWFE regressions arises from different treatment groups receiving the same treatment at different points in time, and then remaining treated. Estimation via TWFE regressions then yields "forbidden comparisons," as newly treated entities are compared to already treated entities. This may result in biased estimations of the treatment effect as these "forbidden comparisons" yield negative OLS weights, because the comparison of newly treated to already (and still) treated might suggest no treatment effect for the newly treated. Under heterogeneous treatment effects or treatment effects that unfold over time, such comparisons might even yield negative treatment effect estimates from these comparisons. Note, therefore, that in our setting, no entity remains treated. This is, exporter-importer-dyads enter and exit the treatment status depending on the conflict status, as well as the similarity and relevance characteristics of the dyad and another third country. In our sample, the average dyad has a conditional likelihood to remain treated for a second year of 45%. Hence, more than half of the treated dyads exit the treatment status already one year later. The likelihood that a dyad remains treated for three years in a row lies even lower at 26%. Only 1% of the entities in our sample are treated for more than half the years they appear in our sample. Therefore, our regressions should be interpreted as common panel regressions with a dichotomous treatment that switches on and off as opposed to a staggered DiD analysis where entities would enter the treatment status once and then stay treated. This makes it unlikely that the occurrence of negative OLS weights due to "forbidden comparisons" would bias our estimates of the treatment effects. As a specific robustness check in this direction, we drop all dyads that are treated in more than 80% of the years in our sample period. If negative weights were problematic, these observations would be the most likely ones to bias our results as their high treatment propensity would subject them to negative weights in the staggered DiD sense. Their omission does however not at all affect our results (see Appendix Table 3-A2).

Data. Our empirical analysis draws from various data sources related to civil conflict and international trade. For our main analysis, we include trade data for the manufacturing and primary sectors. Addressing the primary sector separately is important as civil conflicts predominantly erupt in resource-abundant countries (Ross, 2015).[37]

Manufacturing data come from the Comtrade dataset, which includes bilateral trade flows between 1980 and 2018 of approximately 180 countries.[38] As a measure for trade in primary goods, we use commodity trade data from CEPII's BACI dataset, which consists of yearly bilateral trade flows at the 6-digit HS level. According to recent advancements in the international trade literature, bilateral trade flows alone are not sufficient for a reliable empirical analysis. As Yotov (2021) shows, international trade flows need to be complemented with intra-national trade data to obtain unbiased and consistent estimates within the gravity framework. Unfortunately, the availability of consistent internal production data is still limited. Therefore, we combine several data sources to maximize the coverage across countries, sectors, and years. We follow the literature on computing internal trade flows (Baier et al., 2019). For the manufacturing sector, we compute internal trade as the difference between total manufacturing production and total manufacturing exports. To quantify total manufacturing production, we draw on data from the INDSTAT database. We proceed similarly to compute internal trade flows in the primary sector. Here, we use commodity production data from Fally and Sayre (2018). The authors combine production data of minerals, agricultural commodities and fuels from the British Geological Survey, the FAO and the Global Trade Analysis Project (GTAP). Based on these data, we compute internal trade flows for about 200 countries and across 169 commodities between 1995 and 2014. We complement our dataset with information on PTAs from CEPII's Gravity database.[39]

To identify civil conflict, we use the UCDP/PRIO Armed Conflict Dataset version 19.1 (Sundberg and Melander, 2013). We follow the established definition and code a country to experience a civil war in a given year if it has experienced violent events between government troops and a non-governmental entity, and if the number of battle-related deaths exceeded the threshold of 25 casualties. Our main dataset comprises 179 countries over the years 1995–2014. Table 3-A1 reports descriptive statistics of our main variables.

[37]The relationship between natural-resource abundance and the likelihood of conflict depends on several factors, such as political stability, inequality, or type of resources (see, e.g., Bazzi and Blattman, 2014, among others).

[38]Trade values are primarily measured through imports, as these are usually more precisely computed. We complement missing import data with exports between the same dyad and year to maximize coverage.

[39]The PTA variable is based on the RTA-IS dataset of the World Trade Organization (WTO) and is constructed out of information on Partial Scope Agreements (PSA), Free Trade Agreements (FTA), Customs Unions (CU) and Economic Integration Agreements (EIA).

3.3 Main Results

This section discusses our main findings. We start with a depiction of significant trade relocation effects from civil conflict in the agricultural and manufacturing sector, and explain why such effects do not occur in the minerals and fuels sectors. Afterwards, our discussion turns to the persistence of these effects, for which we find specific evidence in the manufacturing sector. Additionally, we provide evidence that shock-induced international trade policy works as a possible channel for persistence. The section concludes with an analysis of the global welfare effects of civil conflict and the related trade relocation.

Trade Relocation. Table 3.1 presents our main results, which are obtained by estimating Equation 5 with the PPML estimator. We use 20 exporter groups to measure similarity and define an importer's top trade-partners as those that together provide 50% (Panel A) or 66% (Panel B) of the country's imports to measure relevance. Column 1 presents the results from estimations across all sectors, Columns (2)–(5) show results based on sector-specific trade flows, reducing the sample to trade flows in agricultural, minerals, fuels, and manufacturing goods, respectively. All regressions include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed-effects and control for bilateral trade agreements and directional sanctions.

When pooling all sectors together, we do not find any evidence of trade relocation in response to civil conflict in another country. Both specifications in Panels (A) and (B) return relatively precisely estimated zero-effects for trade relocation.[40] Looking at the four main sectors separately, we do, however, find that this non-finding masks significant sectoral heterogeneity. Columns (2)–(5) report significantly positive trade relocation effects in agricultural and manufacturing goods as well as zero-effects in minerals and fuels trade. On average, civil conflict in a relevant and similar country k increases manufacturing exports from i to j by around five to six percent. For agricultural goods, we find similarly strong effects for the broader relevance definition in Panel B and estimate an even larger effect of about eleven percent in Panel A. Overall, we conclude from these findings that productivity shocks from civil conflicts in relevant third countries significantly increase trade in manufacturing and agricultural goods. We interpret this as an impact on directional trade preferences; the high dimensional fixed effects and our control variables hold multilateral resistances, country-specific productivity shocks as well as bilateral characteristics and trade policies constant. Still, we find that the reduced productivity of a third country that i) was among an observed dyad's main exporters and ii) produced similar goods as that same dyad's exporter significantly increases country j's demand for goods from country i.

This finding does not apply to the minerals and fuels sector. There are two potential

[40] The results are basically identical if we do not control for PTAs or sanctions (not shown for brevity).

Table 3.1 – Trade Relocation Main Results, 1995-2014

Dependent:	Exports from country i to country j				
	Pooled	Agricult.	Minerals	Fuels	Manufact.
	(1)	(2)	(3)	(4)	(5)
Panel A:	50% top trade-partners				
Conflict in country k	0.01	0.11**	-0.03	-0.03	0.06***
	(0.02)	(0.04)	(0.07)	(0.04)	(0.02)
Panel B:	66% top trade-partners				
Conflict in country k	0.02	0.06**	-0.05	-0.03	0.05***
	(0.01)	(0.02)	(0.05)	(0.03)	(0.02)
Observations	1,026,127	376,755	217,218	90,947	341,207
Exporter × sector × year FE	✓	✓	✓	✓	✓
Importer × sector × year FE	✓	✓	✓	✓	✓
Exp. × imp. × sector FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, pooled over all sectors in column 1 and disaggregated by sectors in columns 2-5. The explanatory variables take a value of 1 if (i) country k had a conflict in the previous year, (ii) country k was a top exporter for country j in the pre-conflict year and (iii) country k and country i were similar exporters in the pre-conflict year. Similarity is measured by being in the same exporter-cluster, with a total number of 20 clusters. Top trade-partners are those that jointly provide 50% (Panel A) or 66% (Panel B) of the country's imports. We estimate all specifications using the PPML estimator and include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed effects while controlling for bilateral sanctions and trade agreements. Standard errors in parentheses are clustered at the dyad-sector level, * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$.

explanations for the non-findings in these sectors. We would not expect to see trade relocation occurring if either the imported goods cannot be substituted by alternative exporters, e.g. because they are too specific or require significant FDI, or because civil conflicts do not necessarily decrease a conflict country's exports in the first place. At least for fuel exports, we find evidence for the latter explanation. As we discuss in more detail in Appendix 3-E, we find that civil conflicts depress exports in all sectors but in fuels, which confirms prior empirical and anecdotal evidence that warring parties have a joint interest of keeping up oil exports to finance their war efforts (Bazzi and Blattman, 2014). Hence, we would not expect to find trade relocation in the fuels sector. Yet, even though anecdotal evidence suggests that several minerals like diamonds or gold fulfill a similar function as fuels to finance civil wars, Appendix 3-E demonstrates that countries export less minerals when they are at conflict.

A possible explanation why we see no trade relocation in minerals trade is another heterogeneity within the minerals sector. As we discuss in more detail in Section 3.4 below, we find significant evidence of extensive trade relocation in minerals trade for importers that invested high amounts of FDI in the conflict country before conflict onset.

On the other hand, we find no evidence of trade relocation for less FDI-intensive value chains. While this requires more investigation outside the scope of this chapter, it appears that minerals exports can be distinguished into two groups: *i*) low capital intensive minerals (e.g., diamonds, gold) that can be produced by warring rebels/governments and whose production can continue during war, and *ii*) high capital intensive minerals (e.g., copper, coal) where foreign investors can relocate their supply chains to other countries during a conflict. Of course, our observations are based on observable, official trade flows only. However, as we construct our dataset from importer-reported trade flows, a potential measurement error from increased illegal trade flows is likely minimal. Especially so for trade in fuels, where the undisclosed shipment of significant amounts is logistically rather challenging.

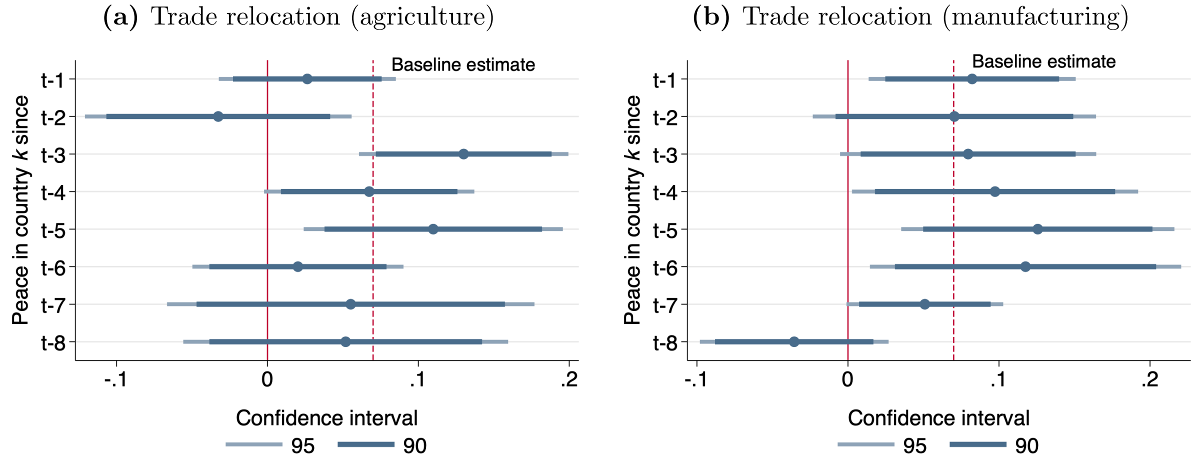
To summarize, we find that civil conflicts provoke sizeable trade relocation effects in agricultural and manufacturing trade, do not change trade relationships in the fuels sector, and exhibit mixed effects in the minerals sectors. Next, we investigate how long these effects persist. Do trade flows return back to normal when a conflict is resolved?

Effect Persistence. Considering the various micro-economic mechanisms that play out when trade flows relocate from one country to another, there is reason to expect that a temporary relocation can turn persistent. As soon as retailers or producers of country j start importing their goods from country i instead of country k, they establish new connections and trade networks with exporting firms in country i. Companies in countries i and j integrate their supply chains and establish international branches via FDI. These newly established connections may, in turn, induce national governments to sign new trade agreements with each other. This re-drawing of international cooperation may persistently decrease bilateral trade costs. According to dynamic equilibrium theory, a one-time shock can hence alter allocations and bilateral preferences such that economies end up converging to a new long-run equilibrium (Allen and Donaldson, 2020, Nitsch, 2009). In our case, this means that new supply chains and trade agreements tend to stay in place when a conflict ends, and trade relationships are unlikely to return to pre-conflict levels once country k resolves its conflict. Such a restructuring of international trade flows can hence exacerbate the conflict trap by pushing countries into the fringe of international trade, which is one explanation why conflict-ridden countries lack economic development in the long-run (Collier et al., 2003).

We analyze relocation persistence by estimating specifications similar to those presented in Equation 5. Instead of an indicator variable for country k being at war, we code how many years ago exporter k's civil war ended. Moreover, to consistently define the similarity and relevance conditions over time, we use the values from the year prior to conflict onset in country k.

Figure 3.2, Panel (a) displays estimates for shipments from exporter i to importer j in

Figure 3.2 – Relocation Persistence



Notes: This figure shows the coefficients and confidence intervals from eight different regressions, each regressing exports from country i to country j on a lagged value of our relocation propensity variable in the agricultural (Panel (a)) and manufacturing (Panel (b)) sectors. The control variables and fixed effects are as in Columns (2) and (5) of Table 3.1. The dashed vertical lines represent the baseline estimates from Columns (2) and (5) of Table 3.1, respectively. The light and dark blue lines depict 95% and 90% confidence intervals based on standard errors clustered at the dyad-sector level.

the agricultural sector up to eight years after the end of a civil conflict, Panel (b) shows the same for manufacturing trade.[41] The specifications include the same fixed effects and control variables as our main results in Table 3.1. For manufacturing trade, we find that up to seven years after the end of a conflict in country k, shipments from i to j remain significantly higher than before the conflict. What is more, our estimates for the lagged effects center around the baseline estimate for trade relocation, suggesting that once relocated, trade relationships mostly remain in place despite the end of a conflict. Note that the return to an insignificant finding in $t-8$ should not necessarily be interpreted as an end of relocation. Our sample only spans 18 years, and the estimations here require a civil conflict to emerge, end, and remain peaceful for at least eight years during the sample period. Hence, this non-finding likely occurs due to a lack of statistical power, instead of trade relationships suddenly reverting back eight years after a civil conflict. In fact, we only identify 214 potential relocation dyads out of 420,000 observations for conflicts that ended eight or more years ago in the manufacturing sector.

Our evidence for persistence in agricultural trade is not as conclusive; even though some lags suggest significantly positive relocation effects three to five years after the end of a conflict, numerous insignificant results do not allow the conclusion of clearly persistent effects. To conclude, we find that manufacturing trade relationships remain diverted after the end of a civil conflict. Even though a conflict country established peace and had up

[41] We do not report estimates for the pooled sample as well as the minerals and fuels sectors. As Table 3.1 suggests, we do not find any significant effects for persistence trade relocation in these sectors.

to seven years of time to recover economically, importers retain their preferences for the alternative export partner. This leads to the question how relocated trade relationships remain cemented after the initial economic shock dissipated.

Table 3.2 – Linear Probability Model: Forming Preferential Trade Agreements

Dependent:	Likelihood of PTA between country i and country j			
	Agricult.	Minerals	Fuels	Manufact.
Conflict in country k	-0.760*** (0.197)	0.162 (0.222)	-0.072 (0.470)	0.578** (0.279)
Observations	371,561	217,383	92,810	342,338
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exp. × imp.	✓	✓	✓	✓
Sanctions	✓	✓	✓	✓

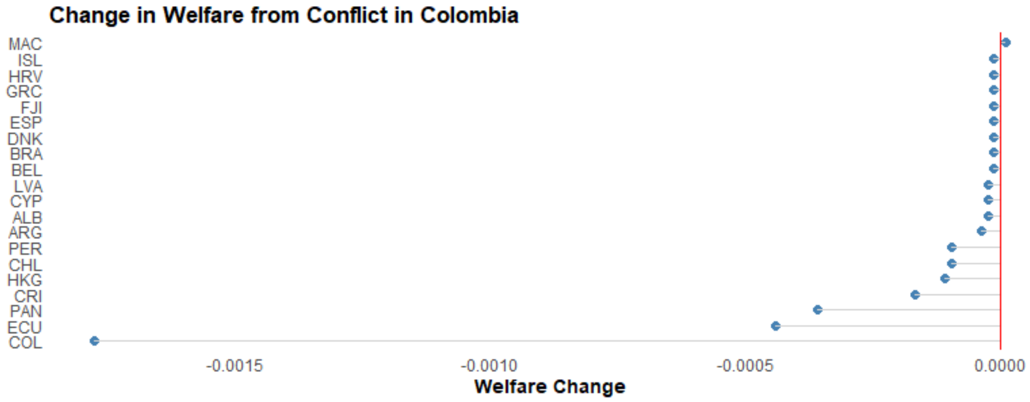
Notes: This table reports results from Linear Probability Models with the likelihood that country i and country j enter a trade agreement as the dependent variable. The explanatory variable is constructed as in Table 3.1, with the top-66% exporters defining relevance, and using 20 exporter groups to define similarity. All estimations include exporter-year, importer-year and exporter-importer fixed effects. We control for prior trade agreements and bilateral sanctions. Standard errors, in parentheses, are clustered at the dyad-sector level, * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$

Persistent trade relocation can come about as countries i and j persistently decrease their bilateral trade costs. During k's civil war, the two countries have an incentive to tighten their trade relationships, something we can observe via the formation of new trade agreements. We construct sector-specific relocation propensity measures as in our main analysis and run bilateral OLS regressions with an indicator variable for signing a new preferential trade agreement (PTA) as the dependent variable. We report the results in Table 3.2. We find a significant increase in the likelihood of entering a PTA if countries experience trade relocation in the manufacturing sector. If relevant exporters of manufacturing goods suffer from civil conflict, the chances to sign a PTA with another, similar exporter increase by up about 0.6 percentage points. This is a sizeable effect, basically doubling the 0.6% unconditional likelihood of a dyad forming a new PTA in our sample. This significantly increased likelihood of strengthening trade relationships after being forced to relocate manufacturing trade flows helps explain our finding of a persistent relocation effect in manufacturing trade.

Surprisingly, however, we find a significantly *negative* effect of about the same size for the likelihood to sign a new PTA in response to trade relocation in the agricultural sector. As we do not find evidence of persistent trade relocation in agricultural goods, we would not have expected to see an increased likelihood of entering a PTA. Yet a decreased PTA likelihood appears puzzling. One possible explanation may lie in local agricultural producers in the importing country being empowered by the reduced import competition during a relevant trade partner's civil conflict, and using this momentum to lobby against

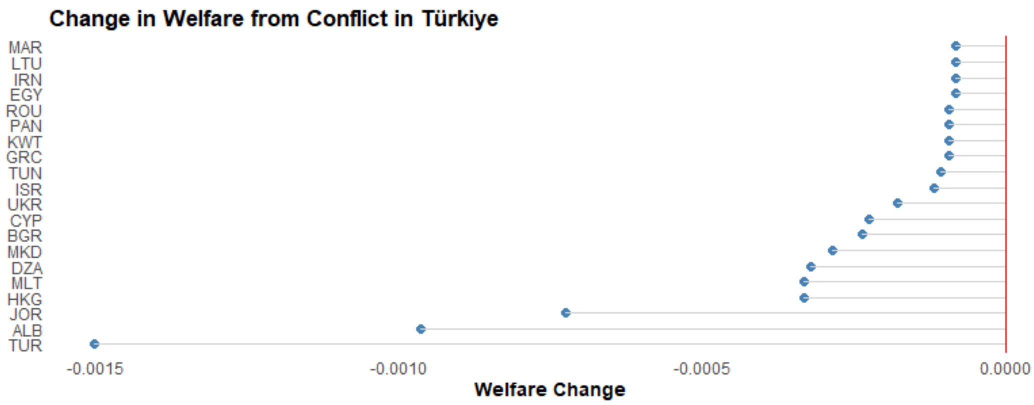
Figure 3.3 – GE Results: Welfare Changes

(a) Welfare Changes, Conflict in Colombia



(b) Welfare Changes, Conflict in Ukraine

(c) Welfare Changes, Conflict in Türkiye



Notes: The graphs report the estimated welfare changes in the general equilibrium due to the civil wars in Colombia (Panel a), Ukraine (Panel b), and Türkiye (Panel c). Each panel reports the 15 countries for whom our estimations reported the largest welfare changes. All estimates are derived based on a Partial Equilibrium regression of exports on peace, comparing the estimated trade flows during peace time to the actual trade flows during the civil war. See Table 3-A5 for the respective Partial Equilibrium results.

lowering trade barriers with other agricultural exporters. Unfortunately, we lack the data to test this mechanisms directly and therefore encourage future research on this effect.

Welfare Effects. To trace the welfare effects of civil conflict and trade relocation, we conduct general equilibrium welfare computations for three case studies. Next to quantifying the trade-related welfare impact of civil conflicts, this analysis helps us shed further light on two questions. First, can importers compensate (most of) the import shock by moving to the second-best import provider, or do they still face a net welfare loss? And second, do these alternative exporters stepping in overall benefit from the civil conflict, or are they also left worse off by the conflict-induced decrease in total global production? As case studies, we look at the recent peaks of civil violence in Colombia, Ukraine and Türkiye, and estimate (i) changes in worldwide bilateral trade flows and (ii) changes in countries' overall welfare.[42]

Following Baier et al. (2019), we first estimate how each of these civil conflicts affects the conflict country's overall exports, and then use this estimate to compute hypothetical trade flows in case the respective conflict never happened. Deriving overall consumption from (hypothetical) internal and international trade flows, we further receive a proxy for countries' overall welfare levels. A comparison of actual to hypothetical trade flows and welfare levels then sketches the general equilibrium effects of the respective conflict. We discuss the GE trade changes in more detail in Appendix 3-D and the welfare effects below.

As depicted in Figure 3.3, for basically every country in our sample, welfare levels are smaller relative to the hypothetical scenario where a given conflict had not occurred. While it is of little surprise that the conflict countries themselves as well as their main importers experience the largest welfare reductions, even those countries that experience bilateral export increases thanks to trade relocation are overall worse off. Indeed, we only estimate a slight welfare increase for Macao in response to the civil conflict in Colombia. Apparently, trade relocation can only partially offset the welfare losses countries encounter due to increased trading costs with the conflict country. Hence, even though trade relocation helps mitigate some of the global loss in trade and welfare due to civil wars, all members of the world economy are individually worse off compared to a world at peace. Note however that the estimated welfare losses are relatively small with significantly less than 0.1 percent for all but the conflict countries. Hence, even though the violent episodes in our case studies decrease global welfare, the overall impact remains rather low, likely because trade relocation helps cushion the conflict's impact on international trade.

[42]Note that we investigate the impact of the Ukrainian civil war that broke out in 2014, where no official international military forces from other countries were involved. We purposefully do not include the Russian attack on Ukraine in our analysis to restrict the general equilibrium results to civil conflicts that follow the definition we used for our main results.

3.4 Extensions

To better grasp the mechanisms that lead to (persistent) trade relocation, we consider various extensions to our baseline estimations. In this section, we test whether our findings change when only looking at long conflicts, and discuss how trade relocation varies with respect to prior global value chain integration.

Table 3.3 – Relocation Heterogeneity: Conflict Duration, 1995-2014

Dependent:	Sectoral exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
Duration of conflict in country k	0.002**	-0.002	-0.007**	0.001**
	(0.001)	(0.002)	(0.003)	(0.001)
Observations	376,755	217,218	90,947	341,207
Exporter × year FE	✓	✓	✓	✓
Importer × year FE	✓	✓	✓	✓
Exp. × Imp. FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, disaggregated by sectors. The explanatory variable follows the relocation propensity coding we used above, but using the number of years at conflict instead of a dummy variable for conflict onset in country k. Similarity is measured by being in the same exporter-cluster, with a total number of 20 exporter groups. Top trade-partners are those that together provide 66% of the country's imports. We estimate all specifications with the PPML estimator and include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed effects and control for bilateral sanctions and trade agreements. Standard errors in parentheses are clustered at the dyad-sector level, * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$

Conflict Duration. In Table 3.3, we repeat our main analysis, but recode the variable for trade relocation propensity to reflect the number of years a dyad is subject to trade relocation from a civil conflict. The results hence should be interpreted as marginal effects, as they reflect the average trade relocation effect per year of conflict. Overall, our findings remain similar to our main findings discussed above. We still find significantly positive trade relocation effects in the agricultural and manufacturing sectors, suggesting a linear increase in trade relocation over the duration of a civil conflict. What is new is that the estimated effect for trade relocation in the fuels sector now turns significantly negative. As discussed above, a null effect for trade relocation in fuels is of little surprise as conflict parties tend to keep exporting fuels to finance their war efforts. Fuel trade relocation turning negative for long-lasting conflicts lends further support for this channel. Apparently, fuel exporters that engage in longer conflicts even increase fuel exports to finance the ongoing battles, in turn drawing away fuel imports from alternative fuel producers.

In a similar vein, Bluhm et al. (2021) argue that civil conflicts can already impact the economy even if they do not yet reach 25 battle deaths. Lingering conflicts, e.g., in the form of violent riots, can induce concerns that the violence will escalate, to which producers and exporters may already respond. We employ their coding of lingering conflicts to our relocation estimate and test for relocation from episodes when countries experience riots or violent protests with at least one casualty, but do not pass the common threshold of 25 battle deaths per year. Table 3-A3 presents mixed results for this lingering conflict category. While the effects overall go mostly in the same direction, we only find significant evidence for manufacturing trade relocation when we use the 66% threshold to define relevance. While this effect is of similar size as for full-scale conflicts, it is not robust to the lower relevance threshold of 50%. Hence, while we cannot rule out that trade relocation already occurs during lingering conflicts, we only find robust evidence of trade relocation for violent episodes that pass the common threshold of 25 battle deaths per year.

Global Value Chain Integration. We expect trade relocation to be most severe for importers whose value chains are not tightly linked to the conflict country. Arguably, it is easier to switch external suppliers of upstream goods for alternative producers in other countries than moving previously offshored, own production capacities from one country to another. In Table 3.4 we analyze whether trade relocation varies conditional on the importance of conflict country k as an FDI destination for firms from importer j. We would expect that substantial amounts of capital invested in conflict country k would reduce the incentive to switch trade partners. We define country k as an important FDI destination if it received more than 10% of importer j's total outward FDI stock prior to the civil conflict. In even columns, we report trade relocation estimates for important FDI-destinations, while odd columns focus on trade relocation away from countries without a significant share of FDI. Note that we do not observe FDI by sector, and hence our sample split builds on the assumption that countries that rely heavily on another country for imports in a specific sector also focus their foreign investments in this country in that given sector.

In the agricultural and manufacturing sectors, we find the expected effect that only less relevant FDI destinations experience trade relocation. In the minerals sector, we find the opposite result; if conflict country k received a significant FDI stock from firms in country j, imports are *more likely* to relocate to another exporter i. This result might hint at the vulnerability of mining-sector FDI to civil conflict. Recent evidence suggests that natural resource mines are preferred targets of violent groups (Berman et al., 2017). The destruction of foreign-held capital together with a more insecure environment for (new) investments might hence encourage firms to divert both FDI and imports to other countries.

Table 3.4 – Relocation Heterogeneity: FDI Destination

Dependent:	Sectoral exports from country i to country j							
	Agricult.		Minerals		Fuels		Manufact.	
Sign. FDI (j to k):	No	Yes	No	Yes	No	Yes	No	Yes
Conflict in country k	0.087*** (0.029)	0.025 (0.045)	-0.046 (0.046)	0.875*** (0.237)	-0.029 (0.031)	0.109 (0.089)	0.029* (0.016)	0.015 (0.026)
Observations	282,597	282,597	162,532	162,532	67,639	67,639	268,228	268,228
Exporter × year	✓	✓	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓	✓	✓
Exporter × Importer	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, disaggregated by sectors. Even columns restrict trade relocation to conflict countries that did receive at least 10% of an importer's FDI before conflict onset, while odd columns look at trade relocation from insignificant FDI locations. The explanatory variables take a value of 1 if (i) country k had a conflict in the previous year, (ii) country k was a top exporter for country j in the pre-conflict-year and (iii) country k and country i were similar exporters in the pre-conflict-year. Similarity is measured by being in the same exporter-cluster, with a total number of 20 exporter groups. Top trade-partners are those that together provide 66% of the country's imports. We estimate all specifications with the PPML estimator and include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed effects, and control for bilateral sanctions and trade agreements. Standard errors in parentheses are clustered at the dyad-sector level, * p< 0.1, ** p< 0.05, *** p< 0.01

3.5 Robustness

We estimate various alternative specifications to test our results for robustness. A first concern of our estimation approach is our selection of cut-offs to code our relevance and similarity conditions. Figures 3-B1 & 3-B2 in the Appendix provide results for our main specification using alternative thresholds to classify relevant and similar exporters, respectively. Figure 3-B1 varies the percentage threshold for import penetration to classify a conflict country as a relevant exporter for a dyad's importer. Our results are somewhat sensitive to the cut-off choice in manufacturing trade, but robust across the various cut-offs in agricultural trade. For manufacturing trade, only cut-offs between 50% and 70% yield significant trade relocation estimates. This non-robustness does however fit the intuition behind our estimation approach. If we consider a too small number of trade-partners, we miss out on relevant relocation cases. This, in turn, results in only a very small number of dyads we code as subject to trade relocation, while many potential relocation cases end up in our control group, biasing the results towards zero. Similarly, a too broad classification of relevant trade-partners adds numerous cases which we would code as subject to trade relocation even though the actual propensity for trade relocation is very low.

In Figure 3-B2, we conduct a similar robustness test and vary the number of exporter groups we use to code exporters as similar. Our results are much less sensitive to the number of exporter groups. Especially for manufacturing trade, we find significantly positive relocation estimates for almost anything between 10 and 32 exporter groups. This robustness resembles the fact that the variation in the similarity classification is rather low across intermediate numbers of exporter groups. For example, countries that rely mostly on agricultural production will almost always end up together in the same exporter group, no matter whether the world is divided into five or forty production clusters. Still, another concern inherent to our estimation approach is that a single exporter group might drive our results. To check for this possibility, we conduct leave-one-out regressions, where we repeat our main estimations but drop one exporter group at a time. As we show in Figure 3-B3 in the Appendix, our results are basically identical regardless of which exporter groups we drop from our sample.

Two further robustness checks specifically concern our coding of exporter similarity. First, instead of K-Means Clustering, we construct a similarity index following Benedictis and Tajoli (2007a). This index measures the correlation of sectoral export values between two countries relative to other countries. We define countries i and k as similar if their export similarity is higher than 0.5, where 1 refers to identical and 0 to non-overlapping export patterns. Second, we change the input dataset for the K-Means Clustering algorithm to allow for importer-specific considerations of which exporters they would treat as similar. Here, we classify all available exporters for each importer separately and include additional variables as inputs to the cluster algorithm. In addition to sectoral production shares, we also include various dyadic determinants of trade costs. Among other things, these are bilateral distance, common official language, and colonial heritage. Arguably, if importers search for substitution possibilities in response to a civil war in one of their main export providers, these cost factors may be as relevant as a country's production capabilities to make a trade relocation decision. We present the results of both alternative specifications in Table 3-A6. Our main results remain qualitatively unchanged. Surprisingly, however, both of these alternative ways of coding yield significantly positive relocation estimates for the minerals sector. Whereas we prefer to interpret these findings with a grain of salt due to the missing robustness of the estimates for this sector, we definitely cannot rule out the occurrence of trade relocation in the minerals sector.

Next, we want to rule out the possibility that instead of the interaction of the similarity and relevance conditions, one of these conditions alone produces our results. Theoretically, our identification approach might mechanically single-out much-trading dyads or countries exporting specific goods via the relevance or similarity classification, respectively. While the fixed effects in our regressions directly control for each indicator variable, there remains the possibility that due to missing variation in either one of the two conditions, the other may alone drive the effects. As our results are less sensitive to the number of exporter groups as shown in Figure 3-B2, one concern could be that the condition of similarity is redundant. To check whether indeed the interaction of both variables is generating our results, we invert either the similarity or the relevance classification and repeat our main estimations. Table 3-A7 reports the results. In Panel A, we use the 33% least important trade-partners to importer j, while still using the original similarity classification between conflict country k and exporter i based on 20 exporter groups. We find no evidence that conflict in less-important trade partners leads to trade relocation. In Panel B, we retain the original relevance classification of the top 66% trading partners, but turn around our similarity classification to include all exporters with an exporter similarity index below 0.2. We do not find significant trade relocation effects. Overall, we conclude from these falsification tests that our identification approach indeed captures trade relocation propensity, as both relationships to the conflict country, i.e., the exporter's similarity as well as the importer's relevance, are needed together to produce our main results.

Finally, Table 3-A8 presents additional results in which we slightly change our main specification. In Panel A, in addition to our standard relocation propensity variable based on conflicts with at least 25 battle deaths, we include a similar indicator for relocation propensity based on large conflicts with more than 1,000 battle deaths. Interpreting the two coefficients jointly hence tells us whether the effect differs between small and large conflicts. The estimated coefficients for small conflicts remain basically unchanged, whereas the indicator variable based on big conflicts yields insignificant or even negative coefficients. These results should be treated with caution though, as the number of large conflicts in our sample is relatively small. If at all, the negative coefficients might be interpreted as a hint on diminishing trade relocation with respect to conflict intensity, as most relocation takes place already in the beginning of a conflict.[43] In Panel B, we use the number of conflict countries that fulfill the relevance and similarity conditions instead of an indicator that the conditions are fulfilled for *any* country to estimate the intensive margin of trade relocation. The coefficients are almost identical to our main results, only in the agricultural sector the effect size decreases slightly. In Panel C, we estimate trade flows in the same year as the conflict in country k. Again, this alternative way of coding does not alter our results.

Finally, Panel D looks at international instead of domestic wars. Surprisingly, we find significantly negative coefficients for trade relocation in the agricultural and minerals sector, while the results for manufacturing turn insignificant. Again, these results should be interpreted with caution however as the number of international wars during our sample time frame is small. In addition, the mechanisms behind trade relocation in international wars differ remarkably from trade considerations in domestic war settings. As the Russian invasion of Ukraine demonstrated, international wars complicate firms' trading decisions further as global alliances as well as diplomatic relationships with perpetrator-

[43] For example, Bluhm et al. (2021) show that most conflicts escalate slowly from small to big conflicts, giving countries time to relocate trade flows before large conflict episodes unfold.

and victim-states lead to sanctions, blockages, or other political barriers to trade.

3.6 Conclusion

This chapter introduces a novel estimation approach for trade relocation effects that result from economic shocks in third countries. We extend the structural gravity model of international trade to allow for unilateral economic shocks, which usually get omitted by the model's country-year fixed effects, to affect bilateral trade between other countries via changes to the overall competition on international markets (Anderson and van Wincoop, 2003, Head and Mayer, 2014). In the short-run, a reduced competitiveness of one country can thus increase trade between other countries as alternative producers climb up the ladder and become relatively more attractive trading partners. Such short-run trade increases can spur market integration, e.g., via signing PTAs, which in turn can persistently decrease bilateral trade costs and provoke a persistent relocation of international trade.

We estimate the trade relocation effects of civil conflicts, which have been shown to significantly depress countries' export capacity (Novta and Pugacheva, 2021). On average, we find that similar and relevant dyads increase bilateral trade flows by about 6% in the agricultural and manufacturing sectors in response to civil conflict in a third country. At the same time, we find no trade relocation effects in the fuels sector. What is more, we find that in the manufacturing sector, trade relocation persists still seven years after the end of a civil conflict due to reduced bilateral trade costs via PTAs. Hence, civil conflicts can induce long-term economic losses for affected countries as international markets end up in a new equilibrium.

This chapter is the first paper to study the short- and medium-run trade relocation effects of unilateral shocks like civil conflicts. Our results add to prior findings that civil conflicts depress the international trade flows of conflict countries (Martin et al., 2008a) and their neighbors (Qureshi, 2013). Our findings are furthermore relevant for the design of post-conflict recovery policies. After a country resolves its internal disputes, it faces a different network of international trade with increased competition due to persistent shifts in the trade relationships of former trading partners. To reintegrate the now peaceful country back into international markets and support post-conflict recovery, improving the terms of trade, e.g., via the quick resolution of (temporary) preferential tariff margins in the spirit of post-conflict GSP tariff removals, may constitute valuable policy measures. Similarly, conflict-countries themselves may prioritize foreign policy to improve bilateral trade and hence spur the recovery of local production capacities.

Our estimation approach can easily be adapted to other settings. To analyze relocation effects, we construct a relocation propensity indicator variable, which translates the triadic relationship between a conflict country and any trading dyad into a dyadic observation. Besides civil conflicts, the approach can be applied to any other unilateral shock that can significantly alter a country's international competitiveness. Moreover, our estimation approach can be adapted to other bilateral outcome variables like migration or FDI by formulating similarity and relevance conditions that apply to the outcome variable of interest.

3.7 Appendix

Appendix 3-A: Additional Tables

Table 3-A1 – Descriptive Statistics

Variable	Mean (1)	SD (2)	Min. (3)	Max. (4)	Obs. (5)
Panel A: General variables					
Export value (Mio. USD)	111.05	2095.00	0.00	4.1e+05	1,647,134
Production value (Mio. USD)	6.9e+07	4.5e+08	0.28	1.6e+10	8,358
Trade agreement	0.12	0.32	0.00	1.00	1,655,492
Exporter sanctioned	0.08	0.27	0.00	1.00	1,655,492
Internal conflict (>25 deaths)	0.14	0.35	0.00	1.00	1,655,492
Panel B: Cluster variables					
Conflict, 20 cluster, top 50 TP	0.03	0.16	0.00	1.00	1,655,492
Conflict, 20 cluster, top 66 TP	0.05	0.21	0.00	1.00	1,655,492
Conflict, 25 cluster, top 50 TP	0.02	0.15	0.00	1.00	1,655,492
Conflict, 25 cluster, top 66 TP	0.04	0.19	0.00	1.00	1,655,492

Notes: This table provides descriptive statistics of the main variables. The variables in Panels B depict the indicator variable as described in Equation 5. Variables take a value of 1 if exporter k has a conflict, countries i and k were in the same cluster, and country k was a top trade-partner (TP) of country j. The sample consists of 180 countries and the sectors agriculture, minerals, fuels and manufacturing between 1995-2014.

Table 3-A2 – Trade Relocation Robustness: Dropping High Propensity Dyads

Dependent:	Exports from country i to country j				
	Pooled	Agricult.	Minerals	Fuels	Manufact.
	(1)	(2)	(3)	(4)	(5)
Panel A:	50% top trade-partners				
any conflict	0.005	0.113***	-0.024	-0.037	0.048**
(k) cluster (i) top (j)	(0.023)	(0.043)	(0.067)	(0.038)	(0.019)
Panel B:	66% top trade-partners				
any conflict	0.020	0.060**	-0.047	-0.029	0.049***
(k) cluster (i) top (j)	(0.014)	(0.025)	(0.050)	(0.029)	(0.015)
Observations	1,015,239	368,923	215,355	90,258	340,703
Exporter × sector × year	✓	✓	✓	✓	✓
Importer × sector × year	✓	✓	✓	✓	✓
Exp. × imp. × sector	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, pooled over all sectors in column 1 and disaggregated by sectors in columns 2-5. Across all regressions, we drop dyads that are treated across 80% of the years in the sample to rule out that these dyads drive our results. The explanatory variables take a value of 1 if (i) country k had a conflict in the previous year, (ii) country k was a top exporter for country j in the pre-conflict-year and (iii) country k and country i were similar exporters in the pre-conflict-year. Similarity is measured by being in the same exporter-cluster, with a total number of 20 exporter groups. Top trade-partners are those that together provide 50% (Panel A) or 66% (Panel B) of the country's imports. For robustness, we here drop all dyads with more than 80% treated years. We estimate all specifications with the PPML estimator and include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed effects. Standard errors in parentheses are clustered at the dyad-sector level, * p< 0.1, ** p< 0.05, *** p< 0.01.

Table 3-A3 – Trade Relocation from Violent Protests

Dependent:	Exports from country i to country j				
	Pooled	Agricult.	Minerals	Fuels	Manufact.
	(1)	(2)	(3)	(4)	(5)
Panel A:	50% top trade-partners				
any conflict	0.015	-0.016	0.040	0.015	0.016
(k) cluster (i) top (j)	(0.012)	(0.028)	(0.069)	(0.055)	(0.012)
Panel B:	66% top trade-partners				
any conflict	0.049***	-0.021	0.096	0.016	0.052***
(k) cluster (i) top (j)	(0.009)	(0.025)	(0.074)	(0.048)	(0.010)
Observations	1,022,008	371,505	217,218	90,947	342,338
Exporter × sector × year	✓	✓	✓	✓	✓
Importer × sector × year	✓	✓	✓	✓	✓
Exp. × imp. × sector	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, pooled over all sectors in column 1 and disaggregated by sectors in columns 2-5. The explanatory variables take a value of 1 if (i) country k had a violent protest in the previous year, (ii) country k was a top exporter for country j in the pre-conflict-year and (iii) country k and country i were similar exporters in the pre-conflict-year. Similarity is measured by being in the same exporter-cluster, with a total number of 20 clusters. Top trade-partners are those that jointly provide 50% (Panel A) or 66% (Panel B) of the country's imports. We estimate all specifications using the PPML estimator and include exporter-sector-year, importer-sector-year and exporter-importer-sector fixed effects while controlling for bilateral sanctions and trade agreements. Standard errors in parentheses are clustered at the dyad-sector level, * p< 0.1, ** p< 0.05, *** p< 0.01.

Table 3-A4 – Trade Relocation Heterogeneity: Market Share of Conflict Country

Dependent:	Sectoral exports from country i to country j							
	Agricult.		Minerals		Fuels		Manufact.	
Country$k$ > 5% market share:	No	Yes	No	Yes	No	Yes	No	Yes
Conflict in country k	0.054*	0.034	0.087	−0.067	−0.269***	−0.010	0.011	0.060***
	(0.030)	(0.032)	(0.073)	(0.055)	(0.083)	(0.030)	(0.021)	(0.018)
Observations	376755	376755	217,218	217,218	90,947	90,947	341,207	341,207
Exporter × year	✓	✓	✓	✓	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓	✓	✓	✓	✓
Exporter × Importer	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j. The explanatory variable is constructed as in Table 3.1, with the top-66% exporters defining relevance, and 20 clusters defining similarity. To analyze the heterogeneity w.r.t the market share, we interact the explanatory variable with a dummy indicating that country k has a market share of at least 5% in the respective sector. This interaction variable takes the value of 0 in odd columns and 1 in even columns. All estimations are run with the PPML estimator and include exporter-time, importer-time and exporter-importer fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered at the importer-exporter level, * p< 0.1, ** p< 0.05, *** p< 0.01.

Table 3-A5 – PE Results for GE Computation

	Exports from country i to j		
	(1)	(2)	(3)
Peace × International	0.687***	0.410***	0.888***
	(0.149)	(0.139)	(0.127)
N	150,719	150,719	150,719
Country	Colombia	Ukraine	Türkiye
FTA-Control	✓	✓	✓

Notes: This table reports Partial Equilibrium estimates that provide the baseline for the General Equilibrium computations. Results are based on PPML estimations and include exporter-time, importer-time and exporter-importer fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered at the importer-exporter level, * p< 0.1, ** p< 0.05, *** p< 0.01.

Table 3-A6 – Trade Relocation Robustness: Alternative Similarity and Relevance Definitions

Dependent:	Exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
Panel A:	Export similarity >0.5			
Conflict in country k	0.104** (0.043)	0.317** (0.155)	-0.055 (0.036)	0.048* (0.025)
Observations	376,755	217,218	90,947	341,207
Panel B:	Dyadic clusters			
Conflict in country k	0.086*** (0.017)	0.067* (0.038)	0.045 (0.034)	0.065*** (0.016)
Observations	345995	204110	84577	289353
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exporter × Importer	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, disaggregated by sectors. The explanatory variable is constructed as in Table 3.1, with similarity being defined as the two countries having an above 0.5 similarity index, as defined by Benedictis and Tajoli (2007a,b) in Panel A, and the two countries being in the same dyadic cluster in Panel B, and relevance as the top-66% exporter countries. All estimations are run with the PPML estimator and include the trade exporter-time, importer-time and exporter-importer fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered on the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01.

Table 3-A7 – Trade Relocation Robustness: Wrong Similarity and Relevance Conditions

Dependent:	Sectoral exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
	(1)	(2)	(3)	(4)
Panel A:	20 clusters, bottom 33% trade partner countries			
Conflict in country k	-0.048 (0.036)	-0.059 (0.045)	-0.031 (0.026)	-0.000 (0.018)
Panel B:	Dissimilar countries (<20% similarity)			
Conflict in country k	-0.006 (0.025)	0.026 (0.037)	-0.033 (0.034)	0.010 (0.021)
Observations	376,755	217,218	90,947	341,207
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exp. × Imp.	✓	✓	✓	✓
PTA	✓	✓	✓	✓

Notes: This table reports a placebo study to the previous estimations. It shows effects of conflict in country k on exports from country i to country j. The explanatory variable is constructed as in Table 3.1, but, in Panel A, relevance is measured with the 33% less relevant trade partners, and, in Panel B, the similarity is measured with a below 0.2 similarity index, as defined by Benedictis and Tajoli (2007a,b). All estimations are run with the PPML estimator and include the trade exporter-time, importer-time and exporter-importer fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered on the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01.

Table 3-A8 – Trade Relocation: Various Robustness Checks

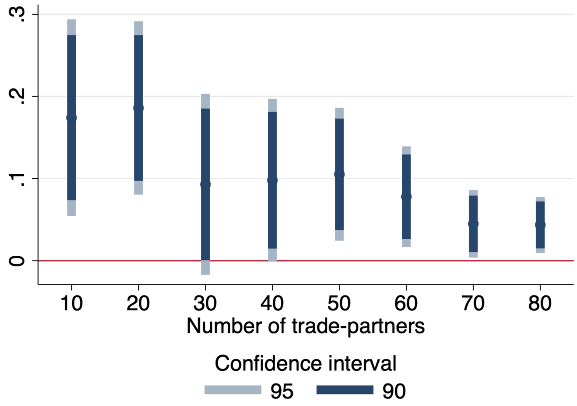
Dependent:	Sectoral exports from country i to country j			
	Agricult.	Minerals	Fuels	Manufact.
Panel A:	Large conflicts			
Conflict in country k	0.081*** (0.024)	-0.040 (0.050)	-0.026 (0.031)	0.056*** (0.016)
Big conflict in country k	-0.091** (0.036)	-0.064 (0.110)	-0.007 (0.056)	-0.090* (0.052)
Panel B:	Intensive margin			
Conflict in country k	0.04* (0.02)	-0.04 (0.05)	-0.01 (0.03)	0.05*** (0.01)
Panel C:	Conflict in same year			
Conflict in country k	0.054** (0.027)	-0.045 (0.054)	-0.026 (0.032)	0.046*** (0.016)
Panel D:	International wars			
Conflict in country k	-0.227*** (0.072)	-0.496*** (0.190)	0.016 (0.049)	0.047 (0.079)
Observations	376,755	217,218	90,947	341,207
Exporter × year	✓	✓	✓	✓
Importer × year	✓	✓	✓	✓
Exp. × Imp.	✓	✓	✓	✓
PTA	✓	✓	✓	✓

Notes: This table reports estimates of the effects of conflict in country k on exports from country i to country j, disaggregated by sectors. In Panel A, the explanatory variables are indicator variables which count the occurrences of (i) country k having had any conflict or large conflicts in the previous year, (ii) country k being a top-66% exporter for country j in the pre-conflict-year and (iii) country k and country i being similar exporters in the pre-conflict-year. In Panel B, the explanatory variable is a continuous variable which counts the occurrences of our diversion propensity indicator for each exporter-importer pair. In Panel C, the explanatory variable is an indicator variable but with conflict measured in the same year as exports. Panel D uses international instead of internal wars. Throughout, similarity is measured by being in the same exporter-group, with a total number of 20 groups. All estimations are run with the PPML estimator and include exporter-time, importer-time and exporter-importer fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors, in parentheses, are clustered on the exporter-importer level, * p< 0.1, ** p< 0.05, *** p< 0.01.

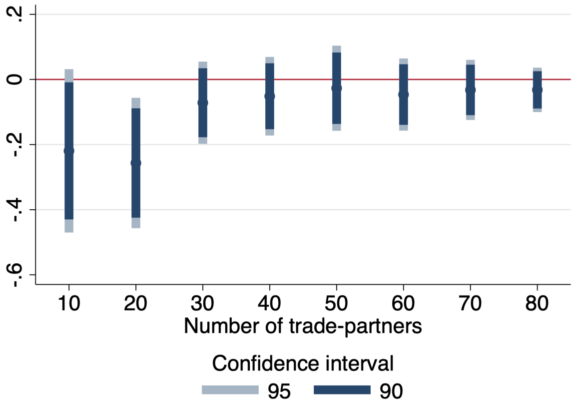
Appendix 3-B: Additional Graphs

Figure 3-B1 – Number of Trade-Partners - Sector Disaggregation

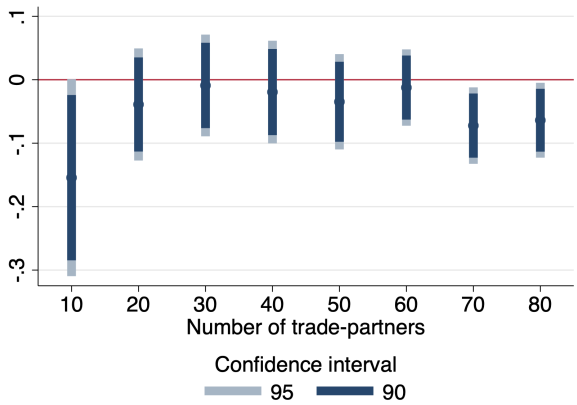
(a) Agriculture



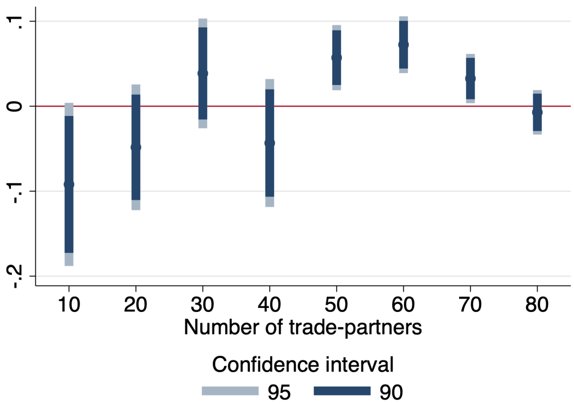
(b) Minerals



(c) Fuels



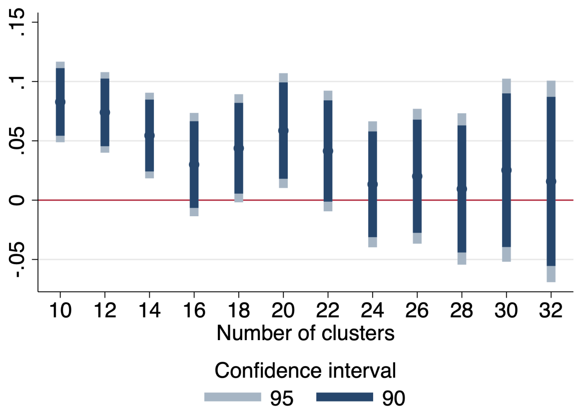
(d) Manufacturing



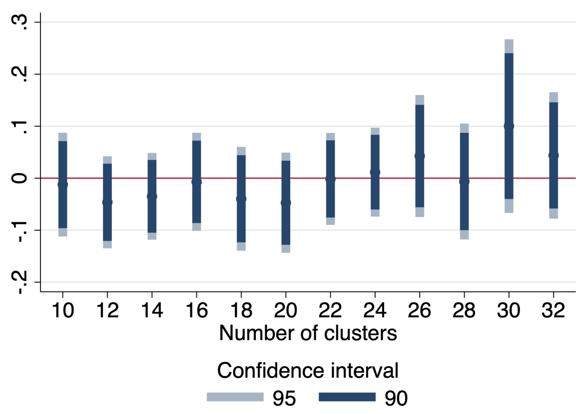
Notes: This figure displays the coefficients of relocation propensity as defined in Table 3.1 with similarity based on 20 exporter groups and relevance on a varying number of trade-partners. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

Figure 3-B2 – Number of Exporter Groups - Sector Disaggregation

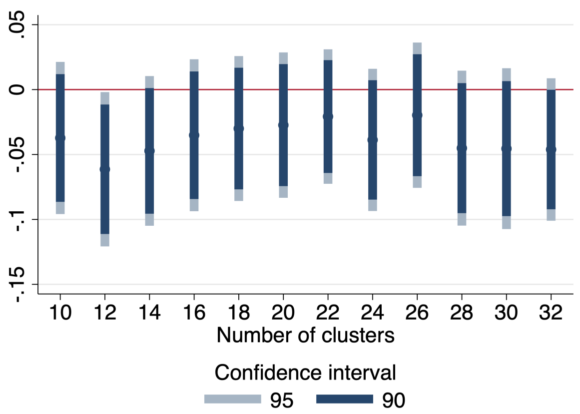
(a) Agriculture



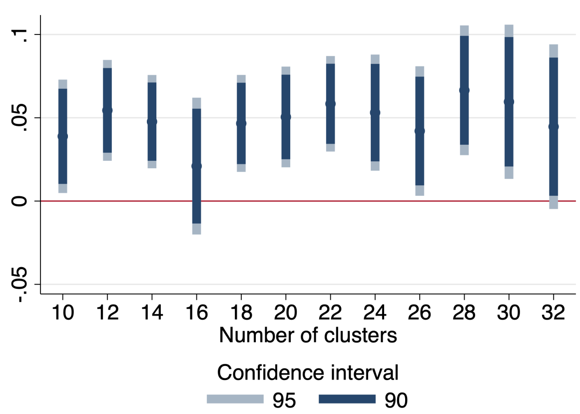
(b) Minerals



(c) Fuels

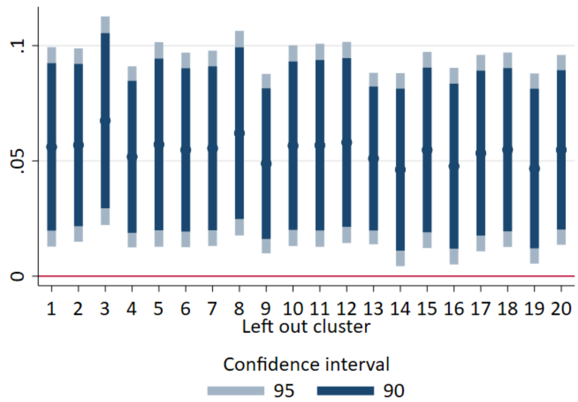


(d) Manufacturing



Notes: This figure displays the coefficients of relocation propensity as defined in Table 3.1 with similarity based on a varying number of exporter groups and relevance on the top 66% trade-partners. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level. The light and dark blue lines depict 95% and 90% confidence intervals, respectively.

Figure 3-B3 – Leave-one-out



Notes: This figure displays the coefficients of relocation propensity as defined in Table 3.1 with similarity based on 20 clusters and relevance on the top 7 trade-partners. Each coefficient represents a regression leaving out one cluster. All estimations are run with the PPML estimator and include exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered on the exporter-importer-sector level.

Appendix 3-C: Derivation of Estimation Equation from Structural Gravity Model of International Trade

Following Anderson et al. (2018), we describe the exports of a variety of goods in sector s from country i to country j in year t with the equation:

$$X_{ijst} = \frac{Y_{ist} E_{jst}}{Y_{Wst}} \cdot \left[\frac{t_{ijt}}{\Pi_{ist} P_{jst}} \right]^{1-\sigma} \tag{6}$$

Exports X_{ijst} are positively related to the product of the exporter's level of production Y_{ist} and the importer's consumption expenditures E_{jst}, relative to total world output Y_{Wst} in that same sector. Trade flows further depend on the bilateral "iceberg trade costs" denoted by t_{ijt}. This term covers, among other things, the distance between two countries or the amount of tariffs paid on shipments. With the elasticity of substitution across varieties $\sigma > 1$, bilateral exports X_{ijst} respond negatively to increases in trade costs t_{ijt}. Finally, bilateral trade depends on the multilateral resistances faced by the exporter and importer, respectively. The outward multilateral resistance Π_{is} describes the exporter's average (inverse) market access to all potential importers. The inward multilateral resistance P_{js} similarly describes the importer's average (inverse) market access to all potential exporters. Both these variables can be thought of as the competition on international markets in sector s that either i or j face with any other country to trade with country j or i, respectively.

Third-Party Effects. There are two ways to let third-party effects enter Equation 6. First, one may view all trade effects that occur due to shocks to any other country than the observed i and j to be included in the multilateral resistance terms Π_{is} and P_{js}. Considering the general equilibrium effects of unilateral economic shocks, any change in one country's export potential affects the trade decisions of all other countries via changes in each export partner's *relative* attractiveness. Such changes in bilateral trade preferences due to deviations of other potential partners' attractiveness are the reason why, among others, Anderson and van Wincoop (2003) and Head and Mayer (2014) mandate the use of exporter-year and importer-year fixed effects in structural gravity estimations. We will follow this convention in all our estimations, such that changes in countries' multilateral resistances will be controlled for and cannot explain the third-party effects we estimate below.[44] Instead, the mechanism we identify encapsulates a *directed* preference change from one importer towards another exporter.

To see how third-party effects can alter the directional preferences of two trading partners, we re-arrange Equation 6 by taking logs and summarizing country-year as well as importer-exporter specific terms:

[44] We thank an anonymous referee for pointing this out.

$$X_{ijst} = exp\left[\lambda_{jst} + \pi_{ist} + t_{ijst}\right] + \eta_{ijst} \tag{7}$$

In Equation 7, λ_{jst} and π_{ist} collect all importer-sector-year specific and exporter-sector-year specific terms, respectively. They hence account for, among other things, $Y_{ist}, Y_{Wst}, E_{jst}, \Pi_{ist}$, and P_{jst} from Equation 6 above. These national incomes and multilateral resistances will later be controlled for by the exporter-year and importer-year fixed effects in our regressions. To trace out the third-party effects on bilateral trade, we can decompose the bilateral term t_{ijst} into two separate components and re-write Equation 7 as:

$$X_{ijst} = exp\left[\lambda_{jst} + \pi_{ist} + \mu_{ijs} + TP_{ijst} + Z_{ijt}\right] + \eta_{ijst} \tag{8}$$

With the term μ_{ijs}, we capture time-invariant components that determine the directed trade relationship between countries i and j. Among other things, μ_{ijs} includes bilateral distance, having a common national language, or sharing a colonial history. Following the conventions in structural gravity estimations, we will control for these time-invariant determinants with dyad-fixed effects below. Hence, the sum $TP_{ijst} + Z_{ijt}$ contains all variation that remains after conditioning on the fixed effects typical to structural gravity estimations.

In Z_{ijt}, we collect all relevant bilateral, time-varying policies that determine the amount of shipments between two countries. Recently, especially two such policies stand out in the literature: Preferential Trade Agreements (PTAs) and sanctions (see, e.g., Dai et al., 2014, Felbermayr et al., 2019). The term TP_{ijst} finally identifies bilateral trade preferences of consumers in importing country j for goods from exporting country i. We expect that third-party effects enter this term in structural gravity estimations despite controlling for the multilateral resistance terms, and that we can empirically identify trade relocation effects via this component.

For example, the term TP_{ijst} would incorporate relative comparative advantages in specific sectors across countries as well as specific tastes of j's consumers for products in sector s produced by exporter i. We expect that these trade preferences TP_{ijst} can change subject to economic shocks in third countries $k \notin i, j$. To illustrate this idea, consider the following case: country j is a net-importer of several goods in sector s. The consumers in country j rank available exporters $i, ..., k, ..., I$ according to their relative comparative advantage in producing goods in sector s for consumption in country j, and allocate their overall demand decreasingly from the exporter with the highest comparative advantage downwards. Assume that in year t_0, the comparative advantage of producing goods in sector s for consumption in country j is strictly higher in exporter k than in exporter i. It follows from the consumers' transitive preferences that country j imports more from k than from i. In year t_1, an economic shock affects country k's productivity and forces it to export fewer goods, or at higher costs. This diminishes the size of country k's comparative advantage, at the benefit of country i. Assuming that producers in i have a similar albeit smaller comparative advantage as k, we would expect its exporters to step up and benefit from the excess demand from j's consumers as k suffers its productivity shock. Whereas we can think about any type of economic shock in this scenario, the remainder of this chapter will empirically investigate the effect of civil conflicts in k on bilateral trade between countries i and j. In the following, we elaborate on how we empirically estimate these triadic effects.

Appendix 3-D: Construction of GE Dataset

Our GE estimates require a symmetric dataset which also includes internal trade flows of all sample countries. We calculate internal trade flows by subtracting a country's exports from its total production. In the next step, we construct a symmetric dataset. This is, we require bi-directional trade flows between all available exporters and importers in the sample as well as non-negative internal trade flows for each country and in every year. Due to differing data availability across years, we restrict our sample to the manufacturing sector and the years 1992-2016. Additionally, we reduce the number of countries to 68 importers and exporters. As a decision rule for our sample construction, we decided to only keep years or countries whose numbers of observations amount to at least 80% of the year and 80% of the importer/exporter with the most observations, respectively. Our results remain unchanged for stricter and looser restrictions.

Appendix 3-E: Direct Effects

A prerequisite for finding significant trade relocation effects of civil conflicts is that conflict countries decrease their amount of exports. Prior findings emphasize that civil wars depress international trade (see, e.g., Bayer and Rupert, 2004, Qureshi, 2013). To replicate these findings with our data and adapt the empirical strategy to the gravity framework of international trade, we follow Head and Mayer (2014) and extend Equation 6 accordingly. When we include country-year and dyad fixed effects, the effect of civil conflict in a country i on that same country's exports cannot directly be estimated as the variable is collinear with the exporter-year fixed effects π_{ist}. We therefore follow Yotov et al. (2016) and include intranational trade flows along with bilateral trade flows in our dataset.[45] This allows estimating the effect of a unilateral shock like civil conflict on bilateral trade by interacting the variable of interest with an indicator variable for international trade flows (Beverelli et al., 2018). We arrive at an estimating equation of the form:

[45] Yotov (2021) provides an extensive overview of the benefits of adding intranational trade in bilateral trade estimations.

$$X_{ijst} = exp\left[\pi_{ist} + \lambda_{jst} + \mu_{ijs} + \beta_1 \cdot (C_{it} \times I_{ijs}) + \gamma \cdot Z_{ijt}\right] + \eta_{ijst}, \tag{9}$$

where η_{ijst} accounts for the remaining variation in X_{ijst} not explained by the fixed effects and control variables. The variable C_{it} indicates the presence of civil conflict in country i at year t, and I_{ijs} indicates international trade flows (i.e., that $i \neq j$). This form of the gravity specification affects the interpretation of the coefficient β_1. Here, β_1 constitutes the elasticity of exports from origin i to destination j in sector s relative to internal consumption of country i to civil conflict emerging in country i.

Table 3-E1 – Direct Effects: Internal Conflicts Hurt Exports

Dependent:	Total exports from country i				
	All sectors	Agri culture	Minerals	Fuels	Manu facturing
	(1)	(2)	(3)	(4)	(5)
Conflict (t-1)	-0.06**	-0.09*	-0.23**	0.03	-0.06**
× international trade	(0.02)	(0.05)	(0.12)	(0.13)	(0.03)
Sanctions	0.02	0.03	0.03	-0.02	0.02
	(0.02)	(0.03)	(0.06)	(0.05)	(0.02)
RTA	0.16***	0.18***	0.15*	0.03	0.17***
	(0.03)	(0.04)	(0.08)	(0.10)	(0.03)
Observations	1,013,193	375,700	216,199	90,632	330,662
Exporter × sector × year	✓	✓	✓	✓	✓
Importer × sector × year	✓	✓	✓	✓	✓
Exp. × imp. × sector	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Notes: This table reports estimates of the effects of civil conflict on a country's exports. We interact a dummy variable for lagged civil conflict with an indicator variable for international trade flows. Coefficients must hence be interpreted as change in exports relative to a country's internal trade. All estimations are run with the PPML estimator, exporter-sector-time, importer-sector-time and exporter-importer-sector fixed effects. Control variables are indicators for trade agreements and bilateral sanctions on the exporter side. Standard errors are clustered at the exporter-importer-sector level, * p< 0.1, ** p< 0.05, *** p< 0.01.

We use the Pseudo-Poisson Maximum Likelihood (PPML) estimator as suggested by Santos Silva and Tenreyro (2006). To account for differences in the duration and velocity of the effect, we lag civil conflict by one as well as two years. The results are presented in Table 3-E1 and confirm our priors based on the literature. Column (1) considers trade data across all four sectors. On average, a conflict country's exports decrease by around 6% relative to the country's internal consumption one and two years after civil conflict, respectively. In columns (2)–(5), we test for heterogeneity across sectors by restricting the sample to trade flows from the respective sector.[46] Overall, the effect of civil conflict

[46]Note that the gravity equation is separable by sectors as outlined in Yotov et al. (2016) and hence Equation 9 can be applied separately by sector.

on international trade is quite heterogeneous. Agricultural exports only suffer slightly one year after conflict with an effect that is barely statistically significant. Exports of mineral goods, however, are significantly reduced by around 21% one year after conflict. For manufacturing exports, we also find a significant reduction of 6%. Interestingly, fuel exports do not appear to decline at all during civil conflict. This could, on the one hand, indicate that importers are so dependent on fuel imports that trade flows continue even in the presence of civil unrest. On the other hand, fuel exports are an important financing tool for civil wars (Bazzi and Blattman, 2014). Therefore, the government as well as the rebels are eager to maintain fuel exports during conflict. Hence, our results suggest that, on average, ongoing civil conflicts depress national exports *relative to internal consumption*.[47] Note however that all these estimates likely constitute lower-bound estimates of the actual effect, since we estimate reductions in international trade *relative to* internal trade. Hence, as internal trade is likely to also be negatively affected by civil conflict, our results mirror the additional deterioration of international trade flows.

Appendix 3-F: Relocation Propensity

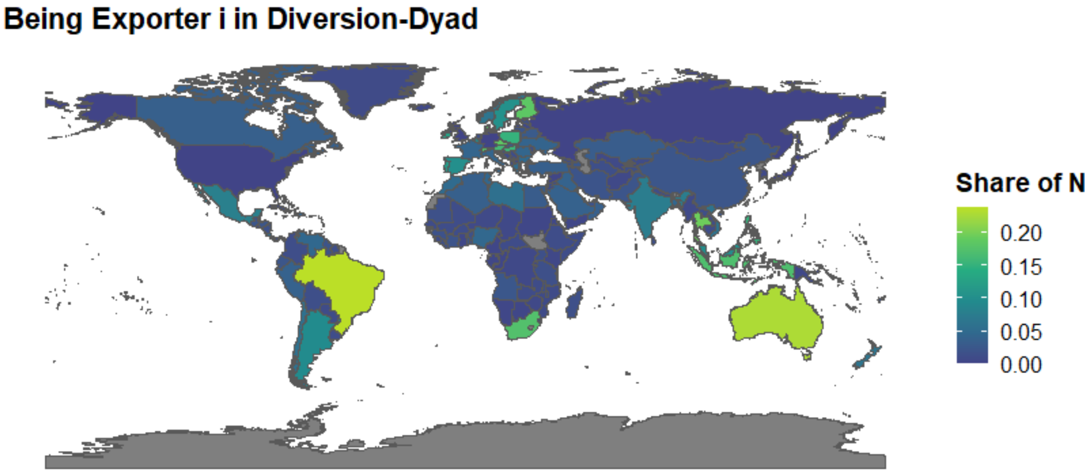
Figure 3-F1 gives some intuition to the distribution of our relocation propensity variable. The two maps report the geographic distribution of the likelihood to appear as exporter i or importer j in a relocation dyad. The odds of being affected as an importer, i.e., having a relevant trade partner starting a civil war, are distributed quite homogeneously across the globe. While East Africa and the Middle East stick out with a slightly higher propensity and Europe appears only rarely affected, the overall propensity is fairly equally distributed across all regions. The likelihood that in at least one of a country's trading sectors a relevant exporter starts a civil war for most countries lies close to 5 percent. The picture is different when looking at the likelihood of being an affected exporter, i.e., the odds that a country with a similar export structure starts a civil war. Here, Brazil and Australia stick out with a very high likelihood of around 20 percent, followed by South Africa, Argentina, Indonesia, Eastern Europe and Scandinavia. On the other hand, the USA and several other countries, especially in Africa, Asia and Central Europe, are almost never coded as exporters benefiting from trade relocation.

In Figure 3-F2, we further investigate the determinants of the similarity and relevance characteristics. Here, we regress the likelihood that a country is a similar exporter (Panel (a)) or a relevant importer (Panel (b)) to a conflict country k on the common gravity variables. As is to be expected, these variables only play little role for the similarity characteristic. Among the bilateral variables, only inverse distance and an indicator for common legal origins are significantly positive, which likely mirrors local clusters of re-

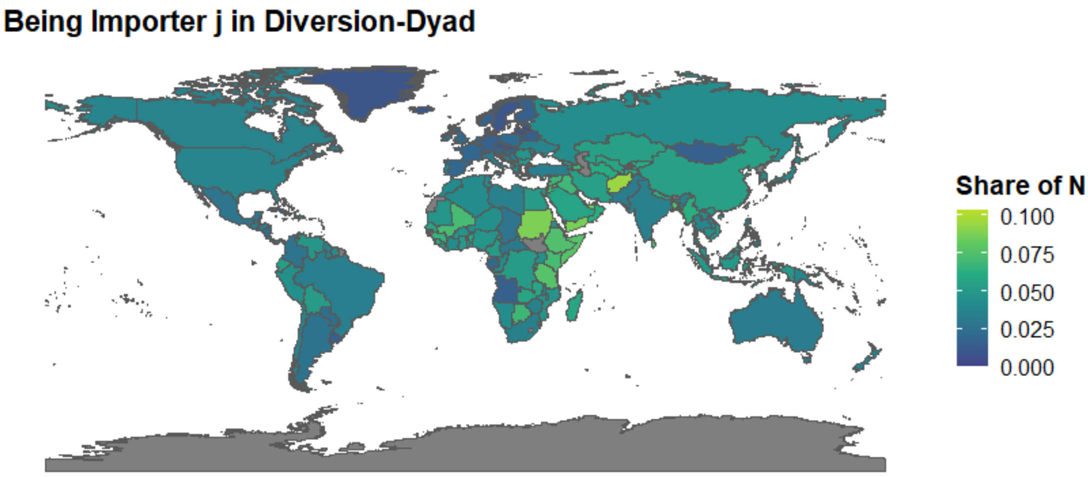
[47]Note again that the interaction term in Equation 9 mandates this interpretation.

Figure 3-F1 – Geographic Distribution of Diversion Propensity

(a) Exporter i



(b) Importer j

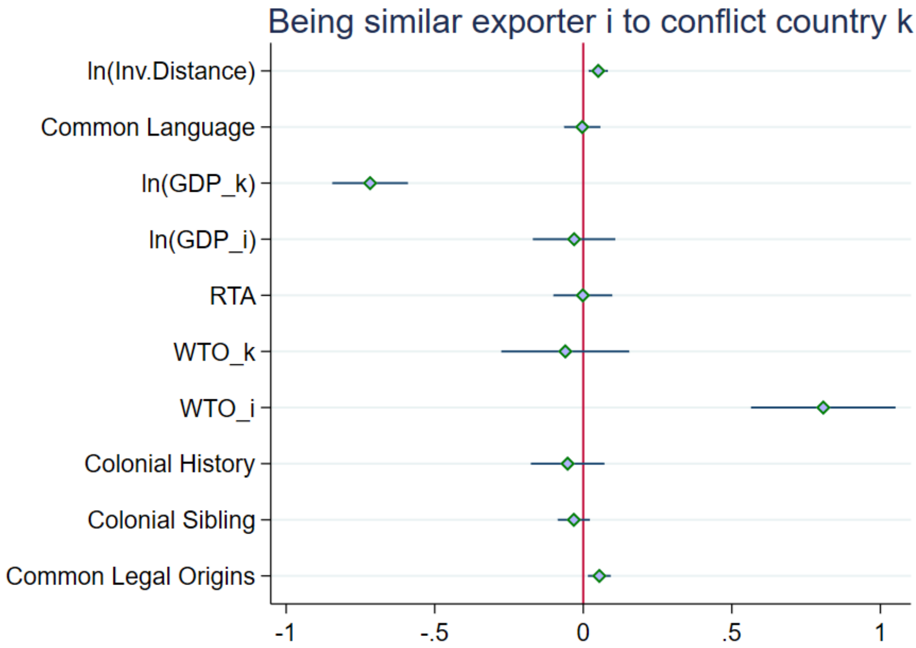


Notes: This figure shows a country's likelihood to appear as an exporter i or importer j in a dyad with positive trade relocation propensity. Panel (a) shows the geographic distribution of the likelihood to be an exporter affected by trade relocation, while panel (b) plots the same distribution for importers. The different shades display the share of a country's observations that it is coded as having a positive relocation propensity. For example, in panel (a) a share of 0.1 means that 10 percent of a country's export observation across all sample years and all importers are coded as being an exporter profiting from trade relocation due to civil conflict in some country k.

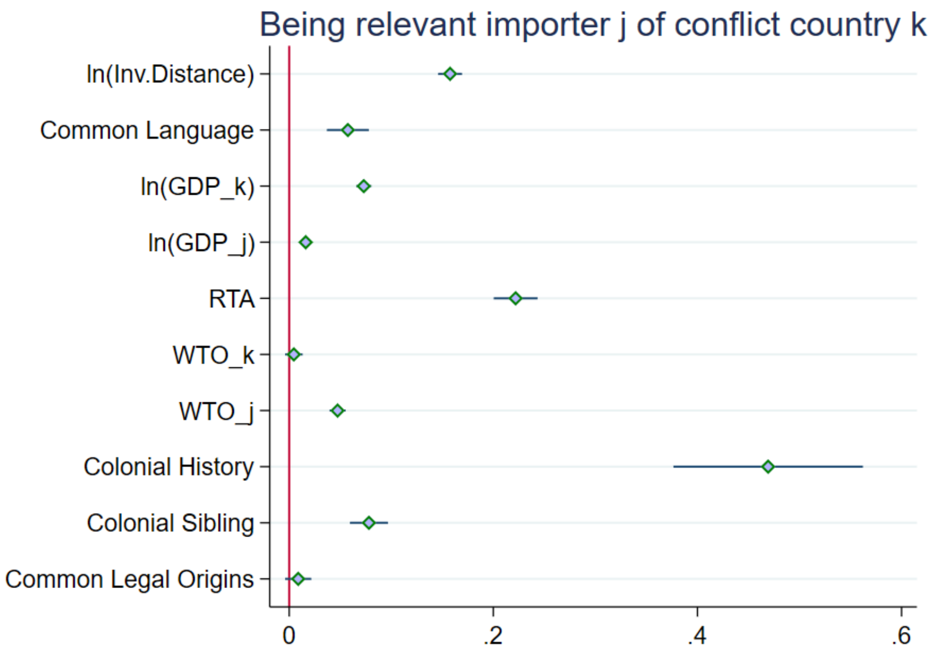
sources or similar production techniques based on the legal environment. Furthermore, conflict countries have on average a lower GDP, while beneficiary exporters are more likely to be WTO members. For the relevance characteristic however, most gravity variables turn out highly significant and with the expected sign. Important trade partners of a conflict country are on average closer and have the same official language or colonial history. Similarly, higher economic masses of both countries j and k as well as an existing Regional Trade Agreement between the two are significant determinants of the

Figure 3-F2 – Explaining Propensity of being i or j

(a) Being Similar Exporter i



(b) Being Relevant Importer j



Notes: This figure reports the results from regressing the status of being a similar exporter i (Panel a) or a relevant importer j (Panel b) for conflict country k on the most common gravity variables. All regressions include importer, exporter, and year fixed effects. Standard errors are clustered at the dyad level. Lines depict 95% Confidence Intervals.

relevance characteristic. This emphasizes that, as is to be expected by construction, the relevance characteristic we identify is strongly related to the classical determinants of bilateral trade.

Appendix 3-G: General Equilibrium

We analyze three case studies in a General Equilibrium (GE) framework. These case studies allow us to focus on specific conflicts and accurately trace the relocation effects. As recent examples of significant violent episodes, we focus on (*i*) the peak of clashes between the Marxist "Revolutionary Armed Forces of Colombia–People's Army (FARC)" rebels and far-right paramilitary forces "United Self-Defense Forces of Colombia (AUC)" in Colombia in the 1990s and until 2005, (*ii*) the Ukrainian civil war from 2014 to present, and (*iii*) the violent 1990s in Türkiye where the "Kurdistan Workers' Party (PKK)" fought for local independence. The case studies were selected based on the significance of the respective conflict shocks (at least two years of violence with more than 1,000 battle deaths) among a handful of countries where international and internal trade data were available during and before or after the conflict period. For these three cases, we proceed in two steps. First, we construct an indicator variable for each case that takes the value of one for all dyads that include the respective conflict country as an exporter during years of peace. We then regress trade on the interaction of this variable with an indicator variable for international trade flows including country-year and dyad fixed effects similar to Equation 9. From this, we receive an estimate for the effect of peace on the respective country's exports relative to its internal consumption of self-produced goods.[48]

Second, we use the respective estimates and compute hypothetical trade changes in the general equilibrium during a conflict-year. Following Baier et al. (2019), we apply a one sector Armington-CES model, assuming a constant trade elasticity of $\theta = 4$.[49] This computation generates counterfactual trade flows for all sample countries in case the civil war in either Colombia, Ukraine or Türkiye had not happened. Finally, the comparison of hypothetical to actual trade flows provides an estimate for the effect of one country's civil war on its and *all other countries'* trade. These computations require a symmetric dataset; i.e., trade flows must be provided for all potential dyads in the sample in every year and always in both directions. Further, for all countries, information on positive internal trade flows must be included in every year. We follow Baier et al. (2019) to

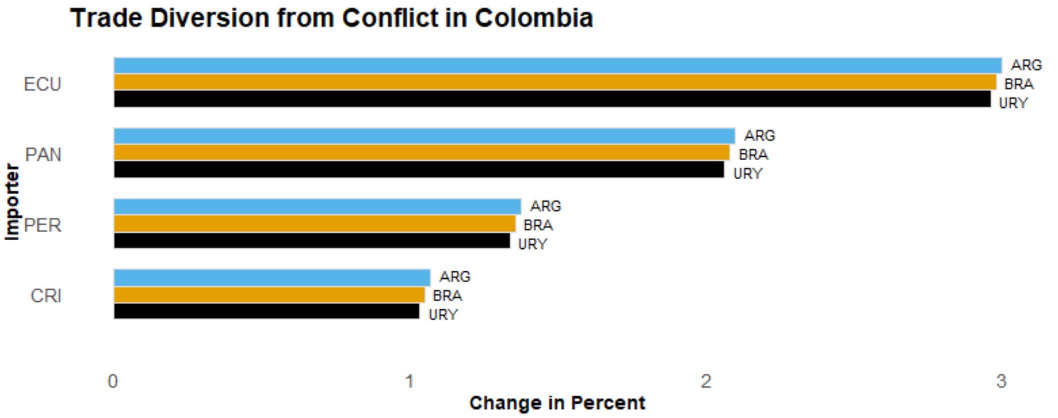
[48] The results are equivalent when estimating the effect of conflict and then computing hypothetical trade flows during a peace year. However, when comparing the hypothetical conflict scenario to the actual peace outcomes, the resulting diversion estimates would have to be inverted to show the trade diversion effects from conflict as opposed to the trade diversion effects from peace. To present the unchanged results, we hence estimate the effect of peace instead of conflict for our General Equilibrium computations.

[49] We run these computations via the "ge_gravity" Stata Command provided by Thomas Zylkin and discussed in Baier, Yotov and Zylkin (2019).

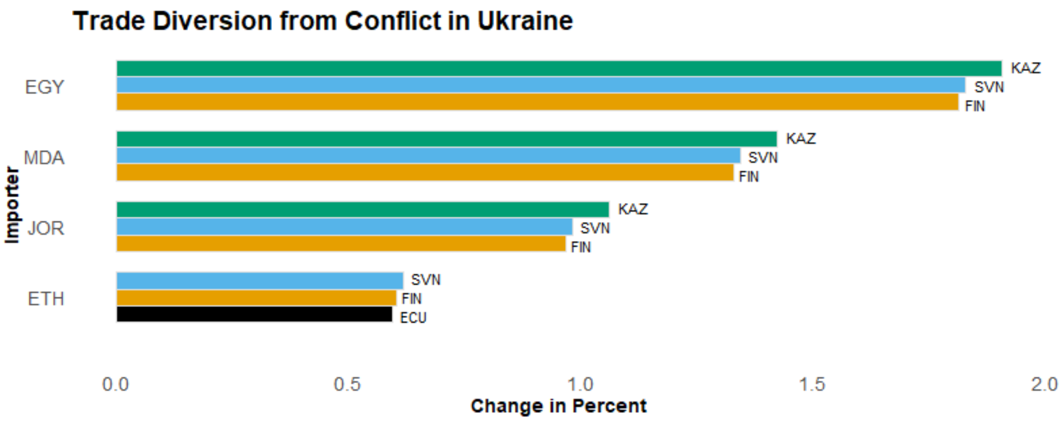
adjust our main dataset accordingly.[50] In the end, we obtain a dataset that contains 81 countries and covers the years 1993–2015.

Figure 3-G1 – GE Results Trade Diversion

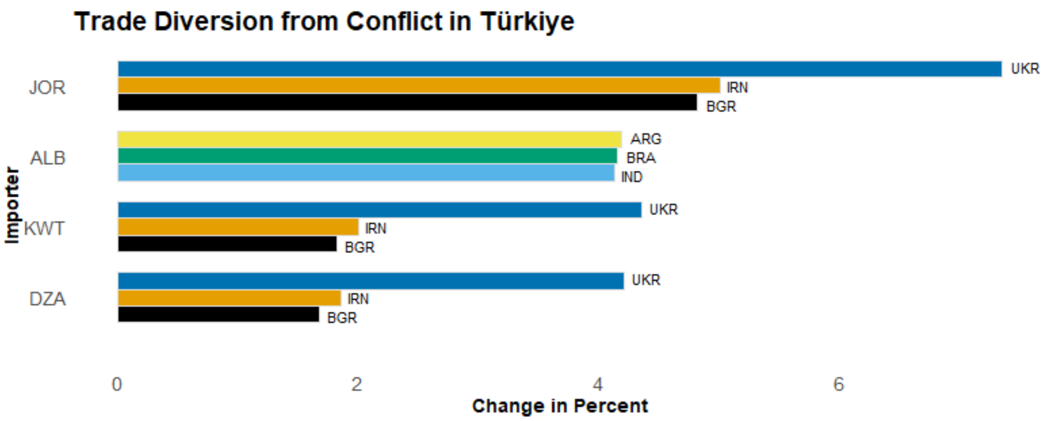
(a) Trade Changes, Conflict in Colombia



(b) Trade Changes, Conflict in Ukraine



(c) Trade Changes, Conflict in Türkiye



Notes: The graphs report the estimated trade changes in the general equilibrium due to the civil wars in Colombia (Panel a), Ukraine (Panel b), and Türkiye (Panel c). See Table 3-A5 for the respective Partial Equilibrium results.

[50] Appendix 3-D provides more details on the dataset construction.

Figure 3-G1 presents the results of the GE analyses for our three case studies. Each panel of Figure 3-G1 reports, for each of the four importers most affected by trade diversion, the export changes for the three origins with the largest export changes. In the case of Colombia, for example, primarily its neighbors Ecuador, Panama and Peru increased imports from various countries by up to three percent. The picture of the beneficiary exporters for Colombia is quite homogeneous. To all four destinations, Argentina increased its shipments the most, closely followed by Brazil and Uruguay. The effects of the civil wars in Ukraine and Türkiye had a larger geographic reach, as both countries are important exporters for Northern African and Middle Eastern countries. During the civil war in Ukraine, mainly other regional exporters increased their shipments to the former destinations of Ukrainian exports. Kazakhstan, Slovenia, and Finland register the largest export increases to Egypt, Moldova and Jordan. In response to the civil war in Türkiye, Jordan, Albania, Kuwait and Algeria registered the largest trade diversion effects. Here however, the group of affected origins is more heterogeneous. Jordan, Kuwait and Algeria mainly turned towards Ukraine, Iran and Bulgaria to substitute for Turkish shipments. Albania, on the other hand, instead increased its shipments rather from large but non-regional suppliers, i.e., Argentina, Brazil, and India.

Trade diversion does however not totally mitigate the welfare loss from civil conflict. In Figure 3.3, we report the estimated international welfare changes in response to the civil conflicts in Colombia, Ukraine and Türkiye. We mostly find negative welfare changes, with the biggest losses borne by the conflict-countries as well as the importers mainly affected. Indeed, even the benefiting exporters bear welfare losses, meaning that their increase in exports could not offset the loss of imports from and exports to the conflict countries. Overall, this emphasizes that, even though trade diversion can mitigate the effects of conflict, global welfare still decreases.

Chapter 4

Human Capital Shocks and Structural Transformation: WWI and Weimar Germany

4.1 Introduction

Population shocks can have large and sustained consequences for local economic development. The literature provides ample evidence that sudden labor supply shocks from out-migration (Hornbeck and Naidu, 2014, Andersson et al., 2022), pandemics (Brainerd and Siegler, 2003, Voigtländer and Voth, 2013a, Karlsson et al., 2014), natural disasters (Vigdor, 2008, Boustan et al., 2012) or wars (Voigtländer and Voth, 2013b) can persistently change economic trajectories. Yet, identifying the causal effect of such labor supply shocks is challenging. For instance, natural disasters and wars usually affect the local infrastructure and capital stocks along with the local population. Similarly, we must assume that individuals' decisions to migrate are endogenous to local economic conditions. The closest setting to an exogenous negative population shock that does not directly affect other economic inputs are pandemics. The Black Death, for instance, killed an estimated 40% of the population in the Middle Ages and had vast economic consequences for affected locations (Voigtländer and Voth, 2013a). But can we directly infer from shocks in pre-industrial economies to more modern settings? Whereas pandemics still leave their toll on today's economies, their scale is not comparable to the Black Death. For instance, about 2% of the population died during the Spanish Flu in the early 20th century (Karlsson et al., 2014), and mortality from Covid-19 was significantly below 0.5% of the population in most countries (Johns Hopkins University, 2022). Very likely, these later pandemics do not constitute a human capital shock as transitory as the Black Death to understand how industrialized economies respond to vast human capital reductions.

We analyze another human capital shock to a modernizing economy that had severe demographic consequences: German casualties during World War I. During the four years of war, an estimated two million German soldiers (about 15% of the male population between 20 and 45) died, and many more were wounded or incapacitated. Notable in our setting is the fact that, different from most other violent settings, the shock did barely affect the capital stock of the economy through war-related destruction.[51] Except for a short period at the beginning of the war, all combat took place outside of German borders, and aeronautics was not advanced enough for large-scale bombardment of industry and infrastructure behind the front lines. Lastly, the war occurred at a crucial time in

[51] See Broadberry and Harrison (2005) for an estimate of the war related destruction of physical assets in the combatant countries. The authors report a 3.1% rate of destruction of German domestic assets as a share of prewar assets.

Germany's economic development. At the turn of the century, Germany was in the midst of following its European neighbors in the structural transformation amid the second wave of industrialization. Whereas Germany was still a largely primary (agricultural) economy in the middle of the 19th century, employment in the secondary (manufacturing) and tertiary (trade and service) sector grew rapidly from German unification in 1871 onwards. A host of labor-saving and labor-assisting technologies became available that transformed the routines of manual labor (Atack et al., 2019). Women started entering the regular workforce in large numbers, often taking up new tasks opened up by the de-skilling of labor brought by the new technologies (Brown and Philips, 1986). In addition, the large-scale promotion of higher education transformed Germany's workforce and society. A new form of White Collar workers emerged. These were well educated and high-skilled individuals who took over lower managerial tasks within firms, ending the aspirations of less educated Blue Collar workers to climb up the ranks with more on-the-job experience (Kocka, 1981). These White Collar Workers were referred to as "Angestellte" vis-á-vis the Blue Collar "Arbeiter." They received better working contracts with higher pay, better job security, and more vacation days and turned into Germany's new social upper middle class.

This chapter investigates how the population shock to the German economy in World War I affected the structural transformation at the local level. We estimate Continuous Difference-in-Differences estimations following Callaway et al. (2021), identifying the average causal response of a marginal increase in a location's casualty rate on various outcomes. Our identification approach relies on the assumption of "conditional strong local parallel trends." We therefore need to assume that, conditional on covariates, locations with marginally different treatment "doses" would have followed the same trend absent the treatment. For this assumption to hold, we introduce treatment quartile fixed effects in all our regressions. This is, we always estimate the marginal effect of an increase in the treatment dose *among counties with a similar treatment intensity.* Therefore, our identification approach allows for selection into higher or lower treatment intensities, i.e., quartiles, but demands a quasi-random distribution of treatment doses within each quartile. In addition, we further account for local institutional and economic factors that may have influenced the casualty rate indirectly via differences in drafting ratios or the deployment to more or less lethal battle scenes of local regiments. Tests for parallel pretrends suggest that the "conditional strong local parallel trends assumption" is likely to hold for our treatment variable.

To derive our treatment variable, we geocoded the birthplaces of 7.2 million German soldiers who were mentioned in the official casualty lists published by the German army during the war. We find significant geographic variation in the casualty rate, i.e., the number of list entries divided by the pre-war male population. We link these data to detailed census records for the pre- and post-war period. Most importantly, we digitized the German business and agriculture census waves from 1907 and 1925, which provide detailed sector level data at the county level. Among other things, we observe the number of employees across 18 sector categories as well as agricultural landholdings by size before and after the war. We find that counties with a higher casualty rate exhibited lower employment growth, especially in the tertiary sector that was on the rise before the war. Crucially, this slower employment growth goes hand in hand with a social transformation: harder hit counties show significantly smaller growth rates in the emerging upper middle class, i.e., Germany's new social strata of White Collar "Angestellte."

A deeper analysis of the mechanisms behind this effect suggests that the main reason behind the decline of the tertiary sector and the upper middle class in counties that felt a higher toll of the war lies in the harder substitutability of White Collar workers. Germany's "Angestellte" were highly educated individuals who enjoyed tertiary education and extensive on-the-job training to specialize in specific tasks. Blue Collar workers, on the other hand, were often barely literate and employed for repetitive manual tasks that seldomly required specialized knowledge, especially in the industrial sector. We find significant evidence that in the industrial sector, war casualties did not affect the employment trend. This is because firms in this sector recruited workers from the informal daily laborer as well as small-scale farming sector to rapidly fill vacant positions. We find that especially subsistence farms disappeared in harder hit counties, and that counties with higher casualties were more likely to concentrate land holdings in bigger farms, and substitute manual labor through mechanization. Yet, these labor reserves in the informal sector and in subsistence agriculture did not help replace high-skilled White Collar employment, which was crucial for the emerging tertiary sector. Hence, counties with more war fatalities saw a decline in the tertiary sector and therefore the new upper middle class which this sector employed.

Our findings contribute to three strands of the literature. First, we contribute to the literature on the effects of negative labor supply shocks. In terms of technology adoption, Hornbeck and Naidu (2014) and Andersson et al. (2022) find that areas more affected by out-migration have higher rates of technology adoption in agriculture. For individual-level outcomes, Goldin (1991) and Goldin and Olivetti (2013) find that the labor supply of women increased due to male mobilization in World War II. Acemoglu et al. (2004) show that this increase in female labor force participation lowered wages because female labor became a better substitute for male labor after the war. In a closely related setting to our study, Boehnke and Gay (2022) and Brodeur and Kattan (2022) show that French casualties in World War I as well as US casualties in World War II, respectively, led to higher female labor force participation after the war. Gay (2023) confirms the positive effect of WWI casualties on female labor force participation in France, and additionally shows that the excess labor demand improved attitudes towards working women. Closer to this chapter, Eder (2022) finds that farmers moved into low-skilled manufacturing jobs in Austria where casualties from World War II were higher. Our findings extend this line of the literature by providing a more detailed look into within-sector changes, shedding light on economic transformations within the agricultural sector, and providing first evidence for the decline of tertiary employment related to human capital shocks.

Second, we contribute to a growing literature which examines the impact of World War I on societies in the interwar period. Acemoglu et al. (2022) use Italian casualties in World War I to argue that the death toll in the war was a major cause for the rise of communism in Italy, which in turn caused the counter-movement and rise of the right-wing government led by Mussolini in the mid 1920s. The impact of local exposure to the war on the rise of the Nazi party in Germany is examined in a similar fashion by Koenig (2023) and De Juan et al. (2023). Both papers establish a direct connection between World War I casualties or the return of veterans to their homes and increased vote shares of extreme right and the Nazi Party in the Weimar Republic. They attribute these effects to the organizational skill of right-wing veterans' organizations and the common experience of suffering and human loss associated with casualties. Our findings suggest another social transformation that WWI entailed: the decrease of Germany's new upper middle class.

Third, several recent studies rely on the raw data set of German casualty lists in World War I from Verein für Computergenealogie (2019a) that we also use in our analysis. For instance, De Juan et al. (2023) investigate the effect of casualties in a county on voting behavior in the post-war era. Ciccone (2021) uses casualty list entries as a negative, exogenous local population shock for the state of Württemberg. Kersting and Wolf (2021) use the names and birthplaces of soldiers to identify patterns in first names as signs of nation-building after German unification in 1871. All these studies rely on the original geocoding provided by Computergenealogie (2019a), which suffers from numerous drawbacks due to imprecise codings. We improve this data set by providing a new and more accurate geocoding for more than 84% of observations in the data set. Hence, we hope that our improved data can also serve as a resource for other scholars working with this data set.

This chapter proceeds as follows. Section 4.2 provides a brief overview of the historical background. Section 4.3 presents our data, before Section 4.4 describes our identification strategy. We then discuss our main results on economic effects in Section 4.5, and investigate the mechanisms behind them in Section 4.6. In Section 4.7, we conduct a number of robustness tests. Finally, Section 4.8 concludes.

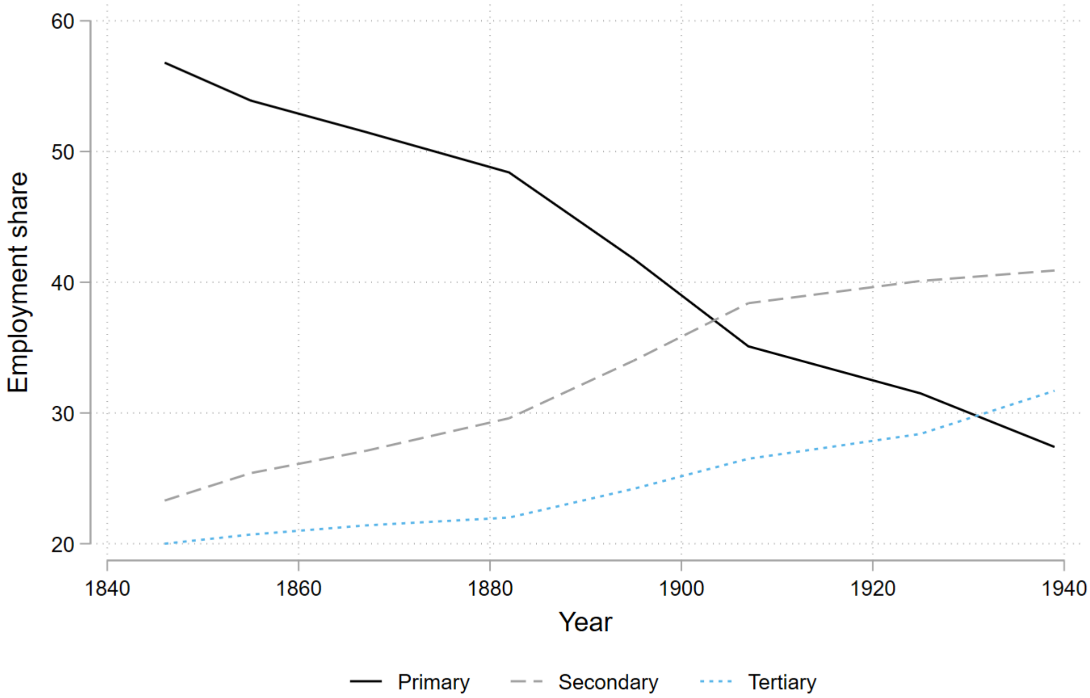
4.2 Historical Background and Theory

The First World War lasted from July 1914 to November 1918 and marked the first fully modern and global war. It further occurred at a critical time of European economic development. Before years of deadly trench warfare cost the lives of millions of soldiers and froze production capacities for war production, Europe experienced almost two centuries of rising incomes amidst two industrial revolutions and the first wave of globalization. This section examines the state of the German economy prior to the outbreak of war, drawing comparisons with other European nations at the time, and delves into the transformative consequences that the war imposed on Germany's local trajectories of industrialization.

Germany's pre-war economy. Germany started industrializing rather late. While large parts of labor in France and the United Kingdom were already employed in the industrial sector at the turn of the 19th century, the German economy was still undergoing its main transformation. Prior to the unification in 1871, the collection of small German states could hardly agree on large-scale economic policies. In addition, despite early efforts of economic integration, e.g., in the form of a German Customs Union called the "Deutsche Zollverein" in 1834, numerous individual currencies and other trade restrictions kept complicating economic cooperation among the German states until 1871 (Stolper, 1950). Therefore, most industrialization efforts remained small-scale, limited to states' borders, and production stayed largely agricultural. Only after the unification, structural transformation occurred at unprecedented rates (see Figure 4.1). Farmers starting seeking industrial employment, and a novel tertiary sector emerged. In particular this tertiary sector experienced unprecedented growth rates, driven especially by the expansion of the railway system in the late 19th century (see, e.g., Hornung, 2015, for an overview). Overall, even though Germany's industrial revolution occurred almost a century later than in England, its quick progress let Germany overtake its industrial role model in terms of industrial production at the eve of the First World War (Stolper, 1950).

Germany not only industrialized late, but also differently. Early on, German states put high emphasis on education. Already in 1820, most states established technical higher education institutions that focused on training engineers for specialized industrial production. At the time of the German unification, these higher education institutions evolved into the first technical universities like the "Technische Hochschule Berlin," which are still among Germany's top universities today (Kocka, 1981). The promotion of education resulted in a stratification into two complementary but socially competing working classes: White Collar employees ("Angestellte") and Blue Collar workers ("Arbeiter"). Figure 4.2 illustrates the emerging importance of White Collar workers. Especially in the tertiary sector, firms transformed their organization of labor: Blue Collar "Arbeiter" specialized on repetitive manual tasks while the bureaucratic responsibilities of, e.g., quality control, logistic and material management, preparing shift schedules, and reporting to firm owners, went almost entirely to the White Collar "Angestellte." This form of within-firm task separation, which finally culminated in different within-firm standings in terms of wages,

Figure 4.1 – Structural Transformation in Germany



Notes: This graph illustrates the sectoral composition of employment in Germany across the census waves in 1846, 1855, 1867, 1882, 1895, 1907, 1925, and 1939 for the German Confederation (pre-1871) and German Empire (post-1871). Data from Rahlf (2022).

employment security, and freedoms, occurred at a rate incomparable to other economies. Crucially, this new working class cut Blue Collar workers' aspirations short. While it was common for more experienced workers to rise in the ranks with on-the-job experience, these promotions became reserved for people with higher education and specific training. This was especially common in the emerging tertiary sector, where employees required specific skill sets that typical workers would not acquire outside higher education institutions (Kocka, 1981).

Over time, the separation of Blue Collar and White Collar workers expanded to a novel social segregation outside work. The better paid, higher skilled, and more valued White Collar workers started viewing themselves as the new upper middle class in German society. They commonly referred to themselves as "private public servants" ("Privatbeamte"), i.e., employees who, because of their type of employment, earned special treatment by society (Kocka, 1981). Overall, Germany experienced a seminal economic transformation during the decades before the First World War. Political change, especially in the form of the German unification, new-found economic liberalism, and high investments in education, produced a sophisticated economy with strong industrial and service sectors, a rich middle class with a special status in society, and more outward-looking, globalized means of production (Stolper, 1950). This period of economic transformation did however come to an abrupt halt with the outbreak of WWI in July 1914.

World War I. The "Central Powers" of Germany, Austria-Hungary and the Ottoman Empire fought the Allied countries of Great Britain, France, Russia and later the United States in a deadly trench warfare where an estimated 10 million of the mobilized 60 million soldiers perished. After initial German advances in the West, offensives on both sides quickly stalled due to technological advances and outdated military tactics. From Flanders in Belgium to the Franco-Swiss border in the South, millions of soldiers died fighting for marginal advances back and forth. On the Eastern Front, German forces repelled an initial Russian advance into East Prussia in August 1914, and fought the following four years exclusively on the territory of the Russian and Austrian Empires. The conflict in the East ended with the Russian surrender and the treaty of Brest-Litvosk in March 1918. After the entry of the United States into the war in April 1917, the situation of the Central Powers quickly deteriorated and a ceasefire was signed in November 1918. The losing Central Powers faced harsh repercussions: The Ottoman Empire and Austria-Hungary were broken up. Germany was stripped of its colonial possessions and lost territory to France, Belgium, Denmark, Lithuania, Poland, and Czechoslovakia.

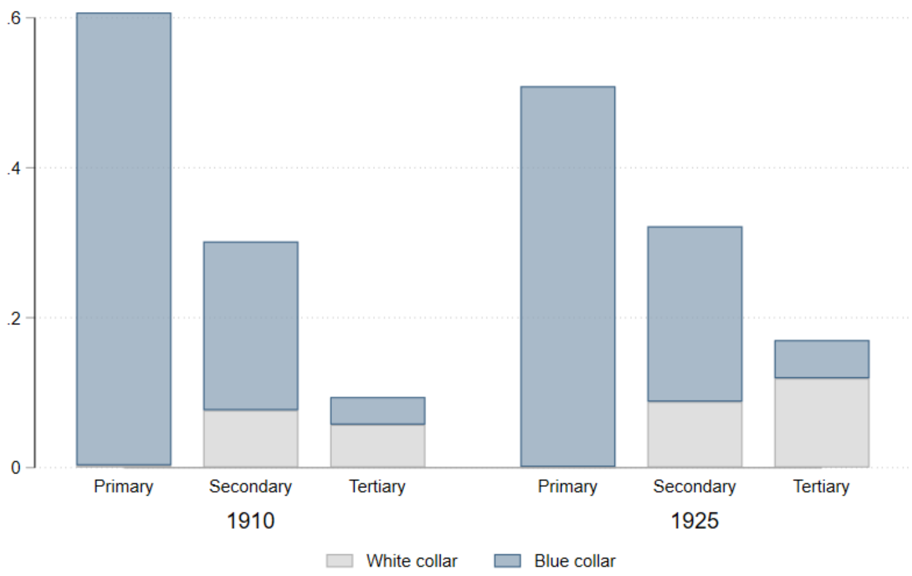
The German war effort involved an almost universal mobilization of men aged 17–45.[52] However, the need for manpower was accompanied by a need for large amounts of military supplies such as guns and ammunition. Over the course of the war, the drafting of soldiers became more and more unequal. Industrial centers became largely spared to keep up military production, while especially agricultural areas had to send their men to war (Afflerbach, 2018). When analyzing the economic consequences of WWI's death toll, it is therefore crucial to account for pre-war economic differences and related differences in drafting ratios, as we discuss in more detail below.

The Consequences of the War. The goal of this chapter is to investigate how the death toll of the First World War impacted the German labor markets amid the structural transformation out of agriculture into modern means of production. Historians already discussed one important economic and social transformation due to the toll of war. The lack of male labor led women start taking up employment in previously male-dominated industries. For instance, the female employment share rose from 7% in 1914 to 23% in 1916 in the chemical industry, and from 24% to 55% in the electrical industry in the same time period (Helfferich, 1925).

Our analysis investigates how local businesses dealt with the war's human capital shock amid the economy's structural transformation. In this analysis, we focus on the

[52] As the German political and military leadership expected a short war, the mobilization rate was high even in war related industries. According to Rasch (2022), on average 30% of the workforce in the industrial centers of the Ruhr were recruited, and in cokeries, this number even went up to 40%.

Figure 4.2 – Structural Transformation and White Collar Workers



Notes: This figure depicts the share of white collar workers ("Angestellte") and blue collar workers ("Arbeiter") in total employment by main sector according to the 1907 & 1925 business censuses.

two defining aspects of the structural transformation in Germany: the transition from agricultural to industrial production and the provision of services in the tertiary sector, and the emergence of White Collar "Angestellte" as the new upper middle class. We theorize that the war's death toll was mainly felt in the White Collar dependent tertiary sector for two reasons. First, White Collar workers were, together with agricultural employees, more likely to be drafted as Blue Collar workers were needed to keep up war production. Second and most importantly, Blue Collar workers lost in war were easier to replace than the well educated and specifically trained White Collar workers.

Since we lack detailed drafting data, our analysis will focus on the latter channel. As we show below, counties that experienced marginally higher fatality rates showed lower growth rates in the tertiary sector and the number of White Collar workers because there existed no able labor reserve to fill open vacancies. Blue Collar workers were largely uneducated and, for most tasks, not specifically trained. When vacancies were left open after the war, these were easily filled with agricultural workers or previously unemployed individuals who were quickly introduced to the repetitive tasks in industrial production. It was much more difficult to replace White Collar workers. While this probably was possible to some extent in the industrial sector where experienced Blue Collar workers could move up the ranks as it was common before the educational revolution in Germany, the tertiary sector faced harder obstacles. Most of the tasks required specific skills and higher education and hence did not allow for an easy replacement by employees who were not directly trained for them. We therefore hypothesize that counties with higher war fatalities see their tertiary sector and upper middle class deteriorate, hence falling behind in the general German trend of structural transformation.

Note finally that two other tolls of the war impacted the German economy. First, the peace treaty obliged Germany to pay high reparations to the Allied countries. These consisted of direct monetary payments as well as deliveries of coal, steel and other industrial goods. While these reparations amounted to a significant share of German industrial production, Germany's industrial base was left largely untouched - with the exemption of the 1923 occupation of the Rhineland. In our robustness section below, we show that our main results are not driven by counties in the temporarily occupied Ruhr territory. Most crucially, there is no evidence that reparation payments correlated with the war's death toll. Hence, it is unlikely that the reparation effects bias our estimations. Second, hyperinflation struck the German economy in 1923, which additionally delayed post-war economic recovery. While also the hyperinflation was a national phenomenon and hardly varied across counties, it is reassuring that most of the outcomes we use in our analysis stem from 1925 and later – a relatively stable time for the German economy in the interwar period up to the Great Depression starting in 1929.

4.3 Data

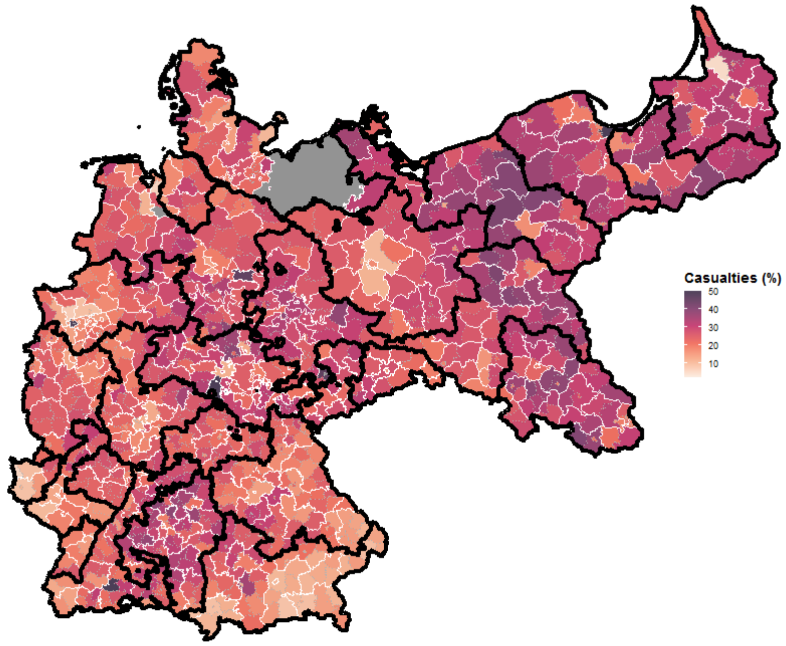
The fact that the war occurred in the midst of Germany's transition to a modern industrial economy allows us to discover local disparities in industrialization. To measure the local shock of the war along with local economic trajectories, we collect a wealth of data on war casualties, employment statistics, mechanization, land ownership, and wages.

Unit of Observation. Our unit of observation are 725 of the 954 German counties ('Kreise') from the 1907 census inside the German post-war borders. The county constitutes Germany's fourth administrative layer. While we leverage data from different years and census waves, we match all to the constant 1907 county boundaries. To do so, we proceed in two steps. First, we link all counties whose borders remain unchanged from the first year of observation in 1907 until our last year of observation in 1933. Second, for counties that split or merge over time but where the outside boundaries remain unchanged, we trace all administrative reforms and aggregate these counties to the biggest common spatial entity. From these two steps, we recover 725 stable counties. For the remaining 229 counties, we were unable to link them over time due to exhaustive redrawings of their boundaries amid large scale administrative reforms, especially in Baden and Thuringia.

Casualty Data. We derive our main explanatory variable from the number of WWI casualties based on the official war records of the German Imperial Army, the so called

'Verlustlisten' (loss lists). Digitized by Verein für Computergenealogie (2019a), these lists contain 8.5 million entries with information on each soldier's name, birthplace, rank, regiment, and the type of casualty reported from slightly wounded to dead.[53] For this study, we geocoded the birthplaces of 7.2 million WWI soldiers at the county level.[54]

Figure 4.3 – WWI Casualty Rate



Notes: This map depicts the share of WWI casualties over the 1907 male population at the county level. Thick black lines depict army district borders. Thin grey borders depict regiment district borders. Thin dotted gray lines depict county borders. Grey areas denote missing data.

Systematic information on the casualty status – that is, whether the entry constitutes a dead or wounded soldier – has been digitized by De Juan et al. (2023). In combination with the population census of 1907, we calculate the share of (i) total casualties and (ii) killed soldiers of the pre-war male population as our two main explanatory variables. Figure 4.3 shows the distribution of the casualty rate, along with the army (thick black lines) and regiment (thin grey lines) districts that oversaw drafting and deployment. Casualties were higher in the more rural areas of East and West Prussia, Pomerania, Posen, and Silesia, and lower in more urban areas such as Berlin, Danzig, the Ruhr area

[53] Note that not all of this information is reported for all entries, and only information on soldiers' names and birthplaces have been digitized systematically.

[54] To this end, we exploited the hierarchical structure of loss list entries that typically consist of the name of the municipality of birth, followed by the county or administrative district and matching birthplace entries with a list of German municipality names in 1910 from Schubert (2020). The remaining uncoded 1.3 million entries were of too broad resolution (e.g., "Bayern"), inconclusive (e.g., "Neustadt", of which there exist more than 40 in Germany), or outside the 1910 German boundaries (e.g., "Zürich"). We describe the geocoding process as well as improvements made vis-à-vis the original coding provided by Verein für Computergenealogie (2019a) in detail in Section 4.9.

as well as in Southern Bavaria and the predominantly French speaking parts of Alsace-Lorraine.

The team of the Verein für Computergenealogie (2019a) already undertook an initial effort to geocode these birthplaces. However, they employed a largely automated geocoding process whose precision and reliability is limited. This is in part due to misleading typos in the underlying information of the lists, and in part because the geocoding algorithm was too imprecise. For example, 955 list entries with the birthplace "Zürich" were assigned to a small hamlet with a few dozen inhabitants of the same name in the county of Steinfurt in Westphalia, instead of the large city in Switzerland. In addition, the geocoding algorithm ignored differences in the spatial resolution of the given information. Among others, entries with, e.g., "Bayern" were assigned to the centroid of the state of Bavaria in the lack of more accurate information. Overall, we expect that such mis-codings can lead to systematic errors in the number of casualties when aggregating them to specific geographic units like the county level. We provide a deeper discussion of our data improvements in the Appendix Section 4-A.

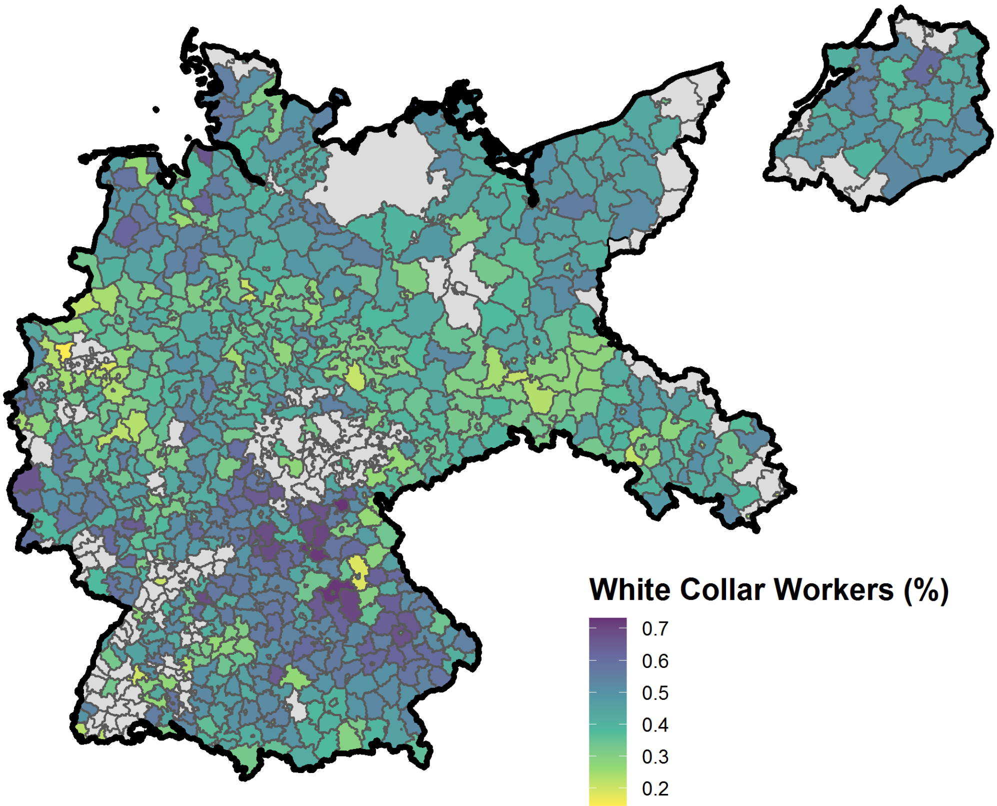
German Business Census. We newly digitized German census data from the German business census waves from 1907 and 1925.[55] The census reports information on company level employment for all companies in Germany. It distinguishes the three main sectors agriculture, industry, and trade, and reports data separately for 18 different sub-sectors ("Gewerbegruppen"). We matched the data at the level of sub-sectors across the two census waves, and merged them with our stable 1907 county boundaries.[56] The census data contain information on the number of businesses as well as employees and worker per county and sector. The 1925 business census wave additionally lists the number of female employees and the capacity of machines and power generators that firms employed. This includes data on wind, water, and thermal power generators as well as electric motors.

Importantly, both census waves list the number of employees separately for White Collar workers ("Angestellte") and Blue Collar workers ("Arbeiter"). From this, we code our first main outcome variable as the share of White Collar workers in total employment. Figure 4.4 illustrates the spatial variation in this share of white collar workers. We find significant variation in the local labor composition. The White Collar share varies from roughly 20% in, e.g., the industrial Rhine valley in the West and up to 70% in Northern Bavaria. Note that these numbers only include employees in businesses, i.e., other workers, e.g., farm laborers, self-employed individuals, or public officials were not

[55]We use the business census as compared to the employment census as the former has more detailed information on the number of businesses, sector level employment, White and Blue Collar workers, and mechanization.

[56]We provide detailed information on the different sector classifications and how they are matched in the Table 4-B2. In addition, the Appendix Tables 4-B8 and 4-B9 illustrate the geographic variation in sectoral employment in 1907 and 1925.

Figure 4.4 – White Collar Worker Share in 1925



Notes: This map depicts the share of White Collar workers in total employment according to the 1925 business census at the county level. Grey areas denote missing data.

enumerated. We separately collected data on agricultural laborers outside agri-businesses as explained in more detail below. We unfortunately lack reliable data on self-employed individuals, public officials, and unemployed people.

In our main estimations, we additionally analyze the effect of WWI casualties on the share of labor in industrial/manufacturing and trade/services jobs in a county to investigate how the human capital shock affected Germany's economic transformation away from the agricultural sector. Note finally that the White Collar share and the sectoral labor composition are strongly intertwined. While the agricultural sector mostly employed Blue Collar workers, the trade/service sector relied mainly on White Collar workers, with the industrial/manufacturing sector employing roughly equal shares of both as illustrated in Figure 4.2.

German Agricultural Census. We further digitized data from the German agricultural census in 1907 and 1925. The census contains data on the total number of farms by county, the total amount of plotted land in hectare, and the number of farm workers employed in a county. As is common in German agricultural censuses, it provides this information separately for five different farm sizes: very small (below 2 ha), small (2 – 5 ha), medium sized (5 – 20 ha), large (20 – 100 ha), and very large (above 100 ha). From the 1925 census, we further leverage data on agricultural machine usage for four major types of agricultural machinery: threshing machines, mowers, seeding machines, and cultivators.

Additional Data. We digitized further data on pre- and post-war local daily reference wages ('Ortsübliche Tagelöhne') from the Proceedings of the Ministry of the Interior ('Zentralblatt des Deutschen Reiches') from January 1914 and the first major post-war revision for January 1922 (Reichsamt des Innern, 1914, Reichsministerium des Innern, 1922). Reference wages reflect regional variation in wage levels and the cost of living, and are representative for the typical local wages paid to unskilled workers (Meerwarth et al., 1932). The reference wages were collected for three different age groups (older than 21, 16 to 21, and under 16), as well as by gender. While generalizable and comprehensive wage data are difficult to come by for our period of observation, these daily reference wages resemble the best available proxy for differences in wage levels. Yet, we will abstain from interpreting them as indicators of average wages but view them solely as the indicators of low-skilled wages in the informal sector as which they were collected. For plenty of reasons, e.g., labor market structures, education premiums, or productivity differences, average wages may deviate from wages of low skilled daily laborers.

Finally, we make use of various other available data sources. First, we use local tax data collected by Brockmann et al. (2023) for 1926, the earliest year available from their data. The authors collected data on the local amount of payroll-, income-, corporate-, and wealth-taxes which allow tracing local differences in skilled wages, corporate gains, and wealth. We further extend our dataset by information from Galloway (2007), who collected various data on employment, wages, and agricultural production from earlier Prussian census waves, starting in 1861.

4.4 Identification

Our goal is to estimate the causal effect of counties' WWI casualties on their economic and social trajectories. For this, we rely on Continuous Difference-in-Differences estimations following Callaway et al. (2021), while at the same time accounting for local differences in the institutional framework that determined counties' exposure to the war.

Main Specifications. Using the terminology of Callaway et al. (2021), we require a strong parallel trends assumption to identify the average causal response of our outcome variables to a marginal increase in the "dose" of WWI casualties. This is, to identify how much a marginal increase in WWI casualties (the "dose d") impacts our dependent variable(s), we need to rule out that counties selected themselves into a given dose d. What is more, Callaway et al. (2021) add that the assumption of what we will term "strong *local* parallel trends" is sufficient for identification when interpreting the results as local marginal effects. In other words, we do not need to assume that no selection at all took place. Instead, it suffices to assume that there is no local selection into slightly higher/lower doses d within a given *neighborhood* of treatment intensities as long as we interpret our results as local marginal effects. In our case, this implies that despite acknowledging that counties with casualty shares of, e.g., 10% and 50% are inherently different from each other, we only need to assume that within a range of, e.g., 40% to 50%, the selection into a given dose d was conditionally random to unobserved determinants of our dependent variables.

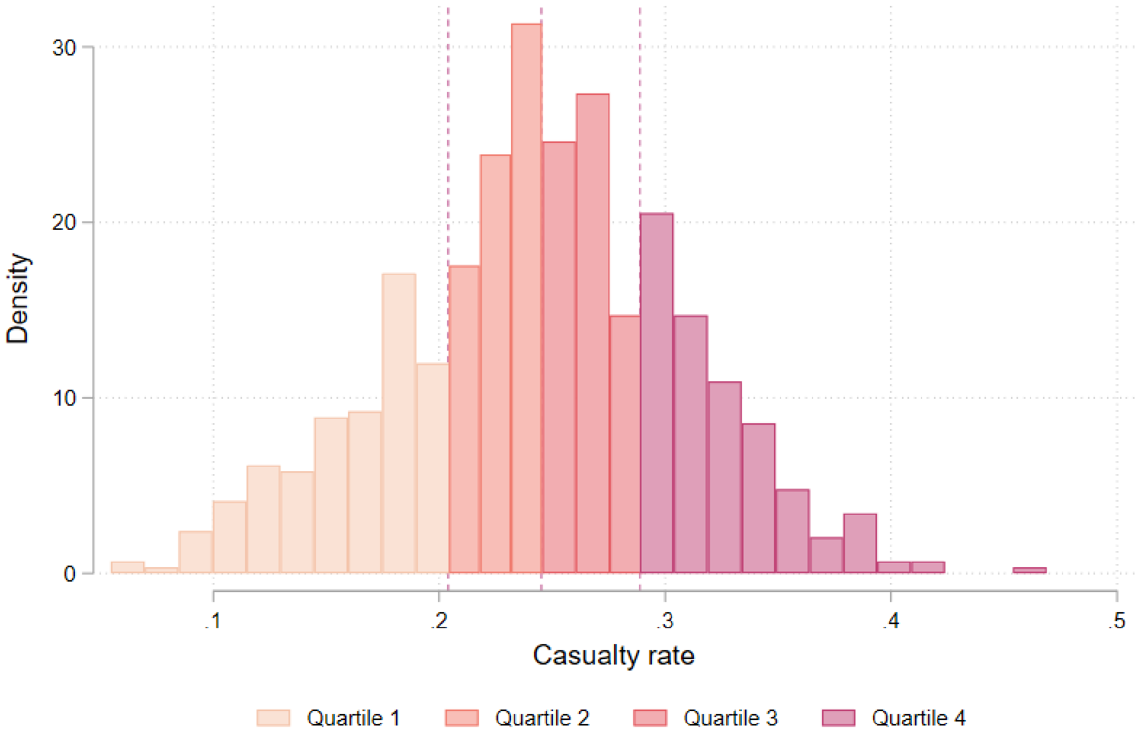
We observe our main outcome variables for each county across two periods t, with $t = 0$ usually referring to the last pre-war census wave from 1907, and $t = 1$ referring to the first post-war census wave in 1925. In addition, we observe each county i's time-invariant treatment intensity (or dose d) $Casualty_i$, which we measure as the share of a county's casualties over its pre-war (1907) male population. We distinguish two different treatment shares based on: (i) the total number of casualties (killed and wounded), and (ii) the number of killed soldiers. Our main regressions for various dependent variables $Y_{i,t}$ take the following form:

$$Y_{i,t} = \pi_i + \gamma P_t + \delta_k Casualty_i^k \times P_t + \beta_1 \mathbf{D_i^k} \times P_t + \beta_2 \mathbf{Z_i} \times P_t + \beta_3 \mathbf{X_i} + \epsilon_{i,t} \qquad (10)$$

Our causal parameter of interest is δ_k, the coefficient of the interaction between each county i's treatment intensity $Casualty_i^k$ and an indicator variable for observations after the war (P_t). We estimate two different specifications every time, switching between the different treatment shares $k \in \{total, dead\_only\}$.

Our estimations hence follow the common two-way fixed-effects setup, with the period-indicator P_t accounting for counties' common trends over time, and the county-fixed effects π_i controlling for time-invariant heterogeneity across counties. In addition to these two fixed effects, we control for quartile fixed effects of our treatment intensity, D_i^k, interacted with the post-war indicator P_t. These quartile fixed effects help establish the strong local parallel trends assumption by reducing the overall variation in the dose (i.e., $Casualty_i^k$) to variation within a neighborhood of similar doses D_i^k. According to Callaway et al. (2021), it is sufficient to assume no sorting into a specific dose d *within the neighborhood of similar doses*. Therefore, when examining the effect of WWI casualties, we condition on casualty quartiles, illustrated in Figure 4.5, to allow for a local interpretation of our results.[57] This is, we estimate the average causal response of a marginal increase in the share of casualties, conditional on a county belonging to the low, lower-medium, higher-medium, or high end of the overall casualty spectrum.

Figure 4.5 – Casualty Quartiles



Notes: This figure illustrates the empirical distribution of our main explanatory variable, the rate of WWI casualties in the pre-war 1907 male population, along with the allocation into quartiles for our identification of local marginal effects in a histogram.

Institutional Determinants of War Exposure. In addition to only comparing counties within the same quartile of war-exposure, we control for two further covariates Z_i to account for institutionally determined differences in counties' exposure to WWI casualties. Once at battle, contemporaneous accounts suggest that individual survival chances were largely determined by chance. As Remarque (1928) remarks in *All Quiet on the Western Front*: "Above all is chance. When a bullet is coming, all I can do is duck. I can neither know nor steer where it lands." The industrial warfare of WWI plays a central role in this, as heavy artillery was used in excess to inflict damage to enemy troops from

[57]Figure 4-B3 in the Appendix additionally shows the geographic distribution of casualty quartiles.

great distance. Not surprisingly, the most common cause of death for German soldiers was artillery fire, accounting for about 50% of total casualties (Reichswehrministerium, 1934).

Still, mobilization and deployment differences constituted important determinants of casualty rates at the county level. In particular the former correlates with counties' economic trajectories, as German manufacturing centers were largely spared from mobilization to continue war production. Not controlling for different mobilization rates would therefore likely invalidate the (strong local) parallel trends assumption we require for identifying a causal effect. To adequately account for this concern, we control for the county-level determinants of mobilization and deployment in WWI Germany.

First, $\mathbf{Z_i}$ contains regiment district fixed effects that control for different deployment patterns within the German army. The German army administration was divided into 25 army districts responsible for the staffing of the different army groups. Figure 4-B1 a) shows the geographic distribution of the army districts. Each of these 25 army districts was further subdivided into regiment districts, which were responsible for recruitment and staffing of specific regiments. We show the distribution of these regiment districts in Figure 4-B1 b). Each of these regiment districts consisted on average of three to four counties.[58] Following the principle of 'Landsmannschaft', soldiers from geographically close areas – that is, counties within the same regiment district – where recruited into the same regiments to increase morale and fighting spirit. The regiments fed into a specific army, which was then deployed in the different war theaters. While mortality rates in different battles of the war varied substantially, mortality within a specific battle was arguably random. As recruits from a given regiment district were deployed together in the same regiment, and this regiment fought the same battles, casualty rates *within* regiments are also plausibly random. With the inclusion of regiment fixed effects, we only exploit variation in WWI casualties between counties within the same regiment district.

Second, we need to control for different mobilization rates across counties in $\mathbf{Z_i}$. Unfortunately, we lack reliable county-level mobilization numbers for WWI in Germany. Controlling for mobilization in our context, however, is crucial as we expect lower mobilization rates in industrial centers as to exempt workers in war related industries from the recruitment process. As we cannot observe mobilization rates directly, we use information on the excess local female population as proxy for mobilization. For this, we use data from the 1916 interwar census, which provides information about the number of men and women by county.[59] $\mathbf{Z_i}$ thus includes the growth rate of the 1916 female share as compared to the 1907 female share, based on the assumption that an increase in the

[58]See Figure 4-B2 for an overview of the distribution of the number of counties per regiment district.

[59]The census provides information for the male population in three categories: civilian males, male military personnel and foreign prisoners of war. We exclude prisoners of war because they were not part of the local male population and we exclude soldiers because they were either on leave or in local training camps at the time of the census.

'abnormal' female share derives from the war-deployed men in the county. An increase in the share of women hence suggests that a county experienced a higher mobilization rate.

We argue that controlling for the casualty rate quartiles, regiment district fixed effects, and female population growth, each interacted with the post-war indicator variable, controls for drafting and deployment characteristics and allows us to establish a local quasi-random variation in casualty rates at the county level. We thus compare counties with slightly differing casualty rates *among counties with a similar exposure to WWI casualties*. Under these circumstances, we argue that the local parallel trends assumption put forward in Callaway et al. (2021) holds. We use data from Prussia in Section 4.7 to demonstrate that our identification approach does not underlie non-parallel pre trends across a wide number of economic variables.

Table 4.1 – Determinants of War Exposure

	Casualty rate			Death rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Prussia	0.037	0.021	0.003	0.016	-0.013	-0.025
	(0.006)***	(0.018)	(0.009)	(0.003)***	(0.022)	(0.019)
	[0.014]***	[0.014]	[0.010]	[0.006]***	[0.019]	[0.018]
Employment: primary	0.022	0.017	0.003	0.010	0.008	0.002
	(0.003)***	(0.004)***	(0.002)	(0.001)***	(0.002)***	(0.001)*
	[0.004]***	[0.004]***	[0.002]	[0.002]***	[0.002]***	[0.001]*
Employment: secondary	0.012	-0.007	-0.001	0.005	-0.003	-0.001
	(0.004)***	(0.004)*	(0.003)	(0.002)***	(0.002)	(0.001)
	[0.009]***	[0.005]	[0.003]	[0.003]***	[0.003]	[0.002]
Employment: tertiary	-0.027	-0.011	-0.001	-0.006	-0.001	0.001
	(0.006)***	(0.009)	(0.004)	(0.003)*	(0.004)	(0.002)
	[0.012]***	[0.011]	[0.004]	[0.005]*	[0.005]	[0.002]
Population	-0.003	-0.014	-0.003	-0.005	-0.005	0.002
	(0.009)	(0.013)	(0.007)	(0.004)	(0.006)	(0.003)
	[0.017]	[0.015]	[0.006]	[0.007]	[0.007]	[0.003]
Urban	-0.009	-0.013	-0.002	-0.003	-0.004	-0.001
	(0.006)	(0.006)**	(0.004)	(0.003)	(0.003)	(0.002)
	[0.008]	[0.010]**	[0.003]	[0.004]	[0.005]	[0.002]
Observations	776	722	722	775	722	722
R^2	0.270	0.727	0.921	0.273	0.679	0.893
Mean DV	0.245	0.245	0.245	0.099	0.245	0.099
Regiment district	.	✓	✓	.	✓	✓
Female share	.	✓	✓	.	✓	✓
Quartiles	.	.	✓	.	.	✓

Notes: Results from Cross-Section OLS Regressions. The outcome variable in Columns (1)–(3) is the share of casualties in the male 1907 population. The outcome variable in Columns (4)–(6) is the share of killed soldiers in the male 1907 population. 'Prussia' indicates that a county belongs to the state of Prussia. 'Employment: primary' indicates the share of agricultural workers in total 1907 employment. 'Employment: secondary' indicates the share of industrial workers in total 1907 employment. 'Employment: tertiary' indicates the share of service workers in total 1907 employment. 'Population' measures logged 1907 population. 'Urban' indicates a county population above 75,000 or a population density above 150. Standard errors clustered by county in parentheses, Conley standard errors using a 100km distance cut-off in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.1 demonstrates our identification approach. We report OLS results with the casualty rate, i.e., the number of wounded and killed soldiers relative to the pre-war male population, as the dependent variable in Columns (1)–(3). In Columns (4)–(6), we repeat the same OLS regressions for the share of killed soldiers only. These cross-section regressions explain the share of casualties or killed soldiers by various pre-war economic factors, such as the county level employment structure and population. In addition, we control for counties belonging to the state of Prussia, as Prussia was the dominant state within the administration and military of the German Empire. Casualty and death rates are likely to be higher in these counties, as Prussian regiments were deemed more reliable due to higher loyalty to the regime and the traditionally higher military spirit in Prussian counties.

Columns (1) and (4) estimate the cross-section OLS regressions without any of our three main control variables. The results demonstrate that, other than the size of a county's population and urbanization rate, all pre-war characteristics correlate significantly with the casualty share and death rate, respectively. We see that, as expected, the share of casualties and deaths was significantly higher in Prussian counties as well as in counties whose population was primarily employed in the agricultural and manufacturing sectors.

In Columns (2) and (5), we add our proxy variables for differences in drafting and deployment. Controlling for deployment to different scenes of the war via regiment district fixed effects and for drafting ratios via the change in the female population share between the pre-war census in 1907 and the interwar census in 1916 significantly reduces the predictive power of counties' pre-war characteristics. Conditional on drafting ratios and deployment, Prussian counties did not face significantly different war exposure than non-Prussian counties. The same holds for counties with higher secondary or tertiary employment. Only agricultural sector employment and urbanization remain significant predictors of counties' casualty rates, requiring even more stringent specifications to account for selection into war exposure.

We therefore follow the recommendation of Callaway et al. (2021) and stress the local parallel trends assumption for Continuous Difference-in-Differences estimations. In Column (3), we additionally add Casualty-Quartile Fixed Effects to investigate how pre-war differences affect a marginal increase in casualty rates. Now, all variables turn statistically insignificant at the 95% level. This mostly holds for the rate of killed soldiers in Column (6) as well. Only agricultural employment remains statistically slightly significant at the 90% level, yet with a very small effect size. In addition, this remaining correlation between agricultural employment and the share of killed soldiers goes into the opposite direction as our main effect estimates presented below. Hence, the remaining bias, if any, would bias our results towards zero.

Overall, we interpret the results in Table 4.1 as encouraging evidence that the necessary covariates we employ in our main specifications alleviate the concern of selection into *marginally* higher casualty rates. The fact that we also observe parallel pre-trends for the subset of Prussian counties where enough pre-war data are available, discussed below, makes us confident that we can interpret our main results as causal evidence for the impact of WW1 casualties on the economic trajectory of German counties.

4.5 Results

WWI casualties had a significant impact on the structural transformation of German counties. This section starts by discussing the war's labor market effects. Our results below demonstrate that marginally higher casualty rates accelerated "first-order" structural transformation, i.e., the movement from agricultural to manufacturing jobs, but slowed down "second-order" structural transformation, i.e., the formation of a strong trade- and service sector. These structural transformation differences lead to changes in the employment and population composition. We find that counties with marginally higher casualty rates experienced a decline in White Collar workers, i.e., the new middle class of the 20th century, both in the labor markets and in the general population. Additional results emphasize that this composition change accrued due to the easier substitutability of low-skilled Blue Collar workers, but did not affect firms' productivity. Finally, we analyze the war's impact on land ownership and find that marginally higher casualty rates significantly contributed to the disappearance of subsistence farming, and that the land owned by these small-scale farmers was absorbed by medium-sized farms, which cultivated bigger areas after the war. To do so, they did not hire more labor but employed more machinery.

Structural Transformation. Table 4.2 shows the results from Continuous Difference-in-Differences regressions following Equation 10, with the logarithm of total employment (Column 1) as well as employment in the agricultural, manufacturing, and trade/service sector (Columns 2–4, respectively) as the dependent variables. All dependent variables derive from the business and agriculture censuses in 1907 and 1925. The business census holds information on employment in firms, distinguished by sector, while the agricultural census contains information on agricultural workers that were operating outside official businesses. We use the sum of workers in agri-businesses from the business census and farm workers from the agricultural census to measure the size of the agricultural sector.

We distinguish between total casualties in Panel (A) and killed soldiers in Panel (B), both as the share in the pre-war male population.[60] Our results demonstrate two things. First, a marginal increase in fatalities significantly reduced the number of employees. Second, this decrease occurs primarily in the agricultural and service sector, while employment in the manufacturing sector follows the general trend. The effect is sizeable. A one percentage point higher casualty rate reduces total employment by around 1.3%, a one percentage point increase in the rate of killed soldiers even by 4.7%.[61] This reduction

[60] Figure 4-B6 in the Appendix illustrates the unconditional correlations between sectoral employment and casualty/fatality rates. Especially for the tertiary sector, the negative correlation between casualty/fatality rates and employment is well visible descriptively.

[61] Note that these percentages refer to different bases, i.e., one percent of a county's male population

Table 4.2 – Employment Effects

	Employment (1)	Employment: primary (2)	Employment: secondary (3)	Employment: tertiary (4)
Panel A: Casualties				
Casualties × post	-1.29378	-1.05081	-0.17806	-1.23613
	(0.65402)**	(0.41107)**	(1.07079)	(0.97718)
	[0.54327]**	[0.27758]***	[0.92060]	[0.66463]*
Panel B: Dead				
Dead × post	-4.65428	-2.03624	-1.39253	-4.18979
	(1.27049)***	(0.74355)***	(2.05606)	(1.74993)**
	[1.51514]***	[0.58249]***	[1.19627]	[0.94258]***
County FE	✓	✓	✓	✓
Post x Regiment District	✓	✓	✓	✓
Post x Female Share	✓	✓	✓	✓
Post x Quartiles	✓	✓	✓	✓
Observations	1,432	1,432	1,432	1,432

Notes: Results from Continuous Difference-in-Differences estimations following Equation 10. Outcome variables stem from the business and agricultural censuses in 1907 and 1925 and measure the logarithm of total employment (Column 1), as well as employment in the agricultural, manufacturing, or service sector (Columns 2–4), respectively. The main explanatory variable is the share of WWI casualties/deaths over the county's pre-war male population. All specifications control for logged population. Columns 2–4 control for logged total employment. Standard errors clustered by county in parentheses, Conley standard errors using a 100km distance-cutoff in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

is most prominent in the tertiary sector, where employment decreases by about 4.2% for a one percentage point higher death rate. This effect is not statistically significant when we look at total casualties including wounded soldiers, whereas we find a significant reduction in agricultural employment for only killed soldiers as well as overall casualties.

Our interpretation of these findings is differential worker movements across sectors, for which we provide additional evidence below. Consider first the null-effect for the manufacturing sector. The fact that the employment in this sector follows the general trend among German counties suggests that lost employees could easily be replaced. Most tasks conducted in the secondary sector did not require specific skills, so new workers could quickly be trained to fill in. The tertiary sector, on the other hand, consists to a large extent of well-trained White Collar workers. These high skilled war casualties were not as easy to replace, which left the vacancies open after the war. As a result, counties with more fatalities were left with a smaller tertiary sector, deviating downwards from the general German trend.

As for the agricultural sector, it is likely that a combination of two developments explains our results. We provide evidence for both in Section 4.6 below. First, the mechanization during the early 20th century made it easier to replace workers in the agricultural sector. In fact, mechanization often functioned as a substitute for labor in the agricultural sector but a complement in the manufacturing sector. Already before the war, mechanization improved the output in the agricultural sector, substituting low-

is a bigger absolute number than one percent of that same county's tertiary employment.

scale farmers for machines and making it possible to cultivate bigger areas with constant amounts of labor. Returning veterans as well as previously unemployed people were therefore relatively more in demand in the manufacturing sector, where they hence faced higher wages and preferred to work. Second, at the turn of the century Germany was still transitioning out of being a mainly agricultural country. As a consequence, a lot of labor was still bound in inefficient, small-scale farming. As we show below, these small subsistence farms were those where employment as well as land holdings disappeared the most.

Note finally that agricultural employment also decreases when looking at the composite casualty rate, suggesting that also returning wounded soldiers who used to work in agriculture leave the sector. While this is no channel we can directly test, it appears reasonable. Returning veterans were often severely injured, making it difficult to employ them in physical farm labor. At the same time, working a machine in a factory required much less physical labor and was possible with even severe war injuries, while they required little to no education or hard-to-acquire skills as, e.g., jobs in the tertiary sector. Second, total factor productivity and hence wages for unskilled workers was likely higher in the manufacturing sector. Hence, small-scale farmers whose production capacity decreased due to war injuries probably arrived at a point where selling their land and taking up work in the industrial sector promised higher pay.

Table 4.3 – White Collar Workers

			White collar workers in...			
	ln(White Collar)	ln(Blue Collar)	Secondary	Tertiary	All Sectors	Population
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Casualties						
Casualties × post	-0.97091	0.38770	-0.12667	-0.38910	-0.30694	-0.09825
	(0.76659)	(1.02384)	(0.16031)	(0.17423)**	(0.16486)*	(0.06766)
	[0.54332]*	[0.84119]	[0.13477]	[0.13248]***	[0.14471]**	[0.05124]*
Panel B: Dead						
Dead × post	-4.78492	-1.63092	-0.16365	-0.90985	-0.67300	-0.50605
	(1.56051)***	(1.98472)	(0.28542)	(0.38004)**	(0.34604)*	(0.14594)***
	[0.86641]***	[1.28910]	[0.21371]	[0.33551]***	[0.25963]***	[0.10970]***
County FE	✓	✓	✓	✓	✓	✓
Post x Regiment District	✓	✓	✓	✓	✓	✓
Post x Female Share	✓	✓	✓	✓	✓	✓
Post x Quartiles	✓	✓	✓	✓	✓	✓
Observations	1,430	1,430	1,430	1,430	1,430	1,430

Notes: Results from Continuous Difference-in-Differences regressions following Equation 10. The dependent variables stem from the business census and measure, from Columns (1) to (6), the log-number of White Collar and Blue Collar workers, and the share of White Collar workers in manufacturing employees, tertiary employees, overall business employees, and in the population. The level of observation is the county level across the two census periods 1907 and 1925. "Casualties × post" indicates the share of casualties in the male 1907 population interacted with a dummy indicating the post-war period. "Dead × post" indicates the share of fatalities in the 1907 male population interacted with a dummy indicating the post-war period. All specifications control for logged population. Standard errors clustered by county in parentheses, Conley standard errors using a 100km distance-cutoff in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Social Transformation. In Table 4.3, we analyze the effect of WWI casualties on various dependent variables that reflect the composition of counties' labor markets and society. The goal of this investigation is to shed light on how WWI affected the new upper middle class in Germany, i.e., the class of White Collar "Angestellte" that started emerging towards the turn of the century and distinguished themselves by being higher skilled and educated, enjoying better working conditions, and receiving higher pay than Blue Collar "Arbeiter." Columns (1) and (2) of Table 4.3 analyze the effect of WWI casualties on the natural logarithm of White Collar and Blue Collar workers in a county, respectively. Again, we distinguish between total casualties and fatalities across Panels A and B. In Columns (3) to (6) we then regress the share of White Collar workers in manufacturing employees (Column 3), tertiary employees (Column 4), overall business employees (Column 5), and the population (Column 6) on WWI casualties.

We find that fatalities from WWI significantly decreased the number of White Collar workers. On average, a one percentage point higher fatality rate decreased White Collar employment by around 4.8 percent according to the result in Panel B, Column (1). This effect is mainly due to soldiers not returning from war, as we find barely significant effects for overall casualties. We also find no evidence for a decrease in Blue Collar employment. This finding adds to our results from Table 4.2 above. Employment in the Blue Collar-heavy secondary sector grew with the general trend, while the White Collar-dominated tertiary sector deteriorated in harder hit counties. Yet, the results in Column (3) suggest that within the manufacturing sector, the share of White Collar employees did not shrink significantly. Apparently, manufacturing firms were able to recruit new White Collar workers to replace the war losses. Even though we cannot test this directly, we suspect that firms returned to earlier practices and promoted experienced Blue Collar workers to White Collar jobs, a career progress that was blocked with the rise of the "Angestellte"–"Arbeiter" distinction (Kocka, 1981).

Therefore, and in line with our results in Table 4.2, the labor shock was mainly felt in the tertiary sector. In the trade and service sector, there were no Blue Collar workers that could compensate the loss in White Collar workers from the war. Similarly, unemployed or workers from other sectors lacked the required skills to fill the open vacancies. As a result, these jobs, and likely firms, disappeared. We see this aggregate effect across various lines: the share of white collar workers in tertiary employment (Column 4), overall employment (Column 5), and the county population (Column 6) decreased significantly. This, hence, accrues to a societal change for the counties that faced higher losses from the war. The new upper middle class that consisted of White Collar workers decreased relative to the overall German trend. While other counties kept specializing in the tertiary sector and promoting their new upper middle class, counties that felt more of the death toll of World War I did not follow this trend of structural transformation.

Table 4.4 – Agricultural Effects

	Total	Very Small <2ha	Small <5ha	Medium <20ha	Large <100ha	Very large >100ha
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log employment						
Casualties × post	-1.23716	-1.77642	-0.36711	0.37064	-1.97442	1.12868
	(0.44471)***	(0.76536)**	(0.47654)	(0.36646)	(0.93345)**	(2.38647)
	[0.31181]***	[0.47832]***	[0.30824]	[0.31033]	[0.69725]***	[1.17090]
Dead × post	-2.48567	-4.20384	-0.35615	0.21245	-4.03054	-0.34757
	(0.84270)***	(1.49288)***	(0.77889)	(0.66622)	(1.69134)**	(4.02021)
	[0.72129]***	[1.46061]***	[0.37448]	[0.50650]	[1.15144]***	[2.57811]
Panel B: Log farm size						
Casualties × post	-0.27050	-1.02169	0.05662	0.75842	-1.55378	0.39090
	(0.21378)	(0.42434)**	(0.28831)	(0.32531)**	(1.05418)	(3.00969)
	[0.16432]*	[0.28993]***	[0.17676]	[0.24829]***	[0.79563]**	[1.75054]
Dead × post	-0.41424	-2.49938	0.37125	1.07104	-1.64098	-4.17911
	(0.38629)	(0.87139)***	(0.53331)	(0.61516)*	(1.79530)	(5.31818)
	[0.24731]*	[0.83132]***	[0.24034]*	[0.50156]**	[1.26130]	[3.86883]
Panel C: Log number of farms						
Casualties × post	-0.88171	-1.76167	0.07139	0.61046	-0.56547	-0.47000
	(0.52968)*	(0.84401)**	(0.27240)	(0.32806)*	(0.45117)	(0.78345)
	[0.39518]***	[0.66580]***	[0.17664]	[0.23501]***	[0.31158]*	[0.13282]*
Dead × post	-2.25306	-4.08647	0.22588	0.79961	-1.21961	-0.92552
	(0.99237)**	(1.59770)**	(0.51896)	(0.61207)	(0.74957)	(1.28062)
	[0.95333]***	[1.59957]***	[0.26138]	[0.43152]*	[0.45143]***	[0.69060]
County FE	✓	✓	✓	✓	✓	✓
Post x Regiment District	✓	✓	✓	✓	✓	✓
Post x Female Share	✓	✓	✓	✓	✓	✓
Post x Quartiles	✓	✓	✓	✓	✓	✓
Observations	1,452	1,452	1,452	1,452	1,452	1,452

Notes: Results from Continuous Difference-in-Differences regressions following Equation 10. Outcome variables stem from the agricultural census and measure the logged number of employees in the agricultural sector (Panel A), the logged farm size in hectars (Panel B), and the logged number of farms (Panel C). Columns distinguish between farm sizes, ranging from very small subsistence-level farms (Column 2) to very large commercial farms (Column 6). The level of observation is the county level across the two census periods 1907 and 1925. "Casualties × post" indicates the share of casualties in the male 1907 population interacted with a dummy indicating the post-war period. "Dead × post" indicates the share of fatalities in the 1907 male population interacted with a dummy indicating the post-war period. All regressions include fixed effects for the treatment-quartile, the regiment district, and the change in the share of women in 1916 to 1907, all interacted with the post-dummy. All specifications control for logged population. Standard errors clustered by county in parentheses, Conley standard errors using a 100km distance-cutoff in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.6 Mechanisms

This section presents additional results that explain the mechanisms behind hard-hit counties' deviation from the German structural transformation trend. Most importantly, we shed light on the employment- and ownership transformation as well as mechanization in the German agricultural sector.

Agricultural Transformation. Our main results suggest that labor moved from the agricultural and unemployment sector into the manufacturing sector to compensate the loss in Blue Collar workers due to WWI fatalities. While we cannot identify such movements directly, we here leverage two additional data sources to look at the effects of such movements at the sending end of the labor movements. This is, we investigate whether the agricultural and unemployment sectors show tendencies that are in line with workers moving out of these sectors.

Using data from the German agricultural censuses in 1907 and 1925, we shed light on the transformation of employment and ownership inside the agricultural sector.[62] Table 4.4 provides estimates for the average causal marginal response of (log) overall employment in agriculture (Panel A), (log) overall farm size (Panel B), and the (log) number of farms (Panel C) to WWI casualties. Interestingly, the agricultural census allows us to test whether the effects differ across farm size categories. This is, we observe all three dependent variables for very small self-sufficient farms (Column 2) up to very large commercial farms with more than 100 hectars (Column 6). Column (1) provides estimates for the aggregate effect across all farm sizes. The distinction reveals an important effect heterogeneity across farm sizes and suggests that WWI casualties promoted a re-orientation within the agricultural sector away from self-sufficient production.

WWI casualties, both in the aggregate and when looking exclusively at fatalities, had a significantly negative effect on the employment, farm size, and number of farms in the self-sufficient category. We also find that large farms reduce the number of employees in counties with higher casualty rates, yet they do not reduce the area they cultivate. Hence, self-sufficient farming seems to disappear more where the toll of WWI was higher. What is more, we find that medium-sized farms increase in size and number, probably absorbing the land made free by self-sufficient farmers exiting the agricultural market. Interestingly, we do not find evidence that employment increased in this or any other category. Even though medium sized farms cultivated bigger areas after the war, they did not higher additional workers. Large farms even reduced their labor force while cultivating a constant area of land. Both was made possible via labor substitution in the form of increased mechanization of the agricultural sector in harder-hit counties.

[62] Figure 4-B7 in the Appendix provides detailed descriptive statistics for the national developments in the German agricultural sector.

Table 4.5 provides suggestive evidence that medium-sized farms employed more threshing machines in counties with higher WWI casualties. Unfortunately, data on machine usage are only available at the county level in 1925, so these results are based on unidentified cross-section regressions and should not be interpreted as causal. Still, this correlational evidence suggests that especially medium-sized farms increased their capital investments. While higher casualty rates are associated with more machine usage in the aggregate agricultural sector, the effect is most predominant in the medium and larger categories. We find no evidence of increased capital investments with respect to WWI casualties in the subsistence-farming sector. Finally, this effect is mostly driven by threshing machines, one of the most labor-saving technologies in the agricultural sector (Caprettini and Voth, 2020).

Overall, our findings for the agricultural sector paint the following picture: In counties with higher WWI casualties, farm labor was not directly replaced. Especially subsistence farms were not continued after the war, either because the original farmer died and nobody took over, or because subsistence farmers found better employment in the secondary sector where firms were eager to replace the Blue Collar jobs left vacant after the war. It followed a consolidation of farm ownership, where medium sized farms took over the land formerly owned by subsistence farmers. The loss of labor was compensated with higher usage of labor-saving technology.

Transformation of the labor reserve. Agricultural laborers were one pool where businesses could recruit workers from to fill open vacancies in the Blue Collar sector. The second pool was the labor reserve, i.e., able workers who were primarily unemployed. At the turn of the century, Germany still hosted a significant amount of daily laborers; rather low skilled individuals who would not be formally employed but offer their services for various manual tasks, often in agriculture or construction. While we lack sufficient data for the size of this informal employment sector to trace labor movements directly, we still can deduct the changes in demand for such workers from wage data.

The German social insurance gathered comprehensive data on reference wages of unskilled workers and daily laborers at the county level before and after the war. These data were meant as guidance for injury benefits and pension payments for workers who had to exit the workforce due to sickness or disabilities. Table 4.6 presents results from Continuous Difference-in-Differences regressions using reference wages in 1914 and 1922 as the dependent variables. From the nature of the data, we can distinguish reference wages by gender and for three age groups, over 21 years, between 16 and 21 years, and under 16 years.

We find that daily laborers' wages grew slower in counties with higher casualties, whereas we do not find significant evidence for wage reactions to WWI fatalities. For adult male workers over the age of 21, a one standard deviation increase in casualties led

Table 4.5 – Agricultural Effects, Mechanization

	Total	Very Small <2ha	Small <5ha	Medium <20ha	Large <100ha	Very large >100ha
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All machines						
Casualty Rate	8.49363	4.57860	7.79902	10.38300	7.40054	7.78657
	(3.23526)***	(3.28988)	(3.23152)**	(3.34297)***	(3.81467)*	(4.64963)*
	[4.09716]**	[3.86781]	[4.50353]*	[4.02260]**	[4.11076]*	[5.24538]
Dead	13.71543	2.07793	9.91192	15.44560	14.65473	18.24046
	(6.40246)**	(5.24243)	(6.36476)	(6.91674)**	(7.44868)*	(9.06822)**
	[7.18833]*	[2.50397]	[6.25446]	[7.80692]**	[8.98425]*	[9.44441]*
Panel B: Threshing machines						
Casualty Rate	11.88732	3.50092	9.87266	13.25231	9.71729	5.74995
	(3.77836)***	(3.27618)	(3.77792)***	(4.06360)***	(3.69968)***	(3.21442)*
	[4.77928]**	[4.17717]	[4.74445]**	[4.91371]***	[4.15418]**	[3.55349]
Dead	15.01375	-0.45315	9.97514	17.12852	14.56019	9.69485
	(7.29256)**	(5.06850)	(7.15286)	(7.82572)**	(7.27274)**	(6.16819)
	[7.81648]*	[4.52229]	[6.88716]	[8.30168]**	[8.45651]*	[6.20097]
Regiment District	✓	✓	✓	✓	✓	✓
Female Share	✓	✓	✓	✓	✓	✓
Quartiles	✓	✓	✓	✓	✓	✓
Observations	398	398	398	398	398	398

Notes: Results from Cross-Section OLS regressions. Outcome variables stem from the agricultural census in 1925 and measure the logged number of general machines (Panel A) and specifically threshing machines (Panel B) used in the agricultural sector (Panel A). Columns distinguish between farm sizes, ranging from very small subsistence-level farms (Column 2) to very large commercial farms (Column 6). The level of observation is the county level in 1925. "Casualty Rate" indicates the share of casualties in the male 1907 population. "Dead" indicates the share of fatalities in the 1907 male population. All regressions include fixed effects for the treatment-quartile, the regiment district, and the change in the share of women in 1916 to 1907. All specifications control for logged population. Standard errors clustered by county in parentheses, Conley standard errors in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

to about one Reichsmark lower wage growth. Compared to an average wage increase in the low-skilled sector of around 20 Reichsmark, this is a quite sizeable effect, especially considering that it is likely the outcome of two opposing effects. On the one hand, one would a priori expect wages to rise in counties with higher casualties or fatalities, as labor becomes scarce. Yet on the other hand, one might expect a selection effect taking place if businesses recruit the most able daily laborers to fill vacancies in the formal Blue Collar sector. This selection effect would lead low-skilled wages to deteriorate as the average skill level among the remaining workers decreases. Our results are in line with both of these effects taking place, while both effects cancel each other out when looking only at fatalities, but the selection effect dominating when also accounting for wounded soldiers. Likely, wounded veterans that return and join the daily laborer market lead to a further decrease in the average skill level, allowing us to identify statistically significant wage effects.

Overall, we interpret the results from Table 4.6 as evidence that businesses also re-

Table 4.6 – Employment Effects, Wages

	Wage (m) (1)	Wage (f) (2)	Wage (m) under 21 (3)	Wage (f) under 21 (4)
Panel A: Casualties				
Casualties × post	-15.78624	-11.45959	-9.36177	-4.53131
	(7.50208)**	(5.91481)*	(5.63858)*	(4.47740)
	[6.22468]***	[5.09007]**	[5.35730]*	[4.33234]
Mean DV	13.007	10.099	9.162	7.174
Panel B: Dead				
Dead × post	-21.98314	-19.52155	-11.09736	-4.97264
	(15.29119)	(12.00273)	(10.83567)	(8.52181)
	[13.99224]*	[11.20067]*	[11.17595]	[8.15955]
Mean DV	13.007	10.099	9.162	7.174
County FE	✓	✓	✓	✓
Post x Regiment District	✓	✓	✓	✓
Post x Female Share	✓	✓	✓	✓
Post x Quartiles	✓	✓	✓	✓
Observations	1,450	1,450	1,450	1,450

Notes: Results from Continuous Difference-in-Differences estimations following Equation 10. Outcome variables are the average male wage (Column 1) and average female wage (Column 2), as well as both respective wages for young workers under 21 in Columns (3) and (4), respectively. "Mean DV" provides the mean of each dependent variable. All specifications control for logged population. "Casualties × post" indicates the share of casualties of the male 1907 population interacted with a dummy indicating the post-war period. "Dead × post" indicates the share of dead soldiers of the 1907 male population interacted with a dummy indicating the post-war period. Standard errors clustered by county in parentheses, Conley standard errors using a 100km distance-cutoff in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

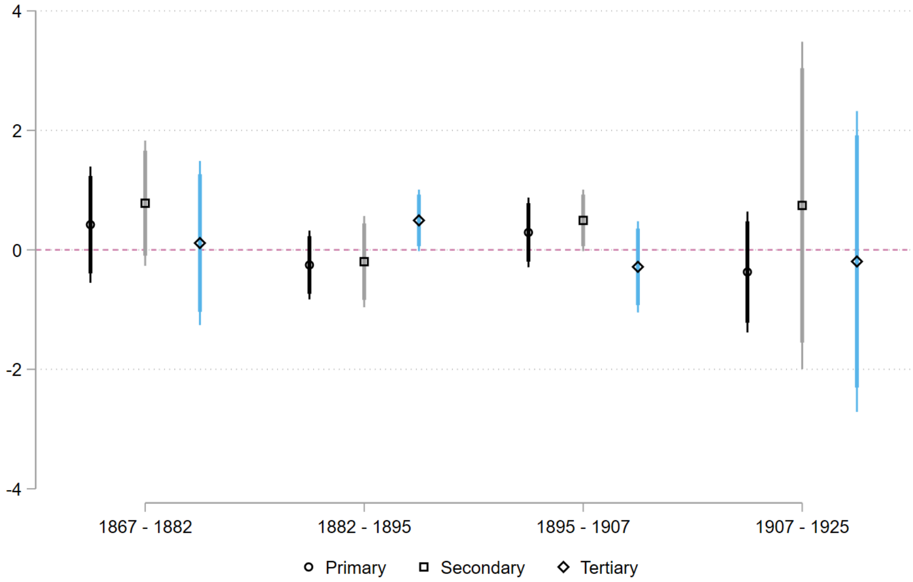
cruited Blue Collar workers from the low-skilled sector. If such recruitment didn't take place, theory dictates that we should observe a significant increase in wages amid higher labor scarcity in harder hit counties. However, our results point to the opposite, supporting the hypothesis that despite lower supply and probably higher demand for workers, wages decreased due to a lower productivity of workers in the sector. Hence, we can summarize the war-induced transformation of the German labor markets as follows: Manufacturing firms recruited labor from the agricultural and informal sector to rapidly replace the Blue Collar workers lost in WWI. Firms in the tertiary sector that relied more on high-skilled White Collar workers lacked this labor reserve and therefore had to leave many positions vacant.

4.7 Robustness

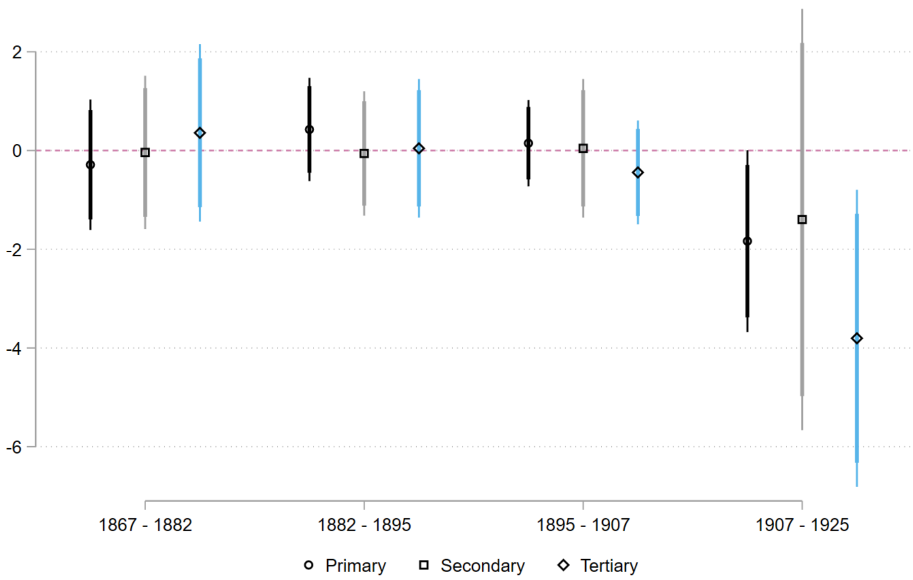
In this section, we first reflect the necessary assumptions to interpret our results as causal. Most importantly, we provide evidence of parallel pre trends to test the plausibility of our strong local parallel trends assumption, and discuss further threats to identification like migration. We conclude the section with a number of alternative specifications to test the robustness of our main results.

Figure 4.6 – Employment Effects, Pre-Trends

a) Casualty rate



b) Dead rate



Notes: This figure depicts the coefficients as well as their 90%- and 95% confidence intervals from the test for pre-trends by estimating the Difference-in-Differences regressions as outlined in Equation 10, but for the periods 1867–1882, 1882–1895, and 1895–1907 and for the harmonized sample of Prussian counties in the Galloway data. Panel a) shows the pre-trends using the casualty rate as the treatment variable, Panel b) using the fatality rate. 'Primary' indicates logged employment in the agriculture sector, colored in black. 'Secondary' indicates logged employment in the manufacturing sector, colored in gray. 'Tertiary' indicates logged employment in the trade and service sector, colored in blue. We report results by pairs of years for the three sectors as indicated on the x-axis. All coefficients derive from separate regressions. All specifications include the female share, army district fixed effects, casualty/dead quartiles, and logged population. Data before 1907 derive from Galloway (2007).

Parallel Trends. According to Callaway et al. (2021), our Continuous Difference-in-Differences estimations require a "Strong Parallel Trends" assumption. This means, there should be no selection into a specific treatment dose, in our case the casualty rate, that correlates with the error term. They further suggest a local interpretation of Continuous Difference-in-Differences regressions. The idea is to allow selection into higher or lower ranges of doses as long as there is no selection into a specific dose among entities within the same range of a dose. From this, we derive our "Conditional Strong Local Parallel Trends Assumption," which postulates that conditional on our Casualty Quartile Fixed Effects and determinants of casualty shares, we should have observed parallel trends among counties with slightly different casualty rates if WWI had not occurred.

It is impossible to test this "Conditional Strong Local Parallel Trends Assumption" directly. However, as is common in the Difference-in-Differences literature, we can investigate parallel pre-trends to check this assumption for plausibility. We use employment data from Galloway (2007), who collected sector-level employment data for Prussian counties in 1867, 1882, 1895, and 1907 to test for pre-trends. Unfortunately, there are no reliable data available for the remaining German states for that time. Therefore, we can only test for parallel pre trends in the Prussian sample. To account for administrative reforms that occurred during this 60 year panel, we harmonize the counties to a constant sample over time, aggregating county mergers and splits to the biggest common entity. We follow Galloway (2007) and distinguish occupations by the first main category assigned by the Prussian statistical offices into agriculture, industry, and trade-related jobs. This distinction follows relatively closely the later distinctions in the full German business census of 1907 and 1925. Figure 4.6 reports the results from Continuous Difference-in-Differences regressions that follow our main specification in Equation 10, but for the periods 1867–1882, 1882–1895, and 1895–1907.[63]

If counties with marginally differing casualty shares followed otherwise parallel growth trajectories during the war, we also should not see any significant differences across counties in the periods before the eventual war casualties were realized. In fact, this holds across all three sectors. We find no evidence of different employment trajectories across counties with marginally different fatality rates. Yet, we can replicate our main results for the Galloway subsample. The final three coefficients in Figure 4.6 reflect our main specifications using our original data from the German business census before and after the war, but for the Galloway subsample of harmonized Prussian counties. We find qualitatively the same effects as for our main sample for the share of killed soldiers. For the total casualty rate including wounded soldiers, the picture is a little mixed. There are two pre-war estimates significant at the 10 percent level. While this can still occur by chance and does not necessarily contradict the assumption of parallel trends, it definitely calls for a cautious interpretation of the casualty results. In addition, we cannot replicate

[63]Table 4-B3 in the Appendix presents those same results in Table form.

the negative effect of casualties on primary sector employment in the Galloway sample. Hence, the investigation of pre trends in the Galloway sample increases our confidence for a causal interpretation of the fatality results, but suggests to take the estimated effects for total casualties with a grain of salt.

In addition to our main outcome variables, we investigate pre-war trend differences for a number of alternative variables. Figure 4-B5 shows coefficient estimates from Difference-in-Differences regressions across various variables on low-skilled wages, agricultural output, and railway expansion, among other things. We find no indication of different pre-trends for any of these additional variables. Overall, we are therefore confident that our Difference-in-Differences estimations identify the causal effects of WWI fatalities, but stress the cautious interpretation of the casualty results.

Birthplaces. Another concern that inherits from the nature of our data is internal migration. Unfortunately, the source of our data only identifies soldiers' birthplaces, not their place of residence before the war. While we lack the data for a systematic check of migration patterns, we can leverage the information on army districts to evaluate how likely internal migration is to introduce a severe bias into our regressions. Soldiers were assigned to regiments according to their place of residence.[64] Therefore, the majority of soldiers' birthplaces should fall into their regiments' borders if the county of birth is identical to the county of residence, i.e., if pre-war migration among soldiers within German borders was low. In Figure 4-B4 in the Appendix, we plot the casualty rates associated with the 49$^{\text{th}}$ Infantry Regiment based in the city of Gnesen in the province of Posen in the most South-Eastern part of the district as an example. The map shows that the vast majority of casualties refer to birthplaces that lie within the borders of the respective army district, indicated by thick black lines. Indeed, this is the case for all list entries for which we have information on the associated army district.

Overall, the figure suggests that assigning casualties based on birthplaces gives a mostly accurate picture of where soldiers lived before the war. Even though internal migration, especially from rural towards urban areas, existed in Germany in the pre-war period (Grant, 2005) and we must acknowledge that soldiers' birthplaces make an imperfect proxy for their place of residence, we are confident that our somewhat noisy casualty measure does not introduce a structural bias into our regressions for two reasons. First, internal migration consisted largely of urban-rural movements. Our Difference-in-Differences identification strategy accounts for urban-rural differences, e.g., in migration rates/directions, via county fixed effects.

Second, our analysis aims to shed light on the local labor market effects of WWI casualties. A priori, it is unclear whether the place of residence or indeed the birthplace

[64] As the number of casualties increased over time, this rule was relaxed significantly (Stachelbeck, 2010).

Table 4.7 – Subsample Analysis

Dropped subsample:	Baseline (1)	Ruhr area (2)	Russian advance (3)	Non-Galloway (4)	Top 10% (5)	Bottom 10% (6)
Panel A: Log(Employment)						
Casualties × post	-1.29378	-1.59497	-1.34954	-0.07564	-0.87419	-2.17863
	(0.65402)**	(0.63824)**	(0.67553)**	(0.55193)	(0.48593)*	(1.02477)**
	[0.54327]**	[0.52151]***	[0.50911]*	[0.23757]	[0.36336]**	[0.79283]***
Dead × post	-4.65428	-5.17785	-4.81723	-1.55133	-3.19806	-8.65803
	(1.27049)***	(1.27159)***	(1.31245)***	(0.61965)**	(1.04608)***	(2.34506)***
	[1.51514]***	[1.69846]***	[1.24322]***	[0.25770]***	[1.12444]***	[2.21589]***
Panel B: Log(Primary Employment)						
Casualties × post	-1.05081	-1.01234	-1.11437	-0.37083	-0.91000	-1.23267
	(0.41107)**	(0.42798)**	(0.42634)***	(0.51243)	(0.60664)	(0.51546)**
	[0.27758]***	[0.29322]***	[0.30250]***	[0.36320]	[0.41412]**	[0.29637]***
Dead × post	-2.03624	-1.84684	-2.14724	-1.83605	-1.63581	-3.09440
	(0.74355)***	(0.74108)**	(0.77378)***	(0.93189)**	(0.90500)*	(1.10358)***
	[0.58249]***	[0.58030]***	[0.64496]***	[0.32530]***	[0.74179]**	[0.36899]***
Panel C: Log(Tertiary Employment)						
Casualties × post	-1.23613	-1.36351	-1.30006	-0.19505	-1.61282	-1.33162
	(0.97718)	(1.00698)	(1.00741)	(1.27455)	(0.92827)*	(1.41153)
	[0.66463]*	[0.65687]**	[0.75800]	[0.50279]	[0.65283]**	[0.80343]*
Dead × post	-4.18979	-4.57117	-4.33323	-3.80464	-5.58000	-4.38710
	(1.74993)**	(1.73070)***	(1.79314)**	(1.52294)**	(1.85512)***	(2.94766)
	[0.94258]***	[1.08596]***	[0.97862]***	[0.90408]***	[1.52174]***	[1.73378]**
Panel D: Log(White collar)						
Casualties × post	-1.28791	-1.44543	-1.29105	-0.80072	-1.46970	-1.42651
	(0.86145)	(0.87697)*	(0.88586)	(1.17087)	(0.91537)	(1.19225)
	[0.61627]**	[0.60018]**	[0.72257]	[0.50554]**	[0.57330]**	[0.75544]*
Dead × post	-4.16870	-4.56796	-4.20630	-3.13087	-5.35209	-4.40813
	(1.64990)**	(1.64448)***	(1.68999)**	(1.28787)**	(1.84557)***	(2.61315)*
	[0.76265]***	[0.92211]***	[0.75010]***	[0.61812]***	[1.39686]***	[1.41660]***
County FE	✓	✓	✓	✓	✓	✓
Post x Regiment District	✓	✓	✓	.	✓	✓
Post x Female Share	✓	✓	✓	✓	✓	✓
Post x Quartiles	✓	✓	✓	✓	✓	✓
Observations	1,432	1,268	1,374	518	1,284	1,250

Notes: Results from Continuous Difference-in-Differences estimations following Equation 10, using subsamples of the basline sample. Column 1 shows the baseline results. Column 2 drops counties affected by the 1923 French occupation of the Ruhr area. Column 3 drops East Prussia. Column 4 drops all non-Galloway counties, reducing the sample to the one used to test pre-trends, and replaces regiment district fixed effects by army district fixed effects. Column 5 drops counties in the highest decile of war exposure. Column 6 drops counties in the lowest decile of war exposure. Standard errors clustered by county in parentheses, Conley standard errors in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of a soldier constitutes the optimal metric here. Contemporary sources suggest that young men often only migrated temporarily. Especially soldiers returning from war often searched employment close to their parental homes. Hence, any impact from wounded or missing men is at least as likely to occur at soldiers' birthplaces as at their pre-war place of residence. We leverage an additional set of lists of missing soldiers that was collected after the war and digitized by the Verein für Computergenealogie (2019b). These lists contain soldiers' birthplaces along with the place of residence of their closest relatives, usually their parents or spouse. We match birthplaces and the residence of the closest relative and find that for about 60% of entries, the address of the closest relative was identical to the place of birth of the missing soldier, accounting only for slight differences in spelling and typos. Note that these 60% only include cases of exact address matches and still exclude cases where soldiers moved within the same village or town. Overall, we are therefore confident that soldiers' birthplaces serve as a valid proxy for the local impact of WWI casualties.

Subsample analysis. Finally, we conduct a subsample analysis for our main results presented above. In Table 4.7, we repeat our main regressions, but alternately drop different subsamples that could affect our results for various reasons. Column (1) reports our main results for comparison. In Column (2), we drop all counties in the Ruhr area that were occupied by French and Belgium troops from 1923–1925. Similarly, in Column (3) we drop the East Prussian counties that were affected by the Russian advance early in the war. We find all our main results to be robust across these two subsamples. In fact, the coefficient estimates across all outcomes are qualitatively identical to our baseline results.

In Column (4), we repeat our main regressions restricting the sample to the Prussian counties in the Galloway sample which we used to test for parallel pre-trends. This subsample analysis seeks to show the comparability between our full German main sample and the much smaller pre-war sample to infer the parallel trends assumption to our main sample. Note that, due to the much smaller sample size in the Galloway sample, we control for army district fixed effects instead of regiment district fixed effects in these regressions. Army districts were one step higher in the administrative hierarchy of the German military and organized the regiment districts we usually control for. These also let us control for deployment differences, albeit at a lower resolution, but leave enough degrees of freedom for statistical inference in the smaller sample. The regression results from the Galloway sample closely match those from our main sample. Even though some coefficient estimates are somewhat smaller than in our baseline results, the effects are overall qualitatively similar.

Finally, we drop the counties with the 10% smallest and highest casualty shares in Columns (5) and (6), respectively. Arguably, counties with especially little or many casualties could be outliers that drive our results, and have the highest odds to be mis-coded due to problems with geocoding. Yet, the results from these two subsamples are very close to our main results. The only findings we are not fully able to replicate is the effect of fatalities on tertiary employment when dropping the counties with the 10% least fatalities. The coefficient estimate in this subsample misses the conventional threshold for statistical significance ($t = 1.49$). Note however that the coefficient size is very close to our original estimate, hence the statistical insignificance is solely driven by the higher standard error. Overall, our results are therefore largely robust to various subsamples.

4.8 Conclusion

This chapter examines how the loss of life during World War I affected the local structural transformation in Germany. The German context of WWI provides an exceptional case study to investigate the effect of a severe population shock in isolation from other economic destruction that usually accompanies such shocks. Due to the fact that fighting mostly took place outside of German territory, the infrastructure and capital stock were left largely untouched. Hence, we can identify the sole effect of the population shock on various economic outcomes.

For our analysis, we collected, combined, and harmonized various unique data sets. First of all, we geocoded more than 8.5 million casualty list entries. What is more, we digitized several censuses and archival data on local economic outcomes, and harmonized them across the German territory to account for various reforms to Germany's subnational borders. This allows us to not only investigate national trends in different economic outcomes, but to also estimate how counties that suffered more under WWI casualties deviated from this national trend in a Continuous Difference-in-Differences setting. Our identification approach relies on a "Conditional Strong Local Parallel Trends" assumption following Callaway et al. (2021), which requires a quasi-random allocation of treatment intensities among units in similar areas of treatment intensity, conditional on covariates. We show that, conditional on treatment quartile fixed effects and institutional determinants of war deployment and conscription, pre-war economic characteristics lose predictive power of local war exposure. Additionally, we observe no evidence of non-parallel economic trajectories before the war.

Our results demonstrate that the labor supply shock induced by WWI casualties had significant effects on counties' structural transformation. Most importantly, we find that harder hit counties fall behind in the transition towards tertiary production and face lower growth rates of White Collar workers, who were the emerging upper middle class at the turn of the century. Investigating mechanisms, we find plausible evidence that these effects accrued due to a lower substitutability of White Collar workers, on which especially the tertiary sector relied. Blue Collar workers in industry could be replaced by agricultural workers and labor reserves from the informal sector, while a loss in agricultural workers could be cushioned by more rapid mechanization. Only in the tertiary sector, where production relied on specific tasks and highly trained individuals, job vacancies remained unfilled.

Our findings shed new light on the harm political violence inflicts on transforming economies. Structural transformation requires establishing new modes of production, which increasingly rely on specific human capital that cannot easily be re-produced once lost. Our findings hence bear important insights for developing countries that are currently undergoing structural transformation: socio-political shocks like civil wars, disease, or brain drain complicate the move to modern production technologies as highly trained employees are both essential and hard to replace once lost.

4.9 Appendix

Appendix 4-A: Geocoding of Casualty Data

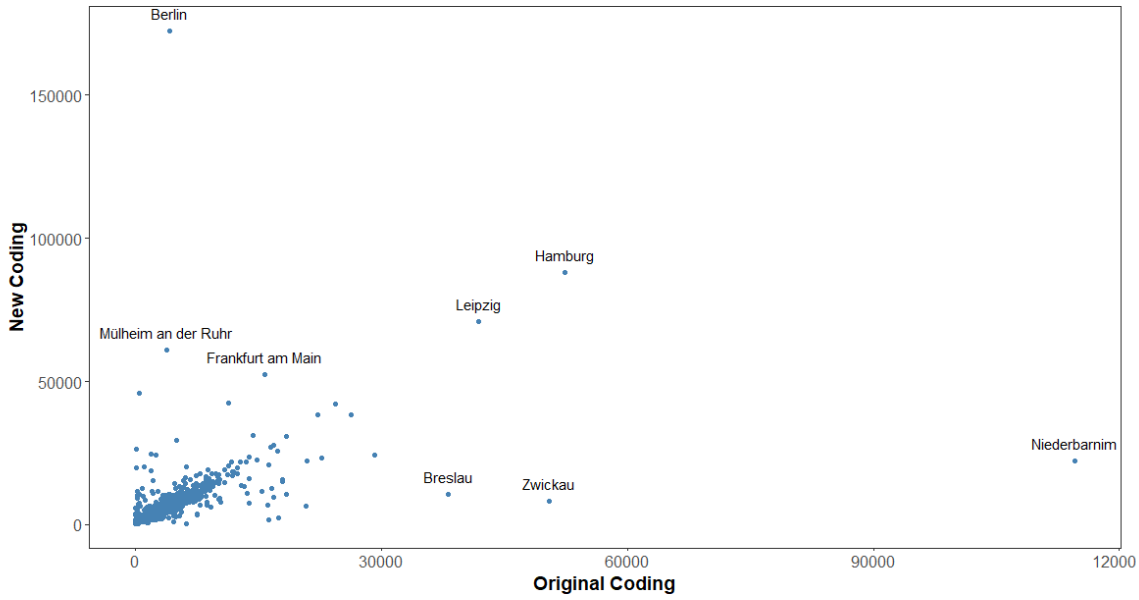
We derive the casualty data as our main explanatory variable from front reports, the so-called "Verlustlisten" or loss-lists, which list the names, ranks, regiments, and birthplaces of soldiers that were wounded or killed during battle. In a crowd-sourcing effort, the Verein für Computergenealogie (2019a) manually digitized the birthplaces of the 8.5 Million entries in these lists. Our geocoding process relies on a string-matching algorithm, which compares the manually digitized location names with a list of communities based on Germany's administrative structure of 1910, i.e., four years before the start of the war, collected by Schubert (2020).

The original entries of soldiers' birthplaces follow no specific structure. The majority of entries consists of only one word, e.g., "Berlin," or a combination of two words separated by a comma, e.g., "Spandau, Berlin." In some instances, an entry contains up to five separate words, all separated by a comma. Unfortunately, these comma-separated location descriptions do not follow a specified structure. In many instances, the first entry identifies a town or village, while the later entries identify higher administrative entities. In several instances however, this order may be reversed, e.g., to "Berlin, Spandau." What is more, information on a higher local level does not always identify an official administrative entity, but local information on the region or the next bigger town. Examples for these are entries of the form "Calw, Schwarzwald" or "Heiligenstadt, Mühlhausen." Such cases complicate a direct matching of entries and our list of communities.

Therefore, we proceeded in the following steps. First, we separated all entries by the comma to attach all distinct location information for each entry. Next, we assigned each list-entry to an entry in our community list, starting from the highest order of information. Our community list contains, for each community, information at five administrative levels, i.e., the "Land," "Provinz," "Bezirk," "Kreis," and "Gemeinde." For each casualty list entry, we started with the last information of the entry's string, for example with "Baden" in the entry "Neuenheim, Heidelberg, Baden." For this information, we started by looking for matches in the highest administrative level, i.e., the "Land," and then moving down to lower levels until a direct match was found. In the example here, we would directly find a match for "Baden" as a "Land." We would then proceed with the second part of information, looking at lower administrative levels in our community list, though subsetted to the region we already identified.

This direct pattern matching involved a number of difficulties. First, several entries were either misspelled or incorrectly typed during the digitization process. Therefore, we corrected numerous entries where we could not identify a direct match. Second, several entries would not result in an exact match. For example, the entry "Mühlhausen" could either refer to the town Mühlhausen near Erfurt in what is today the state of Thuringia,

Figure 4-A1 – Differences in Geocoding



Notes: This graph illustrates the differences in geocoded casualties between the original coding and our coding. On the horizontal axis, we plot the casualties per county according to the original coding, while the vertical axis displays the casualties per county according to our new coding.

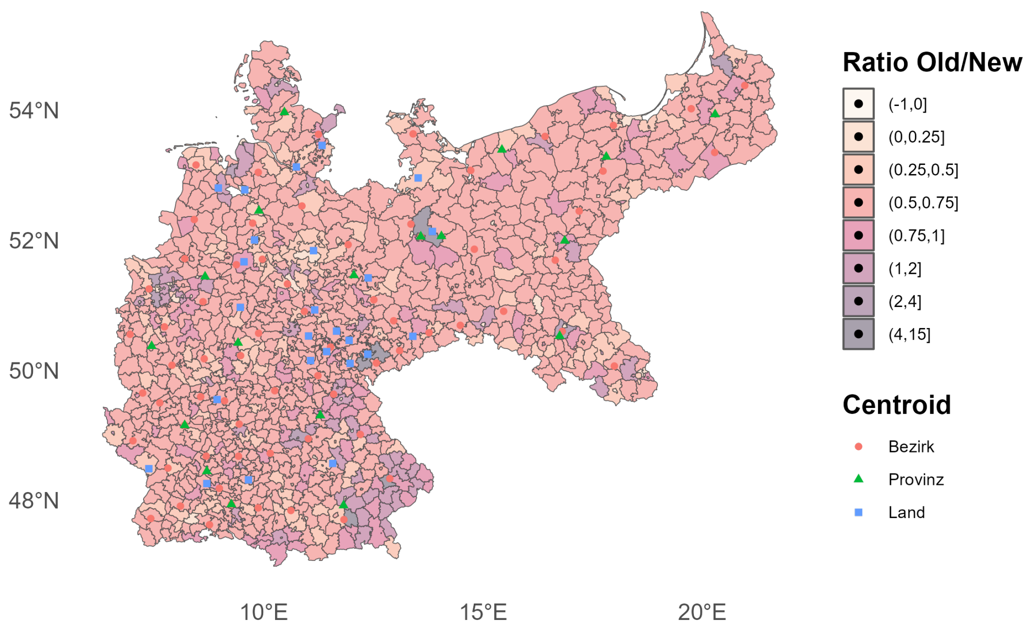
or to the district Mulhouse, which is part of the state or Alsace which today is in France. We worked through all instances of inexact matches manually, sorting the entries to the respective administrative area based on each entry's lower level information.

In the end, we were able to match around 7.5 million of the total 8.5 million entries to an entry at the fourth ("Kreis") or fifth ("Gemeinde") administrative level. Of the remaining entries, many do either not identify a location in Germany (e.g., "Philadelphia, Vereinigte Staaten") or only contain information on a higher administrative level (e.g., "Bayern") that does not allow matching it to the county level.

Our way of coding significantly improves the existing geocoding of casualty entries. The original data set from Verein für Computergenealogie (2019a) was geocoded automatically via the Historic Gazetteer as part of a Master Thesis by Sen (2016). While this automatic coding already delivered a very good spatial overview of casualty entries, it contained a number of structural errors that made the data difficult to use for our research purposes.

Our coding deviates in several ways from the original coding. In fact, the correlation between casualties per county according to our coding and the original coding is only 0.47. Figure 4-A1 illustrates these coding disparities by plotting the casualties per county according to our coding over the casualties per county according to the original coding. Here, we first see three obvious cases where our coding leads to significantly more casualties than the old coding. Most obviously, Berlin sticks out with much more casualties according to our coding. We suspect that several Berlin-entries ended up in the neighboring district Niederbarnim (a significant outlier to the right), as the coordinates assigned to Berlin in the original coding were not accurately matching Berlin's administrative boundaries. Second, Frankfurt am Main and Mülheim an der Ruhr also deviate upwards. Here, we trace the difference to the original coding mistakenly sorting entries like "Frankfurt" to the (much less populated) "Frankfurt Oder" in Eastern Germany. Similarly, several cases for "Mülheim" were mistakenly allocated to Mühlheim am Rhein.

Figure 4-A2 – Geographic Differences in Geocoding



Notes: This map illustrates the geographic variation in the differences in geocoded casualties. The color palette illustrates the ratio of a county's casualties in the original coding vs. our coding. The points depict the geographic centroids of higher administrative units, i.e., districts ("Bezirk"), provinces ("Provinz"), and states ("Land").

There are also several deviations towards the right hand side, i.e., where the original coding suggests higher casualty counts as our coding. These deviations are better illustrated by the map in Figure 4-A2. Here, dark-red to violet colors indicate counties where the ratio of casualties according to the original coding vs. our coding is between 2 and 15. In addition, we plot the centroids of higher administrative units, i.e., districts, provinces, and states. This shows that extraordinarily higher codings in the original geocoding tend to overlap with administrative centroids. For example, Niederbarnim, the large outlier in the scatterplot in the North-East close to Berlin, contains the centroids of both the province Brandenburg and the state of Prussia. The many entries which only held the information "Preußen" or "Brandenburg" were hence sorted into Niederbarnim. Similar cases are, e.g., Breslau, Düsseldorf, Ebersberg, and Landau an der Isar.

Appendix 4-B: Data and Descriptives

Table 4-B1 – Descriptive Statistics

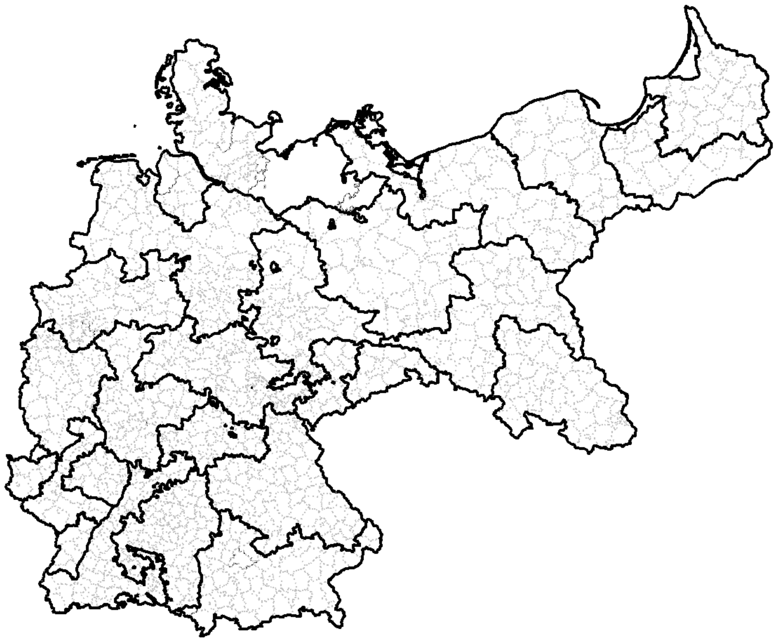
	Count	Mean	SD	Min	Max
Casualty rate	1,432	0.25	0.06	0.05	0.46
Death rate	1,432	0.10	0.03	0.02	0.26
Employment (log)	1,432	9.92	0.66	6.83	12.38
Primary employment (log)	1,432	9.31	0.99	5.64	10.97
Secondary employment (log)	1,432	8.27	1.14	0.00	11.69
Tertiary employment (log)	1,432	7.34	1.02	0.00	11.14
Employment: white collar (log)	1,432	7.77	0.99	0.00	11.26
Employment: blue collar (log)	1,432	8.06	1.19	0.00	11.59
White collar share	1,432	0.14	0.09	0.00	0.50
White collar share: secondary	1,431	0.32	0.10	0.05	0.72
White collar share: tertiary	1,431	0.72	0.11	0.19	0.96
Female share	1,432	0.04	0.06	-0.17	0.63
Population (log)	1,432	10.62	0.63	8.43	13.10
Wages, male over 21	1,431	13.01	10.89	1.60	50.00
Wages, male under 21	1,431	10.10	8.37	1.20	40.00
Wages, female over 21	1,431	9.17	7.78	1.00	40.00
Wages, female under 21	1,431	7.18	5.99	0.70	30.00
Primary employment: total (log)	1,432	9.29	1.04	3.76	10.97
Primary employment: subsistance farms (log)	1,432	7.92	1.08	3.53	10.33
Primary employment: small farms (log)	1,432	7.51	1.25	1.39	9.72
Primary employment: medium-sized farms (log)	1,432	8.02	1.26	1.61	9.75
Primary employment: large farms (log)	1,432	6.75	1.74	0.00	9.34
Primary employment: very large farms (log)	1,432	4.54	2.92	0.00	9.33
Number of all farms (log)	1432	8.29	0.95	4.11	10.15
Number of subsistance farms (log)	1,432	7.59	1.05	3.30	9.96
Number of small farms (log)	1,432	6.46	1.21	1.39	8.47
Number of medium-sized farms (log)	1,432	6.54	1.27	1.10	8.25
Number of large farms (log)	1,432	4.62	1.67	0.00	7.39
Number of very large farms (log)	1,432	1.85	1.54	0.00	5.34
Size of all farms (log)	1,432	9.80	1.27	3.71	11.78
Size of subsistance farms (log)	1,432	6.97	1.08	2.75	9.07
Size of small farms (log)	1,432	7.65	1.23	2.36	9.63
Size of medium-sized farms (log)	1,432	8.78	1.32	2.64	10.63
Size of large farms (log)	1,432	8.03	1.93	0.00	11.03
Size of very large farms (log)	1,432	5.78	3.45	0.00	11.09
Prussian county	1,432	0.55	0.50	0.00	1.00

Table 4-B2 – Sector Structure of the Business Census

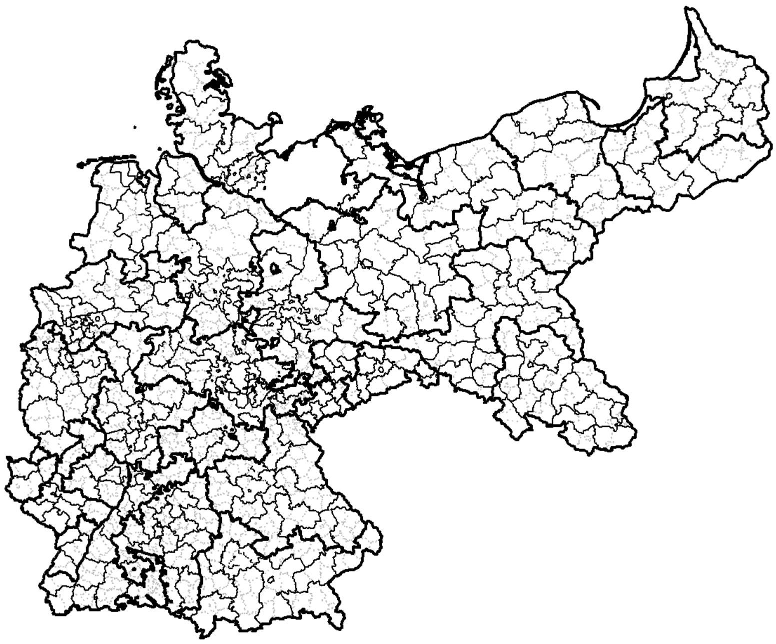
Sector	Subsector	Sub-subsector	Share	War related industry
Primary	Agriculture	Non-agricultural horticulture, animal husbandry, and deep-sea fishing	0.0154	.
Secondary	Mining	Mining, saltworks and peat digging	0.0755	x
Secondary	Mining	Industry of stones and earths	0.0763	x
Secondary	Manufacturing	Manufacture of iron, sheet metal and metal ware	0.0634	x
Secondary	Manufacturing	Machinery, apparatus and vehicle construction	0.0604	x
Secondary	Chemical and Textile Industry	Chemical industry	0.0340	x
Secondary	Chemical and Textile Industry	Textile industry	0.1079	.
Secondary	Consumption goods	Paper industry and multiplying trades	0.0386	.
Secondary	Chemical and Textile Industry	Leather and linoleum industry	0.0205	.
Secondary	Constuction	Wood and cuttings industry	0.0481	.
Secondary	Consumption goods	Food and beverage industry	0.0524	.
Secondary	Consumption goods	Clothing industry	0.0467	.
Secondary	Constuction	Construction (incl. ancillary construction)	0.0446	.
Tertiary	Trade and insurance	Commerce	0.0493	.
Tertiary	Trade and insurance	Insurance	0.0059	.
Tertiary	Transit, gastronomy, other	Transport	0.0487	.
Tertiary	Transit, gastronomy, other	Catering and tavern industry	0.0420	.
Tertiary	Transit, gastronomy, other	Other	0.0176	.

Figure 4-B1 – German Empire, Army Districts, and Regiment Districts

a) Army districts

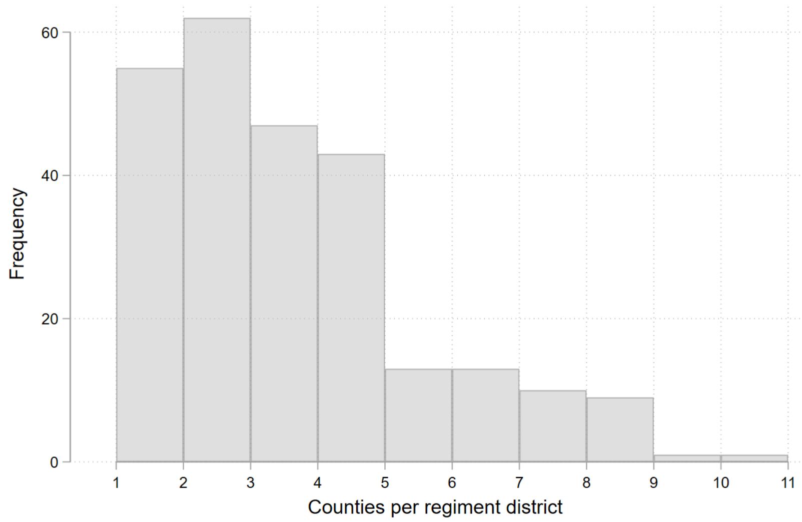


b) Regiment districts



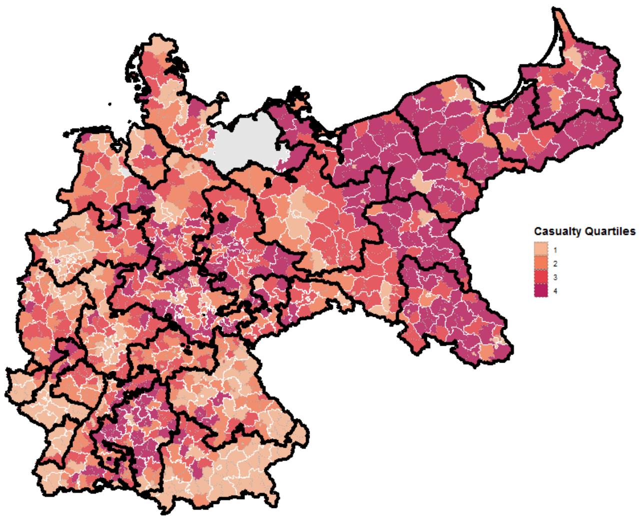
Notes: Figure 4-B1 a) shows the 1914 German army districts ('Armeebezirke'). Thick black lines depict army district borders. Gray borders depict counties. Figure 4-B1 b) shows the 1914 German regiment districs ('Landwehrbezirke'). Thick black lines depict army district borders. Thin black lines depict regiment district borders. Gray borders depict counties.

Figure 4-B2 – Number of Counties per Regiment District



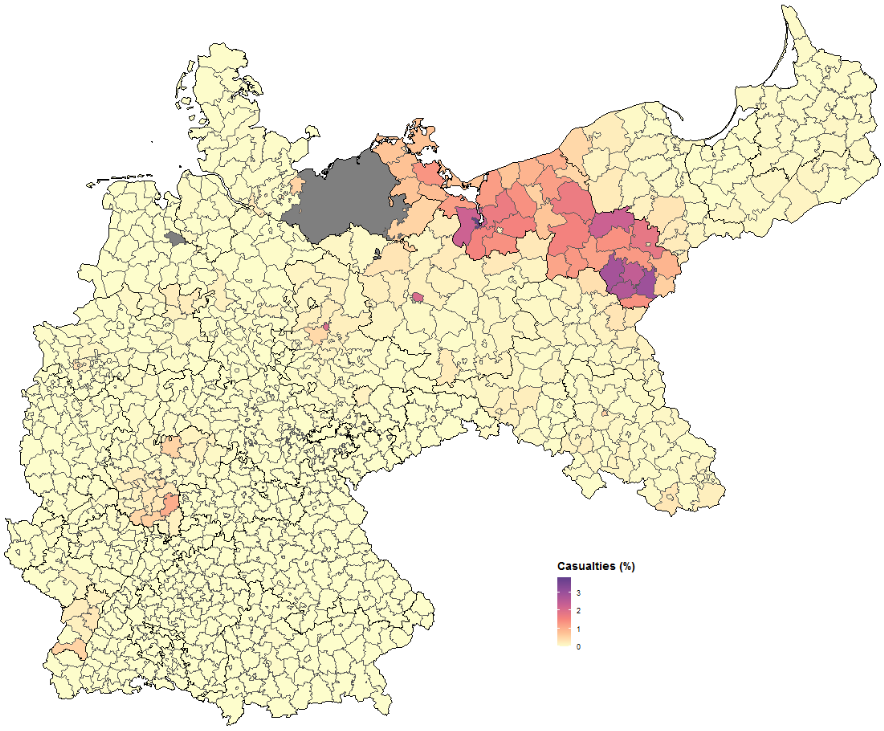
Notes: This graph reports the distribution of the number of counties within a regiment district.

Figure 4-B3 – Casualty Quartiles



Notes: This figure illustrates the geographic distribution of the casualty quartiles for our identification of local marginal effects. Thick black lines depict army district borders. Thin grey borders depict regiment district borders. Thin dotted gray lines depict county borders. Grey areas denote missing data.

Figure 4-B4 – Birthplaces of Casualties from the 49th Infantry Regiment



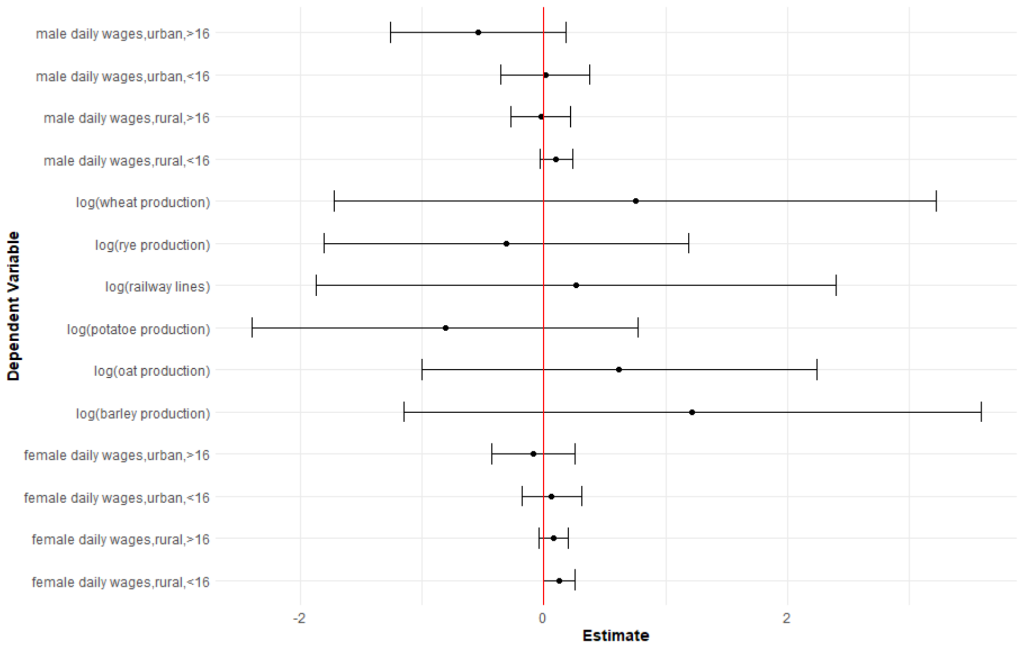
Notes: This map shows, for the 49th Infantry Regiment, which share of the regiment's total casualties fell into each German county. Thick black lines depict army district borders. Thin gray lines depict county borders. Grey areas denote missing data.

Table 4-B3 – Employment Effects, Pre-Trends

	1867–1882 (1)	1882–1895 (2)	1895–1907 (3)	1907–1925 (4)
Panel A: primary sector				
Casualties × Post	0.00568	-0.00390	0.00387	-0.00274
	(0.00512)	(0.00295)	(0.00307)	(0.00535)
Observations	524	524	524	518
Dead × Post	0.00048	0.00546	0.00470	-0.01773**
	(0.00720)	(0.00540)	(0.00505)	(0.00896)
Observations	524	524	524	518
Panel B: secondary sector				
Casualties × Post	0.00972*	-0.00215	0.00514*	0.01111
	(0.00521)	(0.00403)	(0.00264)	(0.01447)
Observations	524	524	524	518
Dead × Post	-0.00203	-0.00206	0.00082	-0.01540
	(0.00828)	(0.00630)	(0.00732)	(0.02121)
Observations	524	524	524	518
Panel C: tertiary sector				
Casualties × Post	-0.00061	-0.00420	-0.00194	0.00261
	(0.00719)	(0.00483)	(0.00395)	(0.01335)
Observations	524	524	524	518
Dead × Post	0.00512	-0.00108	-0.00031	-0.03401**
	(0.00910)	(0.00861)	(0.00506)	(0.01567)
Observations	524	524	524	518
County FE	✓	✓	✓	✓
Post x Army District	✓	✓	✓	✓
Post x Female Share	✓	✓	✓	✓
Post x Quartiles	✓	✓	✓	✓

Notes: Results of Difference-in-Differences regressions following Equation 10. Outcome variables are the shares of a county's employment in agriculture (Panel A), the manufacturing industry (Panel B), and trade-related occupations (Panel C) over total employment. The main explanatory variable is the share of WWI casualties over the county's pre-war male population. All specifications control for logged population. Standard errors clustered by county: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

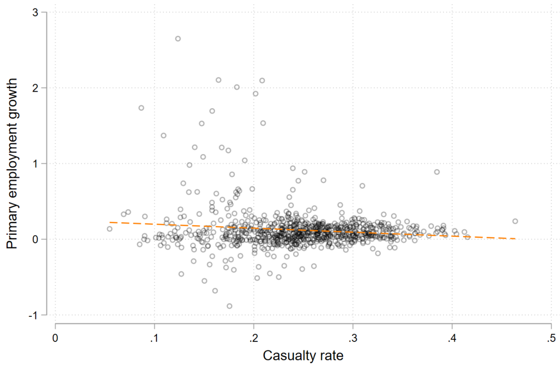
Figure 4-B5 – Additional Pre-Trends



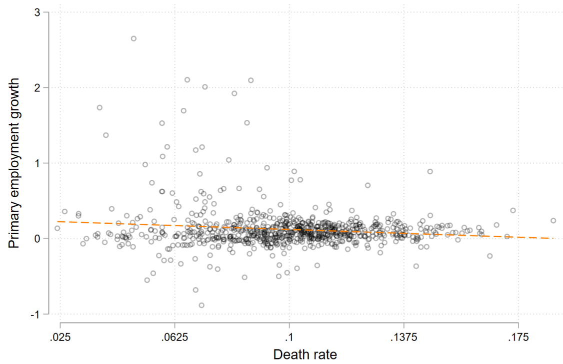
Notes: This graph reports the coefficients and 95% confidence intervals from TWFE regressions. Dependent variables are the length of railway lines in 1856 and 1905, wages in 1892 and 1901, and agricultural production in 1886 and 1896 for Prussia.

Figure 4-B6 – War Exposure and Sectoral Employment Growth

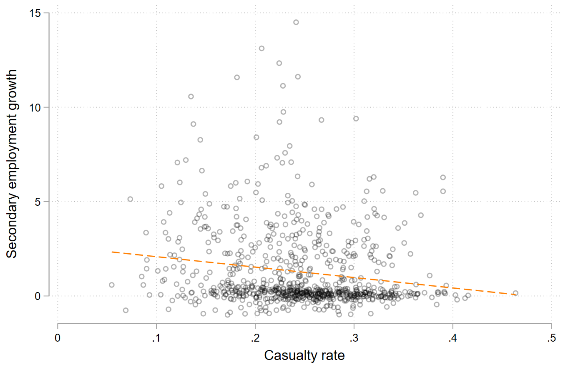
a) Casualty rate: primary



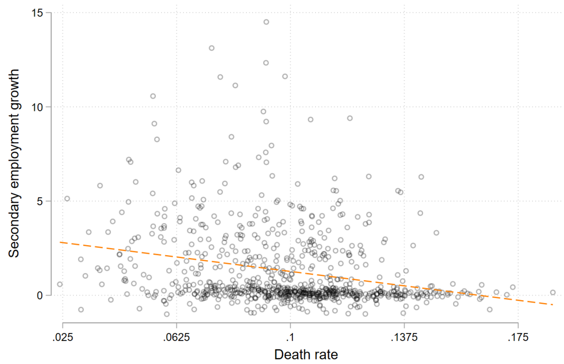
d) Death rate: primary



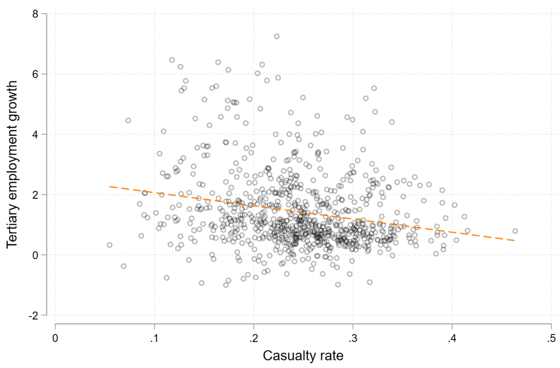
b) Casualty rate: secondary



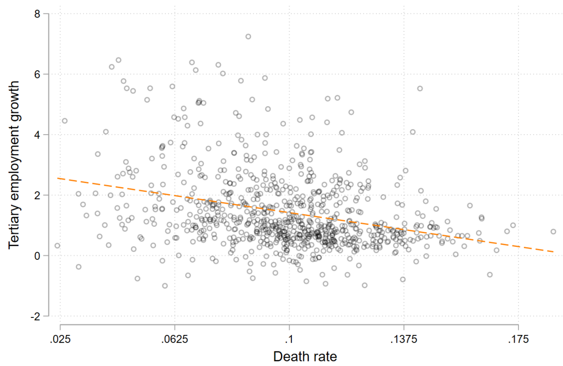
e) Death rate: secondary



c) Casualty rate: tertiary



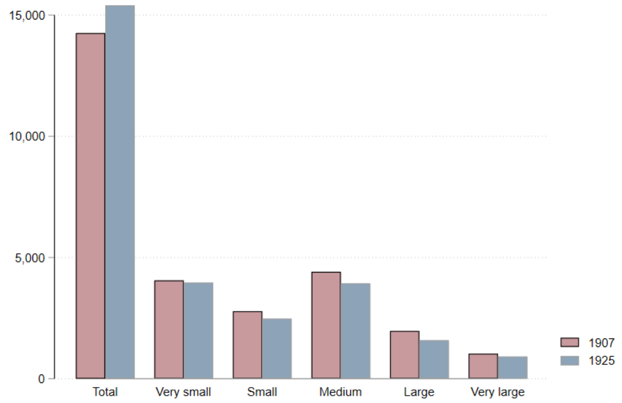
f) Death rate: tertiary



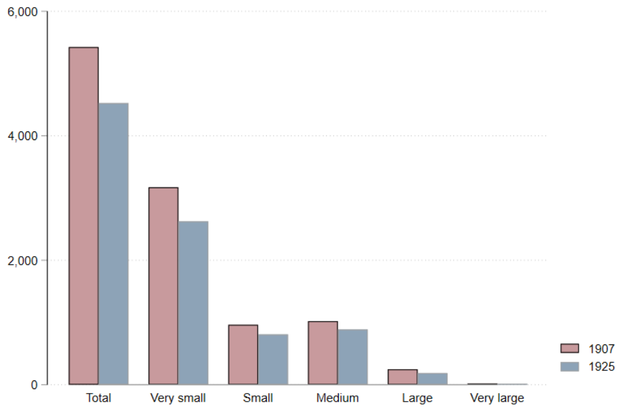
Notes: The graphs report correlations between war exposure and sectoral employment growth rate between 1907 and 1925. Figures a) – c) report correlations for the casualty rate, Figures d) – e) report correlations for the dead rate. Counties based on the final sample and only included once.

Figure 4-B7 – Developments in the Agricultural Sector

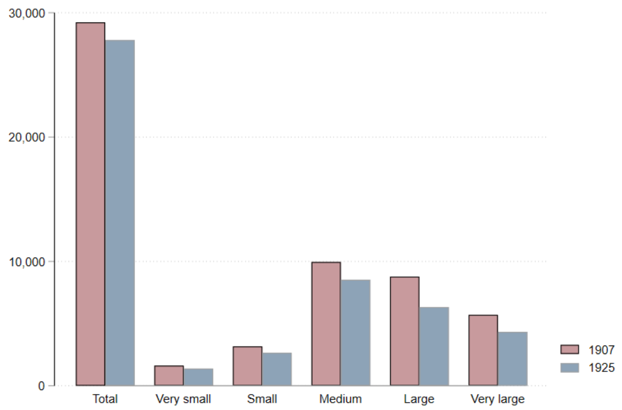
(a) Number of employees



(b) Number of farms



(c) Farm area (in hectares)



Notes: The graphs report the change in agricultural employees (a), farms (b), and cultivated area (c) between 1907 and 1925. We report these numbers separately by farm size, where 'very small' farms constitute farms with less than 2ha area, 'small' farms are between 2–5ha, 'medium' farms between 5–20ha, 'large' farms between 20–100ha, and 'very large' farms are above 100ha.

Figure 4-B8 – Sector Employment Shares, 1907

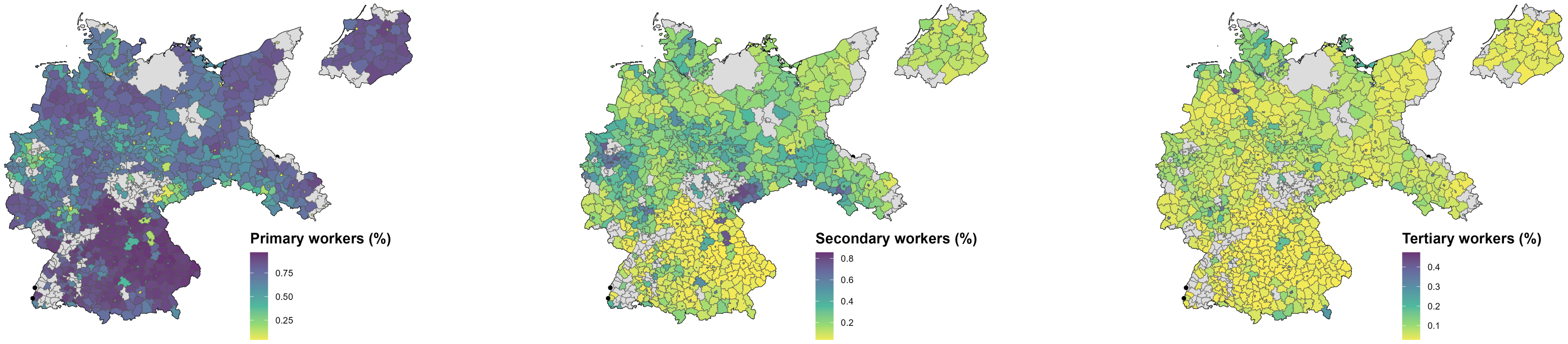
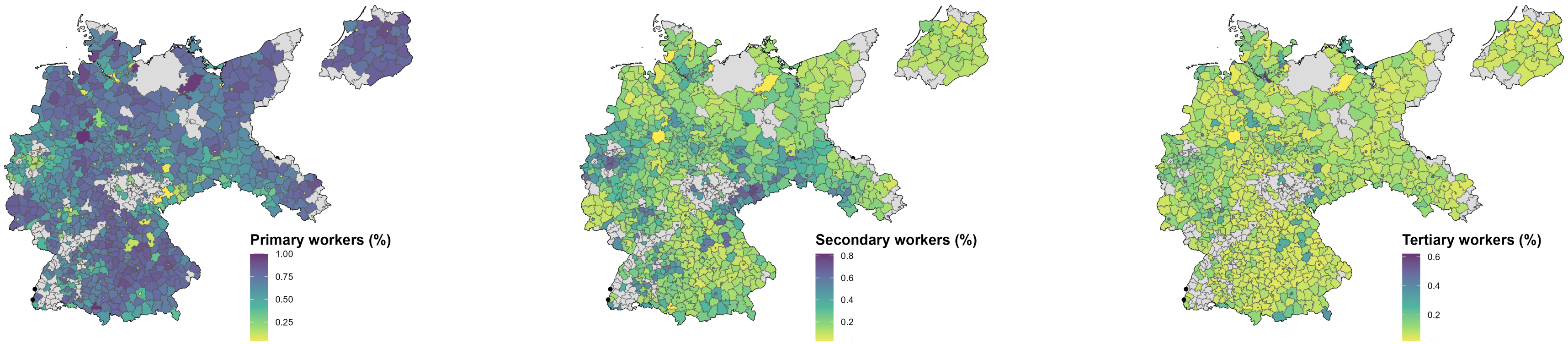


Figure 4-B9 – Sector Employment Shares, 1925



Chapter 5

Independence Movements and Ethnic Politics: The Mau Mau Origins of Ethnic Voting and Distrust in Kenya

5.1 Introduction

The salience of ethnic identities is a prominent feature of politics in many diverse developing countries. In Africa, elections can amount to ethnic censuses at the ballot box (Horowitz, 1985), and generalized trust in others, particularly members of other ethnic groups, is low (Knack and Keefer, 1997, Robinson, 2020).[65] Politicians often exploit such ethnic divisions to mobilize support (Eifert et al., 2010), further weakening electoral accountability and governance. The roots of these social tensions are often traced back to the "divide and rule" approach of European colonizers (Ali et al., 2018) and differences in pre-colonial institutions (Gennaioli and Rainer, 2007, Michalopoulos and Papaioannou, 2013). Nevertheless, there is remarkable variation in the importance of ethnicity in politics among countries that share much of their colonial history. For example, ethnicity is the defining cleavage in Kenya's "high-stake ethnic politics" (Mueller, 2020) whereas it plays less of a role in neighboring Uganda or Tanzania (Miguel, 2004, Conroy-Krutz, 2013, Carlson, 2015, Long and Gibson, 2015).

The aim of this chapter is to show that colonial repression of independence movements and the exploitation of ethnic identities by the colonial power set the stage for ethnic politics and a lack of social cohesion[66] decades later. While others have studied different aspects of how the transition to independent statehood occurred (see, e.g., Garcia-Ponce and Wantchekon, 2018, on democracy) or how nation-building was done post-independence through school curricula (Miguel, 2004) and pro-national propaganda (Blouin and Mukand, 2019), we lack evidence that links the (often violent) path to independence to contemporary ethnic politics and social cohesion. To fill this gap, we exploit the indiscriminate nature of the British response to the "Mau Mau" uprising in 1950s Kenya and local variation in the intensity of the repression.

After their arrival in the 1880s, British settlers claimed some of the colony's most fertile land, while natives where confined to reserves or squatting on white farms (Mosley, 1982, Moradi, 2009, Fazan, 2014). When the acreage per person in the native reserves fell dramatically in the late 1940s, disgruntled farmers, former soldiers, and radical politicians demanded independence and started attacking natives and white settlers who supported the colonial government (Bates, 1987, Anderson, 2005). In response to this nationalist

[65] Identity-based voting is not limited to Africa but has been documented in Latin America (see, e.g., Madrid, 2012) and South Asia (see, e.g., Chandra, 2007).

[66] On foreign intervention and social cohesion during periods of conflict see, e.g., Langlotz (2021).

movement, later coined the “Mau Mau,” Britain set up a system of detention camps and interned anyone they believed to be associated with the uprising. Between 1952 and 1959, the vast majority of three specific Kenyan tribes (the Kikuyu, Embu and Meru) were interrogated and many of them subsequently sent to a camp (see, e.g., Majdalany, 1963, Odhiambo and Lonsdale, 2003, Elkins, 2005). The colonial government sought to frame the uprising as a civil (ethnic) conflict rather than a nationalist uprising. It used members from other ethnic groups and loyalists[67] as fighters, informants, prison guards and overseers (Anderson, 2017). Somewhere between 50,000 and 300,000 people died while being held in a camp or shortly thereafter, while survivors suffered from physical and psychological abuse (Elkins, 2005, Blacker, 2007). Britain was ultimately successful in repressing the rebellion, but the conflict paved the way for independence in 1963 when Jomo Kenyatta, who had himself been detained, became the country’s first president.

We collect a rich body of census and survey data, spanning the period from 1989 until the 2010s, and combine these with archival data on the location of Mau Mau detention camps in the 1950s. We use these data to study two sets of outcomes. First, we examine whether camp exposure affected ethnic allegiances in national politics in the contested 2007 election, as well as contemporary levels of generalized trust and civic engagement. We obtain individual-level votes, a voter’s ethnicity, and basic demographic information from a nationwide exit poll conducted during the 2007 general election (Long and Gibson, 2015). We consider any vote an ‘ethnic vote’ if it is for the presidential candidate preferred by the overwhelming majority of a voter’s ethnic group.[68] We measure the effects of camp exposure on current levels of trust and civic engagement using survey data from the Afrobarometer between 2003 and 2016. Second, we address the lack of hard evidence on the scope and effects of detention (Anderson, 2011). More specifically, we examine whether individuals likely affected by the camps have worse development outcomes in 1989—three decades after the uprising—and today. We focus on a household’s wealth, literacy, and employment using geocoded census data from 1989 and Demographic and Health Surveys conducted between 2003 and 2014.

We analyze the long-run effects of internment on ethnic politics, social cohesion, and individual development outcomes using a triple-difference estimation design that proxies for exposure to detention camps. We focus on the impact of camp exposure on individuals that identify as Kikuyu, Embu or Meru, live within 30 km of a former camp location, and were already born at the time of the uprising. The historical record suggests that the British screened for alleged insurgents solely on an ethnic basis[69] and a significant

[67]The term loyalist refers to individuals who were part of the three Central Province tribes and supportive of the colonial government. It is also often used more specifically to refer to supporters who served in the so-called Home Guard militias or the colonial military, i.e., the King’s African Rifles.

[68]Usually, such a candidate would share the voter’s ethnic affiliation or that of the larger ethnic family or is part of a well-known coalition of ethnic groups.

[69]For a detailed description of the screening process see, e.g., Odhiambo and Lonsdale (2003). Non-Kikuyus (and related tribes) were interviewed and then allowed to return home, while Kikuyu, Embu

share of the 1.5 million Kikuyu, Embu, and Meru were in one of the camps during the state of emergency from 1952 to 1959 (although no precise estimate of the total camp population and their mortality is available). Non-Mau Mau tribes, untreated cohorts, and more distant locations serve as control groups. Our triple-difference design therefore isolates the effect on those that were likely treated by these camps and eliminates bias that may arise from different mechanisms driving selection into camp sites and non-camp sites of Mau Mau and non-Mau Mau tribes, as long as the resulting bias does not change fundamentally for those born before and after 1959. According to contemporary sources, basically all people from the Kikuyu, Embu and Meru tribes, unless known loyalists, were subject to interrogations once encountered by the British authorities, which often involved at least a short incarceration. Even though some Kikuyu, Embu and Meru ended up even supporting the British authorities as, e.g., wards in incarceration camps, they first had to prove their non-involvement in the Mau Mau uprising. We focus on tracing out the direct impact on those that were affected at different points in time (from 1989 until the 2010s). This sets up a relatively strict test where any diffusion of (typically negative) effects of detention to other tribes, sites, or later cohorts are not part of our estimate. Combined with the fact that we follow fewer and fewer survivors over time, we consider most of our results to be a lower bound.[70] We support the validity of our design using placebo checks where we use cohorts that were not immediately affected to construct placebo exposures.

Our analysis establishes two main findings. First, we document that those exposed to detention camps are more likely to vote based on ethnic identity and have a more pessimistic assessment of the trustworthiness of others. For example, they were 13 percentage points more likely to vote for the Kikuyu candidate and outgoing president Mwai Kibaki in the 2007 presidential election, even though they were about 38 percentage points less likely to evaluate him positively. Surveys on attitudes show that camp exposure sharply reduces generalized trust, by more than 80 percentage points, and trust in other people, by about 1.5 categories on a scale of 0 to 3. Moreover, engagement in voluntary community organizations increases by about 0.55 on a scale of 0 to 3. These results confirm that the colonial repression of the Mau Mau significantly altered the fabric of national politics and social cohesion in Kenya by fortifying in-group preferences at the ballot box and raising the level of activity in the local community, i.e., voluntary services for people of the same ethnic group, but at the same time eroding trust in others. Second, we find that detention was a negative shock to the long-term development trajectories of former detainees and affected individuals. Exposure to a camp reduces household wealth by

and Meru suspects almost always faced more intense interrogations.

[70] Theoretically, in our case survivor bias should benefit those better off, i.e., individuals that were hit harder by the emergency become more likely to exit our sample over time, while they simultaneously are more likely to report worse outcomes than those surviving in the sample. Survivor bias should therefore bias our results towards zero.

about one-fifth of a wealth quintile 30 years after the end of the uprising. Today, this effect persists at one-tenth of a wealth quintile. Moreover, literacy falls by 20 percentage points 30 years after the emergency and remains 3.5 percentage points lower for those that survived until the 2000s and 2010s. We also show that the probability of employment of exposed individuals in 1989 is about five percentage points lower, while the likelihood of being out of work or seeking work rises by about one percentage point each.

Our study contributes to a broad literature on nation building and ethnic politics. Widespread ethnic favoritism in the allocation of public funds is one of the key reasons why different ethnic groups compete over control of the central government (Banerjee and Pande, 2007, Franck and Rainer, 2012, Burgess et al., 2015, Kramon and Posner, 2016, De Luca et al., 2018). Once ethnic voting and ethnic favoritism are entrenched, there are only few interventions which appear to be able to (marginally) shift voting behavior.[71] Ali et al. (2018) show that the emphasis on native rule implies stronger ethnic identities in former British colonies (as opposed to French colonies) but this finding cannot explain differences in the prevalence of ethnic politics within British colonies. Our work builds on Garcia-Ponce and Wantchekon (2018), who show that independence movements supported by rural uprisings, rather than urban protest, gave rise to more autocratic regimes. We add within-country evidence explicitly linking the *repression* of the quintessential rural uprising in Sub-Saharan Africa to ethnic voting.

Our work also speaks to a growing literature on the long-run consequences of forced labor, re-education or resettlement camps on local development and social cohesion (see, e.g., Chin, 2005, Dippel, 2014, Lupu and Peisakhin, 2017, Lowes and Montero, 2021, Abel, 2019, Nikolova et al., 2022). The effects of detention and resettlement on development trajectories appear to depend on who went to these camps and what they experienced there.[72] Moreover, the literature typically finds higher levels of trust towards the in-group following traumatic events (see, e.g., Lupu and Peisakhin, 2017, on descendants of Crimean Tatars) or higher levels of trust in general (see, e.g., Abel, 2019, on mixed resettlement camps in South Africa or Lowes and Montero, 2021, on rubber concessions in the Congo Free State).[73] Although the Mau Mau fighters were mostly peasants, the colonial government treated all members of related tribes as suspects and deliberately attempted to break ethnic bonds (both within and across ethnic groups). We show that this translates into both less generalized trust, more civic engagement in the local community, and—going beyond a shift in attitudes and values—a revealed preference for

[71] See, e.g., Ichino and Nathan (2013) on voting in mixed neighborhoods, Casey (2015), Conroy-Krutz (2013), and Carlson (2015) on information, or Arriola et al. (2022) on cross-ethnic endorsements.

[72] Toews and Vezina (2020), for example, show that areas and firms *around* Gulags with a higher share of skilled intellectuals, artists, politicians, and affluent peasants are more prosperous today, whereas Chin (2005) documents large negative effects of internment on labor market outcomes of Japanese-Americans during World War II.

[73] Some studies suggest the opposite. Nikolova et al. (2022), for example, find that trust and civic engagement are universally lower for people that live near Stalin's gulags.

in-group candidates in national elections.

Last but not least, ours is the first study (to the best our knowledge) to quantitatively evaluate the effects of the Mau Mau uprising and study its role in Kenya's post-independence politics. The systematic destruction of records by the colonial government and the British authorities (Anderson, 2011) implies that the voluminous literature on the Mau Mau and their detention is almost exclusively qualitative in nature (see, e.g., the extensive interviews conducted in Elkins, 2001, 2005).[74] By combining archival data on camp locations with geocoded historical census data and contemporary surveys, we show that internment represented a lasting negative shock to the individual development trajectories of likely internees and their relatives. The repression of the Mau Mau movement in Kenya is also a particularly interesting case. The movement was led by the country's largest ethnic group (the Kikuyu) who defined much of post-independence politics, were often accused of favoring their ethnic kin, and violently clashed with the Kalenjin, Luo and Luhya in the aftermath of the disputed 2007 election.

The remainder of this chapter is organized as follows. Section 5.2 provides an overview of the historical context. Section 5.3 discusses the data on internment camps and characteristics of their locations, as well as historical and contemporary outcomes. Section 5.4 describes our triple-difference strategy. Section 5.5 presents the results on the subsequent development trajectories of affected individuals and the impact on ethnic politics and social cohesion. Section 5.6 presents several extensions and robustness checks. Section 5.7 concludes.

5.2 A Brief History of the Emergency

Most historians trace the origins of the Mau Mau uprising to historic grievances over land and increasing population pressures experienced by the Kikuyu on the native reserves (e.g., Bates, 1987, Odhiambo and Lonsdale, 2003, Anderson, 2005, Elkins, 2005). Kenya was one of the few settler colonies in Sub-Saharan Africa and the white settler minority claimed large parts of the fertile land (the so-called 'white highlands,' an area in the central province of Kenya). Since the settler community only numbered a few thousand, most labor was carried out by Africans who were cohabiting on the farm. The remaining native population was assigned land in designated reserves. Increasing mechanization in the 1910s meant that African labor squatting on the farm became redundant, so that the native reserves were becoming increasingly crowded (Fazan, 2014). Similar conditions prevailed in many other parts of Kenya's Central Province.

The colonial government did little to address this problem. Amid heightened grievances, the Kikuyu and related tribes started to form political groups demanding change and

[74] One exception is a descriptive study among 180 former Mau Mau detainees which shows that they experience high levels of post traumatic stress disorder (Atwoli et al., 2006).

opposing (parts or all of) the colonial state. In 1920 the Kikuyu Central Association (KCA)[75] was formed, was banned in 1940, and then reemerged as the Kenya African Union (KAU) in 1944. Both groups challenged the colonial law via petitions and constitutional redresses. The Kenya Land Commission (KLC) established in 1932 was tasked to look into the grievances related to land and to propose lasting solutions for the colony (summarized in Carter, 1934). These, however, were neither far reaching nor adopted by the government.

Growing resentment led a group of several thousand Kikuyu, who were released from sharecropping contracts, to adopt more violent means. The first openly violent act took place on October 9 1952, when a small group of Mau Mau fighters presumably shot Senior Chief Waruhiu in the backseat of his car (Wamagatta, 2016). Numerous attacks followed, often aimed at loyalist Kikuyus, but sometimes involving white settlers. The violence of these attacks stoked widespread fear among the settler community, which pressured the colonial government to react forcefully to the violence. Evelyn Baring—the governor general of Kenya colony—announced a state of emergency immediately after the Waruhiu killing. Jomo Kenyatta, at that time heading the KAU, was arrested together with around 150 other suspected Mau Mau leaders. When these attempts failed to stem the violence, several counter-insurgency laws were announced by the government between January and April 1953. These new laws permitted unhindered information collection about the native population, gave control over any native property to the state, reimposed movement controls in part of the country, and allowed for detention without trial (Anderson, 2005). The colonial government instructed their police and military to systematically investigate anyone suspected of loyalty to the Mau Mau and sentenced these suspects to detention. Lacking actionable intelligence about the Mau Mau, the officials started to engage in a large scale interrogation process termed 'screening.'

The main purpose of screening was to identify those who were loyal to the Mau Mau, either by supporting them directly or by providing shelter and food (Elkins, 2001). British police and military relied heavily on loyal natives who helped to identify whether or not an individual could be attributed to the Kikuyu, Embu, or Meru tribes. Once a suspect was identified, the interrogators often resorted to torture and other brutal examination techniques to determine how loyal a suspect was and if the person was willing to squeal on other potential Mau Mau fighters. The rules restricting the British forces—the King's African Rifles and the Kikuyu Home Guard militia—in their interrogation techniques declined steadily over time. Anderson (2012), for example, discusses how violence was first considered a functional tool of interrogation, while after 1956 systematic torture became widespread.[76] In 2013, Britain apologized for subjecting Kenyans "to torture and

[75]The KCA was a political organization acting on behalf of the Kikuyu community addressing their concerns vis-à-vis the British government.

[76]Britian settled a case brought by four Mau Mau survivors in 2012 and payed 19.9 million GPB in compensation to 5,228 survivors involved in a larger class-action law suit. Anderson (2011) describes the

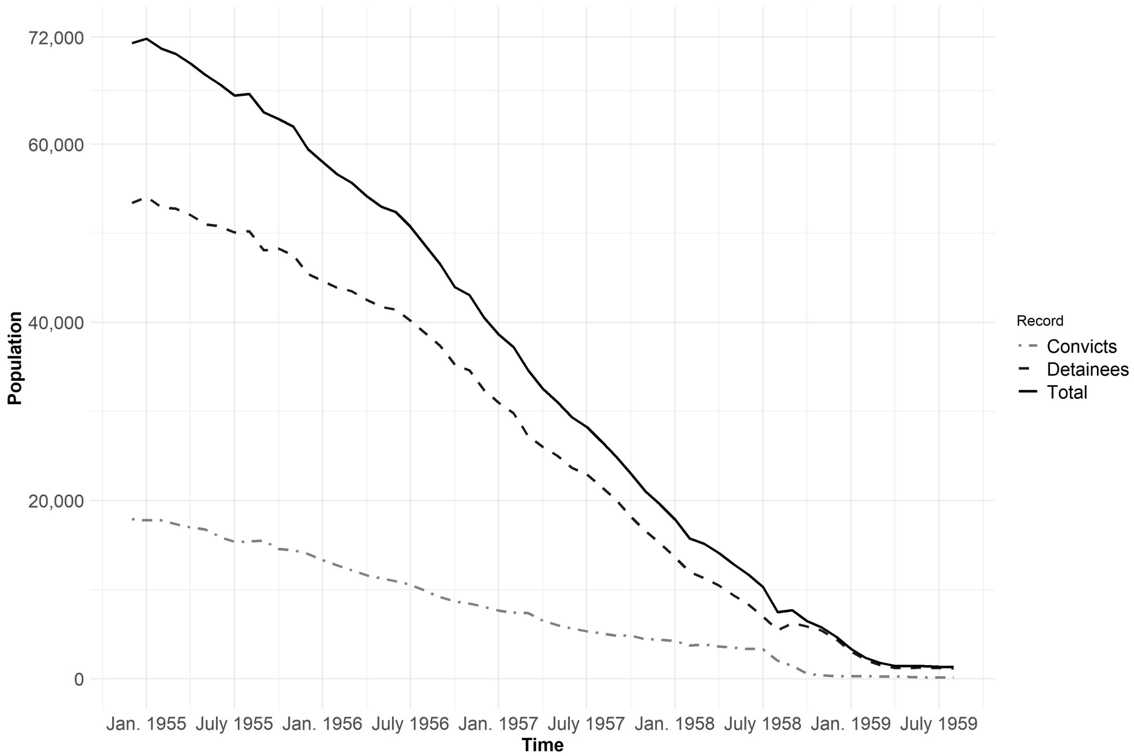
other forms of ill treatment at the hands of the colonial administration" which marred "Kenya's progress toward independence" (William Hague, Foreign Secretary, speaking in the House of Commons on behalf of the government and crown).

The internment camps were organized in a network called the 'pipeline,' in which each inmate was to be assigned to a particular location (Elkins, 2001). Inmates were divided into white, grey or black according to the assessment following the interrogation. Those marked as "white" had confessed and were transferred to camps in their home district with the prospect of eventually being released after additional interrogations and education sessions. Those classified as "grey" were deported to a mid-level work camp for hard labor, re-education and counter-propaganda. Inmates in a grey camp were forced to work in stone pits or similar facilities, e.g., to build the foundation of what is now the Jomo Kenyatta International Airport in Embasaki. Inmates would only leave a grey camp once they were either considered redeemable or hard-core. The latter were designated "black" and deported into exile camps where they often remained until the end of the Emergency in 1959. The repression was disproportionate. The Mau Mau killed 32 white settlers, while thousands of Kikuyu, Embu, and Meru were killed, detained, or both (Odhiambo and Lonsdale, 2003).

The total number of casualties and scale of internment in the British camps is still subject to debate. Elkins (2005) offers an estimate of up to 300,000 Kikuyu, Embu and Meru who are unaccounted for during this period—much more than the 90,000 Mau Mau who were killed according to official numbers (Branch, 2007). Blacker (2007) instead suggests that there were at most 75,000 excess deaths during this period in total. Official sources suggest that about 70,000 people were held in the camps at the height of the Emergency in 1954 (see Elkins, 2001, and Figure 5.1). Many more will have spent at least a few months in the camps during their period of operation from 1952 to 1959. Some have been in over 14 different camps (Kariuki, 1964). We have no record of committals into detention camps under the Emergency Ordinances. However, the steady decline of the average detainee population from 1955–1959, as illustrated in Figure 5.1, hides significant turnover. For comparison, although 25,970 people had been committed to Kenya's non-Emergency detention camps over the course of the year 1954, the daily average population in such camps was only 3,591 (Colony and Protectorate of Kenya, Prisons Department, 1954). Hence, while the average length of the detention sentences during the Emergency may have been longer, the affected population is likely several multiples of the daily average occupancy numbers. The entire Kikuyu, Embu and Meru

allegations brought by Ndiku Mutwiwa Mutua and others. Suspected of giving Mau Mau fighters food, Mutua was dragged out of his hut one morning and violently beaten. After almost losing consciousness, he was driven to a prison where the beating continued. In the camp, Mutua was humiliated, beaten and castrated by European and African officers. Left in his cell to rot, he was rescued by one of the few Mau Mau attacks on a camp. Many of the other camp experiences were similar, often involving hard labor, beatings, torture, castration and rape (Elkins, 2001).

Figure 5.1 – Official Estimates of the Daily Average Detainee Population



Notes: Based on Elkins (2001) who compiled these figures from Monthly Reports of the Ministry of Defence from January 1954 through September 1959.

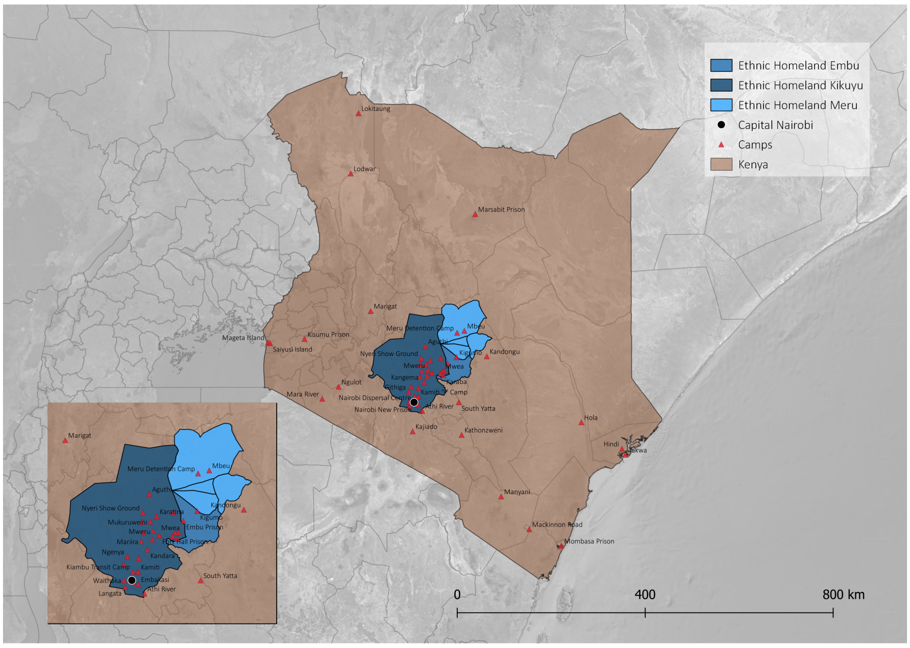
population was only about 1.5 million people according to the 1948 census, about half of whom were children, so that a substantial proportion of the adult population faced a substantive probability of internment.

5.3 Data

Internment Camps. We rely on three primary sources to identify the name, size, type and location of the Emergency detention camps. *i)* Annual reports from the Prisons Department and the Community Development Department of Kenya Colony and Protectorate, *ii)* fortnightly issues of the *Kenya Government Gazette*—the official government source for new legislation and official notices, and *iii)* parliamentary records from the United Kingdom (the UK Hansard). We identify 58 Mau Mau detention camps and prisons in operation between 1952 and 1959. This matches well with the number of camps reported at particular points in time. For example, the Prisons Department refers to 49 special detention and work camps in 1955, after several camps have been closed. We then cross reference our findings with archival records and qualitative information about each camp compiled by Elkins (2001).

Most of these camps were named after the city or township where they were located. We define three precision codes during the manual geocoding process: exact location

Figure 5.2 – Locations of Detention Camps in Kenya



Notes: Red triangles illustrate detention camp sites in Kenya. Homelands of the three Mau Mau-related ethnic groups are added in overlayed patterns.

(accuracy below one km), city or township (accuracy below 5 km), and area or location (accuracy of 5–10 km). For 22 camps we are able to identify the exact location or building of the camp using newspaper articles and other information, for 34 sites we can identify the city or township, and for two sites we are only able to match the camp at a level corresponding approximately to a census location. The camps are plotted in Figure 5.2.

The camps cluster in Kenya's Central Province. The historical homelands of the affected tribes of Kikuyu, Meru and Embu are located in this province. Camps were set up within or close to the home districts of the targeted population. Former inmates were released to their home location, under the auspices of loyalist chiefs and severe movement restrictions, enforced through a passbook system (see, e.g., Anderson, 2000, on the reintroduction of the Kipande). Many camps are located close to Nairobi which was the site of some of the heaviest counter-insurgency crackdowns by British and African forces. Away from the cluster, camps were established further away towards the East African coast or closer to the border region with Uganda and today's South Sudan. The selection of detainees into the different camps occurred via the pipeline, so that inmates followed a progression from their capture until release. Large camps within Central Province, such as the Nairobi Dispersal Center or the Fort Hall Reception Center, served as holding camps in which prisoners were held for a limited amount of time for interrogations. Individuals who confessed were transferred into a camp or prison nearby, e.g., to Mbeu, Aguthi, or Kajiado. Political leaders and others who were judged impossible to be redeemed and re-educated were deported into one of the farther away detention camps like

Lokitaung, Lodwar, Mageta Island, Marsabit, Manyani, or Mackinnon Road. According to former inmates (e.g., Kariuki, 1964), it was not uncommon to be transferred across a variety of camps, and repeatedly be moved up and down the pipeline, until one made it to the district work camp.

Ethnic Politics and Social Cohesion. Individual-level voting information in Kenya is difficult to obtain. Aggregate election results are sometimes available at the level of polling stations, but they lack information on demographics and the ethnic affiliation of voters. We use results from the first nationally-representative exit poll conducted in Kenya during the 2007 election (Long and Gibson, 2015). The pollsters interviewed every fifth voter leaving a voting center. 281 centers were randomly selected using proportional stratified random sampling from constituencies and provinces, resulting in a sample of 5,495 individuals. We geocode the polling stations using the (known) coordinates of polling stations, Google, and a registry of schools (which are typically used as polling stations). We were able to identify the exact location for 93% of all stations.[77] The exit poll is a brief survey meant to be answered in a little more than five minutes but still contains ample data on vote choices, perceptions about government performance, ethnic affiliation, and basic demographic characteristics. The poll has an 82 percent response rate, similar to the Afrobarometer (Long and Gibson, 2015), and records actual vote choices for president, which are gathered during the election, as opposed to party preferences one or two years before/after.

There were three candidates in the 2007 race and each of them received the overwhelming majority of votes from their ethnic group: Mwai Kibaki (94% of Kikuyu vote), Raila Odinga (98% of the Luo vote), and Kalonzo Musyoka (85% of the Kamba vote). We measure ethnic voting as a vote for a presidential candidate for whom at least 80% of a respondent's ethnic group voted.[78] This criterion captures two standing ethnic alliances in 2007 Kenya. The Kalenjin and their leader William Ruto strongly supported Odinga, who received 88% of the ethnic vote. The Meru are of the same ethnic family as the Kikuyu and traditionally support their candidate (87% in 2007).[79] While these high levels of support are reminiscent of "ethnic headcounts," Kenyan elections are no-

[77] We coded the remaining 24 using ward centroids (a ward is the smallest electoral unit which usually contains a handful of polling stations that are in close proximity) and the location of markets.

[78] We follow Huber (2012) and take a group-based perspective of ethnic voting. "A group-centered measure is based on the assumption that ethnicization increases when voting behavior by group members becomes more cohesive, making it easier to predict an individual's vote by knowing the individual's group" (Huber, 2012, p. 987). Clearly, it is easy to predict the vote of, say, a Kikuyu voter simply by knowing their ethnic identity but the same goes for a Meru or Kalenjin voter, even though they do not have a co-ethnic candidate in the race due to cross-ethnic coalitions.

[79] We do not include the Embu (90% of whom supported Kibaki) or other groups for whom we have less than 100 respondents in the raw data. Our results are not sensitive to the choice of the (sufficiently high) percent threshold to identify common voting or whether we include groups with as few as 20 respondents.

toriously close, so that small differences in the ethnic turnout or cross-ethnic voting are enough to swing an entire election. To study if performance evaluations of presidents are affected by internment, we use a question on whether President Mwai Kibaki (the Kikuyu incumbent) kept all, most, some, or none of his promises.

Contemporary outcomes related to social cohesion and civic engagement are from the Afrobarometer survey. Rounds 1–6 of the survey, conducted between 1999 and 2015, have been geocoded by BenYishay et al. (2017), while round 7 includes the GPS coordinates of the interviewed households directly (Afrobarometer Data, 1999–2016). Kenya was part of rounds 2–7 which took place during the years 2003, 2005, 2008, 2011, 2014, and 2016. With 1,104 respondents in the smallest round, and 2,398 respondents in the largest one, this amounts to a total of 11,175 observations. Contrary to the DHS data, the geocoding of households in the Afrobarometer rounds 2–6 was done ex post. This leads to considerable variation in geographic precision. The data contain a categorical precision code that assesses the quality of the provided coordinates, where 1 indicates that the coordinate pair corresponds to an exact location and 6 indicates that a location can only be attributed to an independent political entity. The exact location of a respondent is crucial for our identification strategy, which is why we restrict the sample to the two highest accuracy levels (1 = exact place and 2 = "near" or adjacent). The final sample includes a maximum of 6,160 respondents.

We focus on two trust variables—trust in most people and trust in other people—to investigate how the British detention camps affected general trust levels of citizens related to the Mau Mau uprising, compared to others. Trust in most people is a binary variable, where 1 indicates that "most people can be trusted" and 0 stands for "you must be very careful." The response options for trust in others are categorical and are coded as integer values between 0 and 3, where 0 indicates "not at all," 1 indicates "just a little," 2 indicates "somewhat" and 3 indicates "a lot." In addition we look at two variables indicative of civic engagement. The survey asks whether a respondent is a member in a voluntary association with the categorical response options coded 0 for "not a member," 1 for "inactive member" 2 for "active member" and 3 for "official leader." The level of activity in demonstrations is coded as 0 to indicate "no, would never do this," 1 for "no, but would do if had the chance," 2 for "Yes, once or twice," 3 for "yes, several times," and 4 indicates "yes, often."

Individual Development Outcomes. Our outcome variables for the direct impact of camp exposure on individual well-being—wealth, literacy, employment—are drawn from censuses and household surveys.

For contemporary wealth and literacy, we use three survey rounds: 4 (2003), 5 (2008/2009), and 7 (2014), from the Demographic and Health Surveys (DHS) to extract individual information of 62,584 individuals, including their geographical location (round 6 was not a standard DHS survey).[80] We measure household wealth using an index computed on the basis of the DHS. While the DHS do not track income or expenditures directly, the surveys record several variables that can be linked to economic status. These are, among others, access to electricity, type of roof and floor, or whether the household owns a toilet, a TV, a bike, motorbike, or a car. Based on these indicators, the DHS computes a wealth index using principal component analysis and divides households into quintiles on this index (for details, see, Rutstein et al., 2004). In other words, the DHS data allow us to distinguish households located in the poorest 20 percent in Kenya in a given survey year from those located in, say, the richest 20 percent.[81] As a measure of basic education, we create an indicator for literacy coded as one if an individual can easily read a whole sentence.

We supplement the contemporary data with historical census data from 1989. The 1989 census was one of the most comprehensive earlier censuses conducted in Kenya. It has wide geographic coverage, records literacy and housing conditions in a manner similar to the DHS, and, perhaps most importantly, includes each individual's tribal affiliation.[82] Until the 2010 constitutional reform, Kenya was administratively divided into provinces, divisions, districts, locations and sub-locations—the latter are comparable to census tracts or block groups in the United States and are only a few square kilometers in size in densely populated areas. We geocode each sub-location by combining the individual data with census tabulations and digitized maps.[83] Our final sample includes one million observations (every 20th household) located at the geographic centroid of each sub-location in 1989. For household wealth, we follow the DHS guidelines to construct an index that is strictly comparable to its wealth index quintiles (Rutstein et al., 2004). We base the wealth index only on housing condition indicators shared with the DHS surveys (i.e., type of roof, wall and floor, main source of drinking water, type of sewage disposal, cooking fuel, and type of lighting). Literacy is measured in the individual census as the ability to read and write a simple statement in any language. The census also asks all individuals above the age of 10 for their main occupation during the 7 days preceding the interview. We use indicator variables for "worked for pay or profit," "no work," and "seeking work" to study employment outcomes in 1989 but limit the sample to those

[80]The DHS survey enumeration areas were geocoded on site, allowing us to locate a given individual or household within a range of less than 5 kilometers in Kenya (2 kilometers for urban households). Individuals are split among 399 clusters in the 2003 survey, 397 clusters in the 2008/09 survey, and 1,585 clusters in the 2014 survey, where each cluster contains on average between five and ten households.

[81]Cultural, geographic and other differences across countries can influence what kind of roof or floor can be attributed to wealthier as opposed to poorer households across different countries, but these influences are less relevant in our context, as we are only comparing households *within* Kenya.

[82]While tribal affiliation is still surveyed by the enumerators, the Kenya National Bureau of Statistics (KNBS) stopped releasing this information at the individual level in the decennial censuses after 1989.

[83]Location and sub-location names are missing from the micro-data provided by the KNBS. We match the totals implied by the individual data with census reports and tabulations to recover the names of each geographic entity. The names can then be matched to a digitized map of census sub-locations provided by Kenya's International Livestock Research Institute (ILRI).

aged 16 and older to approximate the working age population.[84]

Geographic and Individual Control Variables. Geographic factors directly and indirectly impact historical outcomes, which then may affect economic development until today (e.g., Nunn and Puga, 2012, Sokoloff and Engerman, 2000). For Kenya, the roots of the Mau Mau conflict can be traced to the alienation of some of the country's most attractive lands by the settlers. The area in the highlands with its mild climatic conditions was particularly attractive to Europeans, much more so than the hot, humid and disease ridden areas near Lake Victoria or the coast around Mombasa. Nairobi lies on a plateau (the low and high highlands) with an elevation of almost 1,800 meters, precipitation is regular, the temperature is moderate, and the disease vector is favorable (Whittlesey, 1953).

We account for this exceptional geography with a variety of control variables derived from raster and vector data: elevation (Jarvis et al., 2008), slope (Jarvis et al., 2008), ruggedness (Nunn and Puga, 2012), wheat suitability (FAO/IIASA, 2011), the length of river and road networks (Natural Earth, 2017), prevalence of the tsetse fly (FAO/AGAH, 2007), and malaria suitability (World Health Organization, 2018), precipitation (Willmott and Matsuura, 2001), and temperature (Willmott and Matsuura, 2001). To extract the relevant information, we partition Kenya into grid cells at a $0.1° \times 0.1°$ resolution (approx. 11km $\times$ 11km). We then spatially join these grid cells with the geolocated survey and census data described earlier. In addition, we control for the great circle distances to Nairobi, the forests in which the Mau Mau fighters were hiding, i.e., the Aberdare Range and Mount Kenya, and the province capitals.

We also use a basic set of individual characteristics that are available in every survey or census that we analyze. Apart from a respondent's ethnic affiliation, these are age, gender, and whether the location is urban or rural. In most surveys, we observe age as a continuous variable, so that we precisely identify the Mau Mau cohort. In the 2007 exit poll, age is recorded in approximately five year brackets.[85] We consider those aged 50 and older to be the relevant Mau Mau cohort.

5.4 Empirical Strategy

Specification. Our approach to approximating the experimental ideal of random assignment to camps is a triple differences-in-differences (DDD) strategy. Triple differences were first introduced by Gruber (1994) and are becoming increasingly popular since they allow for weaker identification assumptions than difference-in-differences (DD) estimation, offer

[84] Other possible answers are "on leave/sick leave," "working on family holding," "student," "retired," "disabled," "home maker," and "other."

[85] The brackets are 18–24, 25–29, 30–34, . . . , 56-60, 60 and above.

estimates of the spillover effect to non-treated units (or bias), and yield the same answer as DD designs if both constituent DDs are unbiased (Olden and Møen, 2022).

We construct the DDD along three dimensions. First, we define an indicator that is unity if an individual identifies with the Kikuyu, Embu, or Meru tribes to select individuals who were likely to have been accused of Mau Mau activities at the time. Second, a detained individual is likely to have lived close to but not necessarily in the immediate vicinity of a camp. We define our baseline measure of proximity as being within 30 km of the nearest former camp (similar to Isaksson and Kotsadam, 2018, and Abel, 2019).[86] Third, we define an indicator for whether an individual was already alive during the time. This allows us to compare those who were born before 1959—the last year of the Emergency—to younger cohorts that were neither alive nor born in a camp or born while at least one of their parents was detained.[87]

All of our regressions are variants of the following specification:

$$\begin{aligned} y_{il} = \; & \beta_1 M_{il} + \beta_2 P_{il} + \beta_3 C_{il} + \gamma_1 (M_{il} \times P_{il}) + \gamma_2 (M_{il} \times C_{il}) + \gamma_3 (P_{il} \times C_{il}) + \\ & \delta (M_{il} \times P_{il} \times C_{il}) + \mathbf{x}'_{il} \boldsymbol{\phi} + \mathbf{d}'_l \boldsymbol{\psi} + \mathbf{z}'_l \boldsymbol{\zeta} + \mathbf{FE}_{il} + u_{il}, \end{aligned} \tag{11}$$

where y_{il} is an outcome for individual or household i in location l. M_{il} indicates whether the respondent identifies as a Mau Mau tribe (either Kikuyu, Embu, or Meru), P_{il} is a dummy variable equal to one if the individual is close to a former camp location (i.e., within 30km) and C_{il} is an indicator for individuals born before 1959. We typically refer to $P_{il} \times M_{il}$ as 'Exposure' and $P_{il} \times M_{il} \times C_{il}$ as 'Exposure $\times$ Cohort.' $\mathbf{x}_{il}$ is a vector of individual level control variables (age, sex, household size), $\mathbf{d}_l$ is a vector of distances to economic or political centers and areas of shelter of the Mau Mau fighters.[88] $\mathbf{z}_l$ are geographic characteristics of the location or enumeration area (urban, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature). $\mathbf{FE}_{il}$ are different sets of fixed effects varying across specifications.

[86] Isaksson and Kotsadam (2018) use proximity as a treatment indicator in a very different context as ours. They leverage survey respondents' proximity to Chinese aid projects to identify treatment by international aid. Abel (2019) uses proximity similar to us, identifying survey respondents living close to former resettlement camps in South Africa.

[87] Similar DDD strategies have recently been used by Muralidharan and Prakash (2017), in their study of cohorts of Indian girls exposed to a cycling program, and Nilsson (2017), who studies the effects of increased alcohol availability for mothers on the long-term labor market outcomes of their children. Our context differs somewhat, in that we do not observe the actual treatment status of each individual but instead recover an intention-to-treat estimate using the DDD parameter as a proxy for exposure.

[88] The distances to Nairobi, Mount Kenya, and the Aberdare Range account for Mau Mau hot spots. Nairobi is located in the Kikuyu homeland and was the site of Operation Anvil. Mount Kenya and the Aberdare Range are the two forest areas where Mau Mau fighters were based and organized their attacks from. This was well known to British officials, who tried to deprive these areas of food supplies and carried out intense raids near the forest boundaries (Anderson, 2012). Finally, we also include the distance to each province capital, as these urban centers may host a camp site but differ on many other characteristics which could be correlated with our outcomes of interest.

We present our main results by incrementally adding higher dimensions of fixed effects. Fixed effects are omitted in the most basic regressions. We then add age, tribe and province fixed effects to arrive at our preferred specification since it can be consistently estimated for all outcomes. For the strictest set of results, we include camp and distance interval fixed effects (running from 0–30, 30–60, 60–90, 90–120, and 120–150 km) and—if the variation in the data permits—interactions between the tribe, distance interval, and age fixed effects. This implies that we can only estimate all constituent terms of the triple interaction in the model without fixed effects. Age, tribe and province fixed effects remove C_{il} and M_{il}. Distance interval effects remove P_{il} and the high-dimensional interactions of the fixed effects absorb all constituent terms but the DDD interaction. Including these fixed effects generalizes the DDD estimator but does not fundamentally alter the interpretation. We only progressively account for systematic differences among non-Mau Mau tribes, age groups/cohorts, province, distance-by-cohort specific factors, distance-by-tribe specific factors, differences in the demographic composition of ethnic groups, and make sure we do not compare respondents across different camp sites. Accounting for these characteristics is important, as it mitigates the influence of compositional changes among treated and untreated groups (Olden and Møen, 2022).

Our baseline results limit the sample to households within 150 km distance to former camp sites. We use two types of standard errors throughout all tables. Errors clustered on the latitude-longitude pair identifying each location allows respondents in the same enumeration area or survey cluster to be arbitrarily correlated. Conley errors with a distance cutoff at 150 km allow for wide-ranging spatial correlation in the responses (Conley, 1999). Both account for the spatial clustering within in the same enumeration area, but the latter also allow for correlation among different enumeration areas.

Interpreting the triple-difference estimate. Our DDD parameter of interest, δ, captures the effect of being exposed to a detention camp and can be decomposed as follows

$$\begin{aligned}\delta = \; & \mathbb{E}[Y|M_1, P_1, C_1] - \mathbb{E}[Y|M_1, P_0, C_1] - \{\mathbb{E}[Y|M_0, P_1, C_1] - \mathbb{E}[Y|M_0, P_0, C_1]\} - \\ & (\mathbb{E}[Y|M_1, P_1, C_0] - \mathbb{E}[Y|M_1, P_0, C_0] - \{\mathbb{E}[Y|M_0, P_1, C_0] - \mathbb{E}[Y|M_0, P_0, C_0]\}),\end{aligned} \tag{12}$$

where $\mathbb{E}[Y|M_1, P_1, C_1]$ is a shorthand for the conditional expectation $\mathbb{E}[Y|M = 1, P = 1, C = 1, \mathbf{x}_{il}, \mathbf{d}_l, \mathbf{z}_l, \mathbf{FE}_{il}]$ and so on. Eq. 12 shows that the DDD estimate is the difference of two differences-in-differences. In the absence of covariates and other fixed effects, it coincides exactly with the differences in the means of these eight different groups. With covariates and fixed effects, it becomes a generalized DDD. Specifically, the first DD eliminates region-specific confounders common to all ethnic groups of the cohort of interest, while the second DD eliminates region-specific differences for those that were born later. The resulting DDD then reflects the impact of camp exposure on individuals of Mau

Mau-related ethnicity that were already born at the time of the Emergency and live near former camp locations. If there is no bias, conditional on observables and unobserved fixed factors, then the second difference over the non-affected cohort can be interpreted as a placebo DD. In that case, we would expect the treatment effect to be zero in the placebo DD and could proceed with DD estimation. To learn whether this is likely, we follow Gruber (1994) and report in Columns (1) and (2) of our main tables the two unconditional DDs before estimating eq. 12.

Identifying variation. The main advantage of our DDD estimation is that it allows us to weaken the required identification assumptions. Instead of requiring that selection into camp locations is the same for Mau Mau and non-Mau Mau tribes, we can allow for differential selection of Mau Mau and non-Mau Mau tribes, as long as this differential is stable across affected and non-affected cohorts. Put differently, if the two DD estimates suffer from the same bias (due to differential selection into particular locations across groups), then the DDD delivers a consistent and unbiased estimate (Frölich and Sperlich, 2019, Olden and Møen, 2022). This is easy to see in eq. 12. Any bias remaining after the first DD (top line) will be cancelled by the second (second line), provided that the bias is stable across both DDs.[89] While we cannot test the identification assumption in our setting, where the differences are not across time and no data prior to the intervention are available, we exploit the wealth of data on untreated units after the event, so that we can construct robustness checks using placebo DDDs for cohorts that were never directly treated.

5.5 Results

We first examine the effects of the Mau Mau uprising and its repression on politics and social cohesion after independence. All tables follow the same structure. For illustration, Columns (1) and (2) present results from the two separate DDs which form the basis of the DDD estimate. From Column (3) onward we present the DDD parameter of interest after accounting for observable differences across individuals and geographic locations.[90] Column (4) adds fixed effects for age, tribe, and provinces, while Column (5)

[89]Note that we could equivalently rearrange the DDD decomposition and interpret the cohort indicator as time (before and after the intervention). While this would bring the interpretation more into line with traditional DDD strategies and would lead us to talk about parallel trends, we do not think this avenue is attractive in our setting. C_{il} only vaguely resembles time, since we compare everyone born before 1959 to everyone born after. Similar applications of DDD in cross-sectional data (e.g., Muralidharan and Prakash, 2017) have used cohorts in lieu of time but then restricted themselves to adjacent cohorts, e.g., girls (boys) aged 14 and 15 versus girls (boys) aged 16 and 17 in two different states. The equivalent parallel trends assumption would be that, in the absence of treatment, the differential among Mau Mau and non-Mau Mau tribes in proximate (treated) places would trend the same way (across cohorts) as the differential among these two groups in non-proximate (untreated) places.

[90]To reduce clutter, we do not report the DDD without control variables and fixed effects. It can be easily calculated by subtracting the estimates provided in Columns (1) and (2). We also do not report

adds camp and distance interval fixed effects and, for everything but the Afrobarometer, also includes higher order interactions of age, tribe, and distance interval fixed effects.

Ethnic Politics. The key question of this chapter is whether the violent repression of the Mau Mau uprising and the deliberate attempt to break ethnic bonds had exactly the opposite effect and strengthened the salience of ethnicity in ways that are still relevant decades later. Table 5.1 addresses this question.

Panel (a) reports the results for ethnic voting. Columns (1) and (2) show estimates of the simple DDs within treated and untreated cohorts. Members of the Mau Mau-related tribes that were alive during the uprising and live close to former detention camps are 11.5 percentage points more likely to vote for their ethnic candidate (Mwai Kibaki) in the 2007 election than those who are not members of these tribes but live close to detention camps. We observe no such differences in the cohort born after the end of the uprising and, hence, no spillovers to other cohorts or selection into camp locations that differs by tribes. Of course, there could still be a diffusion of ethnic voting as a dominant political strategy that is driven by this historical event but does not depend on tribal affiliation or proximity to former detention camps. The DDD results confirm this basic finding. While the estimated effect first falls relative to the standard error in Columns (3) and (4), it is 13.3 percentage points (with a t-statistic ranging from 1.87 to 2.25) in our strictest specification.

Panel (b) shows that the performance evaluation of the Kikuyu incumbent runs in the opposite direction.[91] Columns (1) and (2) show that the raw data suggest that members of Mau Mau tribes that live close to camps today but were born before 1959 were less likely to view Kibaki's performance favorably, although the effect is moderate and statistically insignificant (-0.223 units on a scale of 0 "did not fulfill promises" to 3 "fulfilled all promises"). The relationship becomes significant at conventional levels once we estimate the DDD design with additional control variables and progressively more stringent fixed effects in Columns (3)–(5). Our strictest specification with camp fixed effects, distance interval fixed effects, and the full battery of interacted fixed effects shows that the direct effect of camp exposure reduced the performance evaluation of Kibaki by a little more than one third of a category. The Mau Mau tribes generally view the incumbent positively. Their mean score is close to 2 ("most promises"), so that this is about 20% of the group mean, while the non-Mau Mau score is close to 1 ("some promises").

the coefficients for the remaining DD interactions in the main text, but provide the full tables in the Appendix.

[91] We limit the sample to the same set of tribes with co-ethnic candidates used in Panel (a) to avoid changing the composition of the sample. The results for the full sample are qualitatively similar (not reported). We run OLS regressions instead of ordered logit regressions to avoid the incidental parameter problem due to the high-resolution fixed effects we employ.

Table 5.1 – Exit Polls in 2007: Ethnic Voting and Performance of Incumbent

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel a) Ethnic voting				
Exposure	0.115	0.042	0.032	−0.044	
(P × M)	(0.066)*	(0.037)	(0.040)	(0.047)	
	[0.054]**	[0.035]	[0.039]	[0.041]	
Exposure × Cohort			0.083	0.068	0.133
(P × M × C)			(0.067)	(0.067)	(0.071)*
			[0.068]	[0.060]	[0.059]**
	Panel b) Performance of incumbent (Kibaki)				
Exposure	−0.223	0.076	0.006	0.098	
(P × M)	(0.178)	(0.095)	(0.084)	(0.099)	
	[0.140]	[0.082]	[0.061]	[0.077]	
Exposure × Cohort			−0.296	−0.292	−0.386
(P × M × C)			(0.171)*	(0.170)*	(0.189)**
			[0.138]**	[0.147]**	[0.163]**
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a)	316	2,277	2,593	2,593	2,593
Observations (b)	312	2,253	2,565	2,565	2,565

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual controls for age and sex as well as geographic controls which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition, we control for the distances to Nairobi, the Aberdare Range, Mount Kenya, and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses, respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Our result that camp exposure raises ethnic voting but lowers performance evaluations of the co-ethnic incumbent might appear puzzling. However, one way to think about ethnic voting is that voters discount their performance valuations of the incumbent (or track record of their candidate) and still vote for them if they are an ethnic kin (Long and Gibson, 2015). This is precisely why ethnic voting is detrimental to accountability and governance (Banerjee and Pande, 2007). The effect of camp exposure on ethnic voting is large, even without considering that the intention-to-treat effects documented here are likely to be a lower bound. The Kikuyu alone were 17.1% of the population in 2009. A 13.3 percentage points drop in ethnic voting would have significantly narrowed the gap between Odinga and Kibaki in the flawed 2007 election results.

Trust. We now turn to a test of the long-run effects of detention on social cohesion. Table 5.2 reports the corresponding estimates. The first panel focuses on generalized trust. Recall that the variable is coded as a binary indicator where 1 indicates that "Most people can be trusted" and 0 corresponds to "You must be very careful." The second panel focuses on trust in others which is based on four different categories ranging from 0 ("Not at all") to 3 ("A lot"). As before, we ignore the binary or categorical nature of the dependent variable and interpret the regressions as linear probability models in the case of the former, or like a continuous variable in the case of the latter.

Table 5.2 – Trust Most and Other People

	DD_1 $(C = 1)$ (1)	DD_0 $(C = 0)$ (2)	DDD (3)	DDD (4)	DDD (5)
			Panel a) Trust in Most People		
Exposure	−0.969	−0.058	−0.065	−0.032	0.007
(P × M)	(0.061)***	(0.043)	(0.053)	(0.061)	(0.062)
	[0.049]***	[0.019]***	[0.038]*	[0.024]	[0.031]
Exposure × Cohort			−0.915	−0.792	−0.880
(P × M × C)			(0.092)***	(0.101)***	(0.085)***
			[0.087]***	[0.065]***	[0.093]***
			Panel b) Trust in Other People		
Exposure	−0.998	0.078	−0.111	0.009	−0.312
(P × M)	(0.329)***	(0.156)	(0.209)	(0.233)	(0.243)
	[0.290]***	[0.081]	[0.118]	[0.167]	[0.215]
Exposure × Cohort			−1.095	−1.505	−1.532
(P × M × C)			(0.344)***	(0.419)***	(0.421)***
			[0.318]***	[0.505]***	[0.513]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (a)	220	1,281	1,432	1,432	1,432
Observations (b)	156	1,142	1,235	1,235	1,235

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual control variables for age and sex as well as geographic control variables which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya, and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses, respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

The raw DDs already summarize our main results. Column (1) shows that those who were exposed to detention are about 98 percentage points less likely to trust others and score an entire category lower on the trust in other people index. The placebo DDs in Column (2) cannot confirm similar effects for the non-Mau Mau cohorts. The effect remains large, no matter if we control for observable characteristics at the individual or location level, add age, tribe and province fixed effects, or include camp fixed effects together with distance interval effects in Columns (3)–(5).[92] In all instances, the probability of a respondent indicating generalized trust falls by 79% to 92% and trust in other people falls by up to 1.5 categories. Considering that Kenya is not a high trust society—only 9% of respondents indicate that others can be trusted—this effect size is remarkable. We repeat this exercise using trust in neighbors and relatives as proxies for in-group trust and find comparable results (see Table 5-A2).[93]

These results are unusual as much of the extant literature suggests that the experience of traumatic events *increases* generalized and in-group trust (see, e.g., Abel, 2019, Bauer et al., 2016, Lowes and Montero, 2021). Instead, our results are more in line with Nunn and Wantchekon (2011) who show that the African slave trade was detrimental to in-group and out-group trust. In the case of the Mau Mau uprising, the historical evidence sheds some light on potential explanations for this result. Breaking the cohesion of the Kikuyu, Embu, and Meru was explicit British policy. It is possible that this policy was very effective in reducing trust across the board. Another plausible interpretation of our results is that all of these variables proxy for generalized trust and our result on ethnic voting can be interpreted as greater in-group trust (after all, ethnic voting is a direct measure of revealed in-group preferences).[94]

Civic Engagement. To test whether detention had lasting effects on civic engagement, we study participation in voluntary community associations and participation in demonstrations. Bauer et al. (2016) document a trend towards more pro-social behavior in post-conflict communities. Kariuki (1964) and Elkins (2005) document many instances of voluntary organization of classes, elaborate systems to spread information, and community support that were developed inside the detention camps.

Table 5.3 confirms this conjecture. The simple DDs and the DDDs in Panel (a) consistently show a positive effect of camp exposure on participation in community associations for the affected cohort. In the strictest specification in Column (5) we estimate that exposed individuals increase their engagement by more than half a category from a

[92] Note that we no longer include higher order interactions among fixed effects (Afrobarometer observations are strongly clustered in space so that there is less effective geographic variation than in the other surveys).

[93] In Kenya, the effect of the detention camps swamps the historical legacy of the transatlantic slave trade. Using Nunn and Wantchekon's (2011) preferred specification suggests that a doubling of slave exports per area decreases trust in neighbors by 0.271 units but relatively few Kikuyu were "exported" in the slave trades, so that this effect is only a fraction of the impact of detention.

[94] This would be in line with the positive effects on cohesion among inmates documented in Kariuki (1964) and Elkins (2005), among others. Unfortunately, we cannot study the effects of detention on in-group (co-ethnic) trust directly, as the relevant questions are only included in round 3 of the Afrobarometer which contains too few exposed individuals.

Table 5.3 – Civic Engagement

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel a) Membership in Voluntary Associations				
Exposure	0.530	0.092	0.042	0.148	0.224
(P × M)	(0.269)**	(0.099)	(0.107)	(0.129)	(0.127)*
	[0.035]***	[0.083]	[0.127]	[0.079]*	[0.080]***
Exposure × Cohort			0.391	0.506	0.541
(P × M × C)			(0.291)	(0.309)	(0.327)*
			[0.136]***	[0.144]***	[0.153]***
	Panel b) Active Participation in Demonstrations				
Exposure	0.068	0.003	0.019	0.095	0.182
(P × M)	(0.192)	(0.074)	(0.081)	(0.091)	(0.086)**
	[0.109]	[0.035]	[0.035]	[0.025]***	[0.021]***
Exposure × Cohort			0.109	0.125	0.107
(P × M × C)			(0.212)	(0.234)	(0.231)
			[0.081]	[0.073]*	[0.128]
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (a)	437	3,658	3,881	3,881	3,881
Observations (b)	545	4,094	4,396	4,396	4,396

Notes: The table reports weighted OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual control variables for age and sex as well as geographic control variables which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya, and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses, respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

mean of about 1 "inactive member" towards being an "active member." What is more, we find some evidence that community activity of Mau Mau tribes around camp sites is elevated in general. Panel (b) adds that we observe few differences in active participation in demonstrations. If anything, Column (5) suggests that Mau Mau tribes near former camp locations are generally more active in demonstrations (but the overall mean is 0.57, right between the answers on protest attendance of "no, would never do this" and "no, but would do if had the chance").

Taken together, these results complement our results on ethnic voting and trust. Camp exposure increases revealed in-group behavior in the form of ethnic voting and higher civic engagement but reduces generalized trust.

Wealth. Our second set of results establishes that exposure to detention camps had long-term effects on individual development outcomes, in terms of household wealth, literacy, and employment status. Differences in wealth accumulation between those exposed to the camps and those who were not are of interest for at least three reasons. First, internment translates into a loss of valuable time. Second, anecdotal evidence suggests that detainees were often expropriated, in effect losing the assets they acquired up to the point of incarceration or being forced to divest (see, Kariuki, 1964, who was forced to sell his business on internment). Third and most importantly, systematic abuse during the interrogations and widespread offenses by prison guards are likely to have significantly affected the physical and mental health of detainees. Such negative effects on well-being will have contributed to further income losses and closed off entire career paths.

Table 5.4 – DHS and 1989 Census: Wealth

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel a) DHS 2003-2014				
Exposure	−0.413	−0.411	−0.059	0.002	
(P × M)	(0.108)***	(0.092)***	(0.067)	(0.065)	
	[0.180]**	[0.174]**	[0.084]	[0.056]	
Exposure × Cohort			−0.196	−0.204	−0.188
(P × M × C)			(0.085)**	(0.083)**	(0.086)**
			[0.090]**	[0.076]***	[0.026]***
	Panel b) 5% Sample of 1989 Census				
Exposure	−0.341	−0.461	0.020	0.042	
(P × M)	(0.098)***	(0.105)***	(0.067)	(0.059)	
	[0.103]***	[0.113]***	[0.041]	[0.061]	
Exposure × Cohort			−0.123	−0.129	−0.100
(P × M × C)			(0.053)**	(0.046)***	(0.042)**
			[0.034]***	[0.029]***	[0.027]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a)	6,631	30,811	37,442	37,442	37,442
Observations (b)	152,081	53,737	205,818	205,818	205,818

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a household's head) control variables for age and sex as well as the household size. The geographic control variables include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya, and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses, respectively.

Table 5.4 analyzes the impact of detention on household wealth. For the DHS surveys, we find that households headed by an individual who was alive during the Emergency and exposed to the camps are ranked about one fifth lower in the quintiles of the wealth distribution today (Columns 3–5). This suggests a strong and lasting income effect which is still visible more than 50 years after detention. The results from the 1989 census confirm that this is already visible three decades after the end of the Emergency when a broader set of treated household is in the sample. The effect size falls to about one tenth of a wealth quintile, which may be due to a variety of reasons. For instance, the census data could be more accurate or there is a negative survivor bias, in the sense that households still headed by camp survivors in the 2000s have been less able to adapt to the post-independence economy. DHS respondents were substantially younger during the uprising than census respondents in 1989, which might indicate a stronger effect on younger children. In any case, the census estimates are usually within a standard error of the DHS results.

Columns (1) and (2) reveal how biased simple DD estimates can be. We observe either approximately the same effect in both groups in the DHS data or even less of an effect on the relevant cohort in the census data. However, this "effect" on the non-Mau Mau cohort disappears immediately when we add individual and geographic control variables in Column (3) and remains close to zero in Column (4). The estimate on the $P \times M$ interaction still measures the effect on the placebo cohort, while the coefficient on the $P \times M \times C$ interaction is the effect on the Emergency cohorts. In other words, the estimates in Column (2) can be entirely explained by observable differences across households and locations, while the estimates in Column (1) shrink but remain sizable in the DDD setting.

Literacy. The ability to read and write, our proxy for basic education, is usually acquired in early years during primary school. We focus on early education, as already literate individuals may have had a chance to accumulate more human capital later on. Much of the anticipated negative effects will run through a lack of parental investments while they were interned, rather than rare instances of children being interned or being born inside detention camps (although both did occur). The microeconomic literature stresses the importance of "critical early windows" where shocks can have lifelong effects on cognitive skills (Cunha and Heckman, 2007). In line with this, we consider learning the ability to read and write a crucial indicator of early parental investments which likely has been negatively affected by the detention of the parents.

Table 5.5 shows the corresponding results. The simple DDs and the implied DDD estimates in Columns (1) and (2) of both panels already suggest a strong negative effect on the exposed members of the Mau Mau cohort. As before, we observe that the effect on the post-1959 cohort vanishes once we add control variables and our battery of fixed

Table 5.5 – DHS and Census: Literacy

	DD_1 ($C = 1$) (1)	DD_0 ($C = 0$) (2)	DDD (3)	DDD (4)	DDD (5)
			Panel a) DHS 2003-2014		
Exposure	−0.182	−0.056	−0.034	0.018	
(P × M)	(0.074)**	(0.015)***	(0.016)**	(0.015)	
	[0.065]***	[0.035]	[0.028]	[0.019]	
Exposure × Cohort			−0.086	−0.138	−0.167
(P × M × C)			(0.067)	(0.067)**	(0.073)**
			[0.048]*	[0.017]***	[0.077]**
			Panel b) 5% Sample of 1989 Census		
Exposure	−0.104	−0.045	−0.037	0.009	
(P × M)	(0.026)***	(0.015)***	(0.014)***	(0.011)	
	[0.016]***	[0.017]***	[0.025]	[0.015]	
Exposure × Cohort			−0.056	−0.034	−0.037
(P × M × C)			(0.015)***	(0.013)***	(0.012)***
			[0.022]**	[0.018]*	[0.002]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a)	6,631	30,811	37,442	37,442	37,442
Observations (b)	97,519	676,321	772,439	772,439	772,439

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a household's head) control variables for age and sex as well as the household size. The geographic control variables include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya, and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses, respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

effects. Columns (3)–(5) contain the main results. For the DHS surveys, we find that individuals exposed to detention camps are around 20 percentage points less likely to read and write. This effect is sizable and up to a fourth of the raw probability to be literate in our sample. Our estimates based on the 1989 census data point in the same direction but the effect size is considerably smaller—down to about 3.5 percentage points. The mean level of literacy of the census sample is somewhat lower with about 72% of the population indicating that they are literate. Clearly this only explains a small part of the drop in the DDD estimate. Most of the discrepancy is likely owed to differences in the age composition. Our exposure variable only affects people who were children during the Mau Mau revolt in the DHS surveys but are now in their 40s and 50s, while the relevant cohort in the 1989 census includes all adults older than 30, who will have often been literate before the Emergency. A larger effect on those aged less than ten during the Emergency is also compatible with the earlier finding of lower household wealth in 1989. So far as wealth proxies for the permanent income of parents, a lack of parental resources is a key impediment to investments in children (Carneiro and Heckman, 2003).[95]

Employment. Table 5.6 complements our results on household wealth by studying employment outcomes 30 years after the emergency. Some detainees will have spent up to six and a half years in custody (if we take the total duration of the Emergency as the upper bound). Apart from suffering from physical and emotional trauma, internment translated into loss of valuable time which they could have spent in gaining experience, earning income, and/or obtaining more education. Chin (2005), for example, finds that the internment of Japanese-Americans during WWII in the United States depressed their labor market earnings by 9–13% 25 years later, but did not make them so unsuitable for the job market to change their overall participation rates.

Table 5.6 shows that we find a sizable impact on labor market participation and unemployment. Exposed individuals are less likely to be working for pay and profit, more likely to be out of work, and more likely to be looking for work. The simple DDs in Columns (1) and (2) document discrepancies between participation rates, whose difference goes in the expected direction. However, they also reveal that Kikuyu, Embu, and Meru that live near former detention sites in 1989 but do not belong to the treated cohorts are generally less likely to work, less likely to be out of work, and less likely to be seeking work than people from other tribes in the same area. These results are indicative of differences in selection or sample composition and not to be interpreted causally. The DDDs with control variables and fixed effects in Columns (3)–(5) once again resolve this seeming contradiction. The triple-differences remain stable, suggesting that treated tribes and cohorts near detention sites are 4.9–5.4 percentage points less likely to work, 0.9–1.7 percentage points more likely to have left the labor force, and 1.1 percentage points more likely to be seeking work.[96] The effects on the untreated cohort of Mau Mau tribes often disappear or turn around once differences in the sample composition are accounted for via control variables and fixed effects. Column (4), for example, shows that non-treated Mau Mau tribes near former camps are more likely to work, no more or less likely to be out of work, and somewhat less likely to be looking for work than people from other tribes that live near former camp sites. Introducing camp and distance interval fixed effects in

[95] The coefficient is likely to be underestimated in absolute value for another reason. Political leaders and educated rebels were often giving classes to non-literate inmates, especially in less violent camps and before torture became systematic (see, e.g., Kariuki, 1964). Hence it seems safe to conclude that 3.5 percentage points is a lower bound of the effect of detention on literacy and that this effect is persistent until today.

[96] The remaining 2–3 percentage points are distributed over the other types of activity.

concert with the full battery of interacted fixed effects does little to change the results of the triple interaction. In sum, the camp experience negatively impacted earnings (if we consider asset wealth as a proxy for earnings) and participation rates three decades later. Moreover, the negative effects on literacy documented above are consistent with both deteriorating employment matches and increased withdrawal from the labor market.

Table 5.6 – 1989 Census: Employment

	DD_1 $(C = 1)$	DD_0 $(C = 0)$	DDD	DDD	DDD
	(1)	(2)	(3)	(4)	(5)
			Panel a) Work for pay or profit		
Exposure	−0.133	−0.110	0.019	0.039	
(P × M)	(0.028)***	(0.024)***	(0.012)	(0.017)**	
	[0.057]**	[0.053]**	[0.023]	[0.031]	
Exposure × Cohort)			−0.052	−0.054	−0.049
(P × M × C)			(0.013)***	(0.012)***	(0.012)***
			[0.014]***	[0.015]***	[0.018]***
			Panel b) No work		
Exposure	0.003	−0.015	−0.003	−0.005	
(P × M)	(0.003)	(0.005)***	(0.004)	(0.004)	
	[0.008]	[0.007]**	[0.005]	[0.003]	
Exposure × Cohort			0.017	0.012	0.009
(P × M × C)			(0.004)***	(0.004)***	(0.004)**
			[0.006]***	[0.005]**	[0.005]*
			Panel c) Seek work		
Exposure	−0.002	−0.015	−0.008	−0.008	
(P × M)	(0.001)**	(0.004)***	(0.003)***	(0.003)***	
	[0.001]*	[0.006]***	[0.003]**	[0.003]***	
Exposure × Cohort			0.011	0.011	0.011
(P × M × C)			(0.003)***	(0.003)***	(0.003)***
			[0.004]**	[0.004]***	[0.005]**
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a–c)	222,297	267,446	489,743	489,743	489,743

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a household's head) controls for age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya, and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses, respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

5.6 Extensions and Robustness

We perform several tests to support the causal interpretation of our results and explore potentially relevant heterogeneity in our data. First, we conduct placebo tests on untreated populations. Second, we vary our definition of proximity to the camps to get a sense of how the treatment diffuses around these locations. Third, we explore heterogeneity in camp types to understand if some camps were worse than others. We discuss the findings of the robustness checks below but relegate all tables to the Appendix.

Placebos Cohorts. Our key identification assumption is bias stability, or, put differently, that differential selection into camp locations and non-camp locations for Mau Mau tribes and non-Mau Mau would have been the same in the absence of treatment. We support this assumption by running placebo DDDs where we remove the treated cohort entirely and test for differences among placebo populations that were born after the end of the Emergency. For these tests, we leverage the contemporary surveys (rather than the census data), as these contain a larger number of potentially untreated cohorts than the 1989 data.

The results show that differential selection is stable across the range of key outcomes we consider in this chapter. Our estimates of placebo DDDs in five year intervals are always insignificant at the 5%-level for wealth and literacy (see Table 5-A3), insignificant for ethnic voting and performance of the incumbent (see Table 5-A4), insignificant for all but the cohort that is adjacent to the treated cohort in the case of trust (see Table 5-A5) and always insignificant for civic engagement (see Table 5-A6). Given the large number of placebo tests, we take this as strong evidence that our design is valid.

Proximity. Next we alter the proximity definition to test the effect on those living within 10 to 90 km of a camp site. We exclusively use the census data for these perturbations, since the sample is large enough and contains a density of sub-locations high enough to examine small variations in distance to a camp. In addition to the proximity adjustment, we also vary the cutoff until which the control group is included. This has the effect of comparing individuals which are increasingly close by and hence unlikely to vary on unobserved factors other than exposure to a camp site.

The results show that individuals and households in close vicinity to a camp drive our results (see Tables 5-A7 and 5-A8). In both tables, the first column limits the proximity indicator to those that live within 10 km and then varies the maximum spatial extent of the control group to 60 km, 100 km and 150 km (Panels (a), (b), and (c), respectively). At 10 km, the DDD estimate for wealth is between -0.108 and -0.162 of a quintile, while the estimate for literacy is between -4.6 and -5.1 percentage points. Both are highly significant at conventional levels in all variants. The other columns expand the proximity definition. We observe that the effect persists for those living up to 50 km near a former camp site but shrinks substantially in size and significance. The treatment effect vanishes from 70 km onward.

Camp Types. Finally, we test the sensitivity of our results with respect to the type of camp that an individual was exposed to. Elkins (2001) provides us with a classification of camps which we supplement with estimates of their capacity. We distinguish between special, holding, exile, large, and work camps. Large camps have a capacity of above 5,000 detainees at any point in time. While there is some reason to believe that there may be some heterogeneity in the treatment effects across camp types, it is hard to establish a clear hierarchy. Some exile camps involved comparatively little hard work and abuse, while the treatment of detainees in some work camps was particularly harsh. A change in camp command could alter the experience of inmates completely (Kariuki, 1964).

Tables 5-A10 and 5-A13 show that the effects on wealth and literacy are driven by work camps which make up almost 60% of the camps in our sample. Removing special camps and exile camps for political detainees raises the DDD estimates. Using only work camps also leads to estimates that are about 15–21% larger than our baseline. Note that we drop all those who are close to any of the omitted camp types to not pollute the control group.[97]

5.7 Conclusion

Our study links the violent transition to independence to contemporary ethnic politics and social cohesion. To do so, we explore the long-run effect of the systematic repression of a nationalist uprising during Kenya's late colonial period in the 1950s. This case offers a unique window into understanding how the salience of ethnic identities in politics can come about. The colonial government repressed the emergence of a common national identity by screening everyone that was vaguely associated with the Mau Mau movement and detaining many without cause. In those camps, the government attempted to break ethnic bonds, and subjected inmates to systematic abuse. Today, Kenya is unique among its neighbors in the sense that ethnic allegiances are the defining cleavage in politics and brought the country close to civil war in the aftermath of the 2007 presidential election.

We analyze the indirect effects of camp exposure on ethnic voting and social cohesion, as well as the direct effects on individual well-being. In a first set of results we turn toward voting behavior, trust, and civic engagement to trace out the political implications of the repression. We document that those who were exposed to the detention camps, are more likely to vote along ethnic lines and are even willing to discount their relatively

[97] Tables 5-A11 and 5-A12 repeat this analysis using the Afrobarometer data. The pattern shares some similarities but the limited variation in this survey relative to the census data does not lend itself to drawing more definitive conclusions.

more negative assessment toward their co-ethnic incumbent. We also observe an increase in civic engagement, as evidenced by greater engagement in voluntary community organizations. Nevertheless, the indiscriminate nature of the repression in which both Kikuyu loyalists and members of other ethnic groups were used as informants and guards, led to a deep erosion of generalized trust. In a second set of results we quantitatively establish, for the first time, the long-term effects of the detention camps on those who were likely to be detained. We find that to this day, those individuals and households that were exposed to the Mau Mau camps, are less wealthy, less literate, and have poorer labor market outcomes.

Our research broadly supports the notion that the experience of traumatic levels of violence and war increase local cooperation, even when those affected by such violence suffered deeply. However, the Kenyan case shows that this behavior can have a distinct pro-ethnic feature which can represent a significant challenge for contemporary politics and express itself in low levels of trust towards others in the larger society.

5.8 Appendix

5.8.1 Definition of Variables

Internment Camps

Proximity to Internment Camp: The Proximity to an internment camp is a binary indicator equal to one if the respondent is close to a former camp location, i.e., within 30 km. Locations and Types of internment camps were collected from various sources by the authors. To asses the level of local exactness with respect to the geographic coordinates, we assign three codes of precision: (i) exact location (accuracy below 1 km), (ii) city or township (accuracy below 5 km), (iii) and area or location (accuracy of 5–10 km). Based on historiography we distinguish between special (for hard-code Mau Mau supporters), holding (set up very hastily after mass arrests), exile, large (capacity $> 5,000$ detainees), and work camps (included hard labor). All camps maintained re-education and counter-propaganda. Source: Issues of the Kenya Government Gazette, parliamentary records from the United Kingdom (UK Hansard); Colony and Protectorate of Kenya, Prisons Department (1954), Elkins (2000).

Census Data

Wealth: We construct a wealth index following the method from the Demographic and Health Surveys (Rutstein et al., 2004). Drawing from various survey question on a household's housing conditions (i.e., the type of roof, wall, and floor, the main source of water, the main type of sewage disposal, the main cooking fuel, and the main type of lighting), we run a Principal Component Analysis (PCA) and allocate households to quintile-categories based on their PCA scores. Hence, our wealth variable is a categorical variable, where the values 1 to 5 assign a household to a given wealth category based on their housing conditions stated in the census survey.

Literacy: The 1989 Census asked every person aged 6 years and over: "Does ... know how to read and write a simple statement in any language?". Enumerators could answer this question with 0 (NA), 1 (Yes), or 2 (No). We code this item as an indicator variable, setting all 0-values to missing, and assigning the value of one to any person where the enumerator ticked "Yes," and 0 otherwise.

Employment: The 1989 Census asked every person aged 10 years and over: "What was ... mainly doing during the last 7 days preceding the Census night?". Enumerators could tick one of the following boxes: 01 ("Worked for pay or profit"), 02 ("On leave/sick leave"), 03 ("Working on family holding"), 04 ("No work"), 05 ("Seeking work"), 06 ("Student"), 07 ("Retired"), 08 ("Disabled"), 09 ("Home makers"), 10 ("Other"). We code three indicator variables that indicate with a value of one whether a respondent answered *a*) "Worked for pay or profit," *b*) "No work," or *c*) "Seeking work," respectively.

Afrobarometer Survey Data

Trust in Most People: The question asked in the Afrobarometer Survey is "[...] Generally speaking, would you say that most people can be trusted or that you must be very careful in dealing with people?" with a binary response option where 1 indicates that "Most people can be trusted" and 0 stands for "You must be very careful." The question is available for Kenya in survey rounds 3 and 5. Source: BenYishay et al. (2017), Afrobarometer Data (1999–2016).

Trust in Other People: The question asked in the Afrobarometer Survey is "How much do you trust [... o]ther people you know?" with categorical response options that are coded as integer values between 0 and 3. 0 indicates "not at all," 1 indicates "just a little," 2 indicates "somewhat" and 3 indicates "a lot." The question is available for Kenya in survey rounds 4 and 5. Source: BenYishay et al. (2017), Afrobarometer Data (1999–2016).

Trust in Neighbors: The question asked in the Afrobarometer Survey is "How much do you trust [... y]our neighbors?" with categorical response options that are coded as integer values between 0 and 3. 0 indicates "not at all," 1 indicates "just a little," 2 indicates "somewhat" and 3 indicates "a lot." The question is available for Kenya in survey rounds 3 and 5. Source: BenYishay et al. (2017), Afrobarometer Data (1999–2016).

Trust in Relatives: The question asked in the Afrobarometer Survey is "How much do you trust [... y]our relatives?" with categorical response options that are coded as integer values between 0 and 3. 0 indicates "not at all," 1 indicates "just a little," 2 indicates "somewhat" and 3 indicates "a lot." The question is available for Kenya in survey rounds 3–5. Source: BenYishay et al. (2017), Afrobarometer Data (1999–2016).

Active Participation in Voluntary Associations: The question asked in the Afrobarometer Survey is "[... C]ould you tell me whether you are an official leader, an active member, an inactive member, or not a member: Some other voluntary association or community group?" with categorical response options that are coded as integer values between 0 and 3. 0 indicates "Not a member," 1 indicates "Inactive member," 2 indicates "Active member," 3 indicates "Official leader." The question is available for Kenya in survey rounds 4–7. Source: BenYishay et al. (2017), Afrobarometer Data (1999–2016).

Active Participation in Demonstrations: The question asked in the Afrobarometer Survey is "[... P]lease tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Attended a demonstration or protest march?" with categorical response options that are coded as integer values between 0 and 4. 0 indicates "No, would never do this," 1 indicates "No, but would do if had the chance," 2 indicates "Yes, once or twice," 3 indicates "Yes, several times," 4 indicates "Yes, often." The question is available for Kenya in survey rounds 2–7. Source: BenYishay et al. (2017), Afrobarometer Data (1999–2016).

Demographic and Health Surveys

Wealth: The Demographic and Health Surveys provide a wealth index computed via Principal Component Analysis (PCA). They conduct the PCA based on various variables about a household's housing conditions, e.g., the type of floor, roof, and wall, the type of water, lighting, or sewage disposal, or whether the household owns a bike, motorbike, or car. Based on the result from the PCA, they allocate households to quintiles according to their PCA scores, which results in a categorical wealth variable taking the values 1 to 5. Source: Rutstein et al. (2004), DHS.

Literacy: The DHS surveys ask respondents to read a sentence from a card in the respondent's native language. The enumerate then notes whether a respondent 0) cannot read at all, 1) is able to read only parts of a sentence, or 2) is able to read a whole sentence. We construct an indicator variable that indicates with a value of 1 whether a respondent is able to read a whole sentence, and zero otherwise. Source: DHS.

Exit Polls

Ethnic Voting: The Exit Polls during the Kenya 2007 election asked respondents which presidential candidate they voted for, and at the same time elicited a respondent's ethnic affiliation. We code ethnic voting as an indicator variable which takes the value of 1 if a respondent voted for the same presidential candidate as at least 80% of the respondent's co-ethnics. This is, several ethnic groups had a clearly favored candidate, for whom at least 80% of co-ethnics voted. For these groups, we code the ethnic voting variable as described above. For the remaining groups, the ethnic voting variable always takes the value of zero. Source: Long and Gibson (2015).

Performance of Incumbent (Kibaki): The Exit Polls during the Kenya 2007 election asked respondents: "Thinking about President Kibaki: did he mostly fulfill his promises, only fulfill some promises, or not fulfill promises since the last election?". Respondents could check one of the following boxes: *a*) "All fulfill," *b*) "Mostly fulfill," *c*) "Only some," *d*) "Not fulfill," *e*) "Don't Know," *f*) Refuse to Answer. We code this item as a categorical variable, where the value of 0 indicates the lowest score ("Not fulfil") and 3 indicates the highest score ("All fulfill"). Source: Long and Gibson (2015).

Geographic Control Variables

Elevation: Average physical elevation in meters within a $0.1° \times 0.1°$ grid-cell resolution, i.e., approx. 11×11 km. Source: Jarvis et al. (2008).

Slope: A function of a grid-cells surrounding elevation in degrees within a $0.1° \times 0.1°$ grid-cell resolution, i.e., approx. 11×11 km. Source: Jarvis et al. (2008), authors' calculation.

Ruggedness: Terrain Ruggedness Index in meters within a $0.1° \times 0.1°$ grid-cell resolution, i.e., approx. 11×11 km. Source: Nunn and Puga (2012).

Wheat Suitability: Agro-climatically attainable yield in kilogram of dry matter per hectare within a $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: FAO/IIASA (2011).

River Length: Length of the river network in km within a $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: Natural Earth (2017).

Road Length: Length of the road network in km within a $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: Natural Earth (2017).

Prevelance Tse Tse Fly: Predicted areas of suitability for tsetse fly species Morsitans, Pallidipes, Austeni and Swynnertoni. The index is created by modelling the "known" presence and absence of the flies using a logistic regression of fly presence against a wide range of predictor variables, such as vegetation, temperature, moisture, for a large number of regularly spaced sample points for each area. The index is then aggregated to the $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: FAO/AGAH (2007).

Malaria Suitability: Temperature suitability for Plasmodium vivax transmission globally, calculated using a dynamic biological model and spatial time series across an average year (1950–2000). The index is then aggregated to the $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: World Health Organization (2018).

Precipitation: Long-term average over monthly mean for 1981–2010 in centimeter within a $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: Willmott and Matsuura (2001).

Temperature: Long-term average over monthly mean for 1981–2010 in degree Celsius within a $0.1^\circ \times 0.1^\circ$ grid-cell resolution, i.e., approx. 11×11 km. Source: Willmott and Matsuura (2001).

5.8.2 Descriptive Statistics

The following descriptive statistics are organized by source of data in the main body of the paper and are reported for the regression sample of the main specification.

Table 5-A1 – Descriptive Statistics by Type & Source of Data

	N	*Mean*	*SD*	*Min*	*Max*
Demographic and Health Survey					
Wealth	37,442	3.078	1.428	1	5
Literacy	60,136	0.774	0.419	0	1
Census					
Wealth	206,171	3.286	1.236	1	5
Literacy	773,840	0.727	0.446	0	0
Work for Pay or Profit	646,749	0.202	0.401	0	1
No Work	646,749	0.0377	0.191	0	1
Seek Work	646,749	0.013	0.114	0	1
Exit Polls					
Ethnic Voting	2,593	0.905	0.294	0	1
Performance of Incumbent (Kibaki)	4,302	1.334	0.717	0	3
Geographic Controls					
Elevation	4,900	794.951	629.038	1.000	3,921.923
Slope	4,884	2.015	2.533	0.000	18.920
Ruggedness	4,968	66.371	94.545	0.000	1,135.225
Wheat suitability	4,962	741.324	1,579.290	0.000	5,838.500
Afrobarometer					
Trust in Most People	1,513	0.09	0.29	0	1
Trust in Neighbors	1,524	1.65	0.88	0	3
Trust in Other People	1,307	1.56	0.84	0	3
Trust in Relatives	1,894	2.17	0.87	0	3
Active Participation in Voluntary Associations	4,116	1.05	1.08	0	3
Active Participation in Demonstrations	5,900	0.57	0.88	0	4

5.8.3 Robustness & Extensions Tables

Table 5-A2 – Trust in Neighbors and Relatives

	DD_1 ($C=1$)	DD_0 ($C=0$)	DDD	DDD	DDD
	(1)	(2)	(3)	(4)	(5)
			Panel a) Trust in Neighbors		
Exposure	−0.994	0.283	0.016	−0.053	−0.194
(P × M)	(0.203)***	(0.172)*	(0.182)	(0.185)	(0.249)
	[0.151]***	[0.124]**	[0.121]	[0.095]	[0.119]
Exposure × Cohort			−1.521	−2.207	−2.274
(P × M × C)			(0.339)***	(0.321)***	(0.344)***
			[0.248]***	[0.194]***	[0.197]***
			Panel b) Trust in Relatives		
Exposure	−0.505	0.097	−0.016	0.042	−0.140
(P × M)	(0.184)***	(0.115)	(0.147)	(0.207)	(0.239)
	[0.126]***	[0.114]	[0.113]	[0.131]	[0.171]
Exposure × Cohort			−0.510	−0.828	−0.898
(P × M × C)			(0.206)**	(0.330)**	(0.380)**
			[0.161]***	[0.214]***	[0.279]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (a)	220	1,292	1,442	1,442	1,442
Observations (b)	270	1,611	1,787	1,787	1,787

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual controls for age and sex as well as geographic controls which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A3 – DHS Wealth and Literacy – Placebo Cohorts

	Born before:				
	1969	1974	1979	1984	1989
	(1)	(2)	(3)	(4)	(5)
			Panel a) Wealth		
Exposure	0.055	-0.076	-0.081	-0.051	-0.207
(P × M)	(0.067)	(0.069)	(0.073)	(0.086)	(0.113)*
	[0.118]	[0.120]	[0.116]	[0.107]	[0.106]**
Exposure × Cohort	0.067	0.083	0.069	0.013	0.181
(P × M × C)	(0.073)	(0.066)	(0.069)	(0.078)	(0.109)*
	[0.061]	[0.055]	[0.052]	[0.064]	[0.122]
			Panel b) Literacy		
Exposure	-0.034	-0.035	-0.031	-0.031	-0.029
(P × M)	(0.016)**	(0.016)**	(0.017)*	(0.017)*	(0.018)
	[0.039]	[0.039]	[0.038]	[0.037]	[0.037]
Exposure × Cohort	-0.031	-0.007	-0.017	-0.011	-0.011
(P × M × C)	(0.028)	(0.021)	(0.018)	(0.016)	(0.018)
	[0.031]	[0.025]	[0.022]	[0.017]	[0.021]
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (a)	30,811	30,811	30,811	30,811	30,811
Observations (b)	59,135	59,135	59,135	59,135	59,135

Notes: The table reports OLS estimates. The unit of analysis is the individual. We subset the DHS Sample to people born after the end of the Emergency in 1959. All regressions control for the base levels of the interaction terms. Household's head controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A4 – 2007 Exit Poll – Placebo Cohorts

	Born before:				
	1962	1967	1972	1977	1982
	(1)	(2)	(3)	(4)	(5)
	Panel a) Ethnic voting				
Exposure	-0.039	-0.042	-0.043	-0.038	-0.039
(P × M)	(0.049)	(0.050)	(0.052)	(0.056)	(0.072)
	[0.043]	[0.042]	[0.047]	[0.059]	[0.050]
Exposure × Cohort	0.053	0.037	0.022	0.003	0.006
(P × M × C)	(0.100)	(0.065)	(0.053)	(0.050)	(0.068)
	[0.091]	[0.064]	[0.048]	[0.047]	[0.044]
	Panel b) Performance of the incumbent (Kibaki)				
Exposure	0.039	0.019	0.0005	-0.053	0.029
(P × M)	(0.097)	(0.100)	(0.101)	(0.104)	(0.123)
	[0.085]	[0.092]	[0.091]	[0.080]	[0.102]
Exposure × Cohort	-0.153	0.049	0.087	0.161	-0.001
(P × M × C)	(0.180)	(0.122)	(0.100)	(0.101)	(0.105)
	[0.189]	[0.109]	[0.071]	[0.099]	[0.080]
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (a)	2,277	2,277	2,277	2,277	2,277
Observations (b)	2,253	2,253	2,253	2,253	2,253

Notes: The table reports OLS estimates. The unit of analysis is the individual. We subset the DHS Sample to people born after the end of the Emergency in 1959. All regressions control for the base levels of the interaction terms. Household's head controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A5 – Trust – Altering the Definition of Cohorts

	Born before:				
	1969	1974	1979	1984	1989
	(1)	(2)	(3)	(4)	(5)
	Panel a) Trust in Most People				
Exposure	-0.106	-0.099	-0.068	-0.043	0.017
(P × M)	[0.051]**	[0.069]	[0.051]	[0.065]	[0.058]
	(0.054)**	(0.061)	(0.049)	(0.069)	(0.057)
Exposure × Cohort	0.180	0.071	-0.018	-0.043	-0.113
(P × M × C)	[0.065]***	[0.102]	[0.043]	[0.067]	[0.077]
	(0.071)**	(0.095)	(0.054)	(0.077)	(0.083)
	Panel b) Trust in Other People				
Exposure	-0.144	-0.135	-0.093	-0.048	-0.162
(P × M)	[0.157]	[0.142]	[0.171]	[0.237]	[0.433]
	(0.170)	(0.156)	(0.191)	(0.258)	(0.436)
Exposure × Cohort	0.527	0.284	0.114	0.062	0.163
(P × M × C)	[0.264]**	[0.243]	[0.233]	[0.263]	[0.451]
	(0.281)*	(0.256)	(0.264)	(0.275)	(0.445)
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (a)	1,281	1,281	1,281	1,281	1,281
Observations (b)	1,142	1,142	1,142	1,142	1,142

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A6 – Active Participation – Altering the Definition of Cohorts

	Born before:				
	1969	1974	1979	1984	1989
	(1)	(2)	(3)	(4)	(5)
	Panel a) Voluntary Associations				
Exposure	-0.002	0.034	0.065	0.108	0.011
(P × M)	[0.102]	[0.110]	[0.112]	[0.159]	[0.157]
	(0.112)	(0.118)	(0.122)	(0.176)	(0.184)
Exposure × Cohort	0.229	-0.014	-0.109	-0.151	0.015
(P × M × C)	[0.259]	[0.243]	[0.219]	[0.239]	[0.190]
	(0.293)	(0.245)	(0.219)	(0.241)	(0.209)
	Panel b) Demonstrations				
Exposure	0.006	0.032	0.040	-0.026	0.048
(P × M)	[0.066]	[0.075]	[0.075]	[0.072]	[0.093]
	(0.079)	(0.086)	(0.088)	(0.095)	(0.128)
Exposure × Cohort	0.112	-0.035	-0.044	0.085	-0.039
(P × M × C)	[0.137]	[0.139]	[0.107]	[0.116]	[0.114]
	(0.155)	(0.151)	(0.129)	(0.139)	(0.148)
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (a)	3,658	3,658	3,658	3,658	3,658
Observations (b)	4,094	4,094	4,094	4,094	4,094

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A7 – Census Wealth – Altering the Definition of Proximity

	Proximity within:				
	10 km	30 km	50 km	70 km	90 km
	(1)	(2)	(3)	(4)	(5)
	Panel a) Cutoff = 60 km				
Exposure	−0.095	−0.065	−0.104		
	(0.062)	(0.054)	(0.067)		
Exposure × Cohort	−0.108	0.001	0.033		
	(0.039)***	(0.044)	(0.071)		
	Panel b) Cutoff = 100 km				
Exposure	−0.028	−0.012	−0.005	0.090	−0.409
	(0.064)	(0.056)	(0.060)	(0.066)	(0.211)*
Exposure × Cohort	−0.160	−0.119	−0.124	−0.179	0.201
	(0.037)***	(0.041)***	(0.045)***	(0.045)***	(0.246)
	Panel c) Cutoff = 150 km				
Exposure	−0.033	−0.013	−0.010	0.073	−0.264
	(0.064)	(0.055)	(0.059)	(0.068)	(0.087)***
Exposure × Cohort	−0.162	−0.121	−0.126	−0.182	0.008
	(0.037)***	(0.041)***	(0.044)***	(0.046)***	(0.068)
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (a)	155,251	155,251	155,251		
Observations (b)	202,103	202,103	202,103	202,103	202,103
Observations (c)	205,818	205,818	205,818	205,818	205,818

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A8 – Census Literacy – Altering the Definition of Proximity

	Proximity within:				
	10 km	30 km	50 km	70 km	90 km
	(1)	(2)	(3)	(4)	(5)
	Panel a) Cutoff = 60 km				
Exposure	-0.006	0.017	0.017		
	(0.009)	(0.014)	(0.024)		
Exposure × Cohort	-0.046	-0.023	-0.018		
	(0.012)***	(0.016)	(0.025)		
	Panel b) Cutoff = 100 km				
Exposure	-0.007	0.012	0.010	0.009	-0.066
	(0.008)	(0.011)	(0.012)	(0.008)	(0.064)
Exposure × Cohort	-0.051	-0.035	-0.029	-0.006	-0.077
	(0.011)***	(0.011)***	(0.011)**	(0.012)	(0.134)
	Panel c) Cutoff = 150 km				
Exposure	-0.011	0.004	0.001	-0.001	-0.112
	(0.008)	(0.012)	(0.013)	(0.010)	(0.022)***
Exposure × Cohort	-0.051	-0.034	-0.029	-0.008	-0.019
	(0.011)***	(0.011)***	(0.011)**	(0.012)	(0.042)
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (a)	579,058	579,058	579,058		
Observations (b)	757,986	757,986	757,986	757,986	757,986
Observations (c)	772,460	772,460	772,460	772,460	772,460

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A9 – DHS and Census: Wealth – Exploring Camp Types

	Camp Types					
	All	No Special	No Holding	No Exile	No Large	Only Work
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel a) DHS 2003-2014			
Exposure	-0.059	-0.039	0.057	-0.078	0.057	-0.018
	(0.067)	(0.068)	(0.071)	(0.142)	(0.071)	(0.072)
Exposure × Cohort	-0.196	-0.199	-0.219	0.007	-0.219	-0.199
	(0.085)**	(0.085)**	(0.078)***	(0.181)	(0.078)***	(0.089)**
			Panel b) 1989 Census			
Exposure	-0.013	0.069	0.184	0.012	0.184	0.076
	(0.055)	(0.060)	(0.053)***	(0.112)	(0.053)***	(0.059)
Exposure × Cohort	−0.121	-0.151	-0.167	-0.250	-0.167	-0.146
	(0.041)***	(0.045)***	(0.041)***	(0.097)**	(0.041)***	(0.044)***
Individual controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓	✓
Observations (a)	37,442	37,442	37,442	37,442	37,442	34,359
Observations (b)	205,818	188,980	188,980	188,980	188,980	188,980

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Household's head controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A10 – DHS and Census: Literacy – Exploring Camp Types

	Camp Types					
	All	No Special	No Holding	No Exile	No Large	Only Work
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel a) DHS 2003-2014					
Exposure	0.005	0.009	0.004	0.009	0.004	0.020
	(0.015)	(0.015)	(0.020)	(0.016)	(0.020)	(0.016)
Exposure × Cohort	-0.159	-0.161	-0.143	-0.188	-0.143	-0.178
	(0.079)**	(0.080)**	(0.087)	(0.082)**	(0.087)	(0.082)**
	Panel b) 5% Sample of 1989 Census					
Exposure	0.004	0.002	0.011	0.017	0.011	0.014
	(0.012)	(0.012)	(0.014)	(0.012)	(0.014)	(0.012)
Exposure × Cohort	-0.034	-0.041	-0.030	-0.051	-0.030	-0.039
	(0.011)***	(0.011)***	(0.012)**	(0.012)***	(0.012)**	(0.012)***
Individual controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓	✓
Observations (a)	60,136	57,414	53,824	60,136	53,824	55,123
Observations (b)	772,460	747,777	697,566	698,726	697,566	710,678

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A11 – Trust – Exploring Camp Types

	Camp Types					
	All	No Special	No Holding	No Exile	No Large	Only Work
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel a) Trust in Most People					
Exposure	-0.068	-0.060	-0.059	-0.065	-0.059	-0.080
(P × M)	[0.033]**	[0.028]**	[0.048]	[0.035]*	[0.048]	[0.034]**
	(0.045)	(0.044)	(0.053)	(0.045)	(0.053)	(0.046)*
Exposure × Cohort	-0.889	-0.864	-0.910	-0.908	-0.910	-0.880
(P × M × C)	[0.077]***	[0.077]***	[0.096]***	[0.082]***	[0.096]***	[0.084]***
	(0.089)***	(0.096)***	(0.101)***	(0.104)***	(0.101)***	(0.102)***
	Panel b) Trust in Other People					
Exposure	0.015	0.032	0.002	-0.045	0.002	0.021
(P × M)	[0.106]	[0.106]	[0.134]	[0.110]	[0.134]	[0.104]
	(0.170)	(0.163)	(0.201)	(0.157)	(0.201)	(0.161)
Exposure × Cohort	-1.163	-1.122	-1.344	-1.199	-1.344	-1.176
(P × M × C)	[0.280]***	[0.289]***	[0.305]***	[0.256]***	[0.305]***	[0.290]***
	(0.354)***	(0.367)***	(0.378)***	(0.370)***	(0.378)***	(0.371)***
Individual controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓	✓
Observations (a)	1,501	1,439	1,089	1,367	1,089	1,397
Observations (b)	1,298	1,245	974	1,165	974	1,205

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A12 – Active Participation – Exploring Camp Types

	Camp Types					
	All	No Special	No Holding	No Exile	No Large	Only Work
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel a) Voluntary Associations					
Exposure	0.029	0.071	0.023	0.077	0.023	0.046
(P × M)	[0.071]	[0.080]	[0.080]	[0.073]	[0.080]	[0.076]
	(0.100)	(0.101)	(0.106)	(0.103)	(0.106)	(0.102)
Exposure × Cohort	0.415	0.302	0.514	0.458	0.514	0.417
(P × M × C)	[0.168]**	[0.177]*	[0.188]***	[0.178]**	[0.188]***	[0.161]***
	(0.284)	(0.289)	(0.301)*	(0.306)	(0.301)*	(0.303)
	Panel b) Demonstrations					
Exposure	0.005	0.012	0.045	0.023	0.045	0.041
(P × M)	[0.054]	[0.057]	[0.067]	[0.050]	[0.067]	[0.052]
	(0.076)	(0.076)	(0.088)	(0.076)	(0.088)	(0.075)
Exposure × Cohort	-0.140	-0.121	-0.155	-0.162	-0.155	-0.156
(P × M × C)	[0.057]**	[0.071]*	[0.062]**	[0.072]**	[0.062]**	[0.068]**
	(0.079)*	(0.087)	(0.085)*	(0.086)*	(0.085)*	(0.084)*
Individual controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓	✓
Observations (a)	4,095	3,898	3,263	3,723	3,263	3,786
Observations (b)	4,639	4,404	3,651	4,227	3,651	4,292

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a household's head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A13 – DHS and Census: Narrowing Cohort Windows

	Cohort Windows				
Years around 1959	10	8	6	4	2
	Panel a) DHS: Literacy				
Treatment (P × M)	−0.167	−0.229	−0.281	−0.343	−0.312
	(0.088)*	(0.093)**	(0.102)***	(0.114)***	(0.149)**
	[0.072]**	[0.081]***	[0.099]***	[0.077]***	[0.099]***
	Panel b) DHS: Wealth				
Treatment (P × M)	−0.343	−0.287	−0.382	−0.381	−0.326
	(0.230)	(0.266)	(0.292)	(0.311)	(0.518)
	[0.081]***	[0.068]***	[0.078]***	[0.169]**	[0.187]*
	Panel c) Census 1989: Literacy				
Treatment (P × M)	−0.019	−0.014	−0.016	−0.006	−0.002
	(0.010)*	(0.010)	(0.010)*	(0.012)	(0.014)
	[0.020]	[0.014]	[0.013]	[0.011]	[0.008]
	Panel d) Census 1989: Wealth				
Treatment (P × M)	−0.108	−0.110	−0.108	−0.126	−0.114
	(0.044)**	(0.044)**	(0.045)**	(0.048)***	(0.057)**
	[0.052]**	[0.051]**	[0.050]**	[0.059]**	[0.052]**
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Camp & Distance Interval FEs	✓	✓	✓	✓	✓
Interactions of FEs	✓	✓	✓	✓	✓
Observations (a)	8,917	6,467	4,641	3,001	1,611
Observations (b)	7,915	5,816	4,241	2,829	1,573
Observations (c)	275,085	214,510	166,632	110,288	66,131
Observations (d)	164,907	143,610	121,433	89,336	58,472

Notes: The table reports OLS estimates. Along the columns, we restrict the sample to individuals born in different windows around the end of the Emergency in 1959. For example, Column (1) samples individuals born between 1949 and 1969, while Column (5) samples individuals born between 1957 and 1961. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Household's head controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A14 – Exit Polls in 2007: Ethnic Voting and Performance of Incumbent

	DD_1 ($C = 1$)	DD_0 ($C = 0$)	DDD	DDD	DDD
	(1)	(2)	(3)	(4)	(5)
	Panel a) Ethnic voting				
Exposure	0.115	0.042	0.033	−0.043	
(P × M)	(0.066)*	(0.037)	(0.040)	(0.047)	
	[0.027]***	[0.007]***	[0.040]	[0.036]	
P × C			−0.083	−0.071	
			(0.058)	(0.059)	
			[0.065]	[0.046]	
M × C			0.023	0.012	
			(0.040)	(0.039)	
			[0.061]	[0.043]	
Exposure × Cohort			0.083	0.067	0.133
			(0.067)	(0.067)	(0.071)*
			[0.058]	[0.051]	[0.066]**
	Panel b) Performance of incumbent (Kibaki)				
Exposure	−0.223	0.076	0.006	0.098	
(P × M)	(0.178)	(0.095)	(0.084)	(0.099)	
	[0.143]	[0.026]***	[0.050]	[0.036]***	
P × C			0.187	0.161	
			(0.136)	(0.139)	
			[0.170]	[0.161]	
M × C			0.237	0.237	
			(0.130)*	(0.125)*	
			[0.078]***	[0.052]***	
Exposure × Cohort			−0.296	−0.292	−0.386
			(0.171)*	(0.170)*	(0.189)**
			[0.148]**	[0.145]**	[0.269]
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a)	316	2,277	2,593	2,593	2,593
Observations (b)	312	2,253	2,565	2,565	2,565

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A15 – Trust Most and Other People

	DD_1 ($C = 1$) (1)	DD_0 ($C = 0$) (2)	DDD (3)	DDD (4)	DDD (5)
		Panel a) Trust in Most People			
Exposure	−0.969	−0.058	−0.065	−0.032	0.007
(P × M)	(0.061)***	(0.043)	(0.053)	(0.061)	(0.062)
	[0.049]***	[0.019]***	[0.038]*	[0.024]	[0.031]
P × C			−0.022	−0.119	−0.125
			(0.056)	(0.060)**	(0.063)**
			[0.062]	[0.075]	[0.087]
M × C			0.857	0.829	0.930
			(0.074)***	(0.093)***	(0.080)***
			[0.072]***	[0.064]***	[0.068]***
Exposure × Cohort			−0.915	−0.792	−0.880
(P × M × C)			(0.092)***	(0.101)***	(0.085)***
			[0.087]***	[0.065]***	[0.093]***
		Panel b) Trust in Other People			
Exposure	−0.998	0.078	−0.111	0.009	−0.312
(P × M)	(0.329)***	(0.156)	(0.209)	(0.233)	(0.243)
	[0.290]***	[0.081]	[0.118]	[0.167]	[0.215]
P × C			0.208	0.247	0.307
			(0.166)	(0.235)	(0.246)
			[0.123]*	[0.182]	[0.224]
M × C			1.206	1.605	1.709
			(0.271)***	(0.378)***	(0.374)***
			[0.299]***	[0.414]***	[0.416]***
Exposure × Cohort			−1.095	−1.505	−1.532
(P × M × C)			(0.344)***	(0.419)***	(0.421)***
			[0.318]***	[0.505]***	[0.513]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (a)	220	1,281	1,432	1,432	1,432
Observations (b)	156	1,142	1,235	1,235	1,235

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual controls for age and sex as well as geographic controls which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A16 – Civic Engagement

	DD_1 $(C = 1)$ (1)	DD_0 $(C = 0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel a) Membership in Voluntary Associations				
Exposure	0.530	0.092	0.042	0.148	0.224
(P × M)	(0.269)**	(0.099)	(0.107)	(0.129)	(0.127)*
	[0.035]***	[0.083]	[0.127]	[0.079]*	[0.080]***
P × C			0.068	0.204	0.209
			(0.131)	(0.144)	(0.146)
			[0.095]	[0.106]*	[0.155]
M × C			−0.363	−0.616	−0.648
			(0.225)	(0.247)**	(0.261)**
			[0.095]***	[0.098]***	[0.068]***
Exposure × Cohort			0.391	0.506	0.541
(P × M × C)			(0.291)	(0.309)	(0.327)*
			[0.136]***	[0.144]***	[0.153]***
	Panel b) Active Participation in Demonstrations				
Exposure	0.068	0.003	0.019	0.095	0.182
(P × M)	(0.192)	(0.074)	(0.081)	(0.091)	(0.086)**
	[0.109]	[0.035]	[0.035]	[0.025]***	[0.021]***
P × C			−0.148	−0.085	−0.090
			(0.082)*	(0.092)	(0.094)
			[0.068]**	[0.069]	[0.061]
M × C			0.027	−0.089	−0.044
			(0.197)	(0.215)	(0.209)
			[0.108]	[0.094]	[0.121]
Exposure × Cohort			0.109	0.125	0.107
(P × M × C)			(0.212)	(0.234)	(0.231)
			[0.081]	[0.073]*	[0.128]
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (a)	437	3,658	3,881	3,881	3,881
Observations (b)	545	4,094	4,396	4,396	4,396

Notes: The table reports weighted OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual control variables for age and sex as well as geographic control variables which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5-A17 – DHS and 1989 Census: Wealth

	DD_1 $(C = 1)$ (1)	DD_0 $(C = 0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel a) DHS 2003-2014				
Exposure	−0.413	−0.411	−0.059	0.002	
(P × M)	(0.108)***	(0.092)***	(0.067)	(0.065)	
	[0.180]**	[0.174]**	[0.084]	[0.056]	
P × C			0.072	0.107	
			(0.047)	(0.045)**	
			[0.064]	[0.065]*	
M × C			0.105	0.082	
			(0.068)	(0.068)	
			[0.092]	[0.061]	
Exposure × Cohort			−0.196	−0.204	−0.188
(P × M × C)			(0.085)**	(0.083)**	(0.086)**
			[0.090]**	[0.076]***	[0.026]***
	Panel b) 5% Sample of 1989 Census				
Exposure	−0.341	−0.461	0.020	0.042	
(P × M)	(0.098)***	(0.105)***	(0.067)	(0.059)	
	[0.103]***	[0.113]***	[0.041]	[0.061]	
P × C			0.257	0.200	
			(0.038)***	(0.033)***	
			[0.042]***	[0.052]***	
M × C			0.196	0.184	
			(0.036)***	(0.033)***	
			[0.084]**	[0.054]***	
Exposure × Cohort			−0.123	−0.129	−0.100
(P × M × C)			(0.053)**	(0.046)***	(0.042)**
			[0.034]***	[0.029]***	[0.027]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a)	6,631	30,811	37,442	37,442	37,442
Observations (b)	152,081	53,737	205,818	205,818	205,818

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a household's head) controls for age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively.

Table 5-A18 – DHS and Census: Literacy

	DD_1 ($C=1$) (1)	DD_0 ($C=0$) (2)	DDD (3)	DDD (4)	DDD (5)
			Panel a) DHS 2003-2014		
Exposure	−0.182	−0.056	−0.034	0.018	
(P × M)	(0.074)**	(0.015)***	(0.016)**	(0.015)	
	[0.065]***	[0.035]	[0.028]	[0.019]	
P × C			0.019	0.059	
			(0.040)	(0.037)	
			[0.026]	[0.018]***	
M × C			0.157	0.148	
			(0.052)***	(0.053)***	
			[0.035]***	[0.027]***	
Exposure × Cohort			−0.086	−0.138	−0.167
(P × M × C)			(0.067)	(0.067)**	(0.073)**
			[0.048]*	[0.017]***	[0.077]**
			Panel b) 5% Sample of 1989 Census		
Exposure	−0.104	−0.045	−0.037	0.009	
(P × M)	(0.026)***	(0.015)***	(0.014)***	(0.011)	
	[0.016]***	[0.017]***	[0.025]	[0.015]	
P × C			0.045	0.034	
			(0.012)***	(0.008)***	
			[0.033]	[0.024]	
M × C			0.029	0.033	
			(0.011)***	(0.010)***	
			[0.017]*	[0.014]**	
Exposure × Cohort			−0.056	−0.034	−0.037
(P × M × C)			(0.015)***	(0.013)***	(0.012)***
			[0.022]**	[0.018]*	[0.002]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (a)	6,631	30,811	37,442	37,442	37,442
Observations (b)	97,519	676,321	772,439	772,439	772,439

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a household's head) controls for age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Chapter 6

Conclusion

This thesis analyzes various aspects via which different types of political turmoil can have persistent economic effects. Many of these aspects are not directly visible, yet they are important to take into account when designing reconstruction policies. Often, subtle heterogeneities or externalities associated with (civil) wars or political violence open up additional channels that lead individuals and economies away from their optimal development paths. These general equilibrium effects of political turmoil and other economic shocks require more attention in the economic literature, which often only focuses on shocks' direct effects. This thesis therefore aims to provide novel data, methods, and research questions that help uncover the full effects of political turmoil and other shocks. Finally, the four chapters in this thesis are meant to point policy makers towards one essential question when thinking about how to re-build economies and societies after times of turmoil: *What changes to the general equilibrium did the turmoil provoke?*

Chapter 2 analyzed the general equilibrium effects of civil conflict. Building on a novel measure for subnational conflict exposure, and highlighting that conflict exposure can vary among locations with the same amount of battles due to pre-defined geographic conditions, the chapter highlights that conflict exposure can indeed have long-run consequences on local economies. This finding deviates from prior results in the literature, which argue that locational fundamentals and economic mechanisms lead to an automatic and full recovery from the damages and destruction of war (Davis and Weinstein, 2002, Brakman et al., 2004, Miguel and Roland, 2011).

The key idea of Chapter 2 is to re-think how we evaluate conflict exposure. My main argument here is that it is not the number or mere occurrence of battles in a location that matters for recovery, but whether people and capital still find save havens in this location, or have to abandon it completely in the search for safety. If the latter is the case, i.e., if the conflict is waging across the whole location, people and capital might settle for good in another place. Reconstruction policies that tend to revive the conflict-ridden locations after the war must take these general equilibrium effects into account and set incentives for people and capital to re-settle in the abandoned places.

In Chapter 3, we provide evidence at a global scale for similar general equilibrium effects of civil conflict. Using bilateral trade data for more than 150 countries from 1995–2014 and introducing a novel way of estimating trilateral effects in a bilateral setting, we find that civil conflicts can persistently alter international trade relationships. Our results show that countries rapidly adjust their supply chains in response to a civil conflict erupting at an important trade partner, and that they do not return back to their old import sources once the conflict is resolved. We further find that such trade relocation encourages importers to sign Free Trade Agreements with alternative exporters. These contracts persistently change the relative cost structures of international trade, making it more difficult for the conflict country to re-conquer their standing at international markets once their civil conflict ends.

Much related to Chapter 2, these results caution policy makers to take into account the general equilibrium effects of civil conflicts. While one country is at war, other nations continue their business and try to compensate their own losses from conflict as good as possible. This compensation entails that they build new international cooperations, which in turn lower the value of the conflict country as a cooperation partner. To remedy these general equilibrium effects and help the conflict country to recover their trade relationships once they restored peace, policy makers must be aware that business in other countries went on during the conflict. Temporary preferential trade policies, e.g., offering tariff reductions to newly peaceful countries, can be an easy yet effective policy tool to promote economic recovery.

Chapter 4 analyzed a historic case study to investigate how violent population shocks during transformative economic periods can affect labor markets. Looking at the German economy before and after WWI, we found that the war casualties especially hit the emerging tertiary sector, because this sector was the least able to replace the workers lost in war. For our analysis, we geocoded 8.5 million casualty list entries to soldiers' birthplaces, and digitized detailed census data from before and after the war. At the fourth administrative (county) level, we ran Continuous Difference-in-Differences estimations following Callaway et al. (2021), and found that a marginal increase in casualties led to a significant decrease in tertiary as well as White Collar employment. Our investigation of mechanisms suggested that these effects occur because the agricultural sector could replace labor by machines, and the industrial sector could staff open vacancies with labor reserves from subsistence farming and the informal sector. Only the tertiary sector, which heavily relied on well-educated White Collar workers, had neither technological advances nor proper labor reserves to draw from.

Our results show an important heterogeneity in the labor market effects of population shocks that can be extended to other settings than war. Especially in current developing countries, population shocks through natural disasters, disease, or out-migration due to Brain Drain, can persistently affect their transformation into modern economies. The more so if these shocks hit sectors that rely on well-educated and specifically trained personnel that is difficult to replace. Hence, policy makers must analyze the sectoral impact of population shocks and evaluate whether the specialized labor shortages they induced can be compensated by incentivized migration or additional investments in special education programs.

Finally, Chapter 5 led the focus to long-run individual effects of political violence drawing on Kenya's struggle for independence as another historic case study. In the 1950s, the Mau Mau rebellion fought the British colonizers for a return of (agricultural) property rights and political autonomy. Britain responded by establishing an elaborate detention camp system, which indiscriminately targeted people from the Kikuyu, Embu, and Meru tribes. We collected rich historical census data, geocoded the locations of detention camps, and combined this dataset with current data on individual voting preferences, social and political beliefs, and household characteristics. We estimated triple Difference-in-Differences regressions, where we identify people likely incarcerated in the British detention camps as belonging to the targeted tribes, living close to a detention camp site, and being born before the detention system ended. We find that the affected people are more likely to vote based on ethnicity than performance, and have less trust in others.

These results show that periods of political violence can persistently change the natural chemistry of societies. Indiscriminately targeting people from specific societal groups, and agitating the rest of society against them, sets the seeds for a toxic cohabitation afterwards. Democratic or economic cooperation become difficult, as the violent riots after the 2007 Kenyan election show. Kenya's violent struggle for independence is one case study with its own specific characteristics, but shares a lot of common features with other nations that either violently reached their way to independence from colonial sovereigns, or had to overcome autocratic regimes. It is important to keep in mind that ousting an autocrat or gaining independence from a colonial power is only the beginning of forming a state, society, and economy. Still 70 years later, Kenya's surviving victims of the British detention camps and their descendants keep fighting for reparations and acknowledgment from the British government. Compensating victims of political violence and steering the society to peaceful and effective cooperation is a necessary first step to stable political regimes and economic growth.

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