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Facultad de CC. Económicas y Empresariales

ESSAYS IN DEVELOPMENT ECONOMICS: ON CIVIL CONFLICT IN NIGERIA

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Abstract

The three essays in this dissertation explore the cause and consequences of civil conflict in Nigeria. The first chapter examines the effect of international oil prices on civil conflict in Nigeria. The analysis uses time variation in global oil prices and cross-sectional variation based on the initial distribution of oil production across Nigerian districts. According to our estimates, an increase in oil price increases the risk of civil conflicts in districts that produce oil by 50 percent. Using data on intergovernmental transfers, public attitude surveys, labor outcomes and pollution, we test for popular theoretical mechanisms of the resource curse and show that positive oil price shocks affect conflict through rising competition for resource rents and through a reduction in social capital. We do not find evidence in favor of mechanisms related to changes in the opportunity cost of engaging in conflict or grievances about pollution and its implications.

The second chapter in this dissertation explores the effect of terrorism on child health. We exploit geographical variation in terror attacks of Boko Haram in Nigeria to show that prenatal exposure to fatalities from terror attacks lead to 0.23 percentage point difference in birth weight between exposed and unexposed cohorts. These effects are stronger for mothers with less education. In addition, we find evidence that parents compensate for exposure to terror during pregnancy by increasing investment in postnatal health care.

The final paper in the dissertation examines the impact of violent conflict on household risk coping strategies by estimating the two-fold effect of conflict victimization and income shock on household consumption in Nigeria. Using panel data, we find that shocks reduce food consumption by 17 percent for victimized households but have little effect on non-victimized households. We test for the mechanisms of consumption smoothing and show that victimized households receive more remittances and invest more in informal support groups. Our results are robust to using a sharp increase in local violence as an event study.

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To my family

Chapter 1

Oil Price Shocks and Civil Conflict in Nigeria

1.1 Introduction

Contention over natural resources is a recurrent theme in many civil conflicts. In the last six decades, at least 40% of intra-state (civil) conflict have been associated with natural resources (see, e.g., Lacina and Gleditsch (2005)). Most of these conflicts are located in sub-Saharan Africa. During the period from 1997 to 2014, there have been at least 320 conflict events related to natural resources in the region.[1]

Existing literature generally highlights two factors in explaining this trend. First, the increase in resource rents provide incentives for insurgents to engage in conflict.[2] Second, the decline of wages in the region reduces the opportunity cost of taking part in violent activities.[3] While a large body of cross-country studies provides evidence of these channels of conflict at the macro-level, the evidence of this impact at the micro-level, however, remains

[1]See Raleigh *et al.* (2010).

[2]See Collier and Hoeffler (2004); Fearon (2005); Besley and Persson (2008, 2011); Berman *et al.* (2017)

[3]See, for example, Miguel *et al.* (2004); Brückner and Ciccone (2010); Besley and Persson (2010); Collier and Hoeffler (1998); Bazzi and Blattman (2014).

scarce.[4] A plausible reason for this scarcity is the difficulty of assessing the relation between civil conflicts and the intra-national location of the natural resources that incite those conflicts (Nillesen and Bulte, 2014).[5]

The primary purpose of this paper is to determine the relationship between primary commodities, especially oil, and civil conflicts in Nigeria. The secondary objective is to ascertain the mechanisms that link commodity price shocks to intrastate armed conflicts in Nigeria. Specifically, we use the increase in international oil price to estimate the impact of positive economic shocks on civil conflict in Nigeria. The variation of global oil price can be considered as exogenous to the Nigerian economy as the country produces less than 3% of world oil market.[6] The empirical analysis is based on the combination of an original dataset that documents the location of oil districts, and violent events from Armed Conflict Location Events Data (ACLED). To capture the distinct features of violent events, we define local conflicts by differentiating conflict actors by (i) ethnic group attacks on civilians, (ii) clash between ethnic groups, (iii) government attacks on ethnic groups and (iv) ethnic group attacks on government, in 355 districts over 1998-2012 period.[7]

The identification strategy exploits the impact of oil price on conflicts in districts within Southern Nigeria. The geographical concentration of oil production in the southern part of the country ensures comparability of districts with similar socioeconomic characteristics and minimizes the influence of confounding factors.[8] We argue that conditional on a few

[4]Blattman and Miguel (2010) provides a review of the conflict literature.

[5]Recent studies show the importance of geographical concentration of natural resources for conflict. See Morelli and Rohner (2015); Lujala (2010); Bellows and Miguel (2009); Angrist and Kugler (2008); Dube and Vargas (2013). Caselli *et al.* (2015) show that the presence and location of oil are important predictors of interstate conflict.

[6]In addition, Griffin (1985) show that although Nigeria is part of OPEC, it does not coordinate its production quantities with OPEC because it is a small producer with competitive fringe tendencies.

[7]The definition of conflict events by violent actors captures three distinct features of civil conflict in Nigeria. First, is the two-sided violence between ethnic groups and the government. The second is the clashes between two ethnic groups and the third is the one-sided violence against civilians by ethnic groups. These features show how oil price affect armed conflict through their effect on people living in resource-rich regions, their effect on government and their effect on insurgent movement.

[8]Such as differences in institutional quality and educational attainment. Fenske and Zurimendi (2015) study the effect of an oil price shock on inequality between ethnic groups in Nigeria. They find that high oil prices

district characteristics, the variation in oil price is exogenous to conflict in the region. This suggests a difference-in-difference approach in evaluating the effect of oil price shock on conflict, with the nearest non-oil district forming a credible counterfactual.[9]

The estimation reveals a large impact of positive oil shocks on the likelihood of civil conflict: our results show that, over a 15-year period, oil-price induced shocks led to a 32% increase in ethnic group attack on civilians, a 19% increase in government attacks on ethnic groups; a 20% increase in ethnic group attacks on government and a 68% overall increase in conflict events. In contrast, we find no effect of oil price boom on clashes between ethnic groups.

The paper contributes to the current literature in several ways. First, it provides micro-level evidence consistent with resource rent mechanisms by showing that oil price shocks affect both intergovernmental transfers and violence. These results are in line with the theoretical models which show that rents from resources increase incentives for violence (Grossman, 1995; Bates *et al.*, 2002; Bates, 2008; Besley and Persson, 2010, 2011; Caselli and Coleman, 2013).[10] The results are also consistent with previous cross-country studies which show that rents on primary commodities increase the likelihood of civil conflict especially in sub-Saharan Africa (Fearon and Laitin, 2003; Lujala *et al.*, 2005; Ross, 2004; Humphreys, 2005). In particular, it is consistent with the cross-country analysis of Berman *et al.* (2017) which shows that a rise in mineral prices increases conflict risk in African countries that export primary commodities.[11] Furthermore, it provides indirect evidence of a relationship between federal transfers (from oil revenue) and political corruption, consistent with the studies of Brollo *et al.* (2013), Vicente (2010) and Caselli and Michaels (2013).

Second, it shows that positive oil shocks do not affect individual labor outcomes in

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increase schooling for southern ethnic groups.

[9] A similar approach is used in Dube and Vargas (2013).

[10] It relates to the theoretical work of Tornell and Lane (1999) which show that increase in voracious rent seeking of natural resource revenues occurs after oil windfall. It also relates to the recent research of Lei and Michaels (2014) which show that the discovery of oil fields increase the incidence of internal armed conflict.

[11] This is in contrast to the study by Cotet and Tsui (2013) who find little evidence that oil rents affect political violence.

resource-rich regions. This is consistent with predictions by Grossman (1991), Hirshleifer (1995) and Chassang and i Miquel (2009) that increase in commodity prices enhance the opportunity cost of rebellion. It also relates to studies that show a relationship between negative income shocks and an increase of civil conflict by (Miguel *et al.*, 2004; Collier and Hoeffler, 1998; Dube and Vargas, 2013). Third, it provides direct evidence against grievance mechanisms.[12] Oil extraction may fuel grievance if mining activities destroy ethnic homelands. Using district data on pollution it demonstrates that oil price shocks did not result in conflict in districts with gas flares and oil spills. Finally, it shows evidence that a rise in the global oil price reduces social capital: individuals living in resource-rich regions exhibit lower levels of trust in other ethnic groups and in the local government. This is consistent with the hypothesis that trust is a determinant of civil conflict as argued by Rohner *et al.* (2013b).[13]

The rest of the paper is structured as follows. Section 1.2 briefly reviews the institutional setting for oil industry and related conflict in Nigeria. Section 1.3 describes the data. Section 1.4 outlines the empirical strategy and the results. Section 1.5 discusses mechanisms. Section 1.6 concludes.

1.2 Institutional Setting

Oil exploration in Nigeria dates back to 1908 with the prospect for oil deposits in the southwestern region of the country. In 1956 discovery was made in Oloibiri in the Niger Delta region and crude exports began in 1958. In 1961 total exports were dominated by cocoa, groundnut, and rubber with crude oil at 7.1% of total exports revenue. Between 1965 and 1970, the percentage share of crude oil to export earnings increase from 13.5% to 63.9% to become the leading source of foreign exchange (Obaje, 2009). By 1979, it contributed to 95% of total external earnings and generated 75% of government revenue. The strategic

[12]See, for example, Collier and Hoeffler (2004).

[13]Rohner *et al.* (2013a) find that ethnic fighting reduced trust and increased ethnic identity in Uganda.

importance of crude oil to the Nigerian economy makes it vulnerable to international oil price volatility.

Currently, Nigeria has an estimated 37 billion barrels of proved crude oil reserves and 180 trillion cubic feet (Tcf) of proved natural gas reserves mostly situated along the country's Niger Delta and offshore in the Bight of Benin, the Gulf of Guniea and the Bight of Bonny.[14] Commercial oil production is concentrated within the Niger Delta region situated at the apex of Gulf of Guniea on African west coast. The region consists of nine (9) oil-producing states with over 250 oil producing communities and an extensive network of wells and production-related facilities.[15] Endowed with huge oil and gas fields–half of which are offshore–the region produces over a million barrels of oil per day.

The oil industry operates under a statutory monopoly over mineral exploitation by the Nigerian government and is regulated through the Nigerian National Petroleum Cooperation (NNPC). The NNPC operates through joint ventures and production-sharing contracts with oil majors who are granted territorial concessions (blocs) to extract oil.[16] Oil revenues are distributed to states through a derivation formula with a higher share to oil-producing states and communities.[17] However, intermittent changes to this revenue allocation strategy makes it a source of tension in the Niger Delta region leading to demands for increase in amount of derivation or outright control of the natural resource.[18]

The Niger Delta disputes over resource control started in the early 1990's with the disruption of oil production through protests by the Movement for the Survival of the Ogoni People (MOSOP). By late 1990's conflicts intensified between ethnic groups and government due to grievances of environmental and development neglect.[19] Specifically, the ethnic

[14]See Oil and Gas Journal (2014).

[15]The states are Abia, Akwa Ibom, Bayelsa, Cross River, Delta, Edo, Ondo, Imo, and Rivers.

[16]The upstream sector is largely dominated by multinational exploration and production companies such as Dutch Shell, Total Fina Elf, ExxonMobil, ENI/Agip, ChevronTexaco and Addax Petroleum.

[17]Oil producing states currently receive 13% of revenue from oil receipts.

[18]The allocation formula of oil revenues has changed eighteen (18) times since 1946. See Ross (2003).

[19]For example, the 1997 protest of 10,000 youths at the Alebiri to end activities of Shell in the district and

groups demanded greater local control and more transparent management of oil revenues, as well as adequate compensation of local communities for negative externalities derived from oil exploitation. By 2005 violent community conflicts in Rivers, Bayelsa, and Delta states numbered between 120-150 per year, and over fifty armed groups with an estimated 20,000-25,000 armed youths were operating in the oil producing region (UNDP, 2007).

The formation of Movement for the Emancipation of the Niger Delta (MEND) escalated the conflict in 2006. The most coherent and trained armed group in the region and estimated to have between 5000-10,000 combatants, MEND claimed responsibility for kidnapping oil workers and attacking onshore and offshore oil facilities whilst generating income for arms through oil bunkering trade (Asuni, 2009).[20] In 2009, more than 20,000 ex-combatants accepted amnesty from the government and have been participating in a program of disarmament, demobilization, reorientation, and reintegration (DDRR) (Francis *et al.*, 2011).

1.3 Data

The research design exploits the fact that conflict intensity within oil districts depend on oil price changes in the world market and group competition for resource rents. This naturally requires data on oil production within the local economy, oil prices, conflict events amongst different groups (both government and ethnic groups); district level income and state revenues. Table 3.1 presents the descriptive statistics used in the analysis.

We use conflict data recorded by Armed Conflict Location and Event Data Project (ACLED). The data covers all countries in sub-Saharan Africa from 1997-2015. The data contains real-time reports on daily violent and non-violent events such as battles, riots, protests, violence against civilians by political actors including rebels, governments, communal

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the 1998-99 mobilization of the Ijaw from the Ijaw Youth and National Council led to conflict with government forces and deepened the political disorder across the region (Watts, 2004).

[20] It is estimated that between 70,000 and 300,000 barrels per day (more than 12% of daily average oil production) are lost to illegal oil trade. For instance, Nigeria lost 136 million barrels of oil with an estimated value of $11 billion to oil theft and sabotage between 2009 and 2011 (Kent, 2013). Further estimates show a loss of 84.8m million barrels at the cost of $6.7 billion to oil theft in 2013 (Wallis, 2015).

groups.

The empirical analysis focuses on conflict events in Nigeria between 1998-2012, a 15-year window that captures conflict trends. Events are observed at district level over time using data specific information on date, location, event type, geographic coordinates and contextual notes. To capture local level violence cycle and distinguish who attacks, conflict actors are aggregated into groups defined as ethnic group attacks on civilians, clashes between ethnic groups, government attacks on ethnic group and ethnic group attacks on the government.

To identify producing districts, we collate firm-level information on district oil production from Nigerian National Petroleum Corporation (NNPC) annual reports. To complement this, additional information from secondary sources are used, including annual reports of oil firms and concession maps to locate exploration districts. The full dataset shows an average annual number of 155 oil wells of more than 1700 million barrels in production in 46 oil-producing districts across 9 states over the sample period.[21]

Figure 1.1 shows the districts that produce oil during our sample period. The map shows the oil-producing districts to be concentrated in the southeastern part of the country. Not all districts in the south, however, produce oil. To ensure comparability of similar districts, the sample is restricted to the southern region to exclude regional differences biasing the results.[22] The paper uses non-oil producing districts within the southern region as a control group.

Oil price measure is the average annual spot oil price from the West Texas Intermediate (WTI) series.[23] The variation in global oil price can be safely assumed to be exogenous since

[21]The oil-producing states are Abia, Akwa Ibom, Bayelsa, Cross River, Delta, Edo, Imo, Ondo, and Rivers. There have also been recent oil discoveries in Anambra and Lagos.

[22]There are no oil districts in the North and there is a huge economic disparity between the North and South. The poverty rate in the north is twice that of the south region. The northern region accounts for the majority (66%) of the poor in the country. See World Bank (2014).

[23]The choice of price index choice relies on the fact that the United States has traditionally been the largest importer of Nigerian oil until 2012. India is currently the largest importer of Nigerian crude oil at 370,000 bbl/d while the United States is the 10th largest importer at 60,000 bbl/d. Europe remains the largest-regional importer of Nigeria oil at 900,000 bbl/d. See UNITED STATES

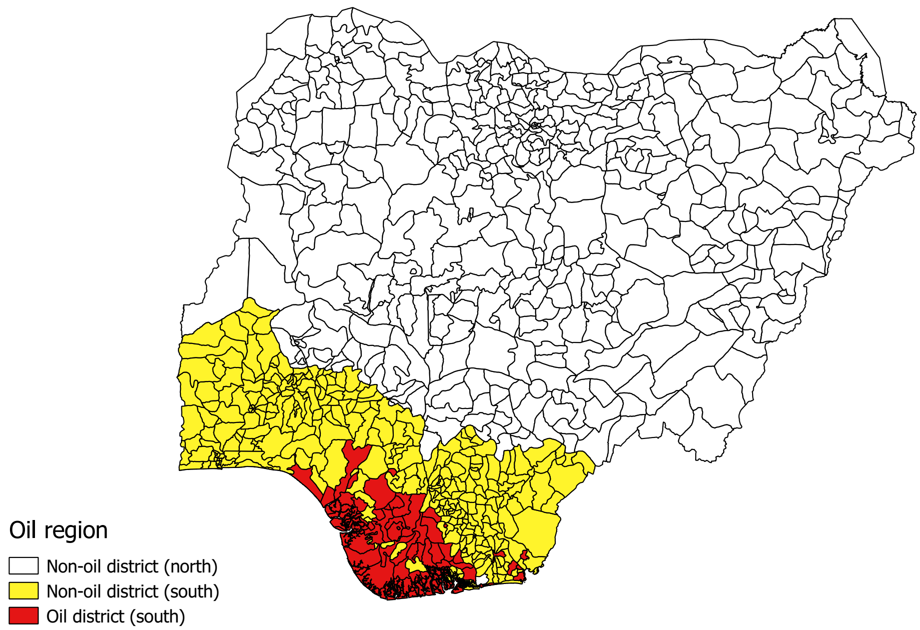

Figure 1.1: *Oil production districts*

Nigeria produces less than 3% of world oil supply.[24] The study focuses primarily on the period 1998-2012 because it contains oil shocks of sign and magnitude comparable to those of the 1970s. Oil price rose in 1998 from \$14.39 per barrel to a high of \$99.57 per barrel in 2008 before a collapse to \$61.69 per barrel in 2009.[25] The substantial variation in oil price levels over this period means that it is possible to make inferences about changes in violent events in Nigeria by comparing oil price levels within a relatively narrow time window.

To motivate our story, Figure 1.2 shows a graphical display of international oil price

ENERGY INFORMATION ADMINISTRATION (U.S.EIA) (2015). The oil price data is available at http://research.stlouisfed.org/fred2/series/OILPRICE/downloaddata?cid=98.

[24]See http://www.eia.gov/cfapps/ipdbproject/iedindex3.cfm?tid=5&pid=53&aid=1.

[25]At the nominal price, it was at an all-time high of \$145 per barrel on July 3, 2008.

movement and our conflict outcomes. The graph reveals that the timing of the changes in global oil price is consistent with the timing of attacks in oil districts. This is particularly evident for events such as ethnic group attack on civilians, government attack on ethnic groups and ethnic group attack on government, which show a notable rise following an increase in oil price. The close relationship between the timing of changes in oil price and local conflict events in districts that produce oil rules out the possibility that pre-trend conflict events drive our results.

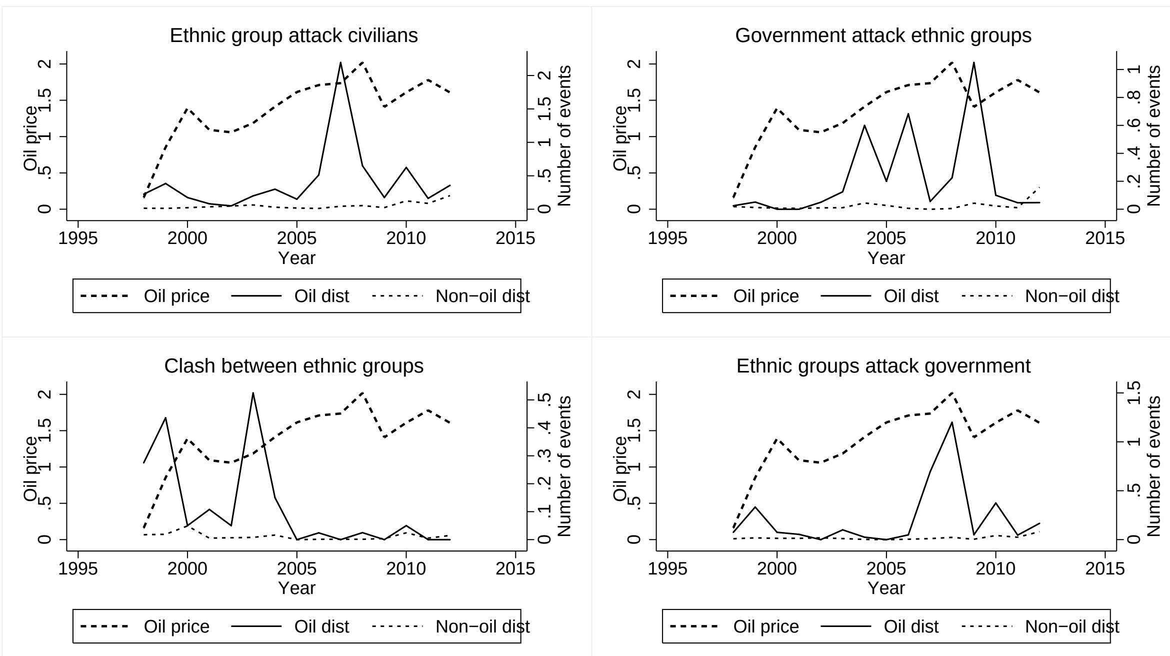

Figure 1.2: *Oil Price and Conflict in Oil and Non-oil districts*

Mechanisms are investigated using labor outcomes data from household surveys and data on intergovernmental transfers to local districts. Specifically, data on wage and employment are from the Nigeria General Household Survey of 2006-2012. The surveys provide information on age, sex, marital status, district, wages, hours worked, employment and migrant status. The sample includes all persons born in 774 districts that are at least 15 years and not more than 65 years between 1998 and 2013.[26] A panel of individuals are

[26]The minimum age for work in Nigeria is 12 years.

defined according to their age, gender, the state of birth and educational level attained. Using this panel, monthly real wage and hours employed are calculated to estimate the opportunity cost of civil conflict.[27] Additional details on data characteristics are provided in the online appendix.

Intergovernmental transfers from the Federation Account are overwhelming the primary source of revenue for subnational governments in Nigeria.[28] Federally-collected revenues are shared between the federal level, the 36 states (plus the Federal Capital Territory) and the 774 local governments (districts) of the country. According to the current revenue allocation formula, 55% of the transfers are allocated to the Federal government, 25% to state governments and 21% to local governments.[29] The fact that revenue from oil represents a substantial share of total public sector income means that the sharing of oil revenue dominates intergovernmental relations in Nigeria and is also a potential source of appropriation. The allocation of revenues to the distinct levels of government are published monthly by the Office of the Accountant-General of the Federation.[30] Using this inventory, we aggregate monthly allocations to generate annual allocation figures at the district level.

The impact of oil price on districts that experience pollution is explored using data on oil spills and gas flaring.[31] The data on oil spills is from the National Oil Spill Detection and Response Agency (NOSDRA), a government institution which oversees incidents of oil spills.[32] The data provides daily information on oil spills including incident date, company,

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[27] Although we do not observe the same individuals over time, the availability of the surveys allows us to observe the average wage and employment within the specified age groups over time. A similar method is used in Dube and Vargas (2013).

[28] Internal revenues of most states are below 10% of their total revenues.

[29] For states that produce oil, the amount each state receives varies according to the number of local governments within the state and the amount of oil produced (oil producing states receive an additional 13% from the Federation Account).

[30] This is available at the Office of the Accountant-General of the Federation website from 2007. See http:www.oagf.gov.ng/

[31] Between 1976 and 2001, a total of 6,817 oil spills have been recorded in the Niger Delta with only 70% of the oil spills being recovered(UNDP, 2006) Satellite estimates show Nigeria as the second largest gas flaring country after Russia at an average of 23 billion cubic metres of natural gas(World Bank, 2007).

[32] The data is available at https://oilspillmonitor.ng/

estimated quantity spilled, cause, site location, district, spill area habitat and states affected. Our sample includes all districts with onshore oil spills from 2006.[33] The gas flaring data is calculated using oil field by firm information on gas flares from NNPC annual reports. The data include information on the amount of gas produced and flared by firms at district level from 1998-2012.

Finally, we use data from Afrobarometer of 1999-2013 to explore alternative accounts relating to local perception of institutional quality such as perceptions relating to the provision of public goods, corruption, communal violence, and poverty.The next section describes the estimation strategy and presents the results.

1.4 The Effect of Oil Price Shocks on Civil Conflict

1.4.1 Empirical Strategy and Results

The empirical strategy follows a difference-in-difference estimation with the exogenous variation from annual oil price movement in the international market.[34] The oil location variable is a dummy variable for districts that produced oil in 1998. Using oil location in the first year of the sample ensures that conflict events over the analysis period are not correlated with local oil supplies or oil field discoveries.

$$Y_{jrt} = \alpha_0 + \alpha_1(Oil_j \times OilPrice_t) + \alpha_2 X_{jrt} + \delta_j + \gamma_t + \lambda_r t + \epsilon_{jrt}, \tag{1.1}$$

Y_{jrt} is conflict outcomes comprising ethnic group attacks on civilians, government attacks on ethnic group, ethnic group attacks on government and clashes between ethnic groups in district $_j$, region $_r$ and year $_t$. In the main specification, the different conflict outcomes, Y_{jrt}, are regressed on $(Oil_j \times Oilprice_t)$, an indicator variable that captures the interaction between districts that produced oil in 1998 (a dummy variable equal to one, and zero

[33]The year from which the data became available.

[34]The international oil price is plausibly exogenous to Nigeria because it is a produces a small amount of world oil production. See https://www.eia.gov/beta/international/data.

Table 1.1: *Descriptive Statistics*

	Observation	Mean	Std. Dev.	Min.	Max.
Panel A: Conflict outcomes					
Ethnic groups attack civilians	5325	0.095	0.738	0	30
Clash between ethnic groups	5325	0.027	0.413	0	14
Government attack ethnic groups	5325	0.045	0.394	0	14
Ethnic groups attack government	5325	0.042	0.401	0	14
Government repression	5325	0.023	0.244	0	7
Clash between political parties	5325	0.017	0.167	0	4
Panel B: Commodity prices					
Log oil price, millions of 2013 naira per barrel	5325	0.211	0.586	-1.818	0.703
Log cocoa price, millions of 2013 naira per metric ton	5325	-3.025	0.311	-3.969	-2.628
Log oil palm price, millions of 2013 naira per metric ton	5325	0.325	0.278	-0.390	0.701
Log rubber price, millions of 2013 naira per pound	5325	-1.703	0.549	-3.300	-0.893
Panel C: Demographics					
Population in 1991 (log)	5325	9.387	4.698	0	14.264
Average years of schooling in 1990	5325	0.883	1.908	0	12.300
Percent of households with primary school education in 1990	5325	9.489	19.158	0	96.600
Geographic area (square kilometers)	5325	0.537	0.597	0.010	5.018
Panel D: Intergovernmental Transfer					
Log district revenue, millions of 2013 naira	3548	5.012	7.288	5.048	22.260
Log tax revenue, millions of 2013 naira	3548	12.950	7.563	2.999	21.522
Panel E: Household Labor outcomes					
Log real wage (2006-2012)	28256	4.646	0.757	1.833	8.075
Log labor hours (2006-2012)	28256	5.032	0.150	3.738	5.075
Panel F: Housing & Infrastructure					
Log annual house rent	32924	9.52	1.22	0	18.79
Rooms at home	58744	2.90	3.83	0	99.00
Households with piped water	58904	0.42	0.49	0	1
Households with electricity	58904	0.57	0.49	0	1
Households with toilets linked to main network	58904	0.24	0.43	0	1
Households with concrete floor	58904	0.76	0.43	0	1
Households with garbage collection	58904	0.13	0.33	0	1
Kilometers of paved roads	5325	16.412	17.989	0	118.00
Panel G: Poverty perception					
Households without food	2604	0.90	1	0	3
Households without water	2604	1.01	1.08	0	3
Households without medical care	2604	0.88	1.02	0	3
Panel H: Trust Outcomes					
Intra-group trust	1285	0.97	0.89	0	3
Inter-group trust	1267	0.78	0.80	0	3
Trust of president	2553	0.79	0.91	0	3
Trust of local council	2511	0.69	0.83	0	3
Trust of parliament	2506	0.74	0.83	0	3
Panel I: Political & Community Conflict					
Conflict between political parties	2487	2.15	0.83	0	3
Community violence	1285	1.50	0.96	0	3
Violence between ethnic groups	1236	1.98	1.03	0	3
Panel J: Public Services Provision					
Local roads maintenance	2567	0.87	0.84	0	3
Local community cleaning and sanitation	2543	1.08	0.91	0	3
Use of local government revenue	2320	0.85	0.68	0	3
Local health standards	1255	1.11	0.87	0	3

otherwise) and changes in international oil price (stated in natural log terms to capture the percentage change effect of oil price), a vector of covariates X_{jrt} and district fixed effects δ_j, year fixed effects γ_t and regional linear trends $\lambda_r t$ and error term ϵ_{jrt}.

The coefficient of interest, α_1, is the percentage effect of an oil price shock on civil conflict. The vector of covariates, X_{jrt} , include pre-sample indicator variables such as a natural log of population, percent of households with primary school education and geographic area.[35] In addition, the regional linear trend variable, $\lambda_r t$, controls for regional economic differences and oil concentration.

Equation 1.1 is estimated using ordinary least squares (OLS) for the different conflict events. Limiting the sample to these events is important because it captures the violence cycle and helps to disentangle the effect of oil from the effect of other time varying factors that influence conflict in Nigeria. The baseline analysis focuses on districts within the southern region to control for the economic disparity between the north and the south and also account for district proximity to oil location. For example, all oil districts are within the southern region and the terrorist attacks of Boko Haram are mainly concentrated in the northern region. This and other potential confounding factors make observations outside the southern region less informative about the effect of oil price on civil conflict. A potential concern in using OLS is that it produces a biased estimate of α_1 because variables cause ϵ to be correlated with time and, thus with $(Oil_j \times Oilprice_t)$. These confounding factors are addressed using a difference-in-differences (DD) analysis. The DD analysis addresses this endogeneity by considering an arbitrarily narrow geographic unit around districts that produce oil as a credible counterfactual. Within this unit, the unobserved factors influencing conflict are likely to be similar so that observations in contiguous districts that do not produce oil provide a comparison group for observations in districts that produce oil.[36] Thus α_1 captures the difference over time in the average difference of conflict outcomes in

[35]The pre-sample variables are between 1990 and 1991. All variables are at the district level.

[36]This removes the bias resulting from differences between oil and non-oil districts. We also control for time invariant factors across districts using district fixed effects.

the two locations. In all specifications, the standard errors are clustered at the district level.

1.4.2 Characteristics of Oil and Non-Oil Districts

Table 3.2 examines the characteristics of districts that produced oil in 1998 and districts that did not produce oil in that year. There are no significant variations in household access to public services or proportion of literate households. Nevertheless, differences between oil and non-oil districts as measured by population, percent of households with primary education and the geographic area could confound the effects of oil price on civil conflict. These variables are included in the regression analysis as controls.

1.4.3 Results

Table 3.3 reports the estimates of the effect of oil price shocks on civil conflict. It starts with a baseline regression and then adds controls. For each conflict outcome, the table reports the coefficient and standard error for $Oil_j \times Oilprice_t$. Except for clash between ethnic groups, all coefficients are positive and statistically significant.

The magnitude of these effects is meaningful. In our study sample, the average number of oil districts is 0.113, which, combined with our main results, suggest that the rise in oil price between 1998 and 2012 led to 0.04 more ethnic group attack on civilians, 0.02 more government attacks on ethnic groups, 0.03 more ethnic group attacks on government and 0.08 more conflicts in the average oil district, as compared to the non-oil districts. Compared to the respective means of each conflict outcome, this signifies an increase of 38% (column 2), 50% (column 6) and 59% (column 8). The overall effect indicates a 50% increase in conflict events (column 10).

There are three potential biases with this estimate. The first is that the focus on the southern part of the country may mask spillovers of conflict from oil district and thereby lead to the contamination of non-oil Southern districts. Second, the estimate may be capturing events unrelated to oil shocks. The third relates to conflict endogeneity.[37] Table

[37] The fact that conflict began in the early 1990s (before our sample period) may explain the current episodes

Table 1.2: *Characteristics of oil and non-oil districts*

	(1) Mean (oil district)	(2) Std. Error (oil district)	(3) Mean (non-oil district)	(4) Std. Error (non-oil district)	(5) Difference in Mean	(6) Std. Error of difference	(7) Observations
Population 1991 (in natural log)	14.772***	(2.553)	11.458***	(0.448)	3.314*	(1.896)	774
Geographic area (square kilometers)	0.081***	(0.029)	0.120***	(0.005)	-0.039*	(0.023)	774
Average years of schooling (1990)	0.785***	(0.236)	0.468***	(0.053)	0.317	(0.231)	774
Percent of households with secondary education (1990)	2.737**	(1.086)	1.760***	(0.246)	0.978	(1.080)	774
Percent of households with primary education (1990)	9.460***	(2.386)	5.134***	(0.531)	4.326*	(2.313)	774
Fraction literate within households (1990)	9.277***	(2.994)	6.197***	(0.667)	3.080	(2.916)	774
Percent of households with water linked to main-network (1990)	4.930**	(2.115)	3.051***	(0.483)	1.879	(2.119)	774
Percent of households with toilet linked to main-network (1990)	2.965*	(1.789)	2.168***	(0.411)	0.797	(1.806)	774
Percent of households with electricity (1990)	8.030**	(3.763)	7.064***	(0.844)	0.966	(3.701)	774

Notes: Oil districts are districts that produced oil in 1998 while non-oil district are districts that did not produce oil in 1998. Each row is a separate regression of pre-sample characteristics on indicators of oil and non-oil districts. Columns (1)-(4) show coefficients and standard errors for each district. Column (5)-(6) show the difference in characteristics between the districts. Column (7) is the number of observations.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

A1 addresses the first and second bias by excluding districts close to oil districts (Panel A), including northern districts (Panel B), excluding Boko Haram attacks (Panel C) and using only northern districts as a control group (Panel D). Table A2 controls for pre-sample conflict outcomes to account for possible correlation with past violent events in the region. The magnitude of the coefficients remains large and significant across the various samples and controls. In addition, Table A3 reports estimates from a specification with Poisson fixed effects. The estimates are significant across the four coefficients and are informative about the price effect of oil on civil conflict. The next section explores the possible channels through which oil affects conflicts.

of violence which will bias the estimates

Table 1.3: *Oil Price Shock and Civil Conflict*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	Ethnic group attack civilians		Clash between ethnic groups		Government attack ethnic group		Ethnic group attack government		Total conflict	
Oil district × log oil price	0.324**	0.319**	-0.060	-0.064	0.203***	0.199***	0.223***	0.220***	0.689**	0.673**
	(0.162)	(0.159)	(0.048)	(0.050)	(0.052)	(0.050)	(0.078)	(0.077)	(0.283)	(0.278)
Controls										
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-level covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,325	5,325	5,325	5,325	5,325	5,325	5,325	5,325	5,325	5,325

Notes: Each column represents a separate regression. Oil price shock is the interaction between global oil prices (in log terms) and districts that produced oil in 1998. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is 1998-2012. The district-level covariates include log of population in 1991, average years of schooling among people aged 15 and above in 1990, percent of households with primary school education in 1990 and geographic area in square kilometers. Each conflict event capture annual number of attacks or clashes. Total conflict is the sum of conflict events at district level.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

1.5 Mechanisms

This section examines the possible explanations for the likelihood of conflict increase due to positive oil price shocks. Understanding district and household responses to oil price shock are important in explaining the conflict level results as well as for assessing the extent to which experience in southern Nigeria can be generalized. Overall the evidence indicates that, relative to the non-oil district, positive oil price shocks increase intergovernmental (federal) transfers to oil districts. In contrast, there is no price effect on individual wages or labor hours.

To highlight how natural resource rents increases the risk of conflict this section considers the effect of a rise in oil price on federal transfers to districts. Evidence of an increase in the probability of conflict might be explained by an increase in revenue allocated to oil districts. As described earlier, intergovernmental transfers are the primary source of local government budget and also a source of tension and conflict for districts in the oil region.[38] Thus, an increase in oil price would raise transfers to the district government and provide an incentive for appropriation resulting to conflict.[39] Results indicate a positive and significant increase in federal transfers which we interpret as evidence for the rent-seeking channel. Furthermore, the results also provide evidence to the theory of voracity effect (Tornell and Lane, 1996) which shows that an increase in resource windfall generates demand for more transfers by powerful groups (for example, state and local governments). In other words, an increase in oil price generates an appropriation effect that leads oil districts to demand a greater share of oil revenue through more transfers.

[38] The debate around allocation of federal transfers generated campaigns and eventually led to the creation of new states and local government areas, as local politicians sought to benefit from patronage in the distribution of transfers at state and local levels (Human Rights Watch, 1999).

[39] Little of the revenue from federal transfers to the state and local governments are actually spent on genuine development projects and there appears to be no control or proper audit over spending by state and local governments (Human Rights Watch, 2002).

1.5.1 Intergovernmental transfers

Federal transfers to local districts come from the Federation Account. This account is sustained by oil revenues, company income tax proceeds; customs duties and excise taxes. Oil revenues represent more than 70% of total revenue from the federation account.[40] Prior to intergovernmental transfers, the federation account is subject to initial deductions known as first charges. These first charges are deducted from oil revenues in the federation account and include a 13% allocation of oil revenue to the oil producing states.[41] The remaining amount is distributed amongst the federal, state and local governments according to a derivation formula.[42] To assess the effect of oil price on federal transfers, equation 1.1 is estimated using federal transfers to districts as a dependent variable.

Table 3.4 gauges the effect of price shock on federal transfers to oil districts. Column (1)-(2) show substantial increases in the transfer of oil and tax revenue to districts. The coefficients suggest that a 1% increase in oil price is associated with a 0.02% increase in district revenue and 0.04% increase in the tax revenue transferred to the average oil district relative to non-oil districts. This implies that the rise in oil price by 0.23 log points between 2007 and 2012 increased the allocation of oil and tax proceeds to districts that produce oil by 0.4% and 0.91% respectively.[43]

[40] Revenue from oil is generated from the sale of crude oil and gas; signature bonuses, royalties and petroleum profit tax (PPT) with a rate of 85% (65.75% in the first 5 years of production).

[41] Other charges comprise of external debt service, the share of government production cost of oil, the cost of government-sponsored projects and National Judiciary Council expenditure.

[42] The federal government receives 50.5% of the total revenue, the state governments 25%, the local governments 21%, 1% goes to the Federal Capital Territory Abuja; 2% to the Ecological Fund and 0.5% to the Stabilization Reserve Fund.

[43] It is important to note that the data understate the actual amount received by local governments. For instance, allocation data does not reflect the deduction of the salaries of primary school teachers by the federal government.

Table 1.4: *Oil Price Shock Mechanisms*

	(1)	(2)	(3)	(4)
	Resource rents		Opportunity cost	
Dependent variable	Log district revenue	Log tax revenue	Log labor hours	Log wages
Oil district × log oil price	0.164***	0.343***	-0.065	0.382
	(0.036)	(0.074)	(0.051)	(0.260)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes
Observations	3,548	3,548	28,256	28,256
Sample period	2007-2012	2007-2012	2006-2012	2006-2012

Notes: Each column represents a separate regression. Oil price shock is the interaction between global oil prices (in log terms) and districts that produced oil in 1998. For all regressions robust standard errors clustered at district level are in parentheses.The district-level covariates include log of population in 1991, average years of schooling among people aged 15 and above in 1990, percent of households with primary school education in 1990 and geographic area in square kilometers. Demographic controls include educational level attained, age, age squared, rural status, gender and marital status. Log district revenue is the natural log of annual intergovernmental transfer of oil revenue from the federal government to the local government. Log tax revenue is the local government share of value added tax (VAT) proceeds. Log labor hours is the log of hours worked in the last month and log wages is the log of hourly wages defined as individual earnings per hours worked in the last month.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

1.5.2 Household labor market outcomes

The section examines household labor market outcomes to investigate the opportunity cost effect of oil price shocks.

In the specification below, the log of hourly wage and hours worked, W, of the individual i are regressed on $(Oil_j \times Oilprice_t)$,, a vector of covariates P_{ijrt} and district fixed effects δ_j, year fixed effects τ_t and regional linear trends $\lambda_r t$ and the error term v_{ijrt}.

$$W_{ijrt} = \alpha_0 + \alpha_1(Oil_j \times OilPrice_t) + \alpha_2 P_{ijrt} + \delta_j + \tau_t + \lambda_r t + v_{ijrt}, \tag{1.2}$$

The coefficient of interest, α_1, is the percentage effect of oil price shock on civil conflict. The vector of covariates, P_{jrt} , includes educational attainment, age, age squared, rural status, gender and marital status.

Columns (3)-(4) of table 3.4 display estimates on the effect of oil price shocks on individual labor hours and wages. The specification restricts the sample to include observations from 2006-2012 in southern Nigeria. There is no evidence that positive oil price shocks affect wages or labor hours in oil districts. These results are consistent with Dube and Vargas (2013) which show that oil price shock increase municipal revenue without affecting local labor outcomes.

1.5.3 The Effect of Oil Price Shocks on Districts with Pollution

Pollution from resource extraction potentially affects civil conflict through local community grievance. Thus negative externalities from resource extraction lead to community compensation demands and violence if it causes job loss and negative health effects in the local community. This section examines the likelihood of conflict as a consequence of negative externalities from resource extraction. Specifically, the focus is on possible pollution factors such as gas flares and oil spills. Satellite estimate shows Nigeria as the second largest gas flaring country after Russia at an average flare of 23 billion cubic meters (BCM) of natural gas between 1995-2006 (World Bank, 2007).[44] In addition, in the last five decades, it is estimated that over 546 million gallons of oil have been spilled in Nigeria.[45]

Environment destruction from gas flares and oil spills make communities vulnerable to pollution.[46] To capture the effect of pollution on civil conflict, the specification exploits a triple difference estimation by combining a difference-difference of oil-producing districts and oil price variation with an additional difference of gas flare and oil spill districts to control for all differences across time between districts that are exposed to pollution, relying on the effect of differential changes in international oil prices on oil-producing districts for identification. Thus, the specification identifies the effect of pollution, net of time-varying characteristics that caused one district to be exposed to pollution from oil extraction, on civil conflict. Formally, the specification differentiates between districts with gas flares and oil spills:

$$\begin{aligned} Y_{jrt} = \alpha_0 + \alpha_1(Flare_j \times Oil_j \times OilPrice_t) + \alpha_2 Flare_j + \alpha_3 Oil_j + \alpha_4 OilPrice_t \\ + \alpha_5(Flare_j \times Oil_j) + \alpha_6(Flare_j \times OilPrice_t) + \alpha_7(Oil_j \times OilPrice_t) \\ + \beta X_{jrt} + \delta_j + \gamma_t + \lambda_r t + \epsilon_{jrt}, \end{aligned} \tag{1.3}$$

[44]However, estimates for 2007 to 2011 gas flares show a remarkable decline ranging from 16.3 BCM in 2007 to 14.6 BCM in 201 (World Bank, 2012).

[45]Between 1976 and 2001, a total of 6,817 oil spills have been recorded in the Niger Delta with a loss of approximately three million barrels of oil. More than 70 per cent was not recovered. Approximately six per cent spilled on land, 25 per cent in swamps and 69 per cent in offshore environments (UNDP, 2006). The National Oil Spill Detection and Response Agency (NOSDRA) noted another 2,405 spills between 2006 and mid-2010, with an increasing trend year-on-year: 252 in 2006, 598 in 2007, 927 in 2008 and 628 in 2009, but rising again in 2010 (Francis *et al.*, 2011).

[46]Almost 60% of people in the oil producing district depend on the natural environment for their livelihood(UNDP, 2006) Moreover, ethnic groups in the local communities demand adequate compensation for the negative externalities derived from oil exploitation.

$$Y_{jrt} = \alpha_0 + \alpha_1(Spill_j \times Oil_j \times OilPrice_t) + \alpha_2 Spill_j + \alpha_3 Oil_j + \alpha_4 OilPrice_t$$
$$+\alpha_5(Spill_j \times Oil_j) + \alpha_6(Spill_j \times OilPrice_t) + \alpha_7(Oil_j \times OilPrice_t)$$
$$+\beta X_{jrt} + \delta_j + \gamma_t + \lambda_r t + \epsilon_{jrt}, \tag{1.4}$$

where Y_{jrt} is the outcome of interest (e.g. civil conflict) in district j, region r at time t. $Flare_j$ is a dummy variable equal to one if a district experienced gas flaring (equation 1.5.3) and $Spill_j$ is a dummy variable equal to one if a district experienced oil spill (equation 1.5.3). X_{jrt} includes district level covariates such as rainfall and log of population. δ_j are district fixed effects, γ_t year fixed effects, $\lambda_r t$ regional time trend and ϵ_{jrt} the idiosyncratic error term.

Table 3.5 show estimates on the effect of oil price shocks on civil conflict in districts with gas flares (Panel A). The point estimate on the triple interaction for flares (i.e Flare district $\times$ Oil district $\times$ Oil Price) is negative and statistically significant in all cases. These results provide no evidence of gas flares as a reason for civil conflict. The results of oil spills (Panel B) show no significant effect of pollution spills on ethnic violence.

1.5.4 Alternative Accounts

This section considers alternative accounts of why positive oil price shocks lead to more ethnic violence in oil districts. Since a substantial part of government revenues is from oil sales, this suggests that the government may use excessive force to protect this asset. By this account, the rise in the government attack on ethnic groups may mask repression of civilians in the region. In addition, violence may be related to clashes between political parties or lead to out-migration of individuals from the conflict zone. Table 3.6 shows the results. The first column shows that increase in oil price is associated with government repression. The estimate implies that, compared with non-oil districts, government repression increase by 30% in the average oil district as a result of positive oil price shock. Our results correspond to that of Besley and Persson (2011) which shows that repression is contingent on shocks to

Table 1.5: *Oil Price Shock and Pollution*

Dependent variable	(1) Ethnic group attack civilians	(2) Clash between ethnic groups	(3) Government attack ethnic group	(4) Ethnic group attack government
Panel A. Gas flares				
Flare district× oil district × log oil price	-2.989***	-1.578	-1.346**	-1.818***
	(1.130)	(1.627)	(0.681)	(0.652)
Flare district × log oil price	-0.688	-0.059	-0.056	-0.444
	(0.565)	(0.046)	(0.119)	(0.305)
Flare district ×oil district	1.564	0.899	1.042*	0.779*
	(1.042)	(0.861)	(0.541)	(0.405)
Oil district ×log oil price	1.033*	0.036	0.285**	0.681**
	(0.562)	(0.038)	(0.118)	(0.309)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	5,325	5,325	5,325	5,325
Panel B. Oil Spills				
Spill district × oil district×log oil price	-1.488	-0.474	0.082	3.914*
	(1.763)	(0.427)	(0.935)	(2.291)
Spill district × log oil price	-0.332***	0.452	-0.078	-0.101*
	(0.126)	(0.411)	(0.050)	(0.056)
Spill district× oil district	1.260	0.004	-0.073	-1.529
	(1.177)	(0.040)	(0.513)	(1.013)
Oil district × log oil price	0.312**	-0.063	0.200***	0.202***
	(0.155)	(0.051)	(0.051)	(0.076)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	5,325	5,325	5,325	5,325

Notes: Each column represents a separate regression. In Panel A, the three-way interaction of flare district, oil district and log oil price measures whether oil price shock increases conflict in district that experience pollution from gas flares. The two-way interaction of gas flares and oil district is included as a control. In Panel B, the three-way interaction of spill district, oil district and log oil price estimates whether oil price shock generates conflict in district the experience pollution from oil spills. The two-way interaction of oil spills and oil district is included as a control. For all regressions, robust standard errors clustered at district level are in parentheses. See Table 3.3 for description of district-level covariates and dependent variables.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

sources of resource rents. In columns (2) and (3) we examine whether oil shocks are related to the conflict between political parties and household migration. We find no relationship between the increase in oil and these outcomes.

Table 1.6: *Alternative accounts*

Dependent variable	(1) Government repression	(2) Clash between political parties	(3) Migration
Oil district $\times$ log oil price	0.061***	0.021	-0.007
	(0.019)	(0.021)	(0.008)
Controls			
District fixed effects	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes
State$\times$year fixed effects	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes
Demographic controls	No	No	Yes
Observations	5,325	5,325	48,256

Notes: Each column represents a separate regression. For all regressions, robust standard errors clustered at district level are in parentheses. Government repression is the annual number of government attack on civilians. Clash between political parties is the annual number of clashes between political parties. Migration is an indicator equal to one if the individual resided in the district for less than one year. Demographic controls include age, age squared, education, indicators for female household head, gender, and rural status. See Table 3.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Next, the paper explores the impact of oil prices on social capital and institutional quality. It considers the hypothesis that trust is a determinant of civil conflict. Conflict reduces trust while low trust increases the probability of future conflict. In addition, it examines the effect of a positive oil price shock on individual perception of local conflict and provision of public services. Table 3.7 present estimates of these outcomes. Panel A show estimates for four trust measures. The estimates show that the rise in oil price is negatively correlated with inter-group trust, the trust of local government and trust of ruling party. This is consistent with previous empirical evidence: the effects of an international shock on civil conflict has a negative effect on trust. Districts that produce oil experience a decline in general trust relative to districts with no oil.[47] Additional trust outcomes such as trust of president and trust of parliament also show negative coefficient estimates. The details are reported in the appendix table A4.

An alternative way to assess the impact of oil price shocks on conflict is to examine its effect on violence perception. The paper investigates this in Panel B. There is a positive and significant effect on violence between political parties, ethnic groups and within communities in the oil region. Similarly, our estimate on the perception of public services (Panel C) shows identical results: a rise in oil prices increases the misuse of district revenue and reduces the community sanitation. Following Caselli and Michaels (2013) the online appendix table 5 considers a variety of housing and infrastructure outcomes: annual house rent, a proxy of housing quantity (rooms at home), measures of housing quality such as percentage of households with piped water, electricity, toilets linked to main network, concrete floor, garbage collection and quality of local infrastructure (kilometers of paved roads). The results are statistically insignificant in four cases. For the remaining outcomes, house rent and fraction of households with sewage network access are positively affected by an increase in oil price. In contrast, kilometers of paved roads and fraction of household with electricity have a statistically significant negative coefficient at 1% and 5%, respectively.

It is possible that the conflict may be due to trade shocks unrelated to oil price such as variation in the global price of agricultural commodities. We account for this possibility in Table 3.8 by controlling for the effect of commodity price shocks from the top three agricultural products exported by Nigeria. The result shows that the oil price shock coefficients increase in size and are statistically significant.

We also perform a number of placebo tests to see whether oil price treat other non-oil districts. Specifically, we do this in three ways. First, we artificially vary the timing of oil price shock on non-oil southern districts. Second, we artificially vary the northern districts. Third, we artificially vary the non-oil southern districts and northern districts. If our identification strategy is correct, oil price shock should have a negative or no effect on

[47] See, for example, Rohner *et al.* (2013a) and Cassar *et al.* (2013).

Table 1.7: *Oil Price Shock and Institutional Quality*

	(1)	(2)	(3)	(4)
Panel A. Trust				
Dependent variable	Intra-group trust	Inter-group trust	Trust local government	Trust ruling party
Oil district $\times$ log oil price	-0.136	-0.322**	-0.260*	-0.376***
	(0.200)	(0.152)	(0.135)	(0.143)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State $\times$ year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	1,285	1,267	2,511	2,512
Panel B. Local conflict				
Dependent variable	Ethnic group unfairly treated	Violence between political parties	Violence between ethnic groups	Violence within community
Oil district $\times$ log oil price	0.216	0.378***	0.701***	0.501*
	(0.253)	(0.143)	(0.223)	(0.271)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State $\times$ year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	2,398	2,604	1,236	1,285
Panel C. Public services provision				
Dependent variable	Use of district revenue	Local roads	Local community cleaning	Local health standards
Oil district$\times$ log oil price	-0.234*	-0.199	-0.476***	-0.335
	(0.140)	(0.164)	(0.176)	(0.230)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State $\times$ year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	2,320	2,567	2,543	1,255

Notes: Each column represents a separate regression. Panel A captures individual perceptions on trust, Panel B capture perceptions on local conflict and Panel C considers perceptions on public services provision. For all regressions, robust standard errors clustered at district level are in parentheses. Observations are at individual level. Demographic controls include age, age squared, an indicator for rural, gender and employment status, five living conditions fixed effects, five education fixed effects and 34 occupation fixed effects. See Table 3.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

conflict in a district that does not produce oil. Our results in Table A6 show that increase in international oil price is not associated with conflict in non-oil districts. Across different conflict outcomes, we find a negative relationship between positive oil price shock and civil conflict in districts that are non-oil.

Table 1.8: *Accounting for Agricultural Price Shocks*

Dependent variable	(1) Ethnic group attack civilians	(2) Clash between ethnic groups	(3) Government attack ethnic group	(4) Ethnic group attack government
Oil district×log oil price	0.321**	-0.066	0.195***	0.221***
	(0.160)	(0.052)	(0.049)	(0.077)
Cocoa district×log cocoa price	0.011	-0.009	0.015**	0.014**
	(0.011)	(0.010)	(0.006)	(0.006)
Oil palm district× log oil palm price	0.040	-0.055*	0.005	0.043
	(0.054)	(0.032)	(0.019)	(0.036)
Rubber district × log rubber price	-0.069	0.010	-0.012	-0.036
	(0.061)	(0.017)	(0.021)	(0.026)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	5,325	5,325	5,325	5,325

Notes: Each column represents a separate regression. For all regressions, robust standard errors clustered at district level are in parentheses. See Table 3.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

1.6 Conclusion

Using a difference-in-difference approach, this paper shows that oil price shocks influence civil conflict. Higher oil prices increase the likelihood of ethnic group attacks on civilians, government attacks on ethnic groups and ethnic group attacks on the government. The magnitude of these effects is similar to previous studies on economic causes of conflict. Our results are robust to including northern districts, excluding contiguous non-oil southern districts, excluding Boko Haram attacks, using northern districts as a control group; accounting for agricultural price shocks and pre-sample conflict outcomes.

Consistent with the view that resource rents are an important determinant of civil conflict, the paper shows that oil districts receive relatively more federal transfers in years of higher oil prices. There is no evidence that oil prices induce conflict through opportunity cost or pollution channels. In contrast, there is evidence showing a strong correlation between government repression and an increase in global oil prices. The paper also explores the effect of oil prices on social capital. It finds that a rise in the price of oil reduces trust between ethnic groups, lowers trust in the local government, the ruling party, the presidency, the parliament and the armed forces. Moreover, it has a negative effect on the local perception of public services such as the use of district revenue and community sanitation. In summary, the results confirm the notion that conflict along ethnic lines is related to contention over economic resources.

The findings of this study have a number of policy implications. First, the analysis suggests that volatility in revenue from natural resources plays a significant role in causing incidents of civil conflict. Second, they imply that government dependence on oil revenues influences its response to conflict. Given that oil is the primary export commodity of the country, any disruption in oil supply by rebel groups would instigate a government reprisal. Third, they indicate that careful monitoring of fiscal transfers to lower levels of government reduces the risk of appropriation by rebel groups. The essential contribution of this study has been to confirm the impact of positive oil price shocks on the risk of civil conflicts. It would be interesting to assess the effects of oil price shocks on political competition to determine whether the state dependence on the revenue of natural oil resources provides incentives for politicians to run for office.

Chapter 2

Terror and Birth Weight: Evidence from Boko Haram Attacks

2.1 Introduction

The April 2014 kidnapping of 276 female students by Boko Haram in Chibok, Nigeria, has renewed attention on the effects of extreme forms of terror activity. The kidnap adds to a 7-year history of violence. Counting all incidents and years since 2009, the yearly prevalence of terror events related to Boko Haram, according to the Armed Conflict and Location Event Data Project (ACLED), is over 300, with a peak of more than 400 in 2015. The cumulated death toll of these terror events exceeds 7000 people. The intensity of these attacks makes Boko Haram one of the deadliest terrorist groups in the world.[1]

The purpose of this paper is to examine the impact of *in utero* exposure to terrorism on birth outcomes. We study the impact of terror with two motivations in mind. First, we are interested in the indirect effect of terrorism on infant health through birth weight. Second, we explore the effect of terror exposure on parental investments after child birth. The indirect costs of terror can be 10 to 20 times larger than the direct costs (Global Terrorism Index, 2014). Yet it is unclear whether this constitutes a small or big effect on households.

[1] See Global Terrorism Index (2015)

Two arguments give small effect view credence: terror causalities are low when compared to affected population and the large scale of human capital substitution in production. Accordingly, the effect of terror is mitigated by the opportunity and ability to substitute production inputs (Becker and Murphy, 2001; Krueger, 2001). The argument for big effect view is three-fold. First, certain economic sectors are less resilient to terror attacks and suffer more losses. Second, indirect costs of fear can lead to overreaction either by households or the government. Lastly, terror attack increases uncertainty which harms investment (Abadie and Gardeazabal, 2003).[2] The existence of the two effects, big and small, suggests that the cost of terror depends on the magnitude of fatalities and household response to terror.

To quantify the impact of prenatal exposure of terror fatalities on birth weight, we combined detailed violent data from the Armed Conflict Location and Event Dataset (ACLED) with a Demographic Health Survey data conducted approximately four years after the start of Boko Haram attacks. We exploit the variation in terror fatalities during pregnancy trimesters to estimate the effect of terror on infant health. Our identifying assumption is that fatalities from terror attacks are unpredictable and are not correlated with the early-life health of a newborn child. We use a difference-in-differences strategy to study the relationship between terror fatalities and infant health by comparing births over periods where fatalities from terror attacks overlap with pregnancy to those where terror fatalities do not. Our results rely on the assumption that pregnancies are not timed relative to fatalities from terror attacks. In addition, we control for possible confounding factors such as migration, fatalities from other conflict events such as clashes between ethnic groups, household displacement, rainfall and temperature shocks, mother fixed effects, state-by-year fixed effects and linear trends. The results are also robust when fatalities are scaled by district population or fatalities from neighboring districts are used as controls.

Our baseline results show that in targeted districts, terror fatalities reduce the overall birth weight of exposed cohort by 6.05 grams. The bulk of the impact is experienced by children exposed in the first and third trimesters of pregnancy but the effect diminishes

[2]See Krueger (2007) for a review of this literature.

within families (i.e., siblings comparison). In contrast, we find no effect of terror fatalities on the probability of being born with low birth weight. [3]Our examination of possible channels of terror exposure shows that the reduction in the use of prenatal medical care prior to birth accentuates the impact of terror on infant health. We find that exposure to terror fatalities reduces the number of prenatal visits by 10 % in the first trimester and 7.8 % in the third trimester. We also consider the role of postnatal parental investments after terror exposure during pregnancy. Interestingly, the damage to infant health is compensated by an increase in infant postnatal care and duration of infant breastfeeding.

Due to the ongoing conflict with Boko Haram, this paper does not provide a comprehensive analysis of the impact of terror on birth weight. However, it offers new insights on how parental response to prenatal terror shock compensates for the reduction in birth weight. Our results contribute to the growing literature on the relationship between violence and birth outcomes (Camacho, 2008; Mansour and Rees, 2012; Berrebi and Ostwald, 2015; Koppensteiner and Manacorda, 2016). Although these studies provide compelling evidence on the effect of prenatal exposure to violence on pregnancy outcomes, they do not consider the role of parental investments on infant health. Our findings also relate to the growing literature on the long-term effects of low birth weight (Almond *et al.*, 2005; Black *et al.*, 2007; Oreopoulos *et al.*, 2008; Royer, 2009; Currie and Moretti, 2007). We show exposure to terror fatalities does not necessarily increase low birth weights in districts that experience terror attacks.

The remaining part of the paper proceeds as follows: Section 2.2 gives a brief background on Boko Haram and reviews the literature on the costs of terror and the impact of shocks to infant health. Section 2.3 describes the violence and health data that we use in addition to our empirical model. In section 2.4 we present the results and various robustness checks. We discuss our findings in section 2.5 and conclude in section 2.6.

[3]Low birth weight as defined by the World Health Organization (WHO) is the weight at birth below 2,500 grams (5.5 pounds) and is widely held to be an indicator of poor infant health. See World Health Organization (2011).

2.2 Institutional Context and Literature review

2.2.1 A brief history of Boko Haram

Boko Haram, which means western education is forbidden, was founded in Maiduguri in north-eastern Nigeria in 2002. The aim of the group was to create an Islamic state ruled by sharia law (Walker, 2012). The group transition to terror attacks began in July 2009 after a confrontation with the Nigerian police force that led to the death of over 700 people. The scale and intensity of events related to Boko Haram increased from 2012 and reached its peak in 2015. By the end of 2016, they were at least 1500 violent events and more than 10000 deaths associated with the group. The conflict also caused an internal displacement of more than one million people (Internal Displacement Monitoring Centre, 2015).

Figure 2.1 shows the number of events related to Boko Haram. To capture the effect of *in utero* terror exposure on infant health during pregnancy trimesters, the graph show variation in events by year and month of attack. There is a huge disparity in the number of events between 2012 and 2016 with the greatest quantity at 60 events in February 2015. Figure 2 reveals a similar increase in terms of the number of deaths associated with Boko Haram attacks. In this case, the events in February 2015 resulted in 3000 deaths. The average number of events and deaths since the beginning of Boko Haram attacks is quite unprecedented even by Nigerian standards of violent conflict episodes. Between 2009 and 2016, the activities of this group translate to an average of 189 events and 3180 per year. The geographical variation in terror fatalities within this period is shown in Figure 2.3. The map shows a large concentration of fatalities from Boko Haram events in the north-eastern Nigeria especially in the states of Borno, Yobe, Adamawa, and Bauchi.

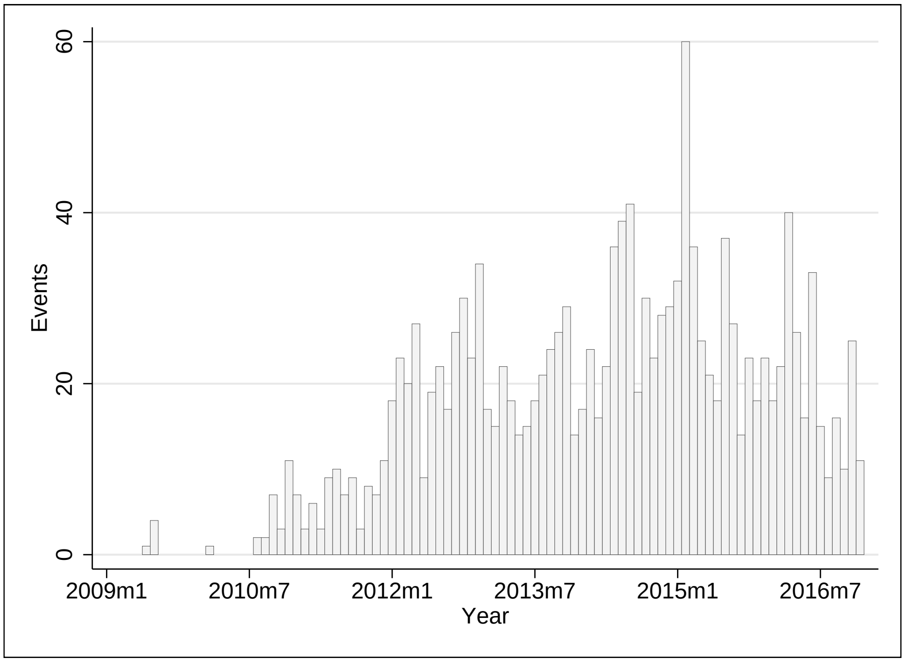

Figure 2.1: *Number of Boko Haram terror-related events by year (2009-2016)*

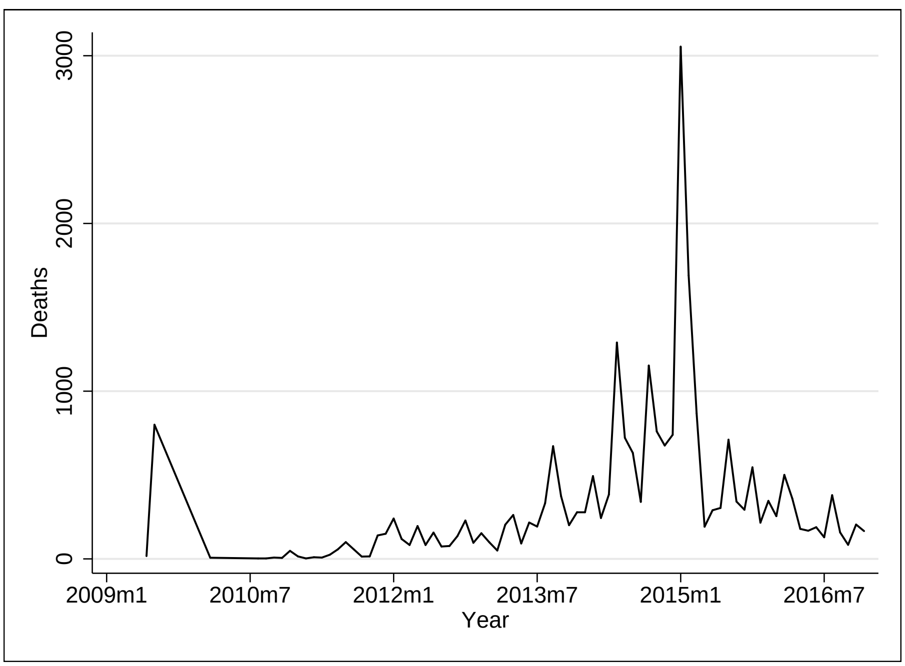

Figure 2.2: *Number of Boko Haram terror-related fatalities by year (2009-2016)*

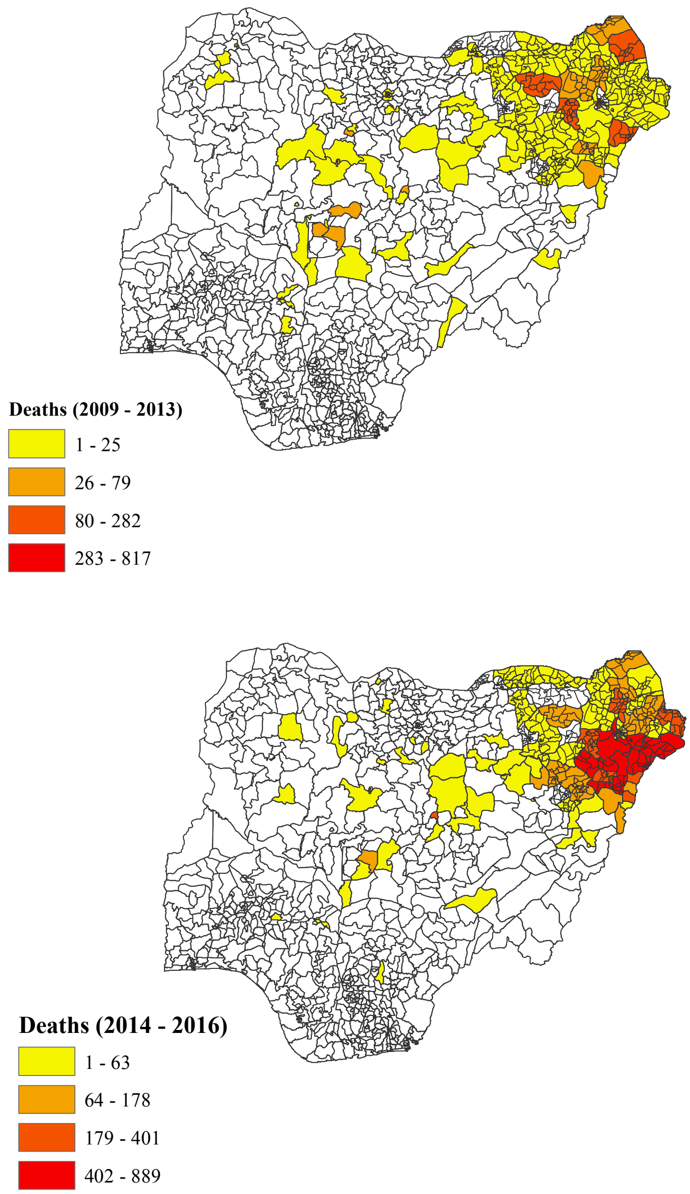

Figure 2.3: *Distribution of Boko Haram terror-related fatalities across districts (2009-2016)*

2.2.2 Prior Literature on shocks to infant health

In recent years, there has been an increasing amount of literature on the effect of maternal trauma on infant health. This trauma could result from several factors such as famine, economic shocks, pollution or conflict.[4]

[4]See Almond and Currie (2011) and Currie (2013) for review.

Famine affects the newborn through the well-documented effect of nutrition on fetal health. Results show that cohorts affected during later stages of pregnancy experienced reductions in birth weight. For instance, Roseboom *et al.* (2011) examined the effect of malnutrition on infants born during the 1944-45 wartime famine in Holland. They found that mid or late gestation exposure to famine resulted in low birth weights, head circumference, and length amongst affected cohorts.[5] Almond and Mazumder (2011) show the effect of maternal fasting on the child *in utero*. Their study show that prenatal exposure to maternal fasting during Ramadan is associated with lower birth weight and has a 22-23% likelihood of leading to the disability of the infant in adulthood.[6] Intrauterine exposure to famine also has long term effects in terms of schooling and socioeconomic outcomes. Relative to unexposed cohorts, cohorts exposed to the Chinese famine (1959-1961) *in utero* attain 0.58 fewer years of schooling (Meng and Qian, 2009); are 3-6% less likely to work and 12-13% more likely to be disabled (Almond *et al.*, 2007). Maternal exposure to a disease environment also has latent effects on children *in utero*. Using 1989-2009 Vital Statistics natality data, Almond *et al.* (2012) found that disease exposure to early childhood increases diabetes incidences and is also associated with poor socioeconomic outcomes and maternal behavior.

Economic shocks around the time of birth are likely determinants of birth weight since it creates maternal stress which affects the child *in utero*. However, findings are less consistent than the famine channel. Dehejia and Lleras-Muney (2004) provide evidence that children conceived during periods of recession are less likely to have low birth weight.[7] In contrast, Van den Berg *et al.* (2006) show that a child born during a recession lives a few years less than a child born during a boom period. Their study indicates that maternal stress from

[5]In a recent study, Hult *et al.* (2010) found that cohorts exposed to undernutrition and in infancy in the Biafra famine during the Nigerian civil war (1967-1970) have increased prevalence of hypertension on order of 9.5 to 24 percent and type 2 diabetes from 8 to 13 percent when they reach the age of 40. Early childhood exposure to the famine led to increased prevalence of adult blood pressure within the range of 9.5 to 16 percent.

[6]A further study by Almond *et al.* (2014) indicates that this affects subsequent academic outcomes of the child at the age of 7. They find that test scores are 0.05-0.08 standard deviations lower for students exposed to Ramadan in the first trimester of pregnancy.

[7]Banerjee *et al.* (2010) find that income shocks decrease height in the long-run but do not affect health outcomes or life expectancy.

economic conditions presupposes the infant to a high mortality rate later in life.

Prenatal exposure to pollution increases the risk of negative health outcomes for the infant *in utero*. A natural experiment study by Currie *et al.* (2009) shows that infants exposed *in utero* to high levels of carbon monoxide (CO) had low birth weight and gestation length relative to siblings and with an increase in death risk of 2.5%.[8] In addition, Coneus and Spiess (2012) find that an average increase in CO exposure in the last trimester reduces birth weight by 289g for infants and increases the likelihood of bronchitis and respiratory diseases for toddlers. Achyuta *et al.* (2016) show that a standard deviation increase *in utero* exposure to PM2.5 leads to a 0.39 percentage point increase in infant mortality.

Exposure to conflict is another potential mechanism that affects infant health. Prior studies show that child *in utero* is affected through psychological stress experienced by the mother. Camacho (2008) show that prenatal exposure to landmine explosions reduces birth weight by 8.7 grams relative to sibling not exposed to explosions. Using 2000-2005 al-Aqsa Intifada in Palestine, Mansour and Rees (2012) find that additional conflict fatality in the first and last trimester increases the probability of low birth weight by 0.003 and 0.002 percentage points respectively. Torche and Shwed (2015) show that exposure to war in the first and second trimesters of pregnancy lowers infant birth weight by 13 and 16 grams respectively. Using a panel data of more than 150 countries and data on terrorism from 1970 to 2007, Berrebi and Ostwald (2015) study the relationship between terrorism and fertility as measured by total fertility and crude birth rates. They find that a standard deviation increase in terror attacks leads to a 1.8 percentage point decrease in fertility rate and a reduction in the number of births by 0.5%. Koppensteiner and Manacorda (2016) estimates the impact of homicide rates on birth outcome in Brazil and finds that additional homicide during the first trimester of pregnancy increases the probability of low birth weight by 0.6 percentage points. The present study confirms previous findings and contributes additional evidence that suggests a relationship between infant health and *in utero* terror exposure.

[8]See Currie (2011, 2013) for recent summaries of this literature.

2.3 Data and Estimation

2.3.1 Data

We use the 2013 Nigeria Demographic and Health Survey (NDHS) data. The NDHS contains data on 38,522 households interviewed between February and June 2013. Information at the household level relates to birth registration, maternal education, and age, marital status, demographic characteristics, maternal behavior during pregnancy (e.g. number of prenatal visits) and infant health at birth. Our analysis uses infant early-life health outcomes, in particular, weight at birth.

Data on fatalities caused by Boko Haram attacks are collected from Armed Conflict and Location Events Data project. The data contains daily on attacks and deaths related to Boko Haram. We collate the information on a monthly basis and construct three terror exposure measures. The first measure is defined as terror fatalities that occur in the first trimester of pregnancy. In other words, terror fatalities that occur 9-6 months before birth. The second measure is terror fatalities that occur in the second trimester of pregnancy (5-3 months before birth). The third measure of terror exposure is terror fatalities that occur in the last trimester of pregnancy (2-0 months before birth). Subsequently, we match our measures of terror exposure with household data from the 2013 NDHS by district of residence.[9] Our analysis is focused on the impact of terror fatalities on infants born to ever married women at the time of the survey.[10]

Table 3.1 provides the descriptive statistics of the data. The full sample comprises of 5189 children born between March 2008 and June 2013 (a period of 64 months). We focus on mothers aged 15-49 at the time of the interview. Each woman is asked a series of fertility and health related questions pertaining to the date of child birth, number of prenatal care visits, place of delivery, medical attention during pregnancy, the weight of the infant at birth, religion and ethnicity. Given that the information on gestation length is not provided in

[9]This is based on the assumption that district of residence is the same as the district of birth.

[10]The upsurge in terror attacks started in July 2009, and June 2013 is the final birth entry in the health survey.

the NDHS, we assume that children born experienced are full-term births with an average gestation length of 40 weeks. The mean birth weight of the infant in the total sample is 2595 grams. 50 percent of the children in our sample weighed less than 2500 grams with an extra 3 percent weighing exactly 2500 grams. The sibling sample is made of 2406 children with an average birth weight of 2564 grams. We capture family characteristics that may influence child health at birth such as mother's age, mother's age at child birth, mother's age at marriage, mother's education, father's education, the age of household head etc.

2.3.2 Empirical model

Before discussing our empirical model, a number of caveats needs to be noted regarding the present study. First, due to data limitation, we can only focus on the effect of terror fatalities that occurred until June 2013. Hence, we underestimate the impact of terror fatalities given the increase in terror intensity between 2014 and 2016 (as shown in Figures 3.4 and 2.2).[11] Second, our analysis captures certain but not all districts that experienced Boko Haram attacks within our sample period. Consequently, we do not estimate the overall impact of Boko Haram attacks in Nigeria as a whole. These caveats are important in the sense that our study captures a partial view of the effect of terror on infant health at birth.

We use a difference-in-differences approach to relate variation in terror fatalities to trimesters of birth of child i as follows:

$$Y_i = \alpha_0 + \sum_{j=1}^{3} \alpha_j \times Trimester_j \times TerrorFatalities_d + \beta X_i + \tau_{yob} + \gamma_{mob} + \lambda_{dist} + \varepsilon_i, \qquad (2.1)$$

where Y_i is the dependent variable of interest. $Trimester_j$ is an indicator variable for the three trimesters of pregnancy; $TerrorFatalities_d$ is the number of casualties from Boko Haram terror attack in district d. The interaction between $Trimester_j$ and $TerrorFatalities_d$ capture terror exposure during pregnancy trimesters. This is measured by fatalities that

[11]The data for this study is restricted to the 2013 Demographic Health Survey of Nigeria. The 2015 DHS survey does not contain data on child health.

Table 2.1: *Descriptive Statistics*

	Full sample		Sibling Sample	
	Mean	SD	Mean	SD
A. Infant outcomes				
Birth weight in grams	2594.52	864.31	2563.86	864.62
Birth weight $<$2500g	0.496	0.501	0.526	0.500
Birth weight $\leq$2500g	0.525	0.500	0.550	0.498
Died within 1 year	0.015	0.121	0.020	0.140
Died within 2 years	0.006	0.076	0.007	0.082
Female	0.493	0.501	0.501	0.501
Twin	0.039	0.194	0.075	0.264
First born	0.275	0.447	0.182	0.386
B. Family characteristics				
Mother's age	30.562	5.929	30.526	4.919
Mother's age at birth	28.227	5.78	28.059	4.967
Mother's age at marriage	20.777	4.699	21.216	4.579
Mother's education	10.771	4.189	11.173	3.916
Father's education	11.081	4.572	11.498	4.073
Age of household head	41.324	11.936	40.762	11.135
Household head is male	0.838	0.370	0.868	0.34
Rural household	0.304	0.46	0.268	0.443
C. Use of medical care				
Number of prenatal visits	7.351	7.578	4.762	6.908
Timing of first antenatal check	2.872	2.263	1.910	2.307
Delivered in hospital or clinic	0.926	0.264	0.937	0.244
Doctor assistance in delivery	0.297	0.457	0.294	0.456
D. Postnatal parental Investment				
Infant postnatal check	0.399	0.49	0.257	0.437
Postnatal checkup by doctor	0.179	0.383	0.114	0.318
Months of breastfeeding$\geq$6 months	0.200	0.400	0.168	0.374
Duration of breastfeeding	1.269	0.503	1.214	0.506
F. Pregnancy Complication				
Any complication	0.654	0.476	0.430	0.496
Anaemia	0.575	0.495	0.383	0.487

Notes: This table provides summary statistics of child health outcomes and mother characteristics. The sample size for the full sample is 5189 while the sample size for the sibling sample is 2406. The sample period is between March 2008 and June 2013.

occur during the first trimester (9-6 months before birth), second trimester (5-3 months before birth) and the last trimester (2-0 months before birth). X_i is a vector of individual characteristics such as gender, twin and birth order, mother's education, mother's age at birth, mother's age at marriage, father's education and occupation, the age of household head, a dummy variable equal to one if household head is male and a rural indicator variable. We control for year of birth (τ_{yob}), the month of birth (γ_{mob}) and district of birth (λ_{dist}). ε_i is a random error term. The trimester parameters measure the extent to which cohorts (*in utero*) exposed to terror fatalities differ from those not exposed to terror attacks. In other words, it captures how pregnancy trimesters are differentially affected by terror attacks in relation to the impact of terror fatalities.

Following Almond *et al.* (2009) we control for the unobservable characteristics of families by restricting the sample to siblings using unique family identifiers. Put differently, we estimate equation (2.1) with family fixed effects to control for family characteristics that might affect fertility decisions with or without exposure to terror attacks.[12] Our estimation, therefore, compares cohorts exposed to terror attacks to their siblings and takes the following form:

$$Y_i = \alpha_0 + \sum_{j=1}^{3} \alpha_j \times Trimester_j \times TerrorFatalities_d + \beta X_i + \pi_{family} + \tau_{yob} + \gamma_{mob} + \varepsilon_i, \quad (2.2)$$

where π_{family} is family fixed effects. Our sample is restricted to 2406 siblings born to 1129 families. The explanatory and control variables are similar to equation 2.1. By restricting our sample to siblings, we compare those *in utero* during Boko Haram terror attack to their siblings. If infant health at birth is affected by terror fatalities, we would expect that those born between July 2009 and June 2013 to have lower birth weights than their siblings, and this difference to be substantial for those born in districts with more terror attacks. This model, therefore, controls for unobserved heterogeneity at the family

[12]This is to remove potentially confounding factors from unobserved characteristics, for instance, family income and selective fertility due to terror attack exposure.

level.[13]

2.4 Results

2.4.1 Baseline Results

Table 2.2 presents our baseline estimates of equation 2.1 for three different dependent variables: birth weight (in grams), low birth weight (a dummy variable when birth weight is less 2500 grams) and very low birth weight (a dummy variable when birth weight is less than or equal to 2500 grams).[14] For each dependent variable, we show two separate regressions, each comprising a different set of control variables. We begin by estimating the impact of terror fatalities on birth weight using equation 1. The results in panel A capture the effects of terror fatalities at any time during pregnancy. Overall, we find that fatalities from terror attacks reduce birth weight of exposed cohorts by 6.05 grams and it is statistically significant at 5% level (column 1). This translates to a 0.23 percentage point difference in birth weight between exposed and non-exposed cohorts. We estimate the effect of terror fatalities during pregnancy trimesters in Panel B. Our results show that additional deaths from terror attacks reduce birth weight by 11.10 grams in the first trimester and 4.68 grams in the third trimester. These results are statistically significant at 5% and 10% level respectively. In contrast, we find no significant effect of terror fatalities on birth weight when we add family fixed effects in column 2.

Next, we analyze the effect of terror on the probability of delivering a child with a low birth weight. Across the full and sibling samples (columns (3) and (4)) we find a positive effect of terror exposure on the infant weighing less than 2500 grams but none of the results are statistically significant. We find similar results when we use the indicator for very low

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[13]This is also similar to the estimation strategy used in Mansour and Rees (2012).

[14]The World Health Organization (2011) defines very low birth weight as birth weights less than 1500 grams (3 pounds 5 ounces). Given that the number of children in our sample with less 1500 grams is below 1 percent, we use a definition of very low birth weight that is similar to that of Mansour and Rees (2012). However, the results remain unchanged when we use this definition.

birth weight (columns (5) and (6)). A possible explanation for these results may be that we do not account for the beneficial effect of early prenatal care on birth weight. Besides, postnatal parental investments may compensate terror exposure during pregnancy. We consider these possible scenarios in sections 2.4.3 and 2.4.5.

Our estimates are comparable to those from previous studies. For instance, Camacho (2008) find that landmine explosions reduce birth weight in the first trimester by 11.6 grams. In the study of the effect of conflict in Palestine on birth weight Mansour and Rees (2012) find that additional conflict fatality in the third trimester reduces birth weight by 2.12 grams for infants exposed during pregnancy. Quintana-Domeque and Ródenas-Serrano (2017) show that an additional death from ETA bomb attacks in Spain reduces birth weight by 0.7 grams in the first trimester of pregnancy.

Table 2.2: *The effect on terror fatalities during pregnancy on birth weight*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of terror fatalities at any time during pregnancy						
Terror fatalities	-6.05*	3.26	0.0038	0.0006	0.0028	-0.0000
	(3.23)	(3.48)	(0.0032)	(0.0008)	(0.0026)	(0.0013)
Panel B: Effect of terror fatalities by trimester						
Terror fatalities: 1st trimester	-11.10**	-0.10	0.0058	0.0012	0.0051	0.0006
	(5.00)	(2.90)	(0.0065)	(0.0013)	(0.0060)	(0.0014)
Terror fatalities: 2nd trimester	-4.16	6.31	0.0054	0.0008	0.0044	0.0010
	(7.22)	(12.47)	(0.0066)	(0.0016)	(0.0060)	(0.0033)
Terror fatalities: 3rd trimester	-4.68*	3.40	0.0008	0.0000	-0.0005	-0.0010
	(2.56)	(2.88)	(0.0014)	(0.0007)	(0.0009)	(0.0014)
Controls						
Family Fixed Effect	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy. The dependent variable in columns (1) and (2) are birth weight in grams while the dependent variable in columns (3) and (4) is a dummy variable equal to one if birth weight is less than 2500 grams. The dependent variable in columns (5) and (6) is a dummy variable equal to one if birth weight is less than or equal to 2500 grams. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head, an indicator variable if household head is male and a dummy variable for rural households. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

2.4.2 Heterogenous effect

Table 2.3 considers our baseline results across different population subsamples such as mother's level of education, the age of mother at birth and mother's employment status. Using variation in education due to school entry policies, McCrary and Royer (2011) find that the education of the mother affects infant health as proxied by birth weight and prematurity. Chena and Li (2009) show that this effect is mainly during post-natal nurturing. We study whether mother's years of schooling mitigate the effect of terror exposure on birth weight by classifying our sample into two categories: mothers with less than 12 years of education and mothers with 12 or more years of education. The results as reported in columns (1)-(4) show that terror fatalities in the first trimester reduce birth weight by 20.78 grams for women with lesser education and by 7.54 grams for women with more education.

Next, we explore the effect of maternal age of childbearing on birth outcomes. Rosenzweig and Schultz (1983) and Rosenzweig and Wolpin (1995) highlighted the impact of age of mother at birth on differences in birth weight but no previous study has investigated the additional effect of terror fatalities. We estimate the effect of maternal age at birth by dividing the sample into children born to women under 28 years and those born to women at age 28 and over.[15] Our results in columns (5)-(8) show that additional terror fatalities reduce birth weight by 17.51 grams in the first trimester for mothers whose age at birth is less than 28 years. In contrast, terror fatalities decrease birth weight by 10.88 grams for mothers with maternal age at 28 and above.

[15] We chose this cutoff to create groups approximately equal in size.

Table 2.3: *The effect of terror fatalities on birth, by mother's education and age of mother at birth*

	Mother's education$<$12 years		Mother's education$\leq$12 years		Mother's age at birth$<$28 years		Mother's age at birth$\leq$28 years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Terror fatalities: first trimester	-20.78***	-47.53	-7.54*	0.65	-17.51***	-42.39	-10.88**	1.33
	(3.77)	(56.18)	(4.09)	(4.25)	(4.65)	(57.27)	(5.28)	(4.72)
Terror fatalities: second trimester	45.33	-22.85	-5.81	7.76	-13.73	-34.75	1.65	11.20
	(36.58)	(33.23)	(8.57)	(13.16)	(10.53)	(93.38)	(5.08)	(10.99)
Terror fatalities: third trimester	-0.62	3.09	-4.53	3.52	-1.73	5.61	-2.22	-7.13
	(3.85)	(7.83)	(4.57)	(3.22)	(2.72)	(6.44)	(2.78)	(11.36)
Controls								
Family Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Observations	1,865	803	3,324	1,603	2,450	1,134	2,739	1,272

Notes: Each column represents a separate regression. The dependent variable is birth weight (in grams). Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. See Table 2.2 for description of family characteristics. Significance levels.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Prior studies have explored the relationship between maternal employment and child development (Gregg *et al.*, 2005; Berger *et al.*, 2005; Baker and Milligan, 2008). We study the effect of maternal employment on birth weight since it is considered to be an important determinant of birth outcomes. We split the sample into working mothers and non-working mothers. We define working mothers as those that were employed within the past one year. The results in Table 2.4 show that, for working mothers, an increase in terror fatalities reduces birth weight in the first trimester by 13.71 grams and in the third trimester by 9.76 grams. The effect for non-working mothers is a reduction by 11.46 grams for terror exposure in the first trimester.

Table 2.4: *The effect of terror fatalities on birth weight, by mother's employment status*

Dependent variable	Working Mothers		Non-working mothers	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	-13.71***	0.38	-11.46**	-0.12
	(3.60)	(3.85)	(5.63)	(5.12)
Terror fatalities: second trimester	4.67	14.54***	-9.34	-18.42**
	(4.99)	(4.00)	(8.77)	(9.28)
Terror fatalities: third trimester	-9.76***	4.49	4.74	10.90*
	(2.67)	(5.67)	(4.42)	(5.97)
Controls				
Family Fixed Effect	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes
Observations	3,991	1,859	1,198	547

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. See Table 2.2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

2.4.3 Use of medical care

Among the plausible explanation for the effect of terror fatalities on birth weight is the reduction in maternal access to prenatal care. Exposure to prenatal care increases birth weight and reduces incidents of low birth weight (Currie and Cole, 1993; Currie and Gruber, 1996; Joyce, 1999). We examine the effect of terror fatalities on prenatal care using four different measures such as the number of prenatal visits, the timing of the first antenatal check (measured in months), doctor assistance in child birth and whether the infant was delivered in a hospital or clinic. Table 2.5 presents the results. We find that additional terror fatalities reduce the number of prenatal visits by 10.3% in the first trimester and 7.8% in the third trimester (column 1). In addition, our estimate with the sibling sample shows a 20.4% decline in prenatal visits for terror exposure in the first trimester (column 2). The results are statistically significant at 1% level. We find no evidence of a delay in the timing of prenatal visits (columns 3 and 4). The probability of doctor assistance during child birth decreases by 0.6 percentage point for an increase in terror fatalities in the third trimester (column 5). In contrast, there is no effect of terror on the likelihood of child birth in a hospital. [16]

As mentioned earlier, our baseline results may be capturing the effect of maternal access to prenatal care. To account for this possibility we repeat our preliminary analysis for the effect of terror fatalities on birth weight and control for the number and timing of prenatal visits. The results, as reported in Table 2.6, are identical to our baseline estimates.

[16] We also explore the possibility that mothers exposed to terror might suffer from anemia or develop pregnancy complications. Table B1 in the online appendix provides evidence of a positive relationship between additional terror fatalities increase the odds of having anemia by 0.41 percentage point in the second trimester and 0.40 percentage point in the third trimester. Furthermore, we find that the probability of pregnancy complications increase by 0.59 percentage point for terror exposure in the third trimester.

Table 2.5: *The effect of terror fatalities on the use of medical care*

Dependent variable	Number of prenatal visits		Timing of prenatal visits		Doctor assistance in delivery		Delivered in hospital	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Terror fatalities: first trimester	-0.103***	-0.204***	0.017	0.007	-0.003	0.001	-0.000	0.000
	(0.027)	(0.070)	(0.013)	(0.020)	(0.002)	(0.002)	(0.002)	(0.001)
Terror fatalities: second trimester	0.062	-0.127	-0.005	0.026	0.010	0.002	0.002	0.001
	(0.050)	(0.135)	(0.016)	(0.041)	(0.010)	(0.007)	(0.002)	(0.001)
Terror fatalities: third trimester	-0.078***	-0.111	0.048	0.039	-0.006***	-0.009	0.004	-0.000
	(0.029)	(0.116)	(0.031)	(0.034)	(0.002)	(0.009)	(0.003)	(0.001)
Controls								
Family Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The dependent variable in columns (1) and (2) is the number of antenatal visits during pregnancy while the dependent variable in columns (3) and (4) captures the timing of the first antenatal check (months) . The dependent variable in columns (5) and (6) is a dummy variable equal to one if a medical doctor assisted in child birth and the dependent variable in columns (7) and (8) is a dummy variable equal to one if the place of delivery is the hospital. The sample period is from March 2008 through June 2013. See Table 2.2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 2.6: *The effect of terror fatalities during pregnancy on birth weight, controlling for use of medical care*

Dependent variable	Birth weight (in grams)		Birth weight<2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities: first trimester	-11.01**	-0.30	0.006	0.001	0.005	0.000
	(5.06)	(2.95)	(0.007)	(0.001)	(0.006)	(0.001)
Terror fatalities: second trimester	-4.60	6.38	0.005	0.001	0.004	0.001
	(7.18)	(12.28)	(0.007)	(0.001)	(0.006)	(7.18)
Terror fatalities: third trimester	-4.44*	3.38	0.001	0.000	-0.000	-0.001
	(2.59)	(3.14)	(0.001)	(0.001)	(0.001)	(0.002)
Controls						
Timing of prenatal visits	Yes	Yes	Yes	Yes	Yes	Yes
Number of prenatal visits	Yes	Yes	Yes	Yes	Yes	Yes
Doctor assistance in delivery	Yes	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

2.4.4 Infant Mortality

In this section, we examine the effect of terror fatalities on the risk of infant mortality. If terror fatalities reduce birth weight and decline in birth weight increases the chance of infant mortality, then terror fatalities should have an influence on infant mortality. Evidence on the relationship between birth weight and infant mortality show that a one standard deviation increase in birth weight is associated with a 2.2 percentage point reduction in infant mortality (Almond *et al.*, 2005). In addition, recent studies on the impact of environmental shocks on early childhood mortality suggest that prenatal exposure to adverse events reduces the chance of infant survival. For instance, Achyuta *et al.* (2016) found that *in utero* exposure to dust pollution in West Africa increases infant mortality by 0.39 percentage points. Jayachandran (2009) shows that prenatal smoke from the 1997 Indonesia wildfire reduced infant survival by 3.5 percent. Kudamatsu *et al.* (2012) demonstrate that African infants face a higher risk of death when exposed to malaria in the first trimester of pregnancy or drought shocks prior to birth.

We estimate the relationship between *in utero* exposure to terror fatalities on infant mortality by using two measures. We define an indicator variable for infants that died within one year of birth and another indicator variable for infants that died within two years of birth. Our results in Table 2.7 show that terror fatalities are not associated with infant mortality within one year (columns (1) and (2)). However, additional deaths from terror attacks reduce the chance of infant survival by 0.04 percentage point in the second trimester at a statistically significant level of 10%, and 0.03 percentage point in the third trimester (column (3)) at a statistically significant level of 5%.

Table 2.7: *The effect of terror fatalities on infant mortality*

Dependent variable	Infant died within one year of birth		Infant died within two years of birth	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	-0.0004	-0.0005	0.0001	0.0005
	(0.0007)	(0.0012)	(0.0001)	(0.0005)
Terror fatalities: second trimester	0.0002	0.0007	0.0004*	0.0006
	(0.0003)	(0.0010)	(0.0002)	(0.0005)
Terror fatalities: third trimester	0.0002	-0.0003	0.0003**	0.0003
	(0.0002)	(0.0013)	(0.0001)	(0.0003)
Controls				
Family Fixed Effect	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes
Observations	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. See Table 2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

2.4.5 Parental Investment

Our results thus far show that *in utero* exposure to terror fatalities reduce the birth weight of the exposed infant. Yet, parental response to terror attacks may compensate or reinforce the effect of this prenatal shock. Previous studies have related differences in postnatal investments to differences in birth weight but no study has examined the effect of terror exposure on parental investments after child birth. For example, Datar *et al.* (2010) found that parents reinforce differences between siblings by investing more in children who have normal birth weight. However, Hsin (2012) shows that parents compensate children with low birth weight additional investment.[17] We consider the effect of parental investment in two ways. First, we ascertain whether parental investments are correlated with terror exposure by estimating equation 1 using postnatal investment measures as dependent variables. Second, we interact postnatal investment measures with our measure of terror exposure and control for it in the baseline regression. This accounts for the mediating impact of parental investment immediately after birth.[18]

We examine the effect of terror exposure on postnatal parental investment by using four different outcomes. First, we use an indicator variable that captures infant postnatal check within 2 months after birth. Second, we define a categorical variable that shows whether the postnatal check was done by a doctor. Third, we define a binary variable if the number of months of breastfeeding is 6 months or greater to capture the threshold effect of breastfeeding.[19] Lastly, we define the duration of breastfeeding as an ordinal variable equal to zero if the mother never breastfed the child; one if the child was ever breastfed but not currently breastfed and two if the child is still breastfeeding.[20]

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[17]See Almond and Mazumder (2013) for a recent review of this literature.

[18]A similar strategy is used in Kelly (2011).

[19]An exclusive feeding of 6 months is recommended by the World Health Organization (World Health Organization, 2001).

[20]There is a large literature on the impact of breastfeeding on child health. See Baker and Milligan (2008) for a review.

Table 2.8: *The effect of terror fatalities on parental investment*

Dependent variable	Infant postnatal check		Postnatal checkup by doctor		Months of breastfeeding≥6 months		Duration of breastfeeding	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Terror fatalities: first trimester	0.018***	0.006*	0.008*	0.017***	0.008***	0.004	0.007***	-0.000
	(0.005)	(0.003)	(0.004)	(0.003)	(0.002)	(0.006)	(0.002)	(0.005)
Terror fatalities: second trimester	-0.016***	-0.004	-0.011***	-0.010**	-0.005	0.011	0.008**	0.020
	(0.006)	(0.010)	(0.004)	(0.005)	(0.010)	(0.008)	(0.003)	(0.023)
Terror fatalities: third trimester	0.009	0.007	0.005	0.002	0.003*	0.012***	0.004***	0.010*
	(0.006)	(0.009)	(0.006)	(0.010)	(0.002)	(0.002)	(0.001)	(0.006)
Controls								
Family Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The dependent variable in columns (1) and (2) is a dummy variable equal one if the infant had a postnatal check within 2 months after birth while the dependent variable in columns (3) and (4) is an indicator variable equal to one if the postnatal checkup was done by a doctor . The dependent variable in columns (5) and (6) is equal to one if number of months of breastfeeding is greater than or equal to 6 months and the dependent variable in columns (7) and (8) is equal to zero, one or two: zero if the mother never breastfed the child; one if the child was ever breastfed but not currently breastfed and two if the child is still breastfeeding . The sample period is from March 2008 through June 2013. See Table 2.2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 2.9: *The effect on terror fatalities on birth weight, controlling for postnatal investments*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of terror fatalities at any time during pregnancy						
Terror fatalities	-5.08	3.70	0.0016	0.0001	0.0003	-0.0008
	(6.39)	(9.04)	(0.0039)	(0.0021)	(0.0025)	(0.0027)
Panel B: Effect of terror fatalities by trimester						
Terror fatalities: 1st trimester	-1.26	-40.68	-0.0317**	0.0150	-0.0633***	0.0244
	(18.88)	(46.55)	(0.0135)	(0.0159)	(0.0124)	(0.0178)
Terror fatalities: 2nd trimester	69.85***	-17.20	-0.0178	-0.0019	-0.0462**	-0.0119
	(17.81)	(71.10)	(0.0111)	(0.0163)	(0.0226)	(0.0233)
Terror fatalities: 3rd trimester	-64.39	3.35	0.0554	-0.0009	0.0484	-0.0014
	(82.05)	(2.17)	(0.0718)	(0.0008)	(0.0641)	(0.0013)
Controls						
Terror fatalities × Postnatal Investment	Yes	Yes	Yes	Yes	Yes	Yes
Family Fixed Effect	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are Boko Haram fatalities that occurred within trimesters of pregnancy. The dependent variable in columns (1) and (2) are birth weight in grams while the dependent variable in columns (3) and (4) is a dummy variable equal to one if birth weight is less than 2500 grams. The dependent variable in columns (5) and (6) is a dummy variable equal to one if birth weight is less than or equal to 2500 grams. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head, an indicator variable if household head is male and a dummy variable for rural households. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 2.8 shows that the probability of infant postnatal check increases by 1.8 percentage point for every exposure to terror fatalities in the first trimester (column (1)). Surprisingly we find that terror exposure in the second trimester reduces infant postnatal check by 1.6 percentage point. The results are statistically significant at 1% level. For our sibling sample (column (2)), there is a 0.6 percentage point chance of postnatal check of the new born child for terror exposure in the first trimester. This estimate is statistically significant at 10% level. Next, we consider the likelihood that the postnatal check is done by a doctor. The results show a similar pattern to that of infant postnatal check. An additional fatality from the terror attacks in the first trimester increases the probability of postnatal check-up by a doctor by 0.08 percentage point but reduces the probability by 1.1 percentage point in the second trimester (column (3)). The statistical significance is at 10% and 1% level respectively. A comparable result is shown when we add family fixed effects in column(4). Terror exposure in the first trimester increases the likelihood of doctor assistance in postnatal check by 1.7 percentage point at a statistically significant level of 1%, and reduces this probability by 1 percentage point in the second trimester at 5% level of statistical significance.

We estimate the effect on terror fatalities on the probability that months of breastfeeding are equal to or more than 6 months. We find that additional deaths from terror attacks in the first trimester increase this probability by 0.8 percentage point and by 0.3 percentage point for terror exposures in the third trimester. The results are statistically significance level at 1% and 10% respectively (column (5)). The estimate in our sibling sample shows an increase of 1.2 percentage point in the likelihood that the number of months of breastfeeding exceeds the recommended threshold (column (6)). Finally, we consider the impact of terror fatalities on the duration of breastfeeding. We find that additional terror fatalities increase the duration of breastfeeding by 0.7 percentage point in the first trimester, 0.8 percentage point in the second trimester and 0.4 percentage point in the third trimester (column (7)). With family fixed effects, the impact increases to 1 percentage point for exposure to terror fatalities in the third trimester (column (8)).

Table 2.9 shows the effects of terror fatalities on birth weight while controlling for the interaction between terror exposure and postnatal investments. We find that the effect of prenatal exposure to terror on birth weight is substantially reduced and not significant. This suggests that parental investments after child birth compensate for terror shock during pregnancy.

2.4.6 Alternative empirical specification

Our baseline specification assumes that terror exposure depends on the number of fatalities from terror attacks. We relax this assumption in two ways. First, we use the continuous measure of terror events at the district level to estimate a model of the form:

$$Y_i = \alpha_0 + \sum_{j=1}^{3} \alpha_j \times Trimester_j \times TerrorEvents_d + \beta X_i + \tau_{yob} + \gamma_{mob} + \lambda_{dist} + \varepsilon_i, \tag{2.3}$$

where Y_i is the dependent variable of interest. $Trimester_j$ is an indicator variable for the three trimesters of pregnancy; $TerrorEvents_d$ is the number of Boko Haram events, with at least one fatality, in district d. The interaction between $Trimester_j$ and $TerrorEvents_d$ captures the number of terror events at district d during pregnancy trimesters j. The control variables are similar to equation 1.

Second, we use an indicator dummy for whether terror attacks overlapped with pregnancy. In other words, we estimate the following:

$$Y_i = \alpha_0 + \sum_{j=1}^{3} \alpha_j \times Trimester_j \times TerrorExposure_d + \beta X_i + \tau_{yob} + \gamma_{mob} + \lambda_{dist} + \varepsilon_i, \tag{2.4}$$

where $TerrorExposure_d$ is an indicator variable that takes the value of 1 for the cohort exposed to a terror attack with at least one fatality between March 2008 - June 2013 in district d. Our control variables are equivalent to those in equation 1.

Table 2.10 shows our results using equation 3. We find that terror events reduce the birth weight of exposed cohorts by 93.21 grams (column (1)). The effect is statistically significant at 5% level. The magnitude of this impact is meaningful. In our full sample, the mean of birth weight is 2595 grams, which suggests that terror events lead to a 4% difference in birth weight between exposed and unexposed cohorts. Considering trimester effects, we find that additional terror events decrease birth weight by -142.10 grams in the third trimester, an effect that is statistically significant at 5% level. In contrast, terror exposure in the first trimester reduces birth weight by -103.84 grams and is statistically significant at 15% level. We also estimate the impact of terror events on the probability of having low birth weight (columns (3) and (4)) and very low birth weight (columns (5) and (6)). Our results show a positive relationship but not statistically significant.

We present results from estimating equation 4 in Table 2.11. Our difference-in-differences estimate (in Panel A) show that, overall, terror exposures reduce birth weight by 151.29 grams (columns (1)) and is statistically significant at 5% level. This is equivalent to a birth weight difference of 6% for infants exposed during pregnancy. Next, we turn to individual pregnancy trimesters (Panel B). Consistent with previous results, the estimated effect is negative for the first and third trimesters. Terror exposure decreases birth weight by 299.92 grams in the third trimester. The result is statistically significant at 10% level.

Table 2.10: *The effect on terror events during pregnancy on birth weight*

Dependent variable	Birth weight (grams)		Birth weight $<$2500g		Birth weight$\leq$2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of terror events at any time during pregnancy						
Terror events	-93.21**	0.46	0.0586	0.0092	0.0360	0.0014
	(40.13)	(36.34)	(0.0417)	(0.0126)	(0.0336)	(0.0172)
Panel B: Effect of terror events by trimester						
Terror events: 1st trimester	-103.84	-26.60	0.0624	0.0223	0.0481	0.0217
	(63.22)	(42.35)	(0.0750)	(0.0190)	(0.0739)	(0.0231)
Terror events: 2nd trimester	-29.28	-50.72	0.0447	0.0105	0.0207	0.0136
	(40.70)	(83.58)	(0.0395)	(0.0199)	(0.0252)	(0.0319)
Terror events: 3rd trimester	-142.10**	60.62	0.0684	0.0005	0.0428	-0.0211
	(68.18)	(63.62)	(0.0741)	(0.0126)	(0.0722)	(0.0180)
Controls						
Family Fixed Effect	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror events are Boko Haram events that occurred within trimesters of pregnancy. The dependent variable in columns (1) and (2) are birth weight in grams while the dependent variable in columns (3) and (4) is a dummy variable equal to one if birth weight is less than 2500 grams. The dependent variable in columns (5) and (6) is a dummy variable equal to one if birth weight is less than or equal to 2500 grams. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head, an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 2.11: *The effect on terror exposure during pregnancy on birth weight*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of terror exposure at any time during pregnancy						
Terror exposure	-151.29**	-7.94	0.0702	0.0134	0.0233	-0.0022
	(64.82)	(103.04)	(0.0614)	(0.0253)	(0.0462)	(0.0324)
Panel B: Effect of terror exposure by trimester						
Terror exposure: 1st trimester	-136.95	-53.24	0.0748	0.0447	0.0591	0.0435
	(128.00)	(84.35)	(0.1278)	(0.0380)	(0.1211)	(0.0462)
Terror exposure: 2nd trimester	78.77	-79.91	-0.0220	0.0084	-0.0523*	0.0066
	(60.45)	(141.69)	(0.0362)	(0.0286)	(0.0286)	(0.0452)
Terror exposure: 3rd trimester	-299.92*	123.65	0.1410	0.0091	0.1017	-0.0196
	(153.19)	(84.29)	(0.1636)	(0.0279)	(0.1560)	(0.0585)
Controls						
Family Fixed Effect	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror exposure is an indicator variable if a Boko Haram event occurred during trimesters of pregnancy. The dependent variable in columns (1) and (2) are birth weight in grams while the dependent variable in columns (3) and (4) is a dummy variable equal to one if birth weight is less than 2500 grams. The dependent variable in columns (5) and (6) is a dummy variable equal to one if birth weight is less than or equal to 2500 grams. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head, an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

2.4.7 Robustness

In this section, we consider several robustness checks. We begin by estimating the effect of terror fatalities on birth weight using an alternative measure of first trimester exposure. Our new measure capture fatalities from terror attacks that occur 8-6 months before birth. We contrast this with our previous measure of first trimester exposure (9-6 months before birth) and present the results in Table 2.12. The estimate is similar to our primary results. Additional fatality from terror attacks reduces birth weight by 11.10 grams in the first trimester.

Our analysis thus far implies that terror fatalities only affect infant health at birth through prenatal exposure. Nevertheless, a potential concern is that terror attacks that occur prior to conception or after child birth may affect birth outcomes. Such an impact would undermine our estimate on the effect of terror fatalities on birth weight of exposed cohorts. We show that this is not the case in Appendix Table B2. We define terror fatalities prior to conception as fatalities from terror attack that occurred 1 to 3 months before pregnancy while terror fatalities after birth are fatalities that occurred 1 to 3 months after birth. The effect of terror fatalities on birth weight is negative and significant for the first and third trimesters exposure and has a larger effect. Additional deaths from terror reduce birth weight by 13.39 grams and 6.53 grams in the first and third trimesters respectively (column(1)). Both estimates are statistically significant at 1% level. We also find that, for every increase in the number of terror fatalities, the infant probability of having low birth weight increases by 1 percentage point and the likelihood of very low birth weight rise by 0.9 percentage points.

We further test our results by controlling for fatalities from other conflict events such as the clash between ethnic groups and religious conflicts. We compute measures of conflict exposure using district level fatalities from violent events that coincide with pregnancy trimesters in our sample period.[21] The findings, as presented in Appendix Table B3 are similar to our main results. We also check whether fatalities from neighboring districts affect our estimates. Although our results (Appendix Table B4) show are robust to this control,

[21] We use the data from ACLED for this computation.

we find evidence that spillovers of terror from adjacent districts reduce birth weight for exposures in the second trimester.

Another concern is that our results may be driven by household migration or possible displacement due to terror. We address the first concern by splitting our sample using a proxy for migration. We consider households that have been away for more than a month within the last 12 months as migrants. We drop the "migrant" households and estimate equation 1. We present the results in Appendix Table B5. The results are identical to the estimates from the full sample, albeit the third trimester is not significant in the non-migrant sample. For the second issue, we use data on the number of internally displaced persons (IDPs) from the United Nations Office for the Coordination of Humanitarian Affairs (UN OCHA) to ascertain at the district level the number of persons displaced each month.[22] We control for the number of people displaced each year at the district level and report the results in Appendix Table B6. Our findings are identical to the baseline results.

Next, we consider the possible effect of climate shocks by controlling for yearly variation in rainfall and temperature at the district level. We use rainfall data from the National Oceanic and Atmospheric Administration Climate Prediction Centre (NOAA CPC) Africa Rainfall Estimation (version 2.0). The rainfall estimates are constructed by accumulating daily rainfall data to produce dekadal (10-day) estimates at about 10km spatial resolution. Our findings, as reported in Appendix Table B7 are robust to this control. It is plausible that our results capture pre-conflict trend or seasonal trend differences (for instance, rainy season and dry season). We estimate equation 1 and control for district time trend and month by year fixed effect. Our results in Appendix Table B8 show a larger and significant effect for prenatal terror exposures in the first and third trimesters within our sample period.

Our baseline specification assumes that the effect of terror fatalities on birth weight is linear. We relax this assumption and explore the effects of terror exposure by the intensity of attacks. We categorize terror intensity by the number of deaths and report the results in

[22] The data can be obtained from the following website: http://www.internal-displacement.org/countries/nigeria.

Table 2.12: *The effect of terror fatalities on birth weight, using alternative measures of first trimester fatalities*

Dependent variable	Birth weight (grams)		Birth weight $<$2500g		Birth weight$\leq$2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities: first trimester	-11.10**	-0.10	0.006	0.001	0.005	0.001
	(5.00)	(2.90)	(0.006)	(0.001)	(0.006)	(0.001)
Terror fatalities: second trimester	-4.16	6.31	0.005	0.001	0.004	0.001
	(7.22)	(12.47)	(0.007)	(0.002)	(0.006)	(7.22)
Terror fatalities: third trimester	-4.68*	3.40	0.001	0.000	-0.000	-0.001
	(2.56)	(2.88)	(0.001)	(0.001)	(0.001)	(0.001)
Controls						
Family Fixed Effect	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy. Terror fatalities in the first trimester measure terror fatalities that occurred 8-6 months before birth. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Appendix Table B9. We replace our terror exposure variable with a dummy variable of 1 if the number of fatalities within trimesters of pregnancy is at least 1 (and 0 otherwise) in panel A, at least 5 in panel B and at least 10 in panel C. The coefficients suggest that prenatal terror exposure reduces birth weight by approximately 300 grams in the third trimester if terror attacks lead to at least one fatality. The magnitude increases for every additional death and the impact become stronger for first trimester exposure and lesser for exposures in the third trimester exposures.

Finally, we test the sensitivity of our results by using the number of terror fatalities per capita at the district level as our independent variable. An advantage of this method is that it avoids that assumption that the effect of an additional death, from terror attacks, is similar across districts. We divide each of our trimester prenatal exposure by population estimates of district d (in hundreds of thousands).[23] The results are reported in Appendix Table B10. The estimated effect for birth weight in the first trimester is negative and statistically significant at 1% level. For the third trimester exposure, the effect is negative but not significant. The probability of having an infant with less than 2500 grams at birth is positive and significant for exposure in the first trimester (an increase of 2.64 percentage point). In addition, the probability of having a child weighing less than or equal to 2500 grams at birth increase by 2.39 percentage points for prenatal exposures in the first trimester.

2.5 Discussion of findings

This paper finds that terror fatalities from Boko Haram attacks have a negative and significant effect on birth outcomes of exposed cohorts. However, it is unclear what would be the long-term effect of being born with a weight that is 6 grams less due to violence. The short-term impact of terror exposure on infant health at birth is contingent upon certain maternal characteristics: additional terror fatality reduces birth weight when mothers have less education, give birth at a younger age or are currently working. In addition, the number

[23]Our population figures are collated from the 2010 - 2016 Annual Abstract of Statistics from the Nigerian National Bureau of Statistics. The data can be accessed from http://www.nigerianstat.gov.ng/

of prenatal visits decreases for terror exposures in the first trimester of pregnancy. The possible implications of our results are as follows. First, the effects of terror exposure appear to operate through two mechanisms. Birth weight decreases when mothers are unable to use medical care services out of fear of an attack. Although better educated mothers were less affected by the terror attacks, our results show that they consumed less prenatal health care as a consequence of terror. The effect of stress is also possible especially if less educated mothers live in districts that experience more terror attacks. This shows that the impact of terror exposure on birth outcomes can be heterogeneous. However, given the cross-sectional nature of our data and the short period of analysis, our estimates should be interpreted with caution. Second, birth weight reduction is likely to affect long-run outcomes such as child height and education. While our study does not estimate the effect of terror exposure on these outcomes, it suggests plausible long-term effects. The study by Black *et al.* (2007) show a 10% increase in birth weight leads to an additional increase of .75 centimeters in adult height and 1 percentage point increase in the probability of completing high school.

Third, postnatal parental investments play a consistent and vital role in moderating the effect of terror exposure on birth weight. Whether these estimates are attributable to differences in initial endowment levels of the parents or damage to physical development growth of the unborn child, cannot be discerned using the DHS data and without additional information on parental investments. Nevertheless, our results show that postnatal investment is important for children exposed to prenatal terror shock.

2.6 Conclusion

Past studies on the consequences of terrorism on birth outcomes focus mainly on its effects through prenatal exposure (Camacho, 2008; Berrebi and Ostwald, 2015). In this paper, we try to estimate the effect of terror exposure on postnatal parental investments. We find that fatalities from Boko Haram attacks exacerbate birth weight differences between exposed and unexposed cohorts across districts but is not associated with differences in birth outcomes within families. We also consider the possibility that postnatal investments by parents may reinforce or compensate birth weight differences as a result of terror exposure during pregnancy trimesters. Our results show that parents compensate for birth weight reduction by increasing postnatal investments of affected cohorts.

An important caveat of our analysis is that we only estimate the effect of terror exposure prior to the escalation of Boko Haram attacks. Hence, the estimated negative relationship between terror fatalities and birth weight understates the true effect of terror attacks on birth outcomes. A further study with more focus on the impact of the intense period of terror attacks is therefore suggested. In addition, we assume that there are no spillover effects of violence on neighboring regions although there may be large spillover costs of hosting refugees from regions with more terror attacks. A future study investigating this would be very interesting. Notwithstanding these limitations, the study suggests that prenatal exposure to terror shocks is harmful to child health whereas postnatal investment is important for reducing this negative effect. Anecdotal evidence shows that Boko Haram conflict has caused food shortages in the northeast of Nigeria (Human Rights Watch, 2016). Consequently, a natural progression of our work is to analyze the effect of terror on other indicators of child health such as height-for-age z-scores and examine whether food security and lack of access to safety nets are possible channels of malnutrition.

In summary, we found that Boko Haram attacks caused health damage well beyond the destruction of lives and property. *in utero* exposure to terror attacks lowers the birth weight of affected cohorts. This effect was identified by geographical variation in terror fatalities across Nigeria. Furthermore, the estimated effect was reduced by within-family comparisons, suggesting that postnatal investments may be a compensating factor.

Chapter 3

Violent Conflict and Household Risk Coping Strategies: Evidence from Nigeria

3.1 Introduction

Violent conflicts affect a large number of households throughout the world.[1] In recent years, several papers have studied the physical, human, as well as the social legacies of violent conflict.[2] Surprisingly, little attention has been given to how households share risk under conflict environments. The lack of research on this theme is striking, given that this question is relevant in understanding the long-term impact of violent conflict on household welfare.

In developing economies, households face considerable idiosyncratic and covariate shocks which result in severe income fluctuations.[3] To survive in this high risk environment,

[1]More than 1.5 billion people live in fragile and conflict-affected states (see World Bank (2011)).

[2]For physical legacies see Davis and Weinstein (2002); Brakman *et al.* (2004); Miguel and Roland (2011). On human legacies, see Bundervoet *et al.* (2009); Minoiu and Shemyakina (2012); Annan and Blattman (2010); Akresh *et al.* (2012). For social legacies, see the recent review of the literature by Bauer *et al.* (2016) as well as Bellows and Miguel (2006); Blattman (2009); Rohner *et al.* (2013a); Cassar *et al.* (2013).

[3]Idiosyncratic shocks are shocks experienced at the household level while covariate shocks are shocks experienced at the community level.

households use several coping strategies ranging from self insurance (such as savings) to informal insurance mechanisms (e.g. transfers and remittances). Yet, little is known about the impact of conflict on risk sharing between households. Economists have documented two main findings with respect to household risk sharing. Households smooth consumption either through self insurance mechanisms such as grain stock or livestock sales (Kazianga and Udry, 2006; Fafchamps *et al.*, 1998) or through informal insurance mechanisms such as relying on informal support networks or receiving remittances (Udry, 1994; Rosenzweig, 1988).

In this paper, we examine the ability of households to smooth consumption when exposed to violence. We focus on violent shocks induced by a sharp increase in local violent events and estimate the influence of this shock on household risk sharing in Nigeria. To analyze this, we use a panel dataset covering over 4000 households in 400 districts from 2010 to 2016. Our empirical model includes an interaction between conflict victimization and suffering negative income shocks. Specifically, we use a difference-in-differences specification, while controlling for household fixed effect, to capture the effect of income shock on changes in consumption between victimized and non-victimized households. Our results show that per capita food consumption of victimized households fall when they experience income shocks. This effect is substantial when compared to non-victimized households. In particular, victimized households witness a 23 percent reduction in consumption after a negative shock. The effects are more pronounced for poorer households that live in rural areas. Our estimates are robust across different victimization definitions and using alternative datasets. Next, we examine whether and how victimized households share risk. We find that households exposed to violence rely on informal rather than self insurance mechanisms to smooth consumption fluctuation. In the aftermath of adverse shocks, households with violence exposure are on average 2 times more likely to receive remittances. This amounts to a 45 percent increase in the remittances received per annum. Households are 12 percentage point more liable to save money with informal support groups but are 15 percent less apt to borrow money from relatives, friends or money lenders.

Several papers have studied the relationship between household conflict exposure and consumption smoothing (Brück, 2004; Verpoorten, 2009; Bozzoli and Brück, 2009; Nillesen and Verwimp, 2010). This study makes several contributions to the current literature. First, while prior studies rely on post-conflict surveys, we use a national representative sample of household data collected prior, during and after a period of intense conflict to estimate the effect of violence on household consumption. Second, the longitudinal nature of the survey ensures that we observe changes in household consumption and violence exposure over time. In addition, the fact that attrition rates are low in the panel sample enhances our confidence that the survey is not affected by non-random selection of conflict survivors. Third, we use conflict-related survey questions at the household and community level to examine how households share risk in the face of idiosyncratic and covariate violent shocks. Furthermore, we show that proximity to violent events plays a key role in household consumption fluctuations.

The findings of this study contribute to a large literature on risk sharing between households (Rosenzweig and Stark, 1989; Townsend, 1994; Udry, 1995; Dercon and Krishnan, 2000; Fafchamps and Lund, 2003; Fafchamps and Gubert, 2007). We provide new evidence for the impact of conflict on household ex-post mechanisms that permit consumption smoothing in the face of adverse shocks. Similarly, by documenting changes in household consumption during an intense violent period we also contribute to research on barriers to household risk sharing (Rosenzweig and Binswanger, 1993; Cole *et al.*, 2013). Given that violence exposure affects household welfare and saving decisions, the results of this paper can be related to the literature on consumption smoothing and social insurance in developing countries (Jacoby and Skoufias, 1997; Chetty and Looney, 2006; Park, 2006; Akresh, 2009).

The paper is organized as follows. In Section 3.2 we review the literature on conflict and household risk coping mechanisms. Section 3.3 covers the institutional setting. Section 3.4 presents the data and descriptive statistics. In section 3.5 we explain the empirical model, display the baseline results and perform robustness checks. We discuss our findings in section 3.6. Section 3.7 concludes.

3.2 Literature review

Our paper contributes to a large and growing body of literature that considers how households insure themselves against consumption fluctuation (see Dercon (2002), Fafchamps (2010) for a survey on this topic). Fafchamps and Lund (2003) and Jack and Suri (2014) document the importance of remittances between households as a form of risk pooling. Udry (1994) and DeWeerdt and Dercon (2006) show that informal loans and insurance help households that suffer negative shocks. Udry (1995) and Park (2006) observe that households use grain sales as a buffer during adverse circumstances. In contrast, Fafchamps *et al.* (1998) and Kazianga and Udry (2006) find that households do not sell livestock after experiencing drought.

Although these findings provide extensive evidence on household risk coping strategies, few studies have investigated the additional impact of violent conflicts on risk sharing between house- holds. Prior literature on this subject shows that conflict environment influences household ability to smooth consumption after income shock. For instance, civil conflict induces agricultural households to change their saving portfolio by reducing buffer stocks such as livestock. A plausible reason is that livestock becomes risky during conflict periods. Not only can it be lost or stolen, it also acts as a sign of wealth which can expose the household to violent attacks. Besides, it can be used as a tradeoff for food exchange. Verpoorten (2009) shows that war-exposed households use cattle sales as a cushion against food price increase during the 1994 Rwanda genocide.

Households in conflict areas may also switch to subsistence farming. There is some evidence that households adopt this coping strategy to minimize income risk relating to food price volatility and conflict-induced market failure. Studies by Brück (2004), Bozzoli and Brück (2009) show that though households in Mozambique use this approach as a wartime survival technique, it reduces their income and has no effect on consumption. Nevertheless, not all farmers engage in subsistence farming as a result of conflict (Nillesen and Verwimp, 2010).

Our results contribute to the literature on the legacy of conflict. Bellows and Miguel (2006, 2009) examine the consequences of household victimization during the 1991-2002 Sierra Leone civil war on local collection action and find that conflict victimization increase the probability of attending community meetings and being a member of a social group by 7 percentage point respectively. Voors *et al.* (2012) show that households exposed to greater levels of violence cultivate more cash crops and invest less in farm improvements. Similarly, Gilligan *et al.* (2014) find that individuals in violence-affected communities invest more in trust-based transactions.

We also add to the literature on armed conflict and child health. Studies by Bundervoet *et al.* (2009) and Akresh *et al.* (2011) suggest that theft and burning of crops and household displacement as a result of violence negatively affect child height-for-age z-scores through a reduction in child nutrition. However, war-induced looting of household assets such as livestock has no effect on child health. Minoiu and Shemyakina (2012, 2014) find that children in households that experience conflict-related victimization have lower height-for-age z-scores. Using the Nigerian civil war as a case study, Akresh *et al.* (2012) document the long term effect of conflict exposure on adult health outcomes. They find that war-exposed girls exhibit a reduction in height of 0.75 centimeters relative to unexposed girls of the same cohort. The present study provides indirect evidence with respect to the impact of violent conflict on child health. We show that household food consumption decrease by a considerable margin if they experience conflict and income shock. This reduction in consumption translates to nutrition deficiency for household members especially dependents.

3.3 Institutional Setting

The primary focus is Nigeria, Africa's most populous and one of the most densely populated, with over 152 people per square kilometer (km).[4] Administratively, the country is divided

[4] Author's calculation using 2006 census data.

into 37 states and further partitioned into 774 districts (also known as local government areas). A major exporter of oil, Nigeria have experienced intermittent episodes of political instability following its independence in 1960 including a brutal civil war in 1967, a series of coup d'état during the 1990s in addition to sporadic ethnic clashes. The key period under study is the upsurge in violent events between 2010 and 2016. Figure 3.1 presents violent conflict events and fatalities between 1995-2016.[5] The yearly trend in conflict episodes and casualties show a disproportionate increase in violence intensity within the last 5 years with a death toll in excess of 10,000 people (see Raleigh *et al.* (2010)). The rise in violence in 2010 coincides with the development of three specific armed groups within the country: Boko Haram in the north, Fulani Militants in the middle belt and Niger Delta Militants in the south.[6]

Boko Haram was originally formed to address the incidents of bad governance in northern Nigeria.[7] The group became violent after an initial political protest in July 2009 (which turned into a riot) resulted in a police crackdown.[8] The frequency of the terror attacks in the ensuing three years prompted the government declaration of a state of emergency in several states in the north by May 2013. Throughout the conflict, more than 8000 civilians have died and over 2 million displaced as a result of Boko Haram (see Human Rights Watch (2016)).

The farmer-herder conflict is another event that has led to considerable loss of life and property damage. The conflict primarily involves two actors: the Fulani herdsmen and indigenous farmers. Partially due to drought and desertification of the north, the Fulani seasonally herd their cattle to the south for grazing and market sale. The lack of grazing

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[5]We define violent events as events with at least one fatality.

[6]See The Economist on "Insecurity in Nigeria: Fighting on all fronts" : http://www.economist.com/news/middle-east-and-africa/21695882-bad-governance-has-bred-uprisings-boko-haram-biafra-fighting-all-fronts and "What is Nigeria's third conflict" : http://www.economist.com/blogs/economist-explains/2016/09/economist-explains-0.

[7]An offshoot of a group known as the "Nigeria Taliban" which initially sought to create a separatist state. See Walker (2012) for more details.

[8]It is estimated that 700 Boko Haram members were killed in addition to their leader Mohammed Yusuf. See Raleigh *et al.* (2010).

routes signifies that the herdsmen sometimes transverse through the arable land of local farmers which give occasion to violence. The intensity of this event increased between 2010 and 2016. For instance, in 2014, a period of heightened violence activity, 17 percent of reported events were related to Fulani militants[9] making them the fourth most deadly terrorist group in the world after Boko Haram, ISIL (the Islamic State of Iraq and the Levant) and the Taliban (see Global Terrorism Index (2015)).

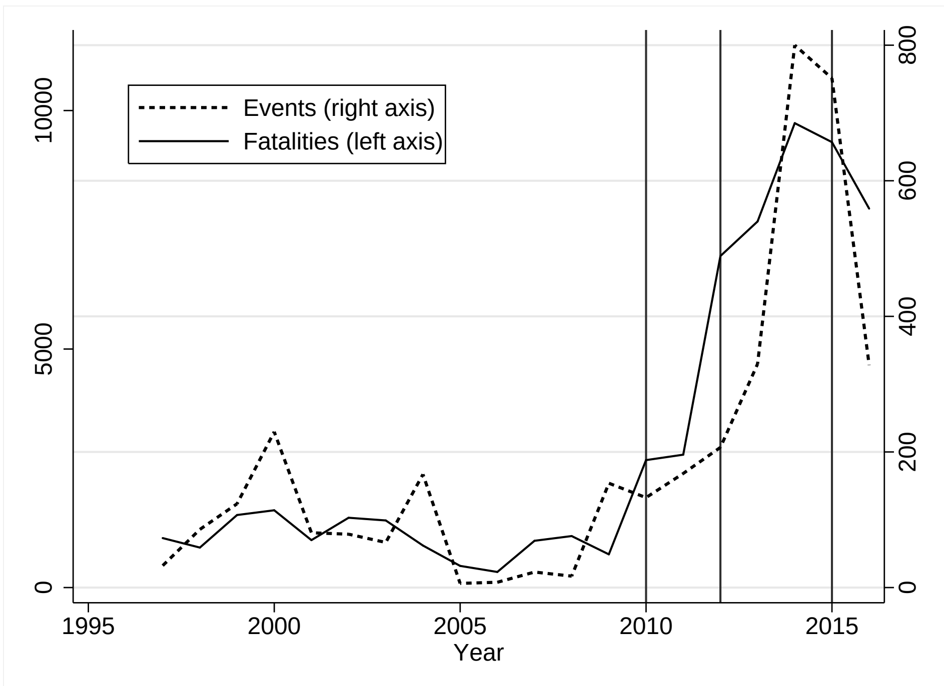

Figure 3.1: *Violent Conflict Trend in Nigeria (1995 - 2016)*
Notes: The solid vertical lines show when the household survey waves were conducted.

Resource-related conflicts in the Niger Delta affect Nigeria's public policy as a result of government dependence on oil revenue. The frequency of this event makes it one of the most common forms of violence in Nigeria. The Niger Delta militants demand more local control and management of oil revenue as well as community compensation for pollution from resource exploration. After a period of high conflict activity in the early 2000s, the militants were granted amnesty in 2009 which led to a decline in violence. However, the government reversal of amnesty budget in 2016 and recent reports of increased attacks on

[9]This is out of 685 violent events reported in Armed Conflict Location and Event Dataset (ACLED).

pipelines and oil platforms have raised concern of a return to more turbulent periods.[10]

As shown in Figure 3.2, the events related to these three groups rose substantially between 2009 and 2016. The number of incidents associated with these groups, as a percentage of the total number of conflict events, rose from 22 percent in 2009 to over 64 percent in 2014 before a slight decline to 58 percent in 2016. Figure 3.3 shows the frequency of deaths as a result of the events from these groups. We practically find a similar but intense pattern in the proportion of casualties. The percentage of deaths from these groups to the total number of conflict deaths increased from 43 percent in 2009 to over 80 percent between 2014 and 2016. To capture the intensity of violence by each conflict actor, Figures 3.4 and 3.5 depict the breakdown the events and fatalities for each group. Events (in Figure 4) related to Boko Haram and Fulani Militants increased steadily after 2009 while Niger Delta militants reduced drastically in later years after a peak of over 80 percent in 2008. The proportion of deaths by each group shows the same trend.

[10]See http://www.economist.com/blogs/economist-explains/2016/07/economist-explains.

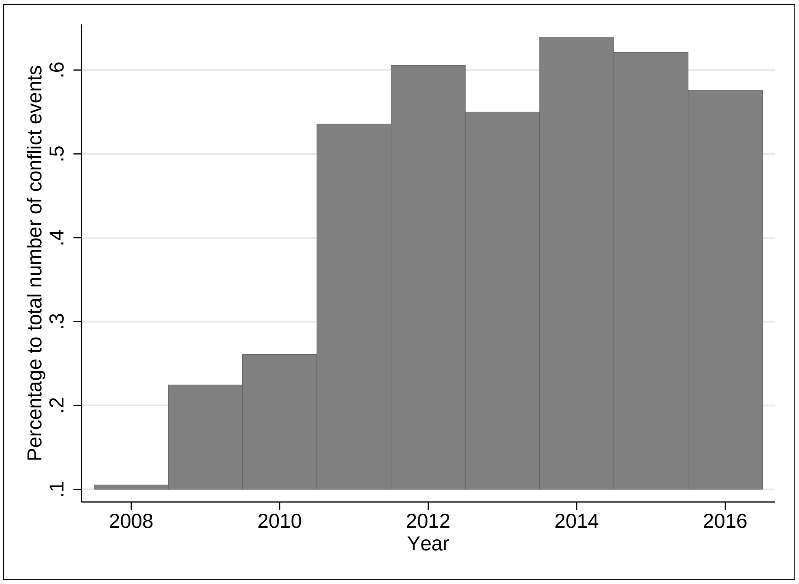

Figure 3.2: *Frequency of events related to Boko Haram, Fulani and Niger Delta Militants (2008-2016)*

Notes: Figures are based on the number of events by conflict actors reported in ACLED. The figures capture the annual percentage change of events related to Boko Haram, Fulani and Niger Delta Militants.

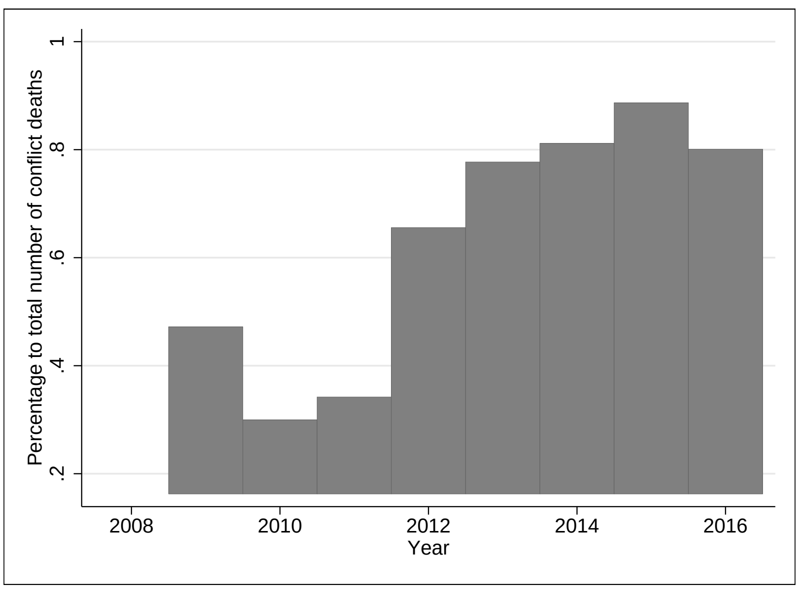

Figure 3.3: *Frequency of deaths related to Boko Haram, Fulani and Niger Delta Militants (2008-2016)*

Notes: Figures are based on the number of deaths as a result of conflict actors reported in ACLED. The figures capture the annual percentage change of deaths related to Boko Haram, Fulani and Niger Delta Militants.

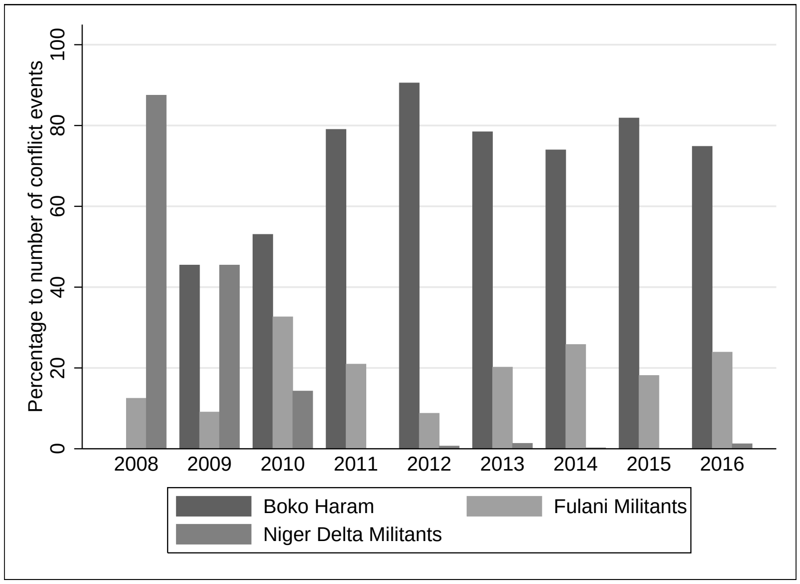

Figure 3.4: *Proportion of events related to Boko Haram, Fulani and Niger Delta Militants (2008-2016)*

Notes: Figures are based on the proportion of annual number of events by the most violent conflict actors as reported in ACLED.

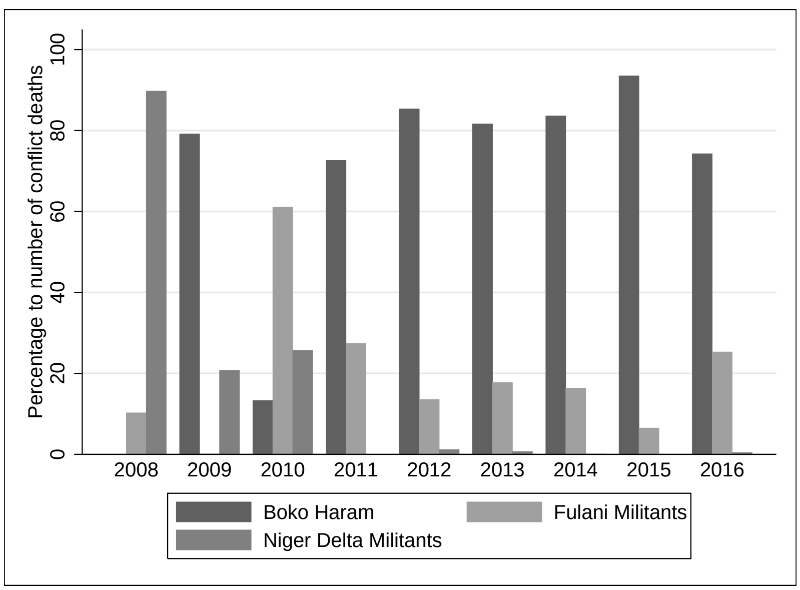

Figure 3.5: *Proportion of deaths related to Boko Haram, Fulani and Niger Delta Militants (2008-2016)*

Notes: Figures are based on the proportion of annual number of deaths by most violent conflict actors as reported in ACLED.

3.4 Data and Descriptive Statistics

3.4.1 Data

The data comes from the living standard measurement survey (LSMS) of Nigeria carried out by the National Bureau of Statistics (NBS) in partnership with the Federal Ministry of Agriculture and Rural Development, the National Food Reserve Agency, the Bill and Melinda Gates Foundation and the World Bank.[11] The LSMS contains three waves of survey conducted in 2010, 2013 and 2016. These surveys are based on a randomly selected panel sample of 5000 households and focus on a wide array of issues relating to agriculture production, income activity, consumption, and expenditure.

The survey data are in three formats. The first relates to household data. This contains detailed demographic information of household members such as age, gender, education level, health status, food and non-food expenditure and geo-referenced data on household location (shown in Figure 3.6). The second concerns household agricultural activity, for example, land ownership, farm labor, crop harvest and animal holdings. The third contains the socio-economic indicators of the community where the household reside, for instance, community organizations, changes in the community and key events.

Given that our objective is to examine the impact of conflict and income shocks on household consumption, we restrict our analysis to the survey questions related to these shocks. Households were asked a number of questions about conflict events experienced by a household member such as whether any family member was killed (not natural death), any member suffered physical aggression (with or without any type of weapon), any member injured/disabled (after direct attack), any member suffered sexual violence, any member forced to work (for free), any member captured/kidnapped/abducted, any member robbed (money or assets), any member made a refugee/internally displaced, family dwelling suffered from robbery, family dwelling burned down/destroyed/seriously damaged/occupied, family land was occupied/expropriated/made unproductive and

[11] The LSMS questionnaires and data are available at NBS website (http://www.nigerianstat.gov.ng/) or at the LSMS-ISA website (http://www.worldbank.org/lsms-isa).

family assets intentionally destroyed/seriously damaged. In addition, for each shock, households were asked questions about each event, such as the year the shock occurred, how many times the event has occurred since 2010 and who was the perpetrator of the event. We construct conflict shocks for each household separately for each year starting from 2010 to 2016. A conflict shock is a dummy variable equal to 1 if household reported experiencing any of the above questions relating to violence. Figure 3.7 shows the evolution of conflict across the country within our study period.

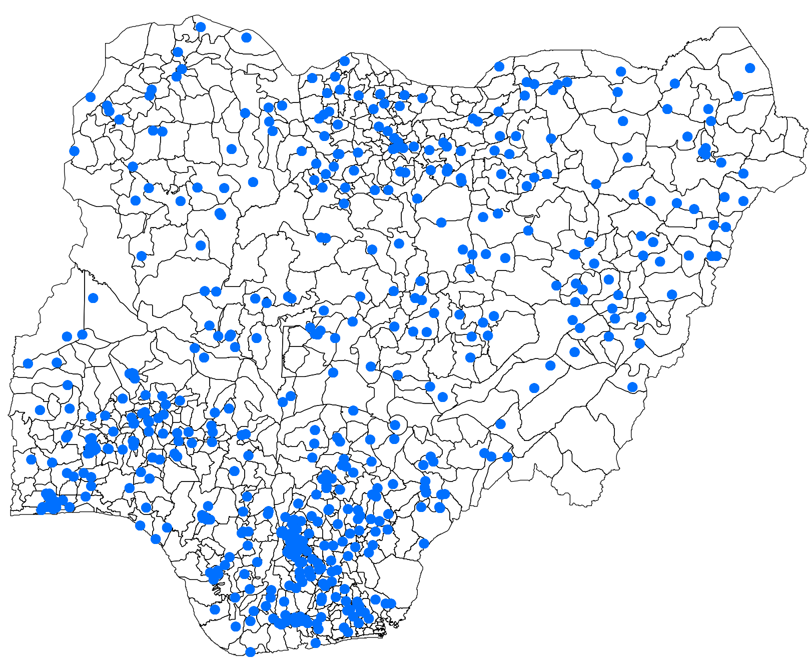

Figure 3.6: *Location of sampled household across Nigeria*

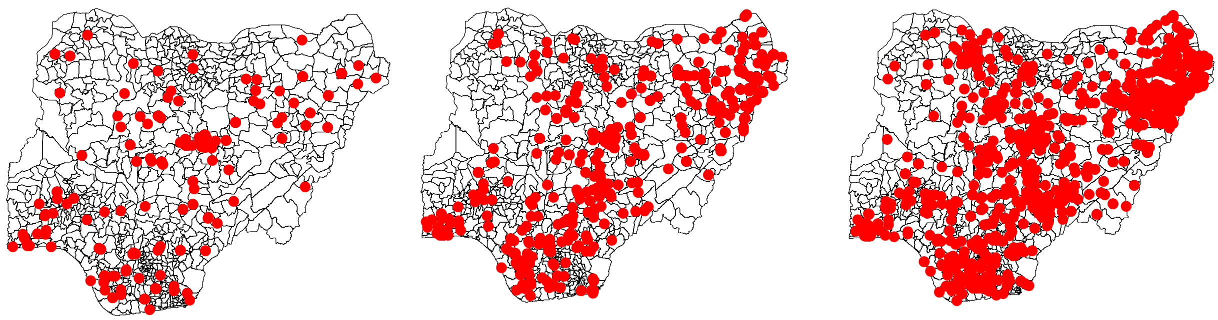

Figure 3.7: *Evolution of conflict across Nigeria (2010-2016)*
Notes: The figures show the progression in conflict events as reported in ACLED. The left panel are events from 2010 to 2011. The middle panel are events from 2012 to 2013 while the right panel are events from 2014 to 2016.

Income shock questions were asked to the household head. The questions relate to shocks suffered by the household such as death or disability of an adult working member of the household, death of someone who sends remittances to the household, illness of income earning member of the household, loss of an important contact, job loss, departure of income earning member of the household due to separation or divorce, departure of income earning member of the household due to marriage, nonfarm business failure, theft of crops, cash, livestock or other property, destruction of harvest by fire, poor rains that caused harvest failure, flooding that caused harvest failure, pest invasion that caused harvest failure or storage loss, loss of property due to fire or flood, loss of land, death of livestock due to illness, increase in price of inputs, fall in the price of output and increase in price of major food items consumed. Households were also asked how many times the event has occurred and in what year the event took place. We define a yearly income shock indicator as a dummy variable for the household affected by any negative shock between 2010 and

2016.

A notable feature of this dataset is the low rate of attrition. For every wave, the survey was administered in two different visits to capture household post-planting and post-harvest activities. From the initial baseline sample of 5000 households, 4916 households were reinterviewed in 2011 (first wave), 4716 households in 2013 (second wave) and 4581 households in 2016 (third wave).[12] In this paper, we use a three-period balanced panel of 4455 households from wave 1 to wave 3. We use wave dummies to control for the difference in the time of survey between each wave. Overall, the attrition rate in our sample (at 9 percent) is significantly less than rates found in the literature on developing countries.[13]

To construct food consumption at the household level, we use two questions relating to meals taken away from home; expenditure on purchased foods or food consumed from household production. With respect to meals taken away from home, households were provided with a list of meals prepared and consumed outside the home and the most knowledgeable person in the household was asked the following: "How much did you or other household members pay, in total in the last 7 days for each meal?".[14] Households were also given a list of food items and the person responsible for food purchases was asked the following: "How much did the household purchase of this item during the past 7 days?".[15] We compute the annual value of household expenditure from these questions and aggregate the values to obtain the total food consumption expenditure. We adjust for the difference in household cost of living by deflating household consumption with a Paasche price index

[12]In other words, of the 5000 households in the post-planting sample in 2010, 98 percent of the households were reinterviewed in the post-harvest survey. Hence, the number of households with complete records in both visits is 4916 in the first wave, 4716 in the second wave and 4581 in the third wave.

[13]Blattman (2009) was able to track 84 percent of an initial household survey in Uganda. Baird *et al.* (2008) report similar magnitude for a youth survey in Kenya. Jack and Suri (2014) also reported an attrition rate of 24 percent in their study of M-PESA in Kenya.

[14]The list of meals outside the home includes local side dishes such as pepper soup, nkwobi, suya; snacks such as sandwiches, biscuits, meat pies, doughnuts; dairy based beverages such as milk, yoghurt, and vegetables such as carrot, pears, sugar cane, plantain.

[15]The inventory of purchased food items include, but is not limited to, grains and flours, starchy roots, tubers and plantain, pulses, nuts and seeds, oil and fats, fruits, vegetables and meat, fish and seafood, milk and milk products, coffee, tea, cocoa and similar beverages, sugar, sweets and confectionary.

following the recommendation of Deaton and Zaidi (2002).

Summary statistics of the sample are presented in Table 3.1. The first panel captures household characteristics. The mean age of the household head between the three waves varied from 49 and 53 years. The average number of years of education of household head is 3 years. The percentage of female head of households increases from 15 percent in the first wave to 21 percent in the third wave. 9 percent of household reported having access to a cooperative institution. The annual household food consumption (in log terms) remains practically unchanged in the surveys. The share of households that reported suffering victimization increased from 1 percent in period 1 to 4 percent in period 3. Households experience of income shock rose by 11 percentage point between period 1 and period 2 but declined by 10 percentage point between period 2 and period 3. The number of community level victimization increased from 36 percent to 42 percent between the three periods.

The second panel provides details on household risk sharing strategies. The number of households that receive remittances rose by 1 percentage point between period 1 and period 3. In contrast, households that save money with informal groups (such as adashi, esusu or ajo) increased from 24 percent in 2010 to 26 percent in 2016. The table also shows an increase of 6 percentage points in the number of households that borrow from informal groups. Household reliance on friends and relatives is also high. 40 percent of households in period 3 reported receiving such assistance. Our measure of grain stored and livestock sales come from household level questions with respect to the amount of harvested crops and livestock sold within each period. We compute the amount of grain stored by taking the monetary value of crops harvested but stored as seeds for the next planting season while the amount of livestock sold is the annual sales value of livestock sold. The last panel shows household head occupation dummies which we define based on industry employed. The majority of household surveyed are farmers.

Table 3.1: *Descriptive Statistics*

	Wave 1		Wave 2		Wave 3	
	Mean	SD	Mean	SD	Mean	SD
Household characteristics:						
Household head age	49.816	15.355	53.111	16.267	53.013	14.558
Education of household head (years)	3.240	3.410	3.091	3.408	2.784	3.227
Household head is female (percent)	0.148	0.354	0.154	0.361	0.212	0.409
Household size	5.622	3.026	5.628	3.183	6.996	3.458
Household dependants	2.671	2.129	2.505	2.122	4.059	2.592
Bank account (percent)	0.306	0.461	0.304	0.460	0.329	0.470
Access to cooperative institution (percent)	0.085	0.279	0.075	0.264	0.089	0.284
Log of annual food consumption per capita (naira)	9.568	1.038	9.420	1.014	9.533	0.987
Household conflict shock (percent)	0.013	0.111	0.014	0.116	0.039	0.194
Negative income shock (percent)	0.292	0.455	0.403	0.491	0.302	0.459
Community conflict shock (percent)	0.360	0.480	0.314	0.464	0.424	0.494
Household risk coping strategies:						
Receive remittances (percent)	0.023	0.150	0.023	0.149	0.034	0.182
Log of remittance amount received (naira)	0.245	1.614	0.231	1.529	0.354	1.907
Use of informal saving groups (percent)	0.240	0.427	0.247	0.431	0.258	0.437
Borrowed money from informal groups (percent)	0.106	0.308	0.111	0.314	0.169	0.375
Borrowed money from friends, relatives or money lenders (percent)	0.211	0.408	0.207	0.405	0.398	0.490
Log of grain stored (naira)	3.956	5.107	7.412	5.291	3.013	4.669
Log of livestock sold (naira)	3.368	4.498	1.920	3.839	1.840	3.817
Occupation of household head (dummies):						
Farmer	0.396	0.489	0.378	0.485	0.006	0.078
Mining	0.004	0.061	0.002	0.049	0.002	0.039
Manufacturing	0.025	0.158	0.032	0.175	0.006	0.077
Professional	0.023	0.149	0.019	0.138	0.003	0.058
In Industry	0.096	0.295	0.098	0.297	0.026	0.160
Sales	0.049	0.215	0.050	0.219	0.011	0.102
Personnel services	0.075	0.263	0.077	0.266	0.010	0.100
Education	0.044	0.205	0.036	0.187	0.041	0.197
Health	0.012	0.110	0.014	0.115	0.010	0.099
Public	0.046	0.210	0.045	0.207	0.042	0.200
Other occupation	0.012	0.110	0.009	0.093	0.005	0.071
Unemployed	0.131	0.337	0.149	0.357	0.735	0.442

Notes: The number of observation in each wave is 4455 households. Household dependants are the number of people in the household with age less than 15 and over 64. Naira refers to the local currency, Nigerian Naira.

3.5 The Impact of Violent Conflict on Household Risk Coping Strategies

3.5.1 Empirical Specification

Our unit of observation is the household. The decision to adopt risk sharing strategies at this level is made by the household head. To identify the impact of violent conflict on household risk sharing, we use a difference-in-difference method that exploits variation in household exposure to violent events over a period of high conflict intensity.

Formally, our baseline econometric specification corresponds to:

$$\begin{aligned} C_{ijt} = {} & \alpha + \gamma Victim_{ijt} + \lambda Shock_{ijt} + \beta Victim_{ijt} \times Shock_{ijt} + \theta X_{ijt} \\ & + \delta_i + \mu_j + \pi_t + \epsilon_{ijt}, \end{aligned} \tag{3.1}$$

where C_{ijt} is the annual food consumption of household i in district j in period t. $Victim_{ijt}$ is a dummy variable equal to 1 if the household reports experiencing conflict shock in within the year and $Shock_{ijt}$ is a dummy which is equal to 1 if household reports experiencing income shock during the year. X_{ijt} is a set of household control variables such as age of household head, years of education of household head, a dummy value of 1 if the household head is female, 12 household head occupation dummies, household size and number of household dependents, dummy variables that capture household use of financial instruments such as bank account and access to cooperative institutions and a dummy value of 1 if household lives in a rural area. We control for household fixed effect δ_i, district fixed μ_j and period fixed effect π_t. ϵ_{ijt} is the error term.

We investigate the mechanisms through which households share risk using the following model:

$$\begin{aligned} R_{ijt} = {} & \alpha + \gamma Victim_{ijt} + \lambda Shock_{ijt} + \beta Victim_{ijt} \times Shock_{ijt} + \theta X_{ijt} \\ & + \delta_i + \mu_j + \pi_t + \epsilon_{ijt}, \end{aligned} \tag{3.2}$$

where R_{ijt} is the risk sharing mechanisms adopted by households such as the likelihood of receiving remittances and the total amount of remittances received, the probability of saving or borrowing money from informal groups, borrowing money from friends and relatives, storing grains or selling livestock. The explanatory and control variables are similar to equation 3.1.

Implicit in equations 3.1 and 3.2 is the control group which consists of households that did not experience victimization. In other words, the empirical strategy relies on the comparison between victimized and non-victimized households. Consequently, our parameter of interest β can be interpreted as the effect of violent conflict on household risk sharing conditional on household being victimized and experiencing a negative income shock. In other words, we test whether victimized households are able to smooth consumption.

Our identifying assumption is that exposure to violent conflict is not driven by household features. A threat to our identification strategy would exist if households were systematically targeted during the upsurge in conflict events. In addition, endogeneity of income shocks with observable household characteristics is plausible.[16] In Table 3.2, we show that exposure to conflict victimization is unrelated to household characteristics. In contrast, income shock is correlated with household consumption changes, education and certain occupation characteristics of household head. This is not surprising given that our data capture self-reported idiosyncratic shocks.[17] We mitigate this concern using two strategies. First, we extend equation 3.1 to include interactions of income shock with household characteristics. Jack and Suri (2014) show that income shocks may be correlated with observable covariates which help households to smooth risk. They recommend the interaction of shocks with household characteristics to control for the impact of these features on the ability of the household to smooth income shocks. In our context, we control for this possibility using the following econometric specification:

[16] Poor and rural households are more vulnerable to income shocks.

[17] Due to data limitation, we do not observe household income shocks prior to conflict intensity nor do we identify episodes of unexpected income change at the household level.

$$C_{ijt} = \alpha + \gamma Victim_{ijt} + \lambda Shock_{ijt} + \beta Victim_{ijt} \times Shock_{ijt}$$
$$+ \theta^S X_{ijt} \times Shock_{ijt} + \theta^H X_{ijt} + \delta_i + \mu_j + \pi_t + \epsilon_{ijt}, \quad (3.3)$$

where X_{ijt} is the same vector of household controls as previously described. Our second strategy uses exposure to negative rainfall shocks, which we examine under robustness in section 3.5.4. In our preliminary specifications, conflict victimization does not appear to a significant determinant of household consumption. We consequently omit this variable in our reported specifications and only present our parameter of interest which is the interaction between household victimization and income shock.[18]

3.5.2 Baseline Results

Table 3.3 presents the baseline results. We report our coefficient of interest, β which captures the interaction of experiencing conflict victimization and a negative income shock. To test the robustness of our findings we gradually add control variables in successive columns. For instance, column (1) reports OLS estimates of equation 3.1 with time fixed effects. We use a panel specification in columns (2) to (5). We also add controls for the district and household fixed effects in column (2) and community fixed effects in column (3). In column (4), we introduce household characteristics while column (5) contains controls plus interactions as specified in equation 3.3. The results show that shocks reduce food consumption of victimized households by 34 percent and it is statistically significant at 1 percent level (column 1). The magnitude of the estimated effect is substantial: for instance, the breakdown of the point estimate (bottom panel; linear combination) shows that victimized households experience a 38 percent reduction in food consumption while non-victimized household only experience a 4 percent decline. The size of the coefficient suggests that conflict victimization leads to at least 9 times more reduction in household food consumption compared to no experience of victimization. The results in column (2) are

[18]The results are available on request.

Table 3.2: *Correlation between Shock Measures and Household Characteristics*

Dependent variable	Conflict shock	Income shock
	(1)	(2)
Age of household head	0.0000	0.0006
	(0.0001)	(0.0004)
Years of education of household head	-0.0001	-0.0068***
	(0.0007)	(0.0019)
Household head is female	0.0066	0.0079
	(0.0058)	(0.0185)
Household size	0.0012	0.0025
	(0.0013)	(0.0033)
Household dependants	-0.0000	0.0038
	(0.0015)	(0.0042)
Household food consumption	0.0026	0.0146*
	(0.0018)	(0.0081)
Rural household	0.0690	0.1456
	(0.0442)	(0.1394)
Bank account	-0.0012	-0.0232
	(0.0036)	(0.0152)
Access to cooperative institution	0.0016	-0.0105
	(0.0043)	(0.0172)
Occupation: Farmer	-0.0005	-0.0611***
	(0.0062)	(0.0223)
Occupation: Mining	-0.0275	-0.1953***
	(0.0423)	(0.0660)
Occupation: Manufacturing	-0.0083	-0.0718*
	(0.0073)	(0.0418)
Occupation: Professional	0.0079	-0.1376***
	(0.0093)	(0.0457)
Occupation: In Industry	0.0179	-0.0832**
	(0.0113)	(0.0350)
Occupation: Sales	-0.0154	0.0150
	(0.0123)	(0.0415)
Occupation: Personnel services	0.0006	-0.0452
	(0.0067)	(0.0302)
Occupation: Education	0.0036	-0.0893***
	(0.0096)	(0.0299)
Occupation: Health	0.0115	-0.1323***
	(0.0191)	(0.0444)
Occupation: Public	0.0102	-0.0753**
	(0.0112)	(0.0293)
Occupation: Other	0.0157	0.0079
	(0.0217)	(0.0550)
Unemployed	0.0043	-0.0620***
	(0.0075)	(0.0200)

Notes: Heteroskedasticity-robust standard errors in parentheses. Each column represent panel regressions with household, district, and time fixed effects. The dependent variable in column (1) is a dummy variable equal to one if household experienced victimization between 2010 and 2016 while column (2) is a dummy variable equal to one if household reported experiencing income shock between 2010 and 2016. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

similar after including fixed effects at district and household level: victimized households find it more difficult to smooth consumption than non-victimized households.

To control for possible differences in the community of residence and household characteristics, we include community fixed effects in column (3) and demographic controls in column (4). The results remain statistically significant at 10 percent level and show a larger effect for victimized households (bottom row). In column (5), we add the full set of controls in addition to the interaction of negative shocks with observable covariates. The coefficient in the bottom panel is comparable across columns (4) and (5). Non-victimized households are able to smooth consumption after a negative shock while victimized households experience a decrease of 17 percent in food consumption.

Table 3.3: *The effect of victimization on household consumption*

Dependent variable	Food consumption				
	(1)	(2)	(3)	(4)	(5)
Victim×negative shock	-0.3409***	-0.3520**	-0.2165*	-0.2285*	-0.2572**
	[0.1262]	[0.1535]	[0.1275]	[0.1260]	[0.1289]
Controls					
District fixed effects	No	Yes	Yes	Yes	Yes
Household fixed effects	No	Yes	Yes	Yes	Yes
Community fixed effects	No	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	No	No	Yes	Yes
Controls + interactions	No	No	No	No	Yes
Observations	12,972	12,972	12,972	12,972	12,972
R^2	0.005	0.298	0.335	0.451	0.454
Linear combination					
Shock, victimized households	-0.3886	-0.3436	-0.2022	-0.1719	-0.1743
	[0.1247]	[0.1546]	[0.1279]	[0.1276]	[0.1280]
Shock, non-victimized households	-0.0476	0.0085	0.0142	0.0565	0.0511
	[0.0193]	[0.0269]	[0.0271]	[0.0240]	[0.0241]
Shock, non-victimized households^b					0.0829
					[0.0265]
Mean of victimized households	0.0219	0.0219	0.0219	0.0219	0.0219

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households^b (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

3.5.3 Mechanisms

In this section, we consider the extent to which victimized households keep up with food consumption during a period of conflict intensity. Prior studies show that households use different ex-post strategies to smooth consumption and nutrition. These strategies are either in form of engaging in informal mutual support network or through precautionary savings such as grain stock or livestock sales. We use equation 3.2 to estimate the extent to which households use these strategies to smooth consumption.

We begin with informal insurance mechanisms (Table 3.4). In particular, we consider the probability that victimized households receive remittances in the event of an adverse income shock. We find that a negative shock leads to a 4.5 percentage point (column 3) increase in the likelihood of receiving remittances for households that experience conflict victimization, an effect that is statistically significant at the 1 percent level. The results are similar in terms of the mean effect of shock on an average victimized household (bottom panel). We also estimate the impact of shocks on the total remittance received. The overall effect shows a 45 percent increase in the amount of remittance received by households that suffer violence exposure. The magnitude of this effect is meaningful especially if we consider the fact that victimized households experience a 17 percent drop in food consumption as a consequence of income shock.

Next, we analyze the effect of an adverse shock on household reliance on informal support networks. The results in Table 3.5 imply that victimized households who suffer adverse income shocks save money with informal groups and borrow less money from friends or relatives. The mean effect translates to a 14 percent rise in savings with informal groups and a 9 percentage point decline in the probability of borrowing from friends. This result is surprising in the sense that negative shocks lead households to dissave or rely on external support to smooth consumption.[19] Nevertheless, the result is plausible if victimized households rely on informal systems of risk sharing as a form of social capital investment.[20]

[19]Studies by Paxson (1992), Udry (1995) and Alderman (1996) show this to be the case.

[20]As shown in Bauer *et al.* (2016), the dependence on victims of violence on informal systems of risk sharing

The results in columns 5 and 6 show a negative but not significant effect for informal borrowing. In Table 3.6, we study the self insurance mechanisms with respect to the total value of household grain stock and livestock sales. We find no effect on the use of these assets as a buffer stock against adverse income shocks by victimized households.

Table 3.4: *Mechanisms: Remittance*

Dependent variable	Receive Remittances			Total remittance received		
	(1)	(2)	(3)	(4)	(5)	(6)
Victim×negative shock	0.0396**	0.0441***	0.0451***	0.4097**	0.4552**	0.4594**
	[0.0160]	[0.0167]	[0.0169]	[0.1695]	[0.1770]	[0.1789]
Controls						
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	Yes	Yes	No	Yes	Yes
Controls + interactions	No	No	Yes	No	No	Yes
Observations	13,365	13,365	13,365	13,365	13,365	13,365
R^2	0.142	0.151	0.153	0.144	0.154	0.155
Linear combination						
Shock, victimized households	0.0393	0.0435	0.0435	0.4087	0.4503	0.4500
	[0.0158]	[0.0164]	[0.0166]	[0.1676]	[0.1742]	[0.1769]
Shock, non-victimized households	-0.0003	-0.0006	-0.0003	-0.0010	-0.0049	0.0056
	[0.0035]	[0.0036]	[0.0039]	[0.0353]	[0.0369]	[0.0393]
Shock, non-victimized households[b]			-0.0016			-0.0026
			[0.0038]			[0.0385]
Mean of victimized households	0.0218	0.0218	0.0218	0.0218	0.0218	0.0218

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable in columns (1) to (3) is a dummy variable equal to 1 if the household received remittance in the past 1 year. The dependent variable in columns (4) to (6) is the log of total amount of remittance received by the household in the past 1 year. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households[b] (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

arise from its value as means of social insurance or protection against conflict risk.

Table 3.5: *Mechanisms: Informal Mutual Support Network*

Dependent variable	Informal saving		Borrowing from friends		Informal borrowing	
	(1)	(2)	(3)	(4)	(5)	(6)
Victim×negative shock	0.1230*	0.1307**	-0.1535**	-0.1528**	-0.0349	-0.0296
	[0.0630]	[0.0636]	[0.0758]	[0.0738]	[0.0438]	[0.0456]
Controls						
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Controls + interactions	No	Yes	No	Yes	No	Yes
Observations	12,659	12,659	9,445	9,445	9,444	9,444
R^2	0.239	0.240	0.227	0.231	0.190	0.193
Linear combination						
Shock, victimized households	0.1397	0.1410	-0.0858	-0.0848	-0.0227	-0.0229
	[0.0633]	[0.0636]	[0.0748]	[0.0741]	[0.0436]	[0.0445]
Shock, non-victimized households	0.0167	0.0179	0.0677	0.0689	0.0122	0.0127
	[0.0104]	[0.0107]	[0.0132]	[0.0134]	[0.0092]	[0.0094]
Shock, non-victimized households^b		0.0103		0.0656		0.0054
		[0.0120]		[0.0156]		[0.0115]
Mean of victimized households	0.0215	0.0215	0.0170	0.0170	0.0171	0.0171

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable in columns (1) to (2) is a dummy variable equal to 1 if the household saved money with an informal groups in the past six months. The dependent variable in columns (3) to (4) is a dummy variable equal to 1 if the household borrowed money from friends, relatives or money lenders in the past six months. The dependent variable in columns (5) to (6) is a dummy variable equal to 1 if the household borrowed money from informal groups in the past six months. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households^b (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

Table 3.6: *Mechanisms: Self Insurance*

Dependent variable	Grain Stock			Livestock Sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Victim×negative shock	-0.3336	-0.2929	-0.0782	-0.0475	0.0632	0.2475
	[0.9340]	[0.9384]	[0.9204]	[1.0683]	[1.0563]	[1.0333]
Controls						
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	Yes	Yes	No	Yes	Yes
Controls + interactions	No	No	Yes	No	No	Yes
Observations	8,383	8,383	8,383	8,019	8,019	8,019
R^2	0.496	0.499	0.500	0.222	0.226	0.230
Linear combination						
Shock, victimized households	-0.6889	-0.6592	-0.6645	0.5349	0.6413	0.6298
	[0.9158]	[0.9201]	[0.9065]	[1.0548]	[1.0408]	[1.0306]
Shock, non-victimized households	-0.3553	-0.3663	-0.3809	0.5825	0.5781	0.5711
	[0.1479]	[0.1487]	[0.1463]	[0.1309]	[0.1304]	[0.1294]
Shock, non-victimized households^b			-0.5864			0.3837
			[0.2020]			[0.1592]
Mean of victimized households	0.0258	0.0258	0.0258	0.0286	0.0286	0.0286

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable in columns (1) to (3) is a the log of total sales value of grain stored by household in the past 1 year. The dependent variable in columns (4) to (6) is the log of total amount of livestock soled by the household in the past 1 year. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households^b (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

3.5.4 Robustness checks

We gauge our findings through several robustness checks. Table 3.7 considers the effect of conflict victimization on household consumption by subpopulation. The results are broken down according to household wealth index and location status and education of household head. We construct the household wealth index using a principal component of household assets across the three waves of surveys and define households as poor if they fall within the bottom three quintiles of the wealth index. We report results by restricting to sample to whether a household is poor, live in rural area and whether household head is educated. We find heterogeneous effects across our sample definition. Our results suggest that poorer (columns 1 and 2) and rural households (columns 3 and 4) that are victimized are unable to smooth consumption after a negative income shock. The effects are also strong for educated household heads (columns 5 and 6).

We now turn to assess the potential bias due to sample attrition or household migration as a result of violence. Although we showed in section 3.1 that our panel is unlikely to be affected by this bias due to the low rate of attrition across the survey waves, it is possible that our results are driven by regions with higher attrition rates or by the movement of households to different communities. We test the sensitivity of our results to three alternative subsamples. First, we restrict our sample by excluding households in the region with most attrition due to violence.[21] Second, we trim our sample by excluding households in the region with most attrition as a result of other factors apart from violence.[22] Third, we exclude households that changed communities within the survey period. We display the results in three headings (according to our sample restriction) in Table 3.8. The basic pattern of the estimates is consistent with the baseline regression estimates in Tables 3.3. We obtain statistically significant results with similar magnitude. We also perform an additional test by reweighing our sample according to the strategy recommended by Fitzgerald *et al.* (1998)

[21] According to the basic information document of the 2016 survey, 364 households from the north east region left the sample due to the violent conflict. See National Bureau of Statistics (2016) for additional information.

[22] 277 households either refused to be interviewed or were simply not found in subsequent surveys (National Bureau of Statistics, 2016).

and show the results in Appendix Table C1. Our estimates are robust to this control.

Our results thus far indicate that violent conflict affects the household ability to smooth risk. Our baseline specification captures violence exposure but did not differentiate between physical exposure (such as the death of a household member) and non-physical exposure (for example, destruction of the household asset). In Table 3.9, we show that the effects are strong for households exposed to physical violence. We also consider two alternative definitions of victimization. First, we take into consideration the fact that households experience of violence may differ according to the number of reoccurrences. We use a survey question on the number of times the violent event occurred since 2010 to capture the intensity of violence exposure. Second, we analyze the effect of conflict shocks at the community level. We report our results in Appendix Tables C2 and C3. Overall, the results are similar but with smaller magnitude.

Table 3.7: *The effect of victimization on household consumption, by household subsample*

	Poor households		Rural households		Educated household head	
	(1)	(2)	(3)	(4)	(5)	(6)
Victim×negative shock	-0.2704**	-0.2717*	-0.2347*	-0.2479*	-0.2784*	-0.2987*
	[0.1344]	[0.1414]	[0.1303]	[0.1362]	[0.1563]	[0.1653]
Controls						
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Controls + interactions	No	Yes	No	Yes	No	Yes
Observations	7,288	7,288	9,014	9,014	8,359	8,359
R^2	0.497	0.501	0.387	0.391	0.467	0.472
Linear combination						
Shock, victimized households	-0.2587	-0.2677	-0.1561	-0.1479	-0.2266	-0.2271
	[0.1329]	[0.1400]	[0.1338]	[0.1373]	[0.1544]	[0.1608]
Shock, non-victimized households	0.0116	0.0076	0.0786	0.0758	0.0518	0.0410
	[0.0319]	[0.0316]	[0.0276]	[0.0275]	[0.0288]	[0.0288]
Shock, non-victimized households^b		0.0040		0.0975		0.0687
		[0.0339]		[0.0311]		[0.0351]
Mean of victimized households	0.0174	0.0174	0.0270	0.0270	0.0205	0.0205

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households^b (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

Table 3.8: *The effect of victimization on household consumption, by subpopulation*

	Excluding North East		Excluding South West		Non-migrant households	
	(1)	(2)	(3)	(4)	(5)	(6)
Victim×negative shock	-0.3140**	-0.3292**	-0.2215*	-0.2320*	-0.2374*	-0.2652**
	[0.1356]	[0.1371]	[0.1262]	[0.1286]	[0.1268]	[0.1297]
Controls						
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Controls + interactions	No	Yes	No	Yes	No	Yes
Observations	11,215	11,215	10,894	10,894	12,663	12,663
R^2	0.449	0.452	0.455	0.459	0.442	0.446
Linear combination						
Shock, victimized households	-0.2651	-0.2670	-0.1588	-0.1557	-0.1768	-0.1728
	[0.1337]	[0.1339]	[0.1281]	[0.1284]	[0.1282]	[0.1286]
Shock, non-victimized households	0.0489	0.0449	0.0627	0.0596	0.0607	0.0541
	[0.0247]	[0.0249]	[0.0251]	[0.0248]	[0.0238]	[0.0237]
Shock, non-victimized households^b		0.0622		0.0763		0.0897
		[0.0265]		[0.0269]		[0.0259]
Mean of victimized households	0.0159	0.0159	0.0281	0.0281	0.0217	0.0217

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households^b (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

Table 3.9: *The effect of victimization on household consumption, using alternative definition of victimization*

Dependent variable	Food consumption				
	(1)	(2)	(3)	(4)	(5)
Victim×negative shock	-0.3094**	-0.3455**	-0.2648*	-0.3004**	-0.3323**
	[0.1473]	[0.1606]	[0.1522]	[0.1471]	[0.1496]
Controls					
District fixed effects	No	Yes	Yes	Yes	Yes
Household fixed effects	No	Yes	Yes	Yes	Yes
Community fixed effects	No	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	No	No	Yes	Yes
Controls + interactions	No	No	No	No	Yes
Observations	12,972	12,972	12,972	12,972	12,972
R^2	0.005	0.298	0.335	0.451	0.454
Linear combination					
Shock, victimized households	-0.3587	-0.3387	-0.2510	-0.2436	-0.2422
	[0.1460]	[0.1613]	[0.1528]	[0.1491]	[0.1496]
Shock, non-victimized households	-0.0493	0.0068	0.0138	0.0568	0.0511
	[0.0192]	[0.0269]	[0.0269]	[0.0239]	[0.0239]
Shock, non-victimized households[b]					0.0901
					[0.0262]
Mean of victimized households	0.0160	0.0160	0.0160	0.0160	0.0160

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. The interaction "victim×negative shock" capture the probability that a household member is physically victimized and the household also experience a negative income shock. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households[b] (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

3.5.5 Using Data on Local Violent Events and Rainfall

As described in section 3, the increase in conflict intensity was primarily due to the activity of three militant groups in different regions of Nigeria. In this section, we capture household exposure to armed conflict and income shocks using two different datasets. First, we use data from Armed Conflict Location & Event Data Project (ACLED) to construct a measure of possible household exposure to conflict. We combine household coordinates from the LSMS survey with geo-coded violent events from the ACLED data. Given that violent events and casualties increased from 2010, we use household proximity to violent events from this period as an indicator of exposure. We define households as being exposed if an event with at least 25 deaths occurred within 1 km of household location.[23] Second, we use rainfall data from the National Oceanic and Atmospheric Administration Climate Prediction Centre (NOAA CPC) Africa Rainfall Estimation (version 2.0). The rainfall estimates are constructed by accumulating daily rainfall data to produce dekadal(10-day) estimates at about 10km spatial resolution. The rainfall estimates are at the district level. We construct a proxy for household income shock using the average 12-month total rainfall estimates from 2001-2015. Specifically, we follow Burke *et al.* (2014) and define rainfall shocks relative to the historical rainfall distribution of the household district.[24] In other words, a district is defined as experiencing rainfall shock if the rainfall is below the 15% quantile of the local rainfall distribution.

We use the following difference-in-differences specification to estimate the effect of violence on household consumption:

$$
\begin{aligned}
C_{ijt} = \alpha + \gamma Conflict_{ijt} + \lambda Rain_{ijt} + \beta Conflict_{ijt} \times Rain_{ijt} \\
+ \theta X_{ijt} + \delta_i + \mu_j + \pi_t + \epsilon_{ijt},
\end{aligned}
\tag{3.4}
$$

[23]We adopt this definition of armed conflict from the Uppsala Conflict Data Program (UCDP). See https://www.prio.org/Data/Armed-Conflict/.

[24]We fit the history of district rainfall into relative rainfall using a gamma distribution. Our results are robust to different gamma definitions such as 5, 10, 20 or 25% quantiles of rainfall distribution. The results are also robust to using rainfall estimates from 1960-1990.

where Conflict_{ijt} is a measure of household exposure to conflict and Rain_{ijt} is an indicator of rainfall shock at the district level. Other control variables are similar to that of equation 3.1. We do not control for interactions between household characteristics and rainfall shocks in this specification since the rainfall measures are uncorrelated with observables (Appendix Table 4).[25]

Our preliminary analysis on the relationship between conflict victimization and household consumption shows that victimized households that experience adverse income shocks are likely to reduce food consumption. The negative and significant effect, as shown in Table 3.9, supports the anecdotal evidence of probable cause of violence escalation within our study period. Victimized households witness a 64 percent decline in food consumption after a negative rainfall shock. Our results are robust to district, household, time and community fixed effects.

3.6 Discussion of findings

The present findings are consistent with the literature on risk sharing between households. As expected, we find that victimized households are able to smooth consumption partially. A possible explanation for this result may be the high level of poverty. Poor households are less prepared to deal with conflict shocks or may tend to reduce consumption despite holding a significant amount of livestock (Kazianga and Udry, 2006). In addition, conflict shocks also affect household income. Successive shocks may, therefore, make consumption smoothing more difficult.

It is important to bear in mind the possible bias in our study. Our results may be affected by household or community self-selection into violence. For example, affluent households or communities with oil may be more at risk of violence due to their wealth, social contacts or resource rents. Furthermore, a non-random sample attrition or household unobserved characteristics may possibly skew our results. Although we show that this is not the case,

[25] Our results, available on request, remain unchanged if we control for them.

Table 3.10: *The effect of victimization on household consumption, using alternative estimation model*

Dependent variable	Food consumption			
	(1)	(2)	(3)	(4)
Conflict×Rain shock	-0.6462*	-1.7276***	-0.9256***	-0.9125***
	[0.3714]	[0.4851]	[0.1071]	[0.1547]
Controls				
District fixed effects	No	Yes	Yes	Yes
Household fixed effects	No	Yes	Yes	Yes
Community fixed effects	No	No	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Household characteristics	No	No	No	Yes
Observations	12,972	12,972	12,972	12,972
R^2	0.005	0.299	0.335	0.450
Linear combination				
Shock, victimized households	-0.7010	-1.5541	-0.8323	-0.7982
	0.3709	0.4992	0.1704	0.1921
Shock, non-victimized households	-0.0547	0.1735	0.0933	0.1143
	0.0179	0.1200	0.1352	0.1128
Mean of victimized households	0.0028	0.0028	0.0028	0.0028

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

our results need to be interpreted with caution. Risk sharing between households also depend on local norms and kinship networks within ethnic groups.[26] The data used for this study does not capture household ethnic characteristics, hence, we cannot reject this hypothesis. A further study with more focus on the effect of violent conflict on the social network among kin is required.

Our findings cannot be easily extrapolated to other developing countries. A replica of the large scale violence outside of the Nigeria context is remote. Differences in the nature of the violent conflict, ethnic composition and traits may make it difficult to infer the possible effects within a distinct scenario. Nevertheless, our results corroborate the ideas of Dercon (2002), who suggested that households in risky environments select risk coping strategies comprising informal insurance mechanisms while disregarding formal credit or insurance markets.

3.7 Conclusion

This paper adds to the growing literature that examines the legacy of violent conflicts. Prior studies have focused on either showing the effect of civil war on physical, human or social capital. Yet, many households live in conflict environments and make decisions as a result of conflict shocks. A natural step is to understand how effective is household risk sharing when exposed to violence, which this paper investigates using the sharp increase of violence in a developing country as a case study.

We estimate the impact of violent conflict on various household risk coping strategies using a panel data of over 4000 households in 435 randomly selected communities in Nigeria. The results show that households exposed to higher levels of violence are more likely to reduce food consumption, receive more remittances, invest more in informal support groups, and borrow less from friends and relatives. While the nature of our data makes it difficult to assess the effectiveness of these mechanisms, this study offers additional insight into the

[26] The importance of extended family and kinship networks as a risk sharing mechanism has been studied extensively in the economic literature. See Fafchamps (2010) for a detailed review.

long-term consequences of adverse shocks highlighted in the literature regarding household consumption smoothing, saving, credit, and insurance.

The upsurge in violence between 2010 and 2016 represents a huge conflict shock (it was equal to more than twice the average number of violent events in the early 2000s) and the large death toll makes it comparable to a civil war.[27] Exploiting household level information on conflict exposure during this period we find that victimized households prefer informal insurance strategies. This is consistent with studies on informal risk sharing.[28] In contrast, we find no evidence that conflict exposure induces precautionary savings.

Given the brief period of conflict exposure, a possible area of future research would be to investigate whether the impact of violent shocks on household risk sharing is permanent. It is also important to understand the reasons for the household's choice of risk strategy. Is it because of changes in payoffs or beliefs? Is it related to prevailing norms of ethnic groups or simply a change in household preference?

[27] According to the definition of Uppsala Conflict Data Program (UCDP), an armed conflict is similar to a war if it comprises between 25 and 999 battle-related deaths. The number of battle-related death during this period exceeded 2500 while civilians deaths were over 6000 (see Raleigh *et al.* (2010)).

[28] Udry (1994) studies the role of loans in risk sharing in Northern Nigeria and find that loan transactions play a key role in risk sharing.

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Appendix A

Appendix to Chapter 1

A.1 Data source and construction

A.1.1 Oil Districts data

The data on oil districts is constructed using the annual statistics bulletin of the Nigerian National Petroleum Corporation (NNPC) from 1997 to 2014.[1] Each statistical bulletin contains production of crude oil, gas and gas flare figures oil fields under different producing companies. Establishing the location of oil at district level involves two steps. First, to build a dataset of oil output by company and oil field; and second, to allocate oil fields to districts using company reports and district records which indicate the local districts where oil fields are situated.[2] In addition, oil producing state portals also indicate the oil producing districts, for example, the website of Delta State government: http://services.gov.ng/delta. We restrict the oil location to districts that produced oil in 1998. Similar restriction is done for gas production and gas flare districts.

[1]The statistical bulletins are available at http://www.nnpcgroup.com/PublicRelations/OilandGasStatistics/.

[2]For instance the Oil spill data reported by Shell Nigeria indicates the oil field and the local district, for example, http://www.shell.com.ng/environment-society/environment-tpkg/oil-spills/data-2011/february.html.

A.1.2 Oil Spills data

Oil spills data is computed from the National Oil Spill Detection and Response Agency (NOSDRA), the government institution which overseas incidents of oil spills.[3] The data provide daily information on oil spills including incident date, company, estimated quantity spilled, cause, site location, district, spill area habitat and states affected. The sample includes all districts with onshore oil spills from 2006 (the year from which the data became available). We aggregate the data by year and restrict the analysis to reported oil spills due to accidents in districts within the south of Nigeria (oil spills due to sabotage are excluded due to endogeneity concerns).

A.1.3 Intergovernmental transfers and district capital expenditure

The data on intergovernmental transfers is from the Federal Accounts Allocation Committee (FAAC) report. It contains monthly distribution of revenue allocation to state government and local government councils by the federation account allocation committee. We collate the data on total allocation to local government from the publication. At the time of writing the following was the path to downloading the data.[4]

The data on district capital expenditure is from the Central Bank of Nigeria Annual Report and Statement of Accounts covering the years 2001-2012. It contains the summary of local government expenditure such as administrative costs, economic services; social and community services and transfers. We aggregate these expenses to ascertain the total capital expenditure at district level.[5]

[3]The data was available at https://oilspillmonitor.ng/ at the time of writing.

[4]From http:www.oagf.gov.ng follow the link to FAAC reports, select month and year then click download.

[5]At the time of writing the data was available from http://www.cenbank.org/Documents/cbnannualreports.asp.

A.1.4 Nigeria General Household Survey

The Nigeria General Household Survey (NGHS) is a nationally representative survey of over 700 districts. We use the rural sample of Employment, Time use and Characteristics of main occupation of surveys covering the years 2003-2012. The survey covers employment and wage data for each household member including wage amount, time unit (daily, weekly, fortnightly, monthly, quarterly and yearly), number of hours worked and in what industry the work was done.

We restrict the wage and labor hours data to household members aged between 18 and 64 years. The wage data is further restricted to observations that capture amounts paid for work performed (this excludes imputed wages for secondary employment/jobs or self-employment). We compute real wage as the individual earnings divided by the number of hours worked in the last month. We focus only on wages in cash (all in-kind wages are excluded). The number of hours worked reflects the hours worked in the last month. District codes are recorded differently across surveys; in order to keep the district codes as consistent as possible, we manually recoded all district codes across all surveys to ensure that codes align.

We also construct district data on housing and infrastructure from the household roster of the surveys. Outcomes include house rent, rooms at home and measures of housing and infrastructure quality such as households with piped borne-water, electricity, sewage networks, concrete floor and garbage collection.

A.1.5 Public Goods Provision

The public goods provision data is from the Nigeria Millennium Development Goals (NMIS) Information System data.[6] NMIS captures data relating to public and private health, education and water facilities by state and local government area (LGA). The data captures surveys over the years 2011, 2012 and 2014. Given that 2014 survey captures more facilities

[6]The NMIS data is available at http://nmis.mdgs.gov.ng/download

across districts, We restrict the analysis to this survey. Outcomes used include number of primary schools and enrollments, total number of teachers and classrooms, primary health clinics and hospitals, improved and unimproved water points.

Additional data set on educational outcomes is from the Nigeria Annual Abstract of Statistics of 2010 - 2012. The document captures district level education outcomes from 2005-2012. The documents are available at http://www.nigerianstat.gov.ng/library. Click on NBS Annual Abstract of Statistics, then on Annual Abstract of Statistics to download the data.

Data on district roads is constructed from a shapefile on Nigerian roads surveyed in 2012 from the African Development Bank.[7] The dataset gives the road extension; whether its paved or unpaved; current condition (good, fair or poor); and the district from which it begins (start node) to the district in which it ends (end node). We combine this with the shapefile on Nigeria districts to ascertain the districts through which the road passes. The combined extension of paved roads in the dataset is 21709 km. From this extension of paved roads, 9663km are in good condition, 7619km are in fair condition and 4427km are in poor condition. The combined extension of unpaved roads in the dataset is 983km. 518km of the extension of unpaved roads are in fair condition while 465km are in poor condition.

A.1.6 Health Data

Data on health are constructed from the Nigeria Demographic health survey over the years 1999-2013. The height-for-age zscore and weight-for-age zscore of the child are constructed following the recommendations of the World Health Organization (WHO) on the computation of these health outcomes. Additional data used relate to infant mortality, height and body mass index of the mother, mother's age, years of education, rural and female household head indicator.

[7] The shapefile can be assessed at http://www.infrastructureafrica.org/documents/type/arcgis-shape-files/nigeria.

A.1.7 Institutional quality data

Institutional quality data is taken from Nigerian Afrobarometer covering the years 1999-2013. We construct variables on individual perceptions on public goods provision, trust, communal violence and poverty. Individual response to questions in the Afrobarometer are categorial ranging from: not at all, just a little, some what, or a lot. To ensure consistency, We follow the strategy of Nunn and Wantchekon (2011) to convert the categorial responses into a variable with a number assigned to each response. In other words, each measure of public goods, trust, communal violence and poverty takes the value of 0, 1, 2, or 3: 0 corresponds to the response "not at all" ; 1 to "just a little"; 2 to "somewhat"; and 3 to "a lot."

A.1.8 International Commodity Price and Agricultural districts data

International commodity (agricultural) price data is taken from the International Monetary Fund (IMF) Commodity market indices. The data is restricted to three products (cocoa, oil palm and rubber) with the highest proportion of exports within the agricultural sector. Data on the districts that produce the agricultural goods are taken from the Nigeria National Agricultural Export Commodities 2007. The survey captures the spread of the cultivation of export crops within Nigeria. the paper uses the data to ascertain the district that produce the top three agricultural export crops within the country.

A.1.9 Rainfall and Population Data

Rainfall data is taken from the National Oceanic and Atmospheric Administration Climate Prediction Centre (NOAA CPC) Africa Rainfall Estimation (version 2.0). The rainfall estimates are constructed by accumulating daily rainfall data to produce dekadal(10-day) estimates at about 10km spatial resolution. The rainfall estimates are at the district level and cover a 12-month period from 2001-2013.

Population data is taken from the NBS Annual Abstract of Statistics covering 2010-2012. The data contains district level population census figures of 1991 and 2006, in addition to, projected population estimates from 2007-2012. Between 1991 and 2006, the Nigerian

Government created new states and districts, in order to keep the district codes as consistent as possible across the years, We manually recode the old district codes to the new district codes.

Table A.1: *Robustness across alternative samples*

Dependent variable	(1) Ethnic group attack civilians	(2) Clash between ethnic groups	(3) Government attack ethnic group	(4) Ethnic group attack government
Panel A. Excluding districts close to oil districts				
Oil district×log oil price	0.310*	-0.079	0.181***	0.226***
	(0.162)	(0.060)	(0.049)	(0.078)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	2,490	2,490	2,490	2,490
Panel B. Including northern districts				
Oil district×log oil price	0.325**	-0.061	0.199***	0.225***
	(0.156)	(0.048)	(0.049)	(0.076)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	11,610	11,610	11,610	11,610
Panel C. Excluding Boko Haram attacks				
Oil district ×log oil price	0.317**	-0.061	0.189***	0.218***
	(0.156)	(0.048)	(0.049)	(0.075)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State ×year fixed effects	Y	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	11,495	11,495	11,495	11,495
Panel D. Including only northern districts as control group				
Oil district × log oil price	0.137*	-0.045	0.071**	0.131**
	(0.081)	(0.054)	(0.032)	(0.055)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	6,885	6,885	6,885	6,885

Notes: Each column represents a separate regression. Panel A excludes southern districts bordering oil districts. Panel B uses northern districts and non-oil southern districts as control group. Panel C excludes districts with Boko Haram attacks within the sample period. In Panel D northern districts are used as control group and the non-oil southern districts are excluded. For all regressions, robust standard errors clustered at district level are in parentheses. See Table 1.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table A.2: *Controlling for pre-sample conflict outcomes*

Dependent variable	(1) Ethnic group attack civilians	(2) Clash between ethnic groups	(3) Government attack ethnic group	(4) Ethnic group attack government
Oil district×log oil price	0.314**	-0.065	0.194***	0.216***
	(0.159)	(0.050)	(0.050)	(0.076)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	5,325	5,325	5,325	5,325

Notes: Each column represents a separate regression. For all regressions, robust standard errors clustered at district level are in parentheses. The district-level covariates include log of population in 1991, average years of schooling among people aged 15 and above in 1990, percent of households with primary school education in 1990 and geographic area in square kilometers, district level conflict events (1990-1997).* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table A.3: *Poisson Fixed Effects*

Dependent variable	(1) Ethnic group attack civilians	(2) Clash between ethnic groups	(3) Government attack ethnic group	(4) Ethnic group attack government
Oil district×log oil price	2.917***	-0.953**	4.883***	3.537***
	(0.735)	(0.417)	(0.620)	(0.706)
Controls				
District fixed effects	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes
Observations	5,325	5,325	5,325	5,325

Notes: Each column represents a separate regression. Each regression uses a generalized linear model with a poisson distribution and a log link function. For all regressions, robust standard errors clustered at district level are in parentheses. See Table 1.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table A.4: *Additional Institutional Quality Outcomes*

Panel A. Trust	(1)	(2)	(3)	(4)	(5)
Dependent variable	Trust president	Trust parliament	Trust military	Trust courts	Trust police
Oil district× log oil price	-0.642***	-0.354**	-0.269*	-0.174	-0.292**
	(0.170)	(0.145)	(0.138)	(0.159)	(0.135)
Controls					
District fixed effects	Yes	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Observations	2,553	2,506	2,535	2,550	2,563
Panel B. Corruption					
Dependent variable	Corrupt president	Corrupt state government	Corrupt parliament	Corrupt local council	Corrupt local chairman
Oil district ×log oil price	0.059	0.100	0.165	0.038	0.243
	(0.145)	(0.138)	(0.137)	(0.131)	(0.148)
Controls					
District fixed effects	Yes	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Observations	2,534	2,536	2,522	2,522	2,538
Panel C. Poverty Perception					
Dependent variable	Without food	Without water	Without medical care	Local markets	Local public service
Oil district × log oil price	-0.245*	0.060	-0.356*	-0.198	0.022
	(0.143)	(0.214)	(0.187)	(0.195)	(0.195)
Controls					
District fixed effects	Yes	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes	Yes
State×year fixed effects	Yes	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Observations	2,604	2,604	2,604	1,289	1,211

Notes: Each column represents a separate regression. Panel A captures individual perceptions on trust, Panel B capture perceptions on corruption of government officials and Panel C considers perceptions on poverty and public goods. For all regressions, robust standard errors clustered at district level are in parentheses. Observations are at individual level. Demographic controls include age, age squared, an indicator for rural, gender and employment status, five living conditions fixed effects, five education fixed effects and 34 occupation fixed effects. See Table 1.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table A.5: *Oil Price Shock, Housing and Infrastructure*

Dependent variable	(1) Log annual house rent	(2) Rooms at home	(3) Households with piped water	(4) Households with electricity	(5) Households linked to main toilet network	(6) Households with concrete floor	(7) Households with garbage collection	(8) Kilometers of paved roads
Oil district × log oil price	1.093***	-0.152	0.073	-0.223**	0.642***	0.105	0.126	-5.603***
	(0.209)	(0.321)	(0.132)	(0.097)	(0.129)	(0.080)	(0.098)	(1.619)
Controls								
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-level covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,924	58,744	58,904	58,904	58,904	58,904	58,904	5,325

Notes: Each column represents a separate regression. For all regressions, robust standard errors clustered at district level are in parentheses. Column (1) is in natural log terms. Column (2) measures average number of rooms at home. Columns (3) -(7) are in percentage terms. Column (8) measures road length at district level. See Table 3.3 for description of district-level covariates.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Appendix B

Appendix to Chapter 2

Table B.1: *The effect of terror fatalities on pregnancy complications*

Dependent variable	Anaemia		Pregnancy complication	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	0.0051	-0.0027	0.0078	-0.0003
	(0.0031)	(0.0034)	(0.0098)	(0.0032)
Terror fatalities: second trimester	0.0041**	0.0036	-0.0090	0.0014
	(0.0019)	(0.0028)	(0.0072)	(0.0039)
Terror fatalities: third trimester	0.0040*	0.0018	0.0062	0.0059***
	(0.0021)	(0.0015)	(0.0042)	(0.0019)
Controls				
Family FE	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy. For all regressions, robust standard errors clustered at district level are in parentheses. The dependent variable in columns (1) and (2) is an indicator variable for mothers with low blood levels during pregnancy while the dependent variable in columns (3) and (4) is an indicator variable for mothers that experienced pregnancy complications. The sample period is from March 2008 through June 2013. See Table 2.2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.2: *The effect of terror fatalities on birth weight, adding fatalities prior to month of conception and after birth*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities prior to conception	7.214**	3.106	-0.001	-0.000	-0.002	-0.000
	(3.529)	(4.233)	(0.001)	(0.001)	(0.001)	(0.001)
Terror fatalities: first trimester	-13.393***	-4.062	0.010*	0.003	0.009*	0.003
	(3.359)	(4.560)	(0.005)	(0.002)	(0.005)	(0.003)
Terror fatalities: second trimester	-3.602	6.888	0.006	0.001	0.005	0.001
	(7.279)	(11.239)	(0.006)	(0.002)	(0.006)	(0.003)
Terror fatalities: third trimester	-6.532***	1.613	0.001	0.000	-0.000	-0.001
	(2.470)	(2.232)	(0.002)	(0.001)	(0.001)	(0.001)
Terror fatalities after birth	4.031	5.147	-0.007**	-0.002	-0.007**	-0.003
	(4.586)	(4.691)	(0.003)	(0.002)	(0.003)	(0.003)
Controls						
Family FE	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy. Terror fatalities prior to conception are fatalities from terror attacks that occurred 1 to 3 months before conception while terror fatalities after birth are fatalities from terror attacks that occurred 1 to 3 months after birth. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.3: *The effect of terror fatalities on birth weight: controlling for fatalities from other conflict events*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities: first trimester	-11.286**	0.535	0.006	0.001	0.005	0.001
	(5.107)	(3.067)	(0.007)	(0.001)	(0.006)	(0.002)
Terror fatalities: second trimester	-4.116	6.190	0.005	0.001	0.004	0.001
	(7.204)	(12.606)	(0.007)	(0.002)	(0.006)	(7.204)
Terror fatalities: third trimester	-4.670*	3.805	0.001	0.000	-0.000	-0.001
	(2.539)	(2.868)	(0.001)	(0.001)	(0.001)	(0.001)
Other conflict events: first trimester	0.156	-0.054	-0.000	-0.000	-0.000	-0.000
	(0.340)	(3.433)	(0.000)	(0.002)	(0.000)	(0.002)
Other conflict events: second trimester	-0.990	0.354	-0.000	-0.001	0.000	-0.001
	(0.654)	(3.708)	(0.000)	(0.001)	(0.000)	(0.001)
Other conflict events: third trimester	-0.501	0.523	-0.000	0.000	-0.000	0.000
	(0.673)	(0.576)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Family FE	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy. Other conflict events are fatalities that occur from clashes between ethnic groups or religious conflict unrelated to Boko Haram attacks. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.4: *The effect of terror fatalities on birth weight: controlling for fatalities from neighboring districts*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities: first trimester	-11.096**	-0.109	0.006	0.001	0.005	0.001
	(5.010)	(2.921)	(0.006)	(0.001)	(0.006)	(0.001)
Terror fatalities: second trimester	-4.158	6.298	0.005	0.001	0.004	0.001
	(7.224)	(12.493)	(0.007)	(0.002)	(0.006)	(7.224)
Terror fatalities: third trimester	-4.675*	3.396	0.001	0.000	-0.000	-0.001
	(2.560)	(2.876)	(0.001)	(0.001)	(0.001)	(0.001)
Neighboring districts: first trimester	-3.319	-19.087	0.006	0.020	0.006	0.018
	(10.692)	(32.049)	(0.009)	(0.030)	(0.009)	(0.029)
Neighboring districts: second trimester	-2.737	-166.725***	0.001	0.008	0.001	0.008
	(6.528)	(30.091)	(0.001)	(0.015)	(0.002)	(0.018)
Neighboring districts: third trimester	27.000	80.588	-0.007	0.005	-0.008	0.004
	(41.437)	(61.749)	(0.006)	(0.007)	(0.009)	(0.008)
Controls						
Family FE	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Neighboring fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy in other districts . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.5: *The effect of terror fatalities on birth weight: controlling for household migration*

Dependent variable	Full Sample		Non-migrant sample	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	-11.10**	-0.10	-11.92***	-0.10
	(5.00)	(2.90)	(4.15)	(2.64)
Terror fatalities: second trimester	-4.16	6.31	-7.69	5.65
	(7.22)	(12.47)	(8.77)	(11.56)
Terror fatalities: third trimester	-4.68*	3.40	-2.63	3.25
	(2.56)	(2.88)	(3.75)	(2.63)
Controls				
Family FE	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No
Observations	5,189	2,406	4,469	2,104

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. See Table 2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.6: *The effect of terror fatalities on birth weight: controlling for internal displacement*

Dependent variable	Full Sample		Sibling sample	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	-11.10**	-10.69**	-0.10	-0.10
	(5.00)	(4.66)	(2.90)	(2.92)
Terror fatalities: second trimester	-4.16	-3.52	6.31	6.29
	(7.22)	(6.66)	(12.47)	(12.75)
Terror fatalities: third trimester	-4.68*	-3.89*	3.40	3.45
	(2.56)	(2.33)	(2.88)	(3.11)
Controls				
Number of people displaced	No	Yes	No	Yes
Family FE	No	No	Yes	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	No	No
Observations	5,189	5,189	2,406	2,406

Notes: Each column represents a separate regression. The dependent variable is birth weight (in grams). Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. See Table 2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.7: *The effect of terror fatalities on birth weight: controlling for rainfall shocks*

Dependent variable	Full Sample		Sibling sample	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	-11.10**	-11.06**	-0.10	-0.12
	(5.00)	(5.01)	(2.90)	(2.91)
Terror fatalities: second trimester	-4.16	-4.16	6.31	6.30
	(7.22)	(7.24)	(12.47)	(12.49)
Terror fatalities: third trimester	-4.68*	-4.70*	3.40	3.38
	(2.56)	(2.55)	(2.88)	(2.89)
Controls				
Rainfall shocks	No	Yes	No	Yes
Family FE	No	No	Yes	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	No	No
Observations	5,189	5,189	2,406	2,406

Notes: Each column represents a separate regression. The dependent variable is birth weight (in grams). Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. See Table 2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.8: *The effect of terror fatalities on birth weight: controlling different time fixed effects*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities: first trimester	-18.389***	-17.996***	0.007	0.009***	0.006	0.008***
	(1.937)	(6.070)	(0.008)	(0.002)	(0.008)	(0.003)
Terror fatalities: second trimester	-2.400	-14.654**	0.004	0.009**	0.004	0.009**
	(7.359)	(7.052)	(0.006)	(0.004)	(0.006)	(7.359)
Terror fatalities: third trimester	-7.640***	-18.099***	0.002	0.010***	0.000	0.009***
	(2.169)	(4.370)	(0.002)	(0.003)	(0.003)	(0.003)
Controls						
District time trends	Yes	No	Yes	No	Yes	No
Month×Year FE	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,189	5,189	5,189	5,189	5,189	5,189

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy . For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.9: *The effect of terror fatalities on pregnancy complications*

Dependent variable	Anaemia		Pregnancy complication	
	(1)	(2)	(3)	(4)
Terror fatalities: first trimester	0.0051	-0.0027	0.0078	-0.0003
	(0.0031)	(0.0034)	(0.0098)	(0.0032)
Terror fatalities: second trimester	0.0041**	0.0036	-0.0090	0.0014
	(0.0019)	(0.0028)	(0.0072)	(0.0039)
Terror fatalities: third trimester	0.0040*	0.0018	0.0062	0.0059***
	(0.0021)	(0.0015)	(0.0042)	(0.0019)
Controls				
Family FE	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks that occurred within trimesters of pregnancy. For all regressions, robust standard errors clustered at district level are in parentheses. The dependent variable in columns (1) and (2) is an indicator variable for mothers with low blood levels during pregnancy while the dependent variable in columns (3) and (4) is an indicator variable for mothers that experienced pregnancy complications. The sample period is from March 2008 through June 2013. See Table 2.2 for description of family characteristics. Significance levels:* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table B.10: *The effect of terror exposure on birth weight: intensity of terror attacks*

Dependent variable	Birth weight (grams)	
	(1)	(2)
Panel A: Terror attacks with at least 1 death		
Terror exposure: 1st trimester	-136.95	-53.24
	(128.00)	(84.35)
Terror exposure: 2nd trimester	78.77	-79.91
	(60.45)	(141.69)
Terror exposure: 3rd trimester	-299.92*	123.65
	(153.19)	(84.29)
Family Fixed Effect	No	Yes
Observations	5,189	2,406
Panel B: Terror attacks with at least 5 deaths		
Terror exposure: 1st trimester	-287.04***	1.37
	(109.99)	(124.49)
Terror exposure: 2nd trimester	-97.73	54.84
	(137.35)	(350.02)
Terror exposure: 3rd trimester	-207.48**	87.89
	(91.41)	(114.92)
Family Fixed Effect	No	Yes
Observations	5,189	2,406
Panel C: Terror attacks with at least 10 deaths		
Terror exposure: 1st trimester	-341.68***	1.33
	(108.72)	(124.49)
Terror exposure: 2nd trimester	-105.66	149.42
	(155.45)	(507.55)
Terror exposure: 3rd trimester	-230.19***	61.93
	(70.07)	(144.14)
Family Fixed Effect	No	Yes
Observations	5,189	2,406

Notes: Each column represents a separate regression. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. All regressions control for year of birth fixed effects, month of birth fixed effects and family characteristics such as infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head, an indicator variable if household head is male and a dummy variable for rural households. Regressions without family fixed effects include district fixed effects. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11: *The effect of terror fatalities on birth weight, scaling fatalities by district population*

Dependent variable	Birth weight (grams)		Birth weight <2500g		Birth weight≤2500g	
	(1)	(2)	(3)	(4)	(5)	(6)
Terror fatalities: first trimester	-34.78***	-0.20	0.0264**	0.0045	0.0239**	0.0023
	(5.59)	(11.55)	(0.0128)	(0.0051)	(0.0120)	(0.0055)
Terror fatalities: second trimester	0.19	20.48	0.0024	0.0007	0.0011	-0.0004
	(10.10)	(13.65)	(0.0061)	(0.0027)	(0.0057)	(10.10)
Terror fatalities: third trimester	-9.81	8.32	0.0017	-0.0002	-0.0006	-0.0018
	(6.94)	(5.14)	(0.0028)	(0.0020)	(0.0017)	(0.0029)
Controls						
Family FE	No	Yes	No	Yes	No	Yes
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	No	Yes	No	Yes	No
Observations	5,189	2,406	5,189	2,406	5,189	2,406

Notes: Each column represents a separate regression. Terror fatalities are fatalities from Boko Haram attacks per district population that occurred within trimesters of pregnancy. For all regressions, robust standard errors clustered at district level are in parentheses. The sample period is from March 2008 through June 2013. Family characteristics include infant gender and birth order, twin indicator, mother's level of education, mother's age, mother's age at marriage, mother's age at birth, father's education and occupation, age of the household head an indicator variable if household head is male and a dummy variable for rural households. Significance levels: * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Appendix C

Appendix to Chapter 3

Table C.1: *The effect of victimization on household consumption, controlling for attrition*

Dependent variable	Food consumption				
	(1)	(2)	(3)	(4)	(5)
Victim×negative shock	-0.3476***	-0.3013**	-0.2375*	-0.2303*	-0.2405**
	[0.1205]	[0.1522]	[0.1334]	[0.1186]	[0.1215]
Controls					
District fixed effects	No	Yes	Yes	Yes	Yes
Household fixed effects	No	Yes	Yes	Yes	Yes
Community fixed effects	No	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	No	No	Yes	Yes
Controls + interactions	No	No	No	No	Yes
Observations	12,972	12,972	12,972	12,972	12,972
R^2	0.005	0.295	0.335	0.447	0.450
Linear combination					
Shock, victimized households	-0.3587	-0.3018	-0.2280	-0.1763	-0.1752
	[0.1189]	[0.1532]	[0.1331]	[0.1190]	[0.1190]
Shock, non-victimized households	-0.0111	-0.0004	0.0095	0.0539	0.0488
	[0.0195]	[0.0231]	[0.0228]	[0.0208]	[0.0208]
Shock, non-victimized households[b]					0.0652
					[0.0238]
Mean of victimized households	0.0252	0.0252	0.0252	0.0252	0.0252

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households[b] (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households.*** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

Table C.2: *The effect of victimization on household consumption, by intensity of violence exposure*

Dependent variable	Food consumption				
	(1)	(2)	(3)	(4)	(5)
Violent events×negative shock	-0.0338*	-0.0352**	-0.0329**	-0.0311**	-0.0306**
	[0.0177]	[0.0147]	[0.0148]	[0.0140]	[0.0145]
Controls					
District fixed effects	No	Yes	Yes	Yes	Yes
Household fixed effects	No	Yes	Yes	Yes	Yes
Community fixed effects	No	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	No	No	Yes	Yes
Controls + interactions	No	No	No	No	Yes
Observations	12,972	12,972	12,972	12,972	12,972
R^2	0.006	0.298	0.335	0.451	0.454
Linear combination					
Shock, victimized households	-0.0898	-0.0320	-0.0212	0.0236	0.0498
	[0.0250]	[0.0305]	[0.0302]	[0.0277]	[0.0291]
Shock, non-victimized households	-0.0559	0.0032	0.0117	0.0547	0.0485
	[0.0190]	[0.0271]	[0.0270]	[0.0242]	[0.0242]
Shock, non-victimized households^b					0.0804
					[0.0266]
Mean of victimized households	0.0746	0.0746	0.0746	0.0746	0.0746

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households^b (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households.*** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

Table C.3: *The effect of victimization on household consumption, by community violence exposure*

Dependent variable	Food consumption				
	(1)	(2)	(3)	(4)	(5)
Community attack×negative shock	-0.1298***	-0.0709	-0.0906*	-0.0772*	-0.0776*
	[0.0389]	[0.0499]	[0.0500]	[0.0440]	[0.0432]
Controls					
District fixed effects	No	Yes	Yes	Yes	Yes
Household fixed effects	No	Yes	Yes	Yes	Yes
Community fixed effects	No	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Household characteristics	No	No	No	Yes	Yes
Controls + interactions	No	No	No	No	Yes
Observations	12,972	12,972	12,972	12,972	12,972
R^2	0.005	0.297	0.335	0.451	0.454
Linear combination					
Shock, victimized households	-0.1361	-0.0434	-0.0464	0.0045	-0.0054
	[0.0307]	[0.0415]	[0.0412]	[0.0362]	[0.0361]
Shock, non-victimized households	-0.0064	0.0274	0.0441	0.0817	0.0753
	[0.0240]	[0.0326]	[0.0329]	[0.0294]	[0.0289]
Shock, non-victimized households[b]					0.0722
					[0.0291]
Mean of victimized households	0.3708	0.3708	0.3708	0.3708	0.3708

Notes: Heteroskedasticity-robust standard errors in brackets. The dependent variable is log of household food consumption per capita. Each column presents the results of a separate regression. Household controls include age of household head, years of education of household head, a dummy value of one if the household head is female, 12 household head occupation dummies, household size, number of household dependants, household rural status, dummy variables of household use of financial instruments such as bank account and access to cooperative institutions. By including interactions in column (5), Shock, non-victimized households[b] (bottom panel) capture the effect for non-victimized households evaluated at the mean characteristics of victimized households.*** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.

Table C.4: *Correlation between Alternative Shock Measures and Household Characteristics*

Dependent variable	Conflict shock	Rain shock
	(1)	(2)
Age of household head	-0.0000	0.0001
	(0.0000)	(0.0001)
Years of education of household head	-0.0001	-0.0004
	(0.0001)	(0.0006)
Household head is female	0.0007	0.0001
	(0.0006)	(0.0029)
Household size	-0.0000	0.0001
	(0.0001)	(0.0006)
Household dependants	0.0002	0.0002
	(0.0002)	(0.0011)
Negative shock	-0.0002	0.0018
	(0.0013)	(0.0062)
Victimized	0.0007	-0.0006
	(0.0005)	(0.0041)
Bank account	0.0013	-0.0044
	(0.0009)	(0.0046)
Access to cooperative institution	0.0005	0.0043
	(0.0024)	(0.0047)
Occupation: Farmer	-0.0005	-0.0063
	(0.0008)	(0.0075)
Occupation: Mining	-0.0004	0.0009
	(0.0006)	(0.0052)
Occupation: Professional	-0.0005	0.0037
	(0.0006)	(0.0115)
Occupation: In Industry	-0.0046	0.0008
	(0.0041)	(0.0107)
Occupation: Sales	0.0051	-0.0016
	(0.0040)	(0.0122)
Occupation: Personnel services	0.0009	-0.0013
	(0.0015)	(0.0072)
Occupation: Education	0.0007	0.0041
	(0.0006)	(0.0061)
Occupation: Health	0.0002	0.0063
	(0.0004)	(0.0081)
Occupation: Public	0.0007	-0.0008
	(0.0006)	(0.0069)
Occupation: Other	0.0087	0.0112
	(0.0086)	(0.0144)
Unemployed	0.0030	0.0011
	(0.0021)	(0.0066)

Notes: Heteroskedasticity-robust standard errors in parentheses. Each column represent panel regressions with household, district, and time fixed effects. The dependent variable in column (1) is a dummy variable equal to one if household is within 1km of a violent event with at least 25 deaths between 2010 and 2016 while column (2) is a dummy variable equal to one if the rainfall is below 15% quantile of the local rainfall distribution. *** Significant at 1 percent. ** Significant at 5 percent. * Significant at 10 percent.