In-utero Exposure to Violence and Child Health in Iraq

Sulin Sardoschau *

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Abstract

This paper examines the impact of exposure to violence during pregnancy on anthropometric and cognitive outcomes of children in the medium-run. I combine detailed household-level data on more than 36,000 children with geo-coded information on civilian casualties in the aftermath of the US invasion of Iraq between 2003 and 2009 and exploit within mother differences in prenatal exposure to violence. I find that one violent incident during pregnancy decreases height and weight for age scores by 0.13 standard deviations and lowers cognitive and behavioral skills of children. Leveraging information on the severity, type and perpetrator of violence, I isolate the effect of stress from access to prenatal care. I show that the results hold when restricting attention to incidents with little impact on the local infrastructure and are largest for more stressful events; primarily those that target the civilian population and involve execution and torture.

Keywords: stress, child health, Iraq

JEL classification: I12 , J13 , O15

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*Humboldt University Berlin, contact: sulin.sardoschau@hu-berlin.de

1 Introduction

The ramifications of prenatal events on child health have far-reaching implications that persist well into adulthood, ranging from consequences for mental health, educational attainment and labor market outcomes (Almond & Currie, 2011; Almond et al., 2018). A large stand of the literature has proposed stress during pregnancy as one of the key drivers of adverse health outcomes for children (Black et al., 2016; Mansour & Rees, 2012; Camacho, 2008). Yet, these studies face two empirical challenges that prohibit a clear conclusion about the role of stress. First, unobserved heterogeneity at the level of the household may lead to differential selection into stressful environments, based on risk aversion, job flexibility or other unobservable characteristics and thus bias the results (Koppensteiner & Manacorda, 2016; Black et al., 2016; Duncan et al., 2016; Currie & Rossin-Slater, 2013). Second, in the context of conflict-related stress, many of these studies focus on secular rises or extreme episodes of violence, such as 9/11 or the al-Aqsa intifada, with potentially large consequences for local infrastructure and access to prenatal care (Eccleston, 2011; Mansour & Rees, 2012; Akbulut-Yuksel, 2014; Lee, 2014; Quintana-Domeque & Ródenas-Serrano, 2017; Lebedinski et al., 2021).

In this paper, I improve on these empirical challenges by i) exploiting within mother variation in exposure to violence during pregnancy, effectively comparing differences in health outcomes between siblings with and without prenatal exposure to violence and by ii) leveraging rich information on the type, severity and perpetrator of violence that allow me to consider incidents with little impact on the local infrastructure and incidents that are arguably more stressful. I investigate this question in an understudied context: insurgent violence following the US invasion of Iraq in 2003. I combine geo-coded information on civilian casualties between 2003 and 2009 from Condra & Shapiro (2012) with detailed information on outcomes for over 36,000 children from the Multiple Indicator Cluster Survey (MISC) conducted in Iraq in 2011. I use information on children's day of birth to trace back in-utero exposure to violence and include mother and children's year of birth interacted with district fixed effects. Additionally, this paper is the first to go beyond birth weight as a primary measure of health, following children throughout their first years of life and tracking their cognitive, motor and behavioral skills.

The identifying assumption relies on the exogeneity of the timing of exposure to violence. I present several pieces of evidence that support the plausibility of this assumption. First, I show in a balance test that experiencing a one-casualty violent incident during pregnancy is not systematically related to an array of household and mother characteristics. In addition, I show that it is unlikely that mothers either anticipate small-scale violent incidents or adjust their behavior differentially ex-post, focusing on selective fertility and migration.

My estimates suggest that one violent incident during pregnancy significantly decreases the height and weight for age Z-scores and reduces children's cognitive and motor skills. The magnitudes are large: in-utero exposure to violence decreases children's height and weight by approximately 0.13

standard deviations below the mean, which translates into 370 grams and 2.7 cm for a one-year old child. Children also perform worse in terms of cognitive and motor skills compared to children without in-utero exposure to violence. It is important to note that - with the exception of a few households in Kurdish districts - all households experience at least one violent incident throughout the observation period between 2006 and 2009. The magnitudes are therefore particularly striking since they capture the marginal effect of an additional violent incident in-utero.

I provide several pieces of evidence consistent with stress as the primary driver of the adverse effects of violence on child health. First, violent incidents in the first trimester are driving the main result. This is in line with the medical literature, documenting that prenatal stress increases levels of Corticotrophin Releasing Hormone (CRH), which regulates fetal maturation and thus increases the risk of adverse birth outcomes especially in the early stages of pregnancy (Beydoun & Saftlas, 2008; László et al., 2014; Glynn et al., 2001). Second, throughout all estimations I restrict attention to incidents that have arguably little effect on the local infrastructure, i.e. violent incidents with only one casualty. I also show that results hold when I only include events where civilians were killed by a direct gunshot, rather than bombings, explosions or airstrikes. Third, I provide evidence that the results are driven by religiously motivated killings that specifically target the civilian population, rather than "collateral" damage from confrontations between coalition and insurgent forces.

I perform several robustness checks to confirm the validity of these findings. While the mother fixed effects account for time-invariant unobserved heterogeneity, there may still be selection into fertility based on levels of violence. This can bias the results if violence is serially correlated and mothers dynamically adjust fertility. For this reason, I include the number of incidents in the three months before conception and the results hold. I also verify that selective out-migration is not a main driver of my results. First, if households move between births due to a violent incident, I consider them as not treated. If these children have worse health outcomes due to in-utero exposure to violence, this would downward bias my findings. In addition, almost all districts experience a violent incident in the observation period and violent episodes (and their location) were difficult to predict. It is therefore unlikely that households move based on (low casualty) incidents. I verify this claim by looking at another survey of Iraqi households that contains detailed information on the migration history of 175,000 respondents and their reasons for moving. In addition, I verify that the results are robust to changes in the measurement of the treatment and outcome and different sample composition.

This paper contributes to the literature on the so-called "fetal origins" hypothesis (FOH). The FOH in economics has been analyzed in six major contexts: nutritional shocks, infectious diseases, exposure to pollution, weather and climate change, use of alcohol and tobacco, as well as maternal stress. Several papers look at mild nutritional shocks in-utero induced by pregnancy during *ramadan* and show that they have a substantial effect on various outcomes, such as income, educational attainment, or mental illness (Majid, 2015; Van Ewijk, 2011; Almond & Mazumder, 2011). Similarly, other papers investigate the role of environmental factors, such as hurricanes, radioactive fallout or pollution, as well as substance abuse during pregnancy in determining health and long-run outcomes of children (Beach et al., 2016; Isen et al., 2017; Almond et al., 2009; Shah & Steinberg, 2017; Ngo & Horton, 2016; Andalón et al., 2016; Dercon & Porter, 2014; Banerjee et al., 2010; Adhvaryu et al., 2014; Nilsson, 2017; Reader, 2023). In addition, some researchers have highlighted the effect of economic conditions during pregnancy on outcomes of children (Dehejia & Lleras-Muney, 2004; Willage & Willén, 2022; De Cao et al., 2022; Reader, 2023).

There is a prominent sub-strand of this literature that tries to isolate the effect of stress. While it is well established in the medical literature that stress and the associated bio-chemical response is harmful to children, it is difficult to disentangle stress from other components in larger population studies. There are no large scale longitudinal data sets that measure CRH or Cortisol levels in mothers.[1] Economists therefore refute to reduced form estimations that use an exogenous event that reasonably affects stress in mothers, such as potential paths of major hurricanes, important sports events or family ruptures (Currie & Rossin-Slater, 2013; Duncan et al., 2016; Persson & Rossin-Slater, 2018).

In the context of conflict and violence, it is difficult to disentangle the effect of stress from other mechanisms. The literature on in-utero exposure to conflict and child health outcomes has been studied in two main contexts, either in one of brief and extreme episodes of violence, such as the attacks on the World Trade Center in September 2001, landmine explosions or the al-Aqsa Intifada (Brown, 2015; Mansour & Rees, 2012; Eccleston, 2011; Camacho, 2008) or systematic "day to day" crime-related exposure to violence in Latin and South America (Nasir et al., 2016; Koppensteiner & Manacorda, 2016; Brown, 2015; Torche & Villarreal, 2014). On the one hand, large scale incidents of violence are difficult to disentangle from other variables that may simultaneously influence child health outcomes, such as deterioration of health infrastructure or behavioral responses of parents. On the other hand, it is hard to argue that regular, crime-related violence is exogenous or unpredictable. Therefore, a major concern in these studies is the selection of households along unobservable characteristics.

Mansour & Rees (2012) is the study that is most closely related to this analysis. The authors draw from the 2004 Palestinian Demographic and Health Survey, which was conducted approximately 4 years after the start of the al-Aqsa Intifada, and find that an additional conflict-related fatality 9 to 6 months before birth is associated with with a modest increase in the probability of a child weighing less than 2500 g. The authors have to rely on self-reported measures of health for children. In contrast to this study, I can draw on indicators of child health that were measured

[1] One exception is Aizer et al. (2016) who collect data from perinatal care centers in the 1960s and show that infants exposed to higher cortisol levels during pregnancy have up to 1 year less school at age 7 than their siblings. However, this study cannot account for selection into fertility in periods of high cortisol levels.

by enumerators at the time of the interview and thus do not suffer from reporting or recall bias. In addition, the authors rely on a sample of 244 mothers in Western Gaza, while I can - for the sample of siblings - leverage information on over 12,000 mothers and 25,000 children. While the authors can account for unobserved heterogeneity at the level of the mother, they acknowledge that the al-Aqsa Intifada came with a lot of restrictions and curfews, potentially restricting access to health care. I can improve on isolating the effect of stress from access to health care by looking at events of the same severity but arguably different levels of stress for mothers. These differences may explain why my estimates substantially deviate from theirs in terms of magnitude.

The paper is structured as follows. In section 2, I briefly describe the Iraqi context and main data sources. I describe the empirical strategy and main results as well as robustness checks in section 3 before providing evidence on the stress mechanism in 3.4 and concluding in section 4.

2 Data and the Iraqi Context

2.1 The US Invasion of Iraq

In 2003, the United States, in conjunction with a coalition of international allies, embarked on a military campaign to oust the regime of Saddam Hussein in Iraq. The decision to invade was made by President George W. Bush and his administration, who posited that Iraq, under the leadership of Saddam Hussein, posed a threat to national security. As the United States and its allies sought to gain support for the war, the administration presented evidence that Iraq was in possession of weapons of mass destruction (WMDs) and had ties to terrorist groups, including Al-Qaida. The Bush administration also claimed that the war was necessary to spread democracy and human rights in Iraq. In retrospect, the evidence presented by the Bush administration to justify the invasion of Iraq has been widely criticized and discredited (Betts, 2007). Despite opposition from several countries such as France and Germany, the coalition launched the invasion on March 20th, 2003, with the stated objective of removing the regime of Saddam Hussein, eliminating the supposed WMDs and ties to terrorist groups and bringing democracy to Iraq.

The coalition, which included the United Kingdom, Australia, Poland, and several other countries, quickly secured control of key cities and infrastructure in Iraq. The initial invasion was marked by the swift collapse of the Iraqi military and the fall of the Hussein regime, culminating in the capture of Baghdad on April 9, 2003. However, the post-invasion period was plagued by a prolonged and violent insurgency. The insurgency was mainly composed of various sectarian and ethnic groups that opposed the foreign presence, the new political order and the lack of services and security (Hashim, 2011). Throughout the war, coalition forces struggled to identify insurgents and gain the support of the local population (Cockburn, 2007). Continuous confrontations and acts of violence between coalition forces, insurgent, and sectarians have caused immense damage in form of civilian casualties. The *Iraq Body Count Project* (IBC) has recorded a total of 200,000 civilian non-combatant casualties since 2003, which is 20 times the number of fatalities in the history of the Israeli-Palestinian conflict.[2]

Aside from its devastating effects on Iraq's economy, the continuous exposure to conflict has also taken a toll on the mental health of the Iraqi population. According to the 2007 Iraq Mental Health Survey, 16.5% of the 32,000 respondents reported symptoms of severe mental disorder; for women this number is even higher at about 20%.[3] About 5% of women in the Iraq Mental Health survey report symptoms of post-traumatic stress disorder (PTSD). Health professionals propose that among all mental disorders, PTSD is one of the main drivers through which parents transmit adverse effects to their children during war-times (Devakumar et al., 2014; Murthy & Lakshminarayana, 2006).

2.2 Data Sources and Descriptive Statistics

Multiple Indicator Cluster Survey. In collaboration with the local government, UNICEF assists countries in collecting and analyzing the situation of children and women through its international household survey initiative, called the Multiple Indicator Cluster Surveys (MICS). The cross-sectional survey in 2011 is the first to be representative on the district level in Iraq.[4] The survey collects detailed information on living conditions, mothers' birth history, and very detailed health outcomes for children under the age of 5, sampling 35,580 households, 56,445 women (age 15-49 years) and 36,599 small children (0-4 years).

Information on infant health outcomes include classical anthropometric indicators such as the Height for Age Z-Score and the Weight for Age Z-Score. These are not self-reported but are measured by the interviewer. The main index is based on the National Center for Health Statistics (NCHS) malnutrition and stunted scale and expresses the distance between an individual child's height/weight and the average height/weight of comparable children in the reference population taking into account the dispersion of the distribution. Additionally, child development indicators are collected in the form of questions on motor skills (child is able to pick up a small object with 2 fingers), cognitive skills (child identifies at least ten letters of the alphabet, reads at least four simple, popular words, knows name and recognizes symbols of all numbers from 1-10), behavior (child follows simple directions, is able to do something independently, gets along well with other children, kicks, bites or hits other children or adults, gets distracted easily, is sometimes to sick to play). These more complex health indicators were not collected in all households but for a sub-sample of about 13,000 children (out of 36,000 children). The randomization was made at the household level such that all children under the age of 5 in the randomly selected household were tested.

[2] Statistics from B'Tselem, an Israeli human rights organization, are used because they are largely considered to have comprehensive and nonpartisan data. Find access here: https://israelipalestinian.procon.org/view.resource.php?resourceID=000639

[3] World Health Organization and Ministry of Health, Iraq Mental Health Survey 2007. Find access here: http://apps.who.int/iris/handle/10665/116610

[4] Previous MICSs carried out in 2000 and 2006 were only representative on the provincial level

Table 1 reports the various characteristics at the household-, parent- and child-level for all households in the MICS data set that have at least one child under the age of 5 (which I call the "full sample") and the sample of households with at least 2 children born in the observation period (i.e., the "sibling sample"). Approximately 20,000 out of the total 35,000 households have at least one child under the age of 5. About 12,000 mothers gave birth to multiple children during the relevant observation period between 2006 and 2009. Parents in the sibling sample are less educated, younger and less wealthy than their counterparts in the full sample. 24,000 children under the age of 5 have a sibling that is also under the age of 5. Health outcomes for children that have siblings of similar age are generally worse, they tend to weigh less and be smaller. With regards to cognitive, behavioral and motor skills, children in both samples perform similarly well. In addition to information on children, there is a vast array of household and parents' characteristics, including detailed information on household size, composition, and wealth, as well as information on parents' education, labor market status, proxies for religiosity and a limited number of attitudinal questions.

Iraq Body Count Project The Iraq Body Count Project (IBC) provides daily geo-coded information on the location of the attack, the perpetrators of the attack (coalition, insurgent, sectarian, unknown), the type of attack (mortars, missiles, suicide attacks, snipers, improvised explosive devices (IEDs), rocket propelled grenades (RPGs), car bombs, and small arms fire, usually with assault rifles. Their data is gathered from media reports, hospital documentation, morgues and other sources. In cooperation with the IBC Condra & Shapiro (2012) assign 19,961 violent incidents to districts for the period between 2003 to 2009, accounting for a total of 59,245 civilian deaths.

Figure 1 depicts the cumulative number of violent incidents (panel a) and causalities involved (panel b) across all 118 districts between 2003 and 2009. Violent incidents cover the whole of Iraq, with the exception of Kurdish districts in the North-Eastern part of the country. Most of the violent incidents occur in more populated districts (the Southwestern part of Iraq is very scarcely populated) including the capital Baghdad. However, there is still substantial variation in exposure to violence across the entire Northwestern-Southeastern strip of the country.

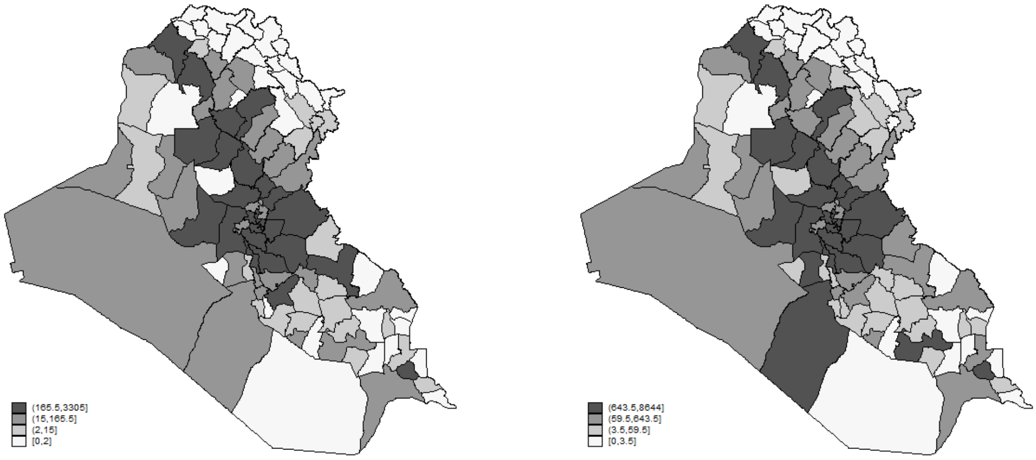
This paper follows Condra & Shapiro (2012) to divide killings into 4 categories: *i*) insurgent killings of civilians that occur in the course of attacking Coalition or Iraqi government targets; this category explicitly excludes insurgent killings that are unrelated to attacks and are better classified as intimidation killings related to dynamics of the civil war, *ii*) Coalition killings of civilians, *iii*) sectarian killings defined as those conducted by an organization representing an ethnic group and which did not occur in the context of attacks on Coalition or Iraqi forces, *iv*) unknown killings, where a clear perpetrator could not be identified. This last category captures much of the violence associated with ethnic cleansing, reprisal killings, and the like, where claims of responsibility were rarely made and bodies were often simply dropped by the side of the road. While all casualties

Table 1: **Summary statistics - 2011 Multiple Indicator Cluster Survey Iraq**

	(1) Full Sample				(2) Sibling Sample			
	mean	sd	min	max	mean	sd	min	max
Household characteristics								
Number of HH members	7.57	3.55	2	36	8.36	4.04	3	36
Number of women 15 - 49 years	1.64	1.06	0	11	1.72	1.16	0	11
Number of children under age 5	1.77	0.96	1	13	2.47	0.87	2	13
Total children aged 2-14 years	3.39	1.87	1	20	3.48	2.09	1	20
Female household head	0.05	0.21	0	1	0.05	0.21	0	1
Education of household head	2.25	0.75	1	3	2.18	0.76	1	3
Rural	0.43	0.49	0	1	0.47	0.50	0	1
Wealth index score	-0.07	1.00	-6	2	-0.17	1.05	-6	2
Observations	20,031				10,294			
Parent characteristics								
Mother's line number	3.64	3.25	1	32	4.28	3.80	1	32
Father's line number	1.89	2.08	0	31	2.25	2.49	0	31
Father's education	2.43	0.78	1	9	2.38	0.77	1	9
Mother's education	2.04	0.71	1	3	1.98	0.70	1	3
Age of woman	29.96	6.99	15	49	28.73	6.34	15	49
Age of husband/partner	34.43	8.27	15	92	33.05	7.59	16	85
Husband is relative	0.60	0.49	0	1	0.61	0.49	0	1
Currently working	0.09	0.29	0	1	0.09	0.28	0	1
Husband/partner has other wives	0.06	0.23	0	1	0.06	0.25	0	1
Observations	22,109				12,328			
Child characteristics								
Female	0.49	0.50	0	1	0.49	0.50	0	1
Twin	0.03	0.16	0	1	0.03	0.18	0	1
Line number	7.25	3.76	2	36	7.56	4.08	2	36
Age	3.83	1.58	1	6	3.84	1.58	1	6
Child Outcomes								
Height for age z-score	-0.57	2.08	-10	10	-0.62	2.04	-10	10
Weight for age z-score	-0.26	1.84	-6	10	-0.30	1.80	-6	10
Identifies at least ten letters of the alphabet	0.29	0.45	0	1	0.29	0.45	0	1
Reads at least four simple, popular words	0.12	0.32	0	1	0.11	0.32	0	1
Knows name and recognizes symbol of all numbers from 1-10	0.21	0.41	0	1	0.20	0.40	0	1
Able to pick up small object with 2 fingers	0.90	0.30	0	1	0.89	0.31	0	1
Sometimes too sick to play	0.54	0.50	0	1	0.54	0.50	0	1
Follows simple directions	0.82	0.39	0	1	0.81	0.39	0	1
Able to do something independently	0.74	0.44	0	1	0.73	0.44	0	1
Gets along well with other children	0.84	0.37	0	1	0.83	0.38	0	1
Kicks, bites or hits other children or adults	0.26	0.44	0	1	0.27	0.45	0	1
Gets distracted easily	0.44	0.50	0	1	0.45	0.50	0	1
Observations	34,660				24,843			

This Table reports summary statistics from the 2011 Iraq Multiple Indicator Cluster Survey on 3 levels: the household level, the parent level and the child level. I report the summary statistics for the full sample as well as for the subset of households that has multiple children under the age of 5, which provides the main source of variation in the mother FE model. The table also shows the main outcomes of interest, including anthropometric (height for age and weight for age z-scores) as well as cognitive, motor and behavioral skills, based on survey questions to the mother.

Figure 1: **Iraq Body Count Data by district: cumulative 2003-2009**



(a) Number of Violent incidents

(b) Number of Civilian Casualties

reported are civilian casualties, the casualties resulting from coalition-insurgent confrontation are considered "non-targeted collateral damage", e.g. civilians dying in the crossfire. Sectarian violence are killings by a clearly identified militia targeted at the civilian population.

The IBC data includes a descriptive section on the type of violent act perpetrated. Using key word searches in the incident descriptions, I created 5 categories of attacks. These categories are not mutually exclusive since the same incident can contain multiple types of violence. I distinguish between suicide bombings, other bombs and explosions (typically so-called Improvised Explosive Devices), airstrikes and missiles (which is mostly long-distance weaponry or military planes), gunfire, and execution or torture. Table 2 shows the breakdown of violent incident by type and perpetrator. Since violent incidents can involve multiple perpetrators and combine different types of violence, the shares reported do not add to 100 percent. Almost 60% of incidents with civilian casualties involved gun violence. Almost one quarter involved executions and torture, which were mostly perpetrated by sectarian agents, who are also the principal source behind violent incidents. However, the majority of suicide bombings were caused by insurgent rather sectarian agents.

One concern is that the IBC data could be subject to reporting bias. On the one hand, reporters may avoid high-violence environments and not report stories from there. On the other hand, the opposite bias could be present if high violence areas attract the most media attention. Condra & Shapiro (2012) address this concern, using 2,612 incidents for which a larger level of aggregation (the governorate) is known but the district is not and analyze whether the proportion of non-attributable incidents at the governorate correlates with levels of violence. The authors do not find a significant correlation that would suggest a systemic bias of non-attributable incidents.

Table 2: **Violent incident characteristics by perpetrator**

Perpetrator	casualties	suicide	bombs	airstrikes/missiles	gunfire	executions/torture
Coalition forces (N=1179)						
mean	5.621	0	0.049	0.2408	0.628	0.007
sd	32.74	0	0.216	0.427	0.483	0.087
Insurgent agents (N=2826)						
mean	3.207	0.182	0.669	0.071	0.264	0.009
sd	5.287	0.386	0.470	0.258	0.441	0.097
Sectarian agents (N=12874)						
mean	2.965	0.026	0.179	0.067	0.699	0.315
sd	11.692	0.161	0.383	0.251	0.458	0.464
Total (N=16879)						
mean	3.191	0.050	0.252	0.080	0.621	0.243
sd	13.573	0.219	0.434	0.272	0.484	0.428

Own classification of type of violence according to descriptions in "Cause of Death" column of the ESOC data (Iraq Body Count Project and Condra & Shapiro (2012) data set), based on keyword search. Multiple classifications for a single violent incidence are possible (e.g. suicide bombings + explosions). Summary stats are provided for incidences with one unique and identified perpetrator; events with unknown perpetrator (N=981) or multiple perpetrators (N=602) are not reported.

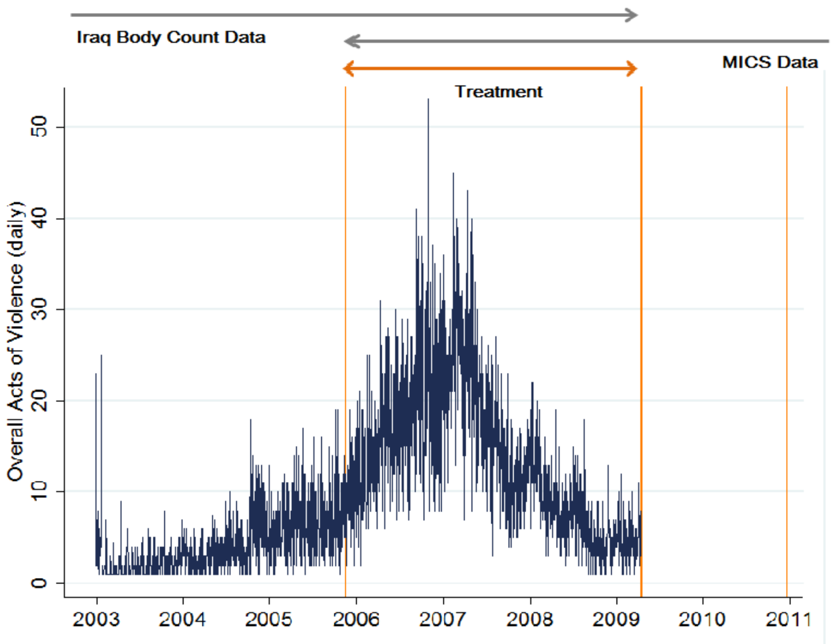
In this paper, I restrict attention to one-casualty incidents only, further alleviating concerns about differential reporting bias based on the severity of violence. I also perform another sanity check on the data, using an alternative data set, namely the unclassified data from the Multi-National Force-Iraq (MNFI) through daily Significant Activity Reports (SIGACTS).[5] The data set covers all known attacks on Coalition forces, Iraqi Security Forces, the civilian population, and infrastructure between 2004 and 2008. The data do not capture the perpetrator of violence and are only recorded if they involve coalition forces in some way. In contrast to the IBC data, they also include incidents with no casualties. Comparing the yearly activities on the district level between the two data sets, there is a very high correlation of about 0.8 which is significant at the 1% level. Given the very limited data sources on district level violent incidents in Iraq, I consider this one of the few possible sanity check for the accuracy of the IBC data.

Linking violent incidents to pregnancies. In order to link district-level data on violent incidents to child health outcomes, I use the exact birthdays of the children under the age of 5 from the MICS 2011 and calculate their period in-utero backward. I drop observations where there is no information on the birthday of the children. The earliest date of conception is five years and 9 months before the time of the first MICS 2011 interview, i.e. September 2005. There will be some measurement error, particularly for the first trimester, as I cannot account for early births. Instead, I assume that all children are carried to full term. Once I have assigned in-utero periods to all children in the MICS data set, I construct the final data set which consists of the overlap period between all recorded incidents in the IBC data set and the earliest birth in the 2011 MICS data set (see Figure 2). This leaves me with the relevant period of observation from 2006 to 2009, comparing

[5]The data is made available to the Empirical Studies of Conflict (ESOC) Project and used in a different context in Berman et al. (2011)

siblings with a maximum age gap of 2.5 years.

Figure 2: **Linking violent incidents to pregnancies: cross-sectional MICS survey 2011 and Iraq Body Count data 2003-2009**



Importantly, for the sake of this analysis, I exclude all events that have more than one casualty in order to limit the influence of large scale attacks that may impact the regional health infrastructure or induce more substantial change in the socio-demographic composition or regional amenities. In a robustness check, I show that the effect is marginally larger when I include violent incidents with more casualties. I also treat violent episodes that last several days as one incident and take the start date of the violent episode. 17,500 of the 18,500 recorded events only last one day. The average duration of a violent episode is between 1 and 1.5 days.

In Appendix Table A.1, I show the average duration and casualties associated with each incident by perpetrator, when I do not restrict the sample to one-casualty events. Half of the recorded violent incidents involve only one civilian casualty. Generally, casualty numbers are in the one digits, only 6% involve more than 10 casualties and the sample includes 70 observations with more than 100 casualties. Events with very high civilian casualties occurred almost exclusively in Baghdad. The vast majority of incidents, more than 70%, involves sectarian forces. However, coalition attacks have been the most fatal ones, resulting, for instance, in a violent episode of 30 days (July 1st to 30th 2006) that took 1098 civilian lives in Baghdad.

3 Empirical Strategy and Results

3.1 Empirical Strategy

I leverage within mother variation in exposure to violence during pregnancy, effectively comparing health gaps between pairs of siblings where one was exposed to violence in-utero and the other one was not to those sibling pairs where either both or none have been exposed to violence. My preferred specification writes as follows:

$$Y_{imd} = \beta_0 + \beta_1 violence_d \times in\ utero_i + \beta_2 X_i + YoB_i \times \theta_d + \gamma_m + \varepsilon_{imd}$$

I observe health outcomes Y of child i born to mother m located in district d in 2011 and trace back their in-utero exposure to violence in each of the trimesters. The treatment variable $violence_d \times in\ utero_i$ is a dummy variable that switches on if any violent incident with one casualty was recorded at the district-level ($violence_d$) when the child was still in-utero ($in\ utero_i$). The vector of control variables X_i includes child-level characteristics, such as the gender of the child, whether he or she has a twin sibling, and their line number (e.g. whether the child is first, second or third born etc.). I also include a year of birth dummy YoB interacted with districts fixed effects θ_d, which capture child age-district specific time trends. Additionally, I include mother fixed effects γ_m, absorbing any time-invariant unobserved heterogeneity at the level of the mother. The main outcomes of interest, weight and height, are measured by the enumerator and presented as Z-Scores. Other health outcomes, such as the ability to recognize letters or numbers are based on survey questions (see Table 1 for the full list of non-anthropometric outcomes). Standard errors are clustered at the district level. The coefficient β_1 can be interpreted as follows: a violent incident involving one casualty during pregnancy (or each trimester of pregnancy) will decrease the height or weight of a child by β_1 standard deviations.

It is important to note that violence varies at the district and day level. The treatment therefore affects everyone that lives in the district at a certain point in time. The identifying variation is at the child level and comes from exposure to violence when it is in-utero. This is why it is possible to include mother fixed effects although outcomes are only observed once in 2011 and mothers are uniformly treated within a district. In order to further illustrate the identifying variation, in Table 3, I show the probability of treatment for the sample of mothers that have more than one child under the age of 5 at the time of the interview. About 5,900 mothers have experienced a violent incident during pregnancy, either for some or all of their children (see second and third row). The overall probability of experiencing a violent incident during pregnancy therefore lies at around 60%. As mentioned above, I compare health gaps of siblings in the second row with health gaps of siblings in the first and last row.[6]

[6] I show in section 3.5 that results are robust to using as a control group only sibling pairs where none have been

It is also worth noting that all children in the data set were exposed to a violent incident at some point after they have been born (except for a few districts in the Kurdish region that have not experienced any violence during the observation period). All of these events will naturally have an impact on child health a few years later. In this context, I only test whether an *additional* incident during pregnancy worsens health outcomes. Ex-ante it is not obvious whether we should observe an effect at all. On the one hand, violence may be "normalized" in these contexts and mothers may respond less to an incident, as compared to rare events which are typically analyzed in the literature (Currie & Rossin-Slater, 2013; Brown, 2015; Duncan et al., 2016; Persson & Rossin-Slater, 2018). On the other hand, there is evidence that even small events in early stages of pregnancy can have significant effects in the long-run and that exogenous shocks are easier to mitigate in later stages of pregnancy and early childhood (Camacho, 2008; Duncan et al., 2016).

Table 3: Probability of treatment: in sibling sample

	number of mothers	share of mothers
No sibling treated	4,246	0.418
Some siblings treated	3,983	0.393
All siblings treated	1,921	0.189
Total	10,150	1

Number of households that have multiple children under the age of 5, of which none were exposed to violence during pregnancy (first row), of which some were exposed to violence (second row), of which all were exposed to violence (third row).

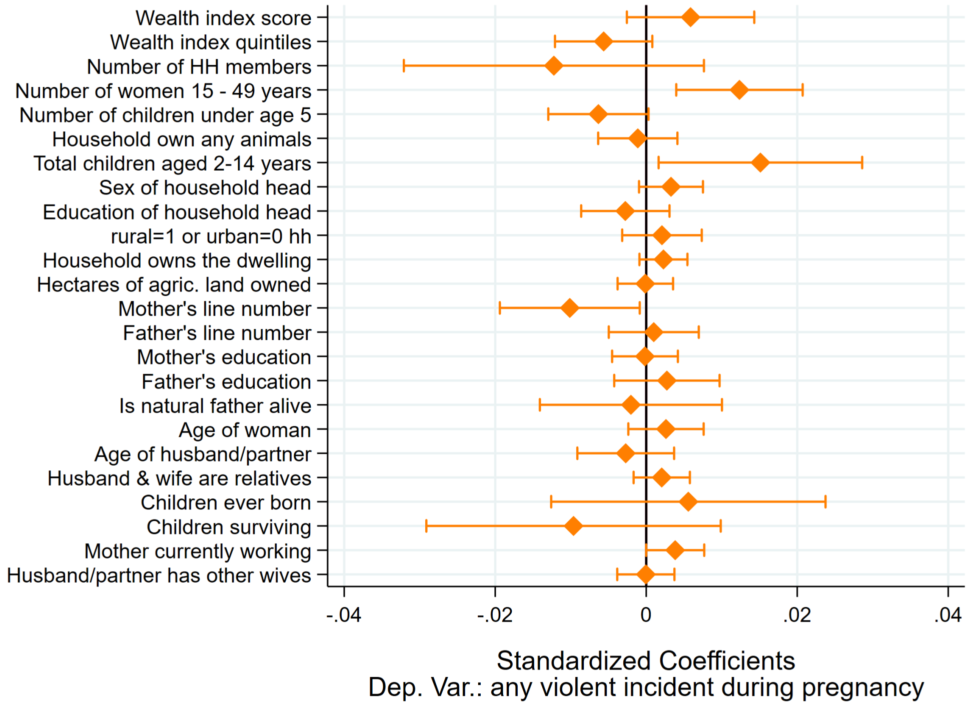
3.2 Threats to Identification

A causal interpretation of β_1 requires that there are no time-varying unobserved characteristics at the level of the mother that determine both the likelihood of being exposed to violence during pregnancy and health outcomes of the child they are carrying. In essence, the identifying assumption relies on the exogeneity of the timing of exposure to violence. In Figure 3, I present a plausibility check for this assumption. Specifically, I predict the probability of treatment (in-utero exposure to a violent incident with one casualty) with an array of mother and household-level characteristics. I include district fixed effects and standardize coefficients for comparability. Reassuringly, almost all variables are not systematically correlated with the treatment and those that are are small in magnitude. Importantly, the mother fixed effects capture all characteristics presented in Figure 3 and others that could be related to selection into treatment and child health outcomes, such as risk aversion, religiosity or attitudes towards contraception etc. Any differences in district characteristics in the year the child was born are captured by the district-year of birth fixed effects, including

exposed to violence in utero.

political stability, ethnic polarization, quality of health infrastructure etc. In addition, any concerns related to the time-varying unobserved heterogeneity at the level of the mother would have to point to variation in a time span of 2.5 years since this is the largest age gap observed between siblings in my data set. More generally, the coefficient β_1 is biased if mothers either i) anticipate small-scale violent incidents or ii) adjust their behavior differentially ex-post.

Figure 3: Plausibility of exogenous exposure to violence in-utero



Note: Plausibility of exogenous exposure to violence during pregnancy. I estimate the following regression: $violence_d \times in\ utero_i = \gamma X_{mh} + \theta_d + \epsilon_{imhd}$. I regress a set of mother and household level characteristics X_{mh} on the treatment variable $Violence_{imhd}$, i.e. a dummy variable that switches on if child i living in household h born to mother m has been exposed to a violent incident with one casualty in-utero. I include district fixed effects θ_d and cluster standard errors at the district level. All coefficients are standardized for comparability.

Selective fertility. First, I only observe pregnancies that resulted in a live birth. It is possible that violent incidents and stress increase the likelihood of miscarriage (László et al., 2014). Consequently, women that have been exposed to violence and experienced a miscarriage will not appear in the treatment group although they are the ones most severely affected by it. Therefore, I may underestimate the true effect of exposure to violence during pregnancy and only quantify the effects conditional on having a live birth.

Second, it is possible that even low casualty incidents are serially correlated and mothers dynamically adjust their fertility in response to episodes of violence. For instance, it is possible that wealthier mothers with access to contraceptives can adjust fertility while others cannot. If there is negative selection into fertility that is time-varying at the level of the mother, I would falsely attribute the adverse health outcomes to violent incidents rather than time-varying unobserved heterogeneity of mothers. In order to address this concern, I include a dummy variable that captures any violent incident in the three months before conception in one specification.

Selective migration. As mentioned previously, the mother fixed effects capture a substantial share of the unobserved heterogeneity associated with both exposure to violence and child health. However, mothers may choose to move in response to exposure to violence. Since I have no information on the migration history and timing of migration (or any other proxy, such as district of birth of mother or father), I cannot control for this problem directly. If there is selective emigration in response to violence, I would consider them as not treated. Consequently, I may under-estimate the true effect of exposure to violence in-utero.

In addition, I focus on low-casualty incidents in a highly violent environment. The decision to emigrate would have to be based on these relatively small scale events. In order to assess the plausibility of this claim, I use a different data source to verify whether internal migration was a major determinant in the demographic composition of districts. The Iraq Household Socio-Economic Survey (IHSES) is a large scale survey conducted by the Iraqi Central Office for Statistics and supported by the World Bank, that covers over 25,000 Iraqi households and over 175,000 individuals in the year 2012. The data set contains detailed information on the migration history of respondents. Approximately 18% of respondents have migrated at some point in their lives. Appendix Table A.8 describes the different reasons for migration for the sub-sample of approximately 1,600 respondents that moved during the observation period between 2006 and 2009. Approximately 6% of the individuals that migrated during this period have done so for reasons of civil conflict, armed conflict, security reasons, or overall forced displacement. Overall, they make up less than 0.1% of the full sample, alleviating concerns that migration is a main driver in the composition of respondents in the MICS.

3.3 Main Results

Anthropometric outcomes. I present the main results in Table 4. I consider the Height for Age Z-Score (HAZ) and the Weight for Age Z-Score (WAZ) in columns 1 to 3 and 4 to 6, respectively. As previewed above, all specifications include child-level controls, mother fixed effects as well as year of birth interacted with district fixed effects (I show in Appendix Table A.2 the evolution of the coefficient of interest when successively introducing controls and fixed effects). The average height and weight as well as the standard deviation of children by age are reported at the bottom.

In columns 1 and 4, I first include a dummy variable for any low-casualty violent incident during pregnancy and find that in-utero exposure to violence significantly reduces the height and weight scores of children. The estimates suggest that one incident decreases the HAZ by 0.135 standard deviations and the WAZ by 0.143 standard deviations, which translates into 370 grams and 2.7 cm for a one-year old child.[7]

Table 4: **Violent incident during pregnancy deteriorates child health**

	Height for Age Z-Score			Weight for Age Z-Score		
	(1)	(2)	(3)	(4)	(5)	(6)
violence$_{all}$	-0.135**			-0.143***		
	(0.0588)			(0.0514)		
violence$_{trim1}$		-0.232***	-0.197***		-0.188***	-0.145***
		(0.0561)	(0.0544)		(0.0417)	(0.0373)
violence$_{trim2}$		-0.0321	-0.0159		-0.0439	-0.0241
		(0.0569)	(0.0583)		(0.0417)	(0.0425)
violence$_{trim3}$		0.108*	0.112*		0.0298	0.0351
		(0.0585)	(0.0592)		(0.0477)	(0.0481)
violence$_{pre}$			-0.116**			-0.142***
			(0.0518)			(0.0414)
Observations	22,640	22,640	22,640	22,640	22,640	22,640
Mothers	10,150	10,150	10,150	10,150	10,150	10,150
Districts	118	118	118	118	118	118
Adj. R-squared	0.277	0.278	0.278	0.313	0.313	0.314
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth × district FE	Yes	Yes	Yes	Yes	Yes	Yes
	mean (cm)	sd			mean(kg)	sd
Age 0	64.22	15.87			6.98	3.39
Age 1	78.65	24.53			10.44	2.87
Age 2	87.18	14.98			12.66	3.45
Age 3	95.26	20.12			14.62	3.36
Age 4	102.26	20.08			16.41	3.34

The dependent variable is the height for age Z-score in columns 1 to 3, and the weight for age Z-score in columns 4 to 6. Average height and weight by age as well as standard deviations are reported at the bottom of the table. The sample of violent incidents is restricted to those with only one casualty. The treatment is measured as a dummy variable for any violent incident during all of pregnancy (col 1 and 4), during each trimester (col 2 and 5) and for the three months before pregnancy (col 3 and 6). Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In columns 2 and 5, I present the results for assigning violent incidents to trimesters of pregnancy. The medical literature proposes that the production of the stress-induced hormone CRH has the most prominent effect on birth outcomes during the first trimester of pregnancy, i.e. children are most sensitive to maternal stress in early stages of pregnancy (Glynn et al., 2001). The estimates confirm this finding as most of the adverse effect of conflict on height and weight comes from events that occurred during the first trimester. The effect is significantly larger and more precisely estimated when I consider the timing of the violent incident. As mentioned in the previous

[7] It is difficult to compare these estimates to others in the literature since anthropometric outcomes are typically measured at birth. For instance, Camacho (2008) estimates that one landmine explosion decreases the birth weight by 8.7 grams. In my estimation children under the age of one will weigh 42 grams less than their unexposed siblings. This is however based on an average treatment effect estimated on a sample of children that are up to five years of age. In addition, other studies like Mansour & Rees (2012) or Duncan et al. (2016) do not present exact estimates; rather they only present evidence on the likelihood that a child weighs less than 2500g at birth.

section, I will have some measurement error around the exact timing of pregnancy since I cannot identify whether the child was born prematurely. This is why I may observe - although imprecisely estimated - a positive effect in the last trimester.

In columns 3 and 6, I include a dummy variable for any violent incident in the three months prior to the conception date in order to account for differential selection into fertility. As anticipated above, I find evidence in line with negative selection into fertility. Children to mothers who were exposed to violence before pregnancy and still get pregnant generally exhibit worse health outcomes both in terms of height and weight. But even conditional on pre-pregnancy levels of violence, the coefficients (albeit marginally smaller in magnitude) are robust and more precisely estimated.

Mitigating factors. In a next step, I investigate potential mitigating factors. In Table 5, I interact the treatment with i) the age of the child, ii) age of the mother and iii) the gender of the child to explore heterogeneous effects in exposure to violence. In contrast to previous studies that measure anthropometric outcomes at birth, I am able to trace HAZ and WAZ for children throughout their first four years of life. This allows me to analyze whether the adverse effects of exposure to violence in-utero can be mitigated over time. In column 1 and 4 of Table 5, I interact violence with the age (in months) of children and find that over time children are able to catch-up with their siblings in terms of health outcomes. The coefficient implies that children need about three years to converge to their siblings. However, as noted above, the largest age gap in the sample is 2.5 years. Therefore, I don't observe full convergence and it is possible that some developmental gaps persist over time.

In a next step, I investigate whether the age of the mother plays a significant role in mitigating the adverse effects. On the one hand, older mothers may be more sensitive to adverse effects of stress since the likelihood of a risky pregnancy increases with age, even in the absence of exposure to violence. On the other hand, older mothers may also have more resources (both financial and social) to mitigate the adverse effects. Since more direct measures of resources are endogenous to exposure to violence (i.e. household income may itself be a consequence of violence) and therefore bad controls, I refute to age as a pre-determined proxy for both resources and risks. In columns 2 and 5, I do not find a significant relationship between the age of the mother and her ability to mitigate the consequences of violence, potentially due to the aforementioned countervailing forces.

Lastly, I investigate in columns 3 and 6, whether the gender of the child exacerbates the adverse effects of exposure to violence. It is possible that parents allocate resources differentially across gender and therefore contribute to a widening health gap among siblings. The coefficient of the interaction term is small in magnitude and negative but noisily estimated, indicating that there are no large differences in the effect of a low-casualty violent incident between boys and girls.

Table 5: **Mitigating factors: children catch up over time**

	Height for Age Z-Score			Weight for Age Z-Score		
	age of child (1)	age of mother (2)	female (3)	age of child (4)	age of mother (5)	female (6)
violence	-0.611***	-0.154	-0.108	-0.621***	-0.308*	-0.129**
	(0.109)	(0.174)	(0.0703)	(0.0930)	(0.176)	(0.0613)
violence × age of child (months)	0.0182***			0.0185***		
	(0.00315)			(0.00284)		
violence × age of mother		0.000683			0.00580	
		(0.00602)			(0.00591)	
violence × female			-0.0554			-0.0279
			(0.0651)			(0.0571)
Observations	22,640	22,634	22,640	22,640	22,634	22,640
Mothers	10,150	10,148	10,150	10,150	10,148	10,150
Districts	118	118	118	118	118	118
Adj. R-squared	0.280	0.277	0.277	0.316	0.313	0.313
Controls	Yes	Yes	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is the height for age Z-score in columns 1 to 3, and the weight for age Z-score in columns 4 to 6. The sample of violent incidents is restricted to those with only one casualty. The treatment is measured as a dummy variable for any violent incident during all of pregnancy. Interaction terms include the age of the child in months, the age of the mother in years, and the gender of the child with female equal to one. Age of child in months is included separately, age of mother is absorbed by mother fixed effects and gender of child is included in the set of controls. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Illness, cognitive skills and behavior. The effects of in-utero exposure to violence can also extend to cognitive, motor, and behavioral skills, as well as vulnerability of the immune system. Some of these outcomes are only available for a small but randomly chosen sub-set of households and are based on survey questions rather than direct measures taken by the enumerator (I list the questions and their coverage in Appendix Table A.3). In columns 1 and 2 of Table 6, I look at the likelihood that a child was sick with a cough or diarrhoea in the last two weeks. This question is available for all households and is less likely to suffer from recall or reporting bias. In column 1, I find that children that were exposed to a violent incident in-utero are more likely to have recently suffered from a cold but do not find a differential effect on the likelihood of diarrhoea in column 2.

Next, I look at the effect on cognitive skills and behavior of children for a small sub-set of approximately 2,200 children and 1,100 mothers. Cognitive skills include, for instance, the ability recognize letters or numbers. Behavioral outcomes include, for instance, whether the child is able to follow simple directions or gets along well with other children. Table A.4 shows the correlation between these variables and HAZ and WAZ of children. Overall the correlation is very low (no higher than 0.05) and is not significant in many cases, indicating that either cognitive skills and behavior are largely orthogonal to anthropometric measures or that there is measurement error or reporting bias on the side of mothers. I create two indicators of cognitive skills and behavior based on the first principle component of multiple questions listed in Appendix Table A.3 and report the results in columns 3 and 4, respectively. As mentioned above, the sample size is reduced by 90%.

Nevertheless, I find a negative effect - albeit noisily estimated - on the cognitive skills and behavior of children.

Table 6: **Violent incident during pregnancy and illness, cognitive skills and behavior**

	likelihood cough (1)	likelihood diarrhoea (2)	1st PC cognitive skills (3)	1st PC behavior (4)
violence	0.0217**	-0.0140	-0.123	-0.0612
	(0.00853)	(0.00882)	(0.151)	(0.178)
Observations	22,588	22,584	2,256	2,147
Mothers	10,129	10,124	1,116	1,063
Districts	118	118	117	117
Adj. R-squared	0.382	0.210	0.588	0.463
Controls	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes

OLS estimates on the likelihood of having had a cough (column 1) or diarrhoea (column 2) in the 2 weeks before the interview. The dependent variables in columns 3 and 4, are the first principle component of all cognitive and motor skills (questions EC8-EC11 of Appendix Table A.3) and behavioral outcomes (questions EC12-EC17 of Appendix Table A.3). These are only available for a random sub-sample of households. The sample of violent incidents is restricted to those with only one casualty. The treatment is measured as a dummy variable for any violent incident during all of pregnancy. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.4 Stress Mechanism

Perpetrator of violence: collateral casualties and directed violence. I leverage information on the perpetrator of violence to argue that some events are likely to be more stressful than others, holding the severity (i.e. the number of casualties) constant. As outlined in section 2, the IBC data allows me to distinguish between three perpetrators of violence: *i*) insurgent killings of civilians that occur in the course of attacking coalition or Iraqi government targets, *ii*) coalition killings of civilians by US or allied forces and *iii*) casualties caused by sectarian militia that did not happen during combat with coalition forces, i.e. these attacks targeted the civilian population directly.[8] These events are arguably more stressful.

I test this hypothesis in Table 7 and distinguish between the three perpetrators in contexts where one unique perpetrator could be assigned to the death of one civilian. I compare health outcomes between siblings with and without pre-natal exposure to violence perpetrated by either sectarian (columns 1 and 4), insurgent (columns 2 and 5) and coalition (columns 3 and 6) forces, dropping from the sample sibling pairs that have been exposed to violence by the other perpetrators.

[8]There may be concerns that sectarian violence is just a proxy for violence of extreme intensity, e.g. long duration and many casualties. While this concern would already be remedied by the restriction to one casualty events, the correlation coefficients in Table A.5 confirm that this concern is not valid. In fact, there is no significant correlation between the duration of an violent incident and the perpetrator, and the number of casualties is even negatively correlated with sectarian caused incidences.

While all violent incidents exhibit a negative sign, the effect is largest and most precisely estimated for sectarian violence. This is in line with the idea that sectarian violence is more stressful for mothers.

Notably, the effect of coalition violence is close to zero. This does not negate the adverse effects of coalition and on child health overall but indicates that they are less likely to operate through the stress mechanism. This suggests that the effects of in-utero exposure to violence on child health can be heterogeneous and - in addition to their effect on local infrastructure - also depend on their psycho-social impact on the civilian population.

Table 7: **Stress mechanism: perpetrator of violence**

	Height for Age Z-Score			Weight for Age Z-Score		
	sectarian (1)	insurgent (2)	coalition (3)	sectarian (4)	insurgent (5)	coalition (6)
violence$_{trim1}$	-0.182***	-0.152**	-0.0325	-0.181***	-0.119**	-0.0187
	(0.0655)	(0.0761)	(0.128)	(0.0502)	(0.0581)	(0.106)
violence$_{trim2}$	-0.0997*	-0.0618	-0.0604	-0.0972**	-0.0621	0.0456
	(0.0594)	(0.0923)	(0.104)	(0.0475)	(0.0716)	(0.0947)
violence$_{trim3}$	0.130*	0.101	-0.123	0.0678	-0.0266	-0.0670
	(0.0686)	(0.0903)	(0.121)	(0.0496)	(0.0572)	(0.108)
Observations	20,821	14,158	12,722	20,821	14,158	12,722
Districts	118	117	117	118	117	117
Adj. R-squared	0.282	0.258	0.250	0.314	0.280	0.277
Controls	Yes	Yes	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is the height for age Z-score in columns 1 to 4, and the weight for age Z-score in columns 5 to 8. The sample of violent incidents is restricted to those with only one casualty. The treatment is measured as a dummy variable for any violent during each trimester. Columns 1 and 5 restrict violent incidents to those that were caused by a direct gunshot. Column 2 to 4 and 6 to 8, look at the perpetrator of violence when they are unique and can be identified, distinguishing between sectarian, insurgent and coalition violence. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Types of violence: stress versus damages to infrastructure. Next, I look at different types of violence, distinguishing between direct gunshots, bombings, airstrikes and missiles, suicide bombings and torture and executions in Table 8. The categories are not mutually exclusive, since - for instance - executions of civilians often involve a direct gunshot, or bombings can include suicide bombings but also other types of explosions. As shown in Table 2, the type of violence is closely linked to the perpetrator of violence. Sectarian violence is largely associated with execution and torture as well as gunfire. Insurgent agents typically revert to suicide bombings. Airstrikes and missiles typically involve coalition forces.

In a first step, I compare types of violence that impact access to pre-natal care to varying degrees. For instance, it is unlikely that the killing of one civilian with a direct gunshot causes damages to the infrastructure or trigger curfews, as those are usually initiated after bombs or explosions or more large scale events. In columns 1 and 6 of Table 8, I restrict the sample of incidents to those that came from direct gunfire, again excluding from the control group mothers that have been exposed to other types of violence during pregnancy. Despite this limitation, I detect an adverse effect - albeit smaller than in the baseline regression - of direct gunshots on height and weight scores of children. Conversely, bombings are more likely to impact the health infrastructure more substantially. I test this in columns 2 and 7 and find - as expected - that the effect size is substantially larger, reflecting a compound effect of stress and access to pre-natal care. Next, in columns 3 and 4, as well as 8 and 9, I look at other events that may have a large effect on the infrastructure. Airstrikes, missiles and suicide bombings negatively impact the height and weight for age Z-score of children, although the estimates are noisy.[9]

Lastly, I investigate another type of violence that is arguably particularly stressful. The majority of sectarian violence is directed at the civilian population and involves gruesome acts of torture and execution, typically involving a public display of violence (either through exposure to the victims of this violence and propagation on social media). This violence is individualized and may not even involve the use of high impact weapons. I look at these incidents in columns 5 and 10 and find a large and negative effect on children's HAZ and WAZ. The effect size is statistically indistinguishable from bombings, suggesting that in some cases the severity of stress may even exceed the compounding effect of access to pre-natal care.

[9]Restricting the sample to these events reduces the number of treated mothers substantially since one-casualty suicide bombings and airstrike are rare: the IBC records 125 suicide bombings with one casualty and 528 airstrikes and missiles with one casulaty compared to approximately 6,000 incidents involving gunfire.

Table 8: **Stress mechanism: type of violence**

	Height for Age Z-Score					Weight for Age Z-Score				
	direct gunshot (1)	all bombings (2)	airstrikes & missiles (3)	suicide bombings (4)	torture & executions (5)	direct gunshot (6)	all bombings (7)	airstrikes & missiles (8)	suicide bombings (9)	torture & executions (10)
$violence_{trim1}$	-0.135**	-0.237**	-0.190**	-0.293	-0.214**	-0.104**	-0.279***	-0.0358	0.0699	-0.162***
	(0.0621)	(0.0913)	(0.0849)	(0.225)	(0.101)	(0.0475)	(0.0909)	(0.0600)	(0.143)	(0.0617)
$violence_{trim2}$	-0.0754	-0.172*	-0.0198	-0.0866	-0.0989	-0.0775	-0.148**	-0.0188	0.0218	-0.0948
	(0.0608)	(0.101)	(0.110)	(0.227)	(0.0984)	(0.0511)	(0.0688)	(0.0912)	(0.206)	(0.0746)
$violence_{trim3}$	0.156**	-0.00726	-0.226	-0.335	0.0641	0.0425	0.00187	-0.163	-0.0977	-0.0328
	(0.0671)	(0.0919)	(0.139)	(0.322)	(0.127)	(0.0541)	(0.0787)	(0.120)	(0.170)	(0.0804)
Observations	20,146	14,283	11,991	9,707	13,998	20,146	14,283	11,991	9,707	13,998
Mothers										
Districts	118	118	115	107	118	118	118	115	107	118
Adj. R-squared	0.269	0.267	0.258	0.241	0.276	0.298	0.291	0.280	0.270	0.305
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is the height for age Z-score in columns 1 to 5, and the weight for age Z-score in columns 6 to 10. The sample of violent incidents is restricted to those with only one casualty. The categories are not mutually exclusive. Columns 1 and 6 restrict violent incidents to those that were caused by a direct gunshot. Columns 2 and 7 report results on all bombings (including suicide bombings). Columns 3 and 8 report results on airstrikes and missiles. Columns 4 and 9 report results for suicide bombings specifically and columns 5 and 10 show results for one civilian casualty killed through torture and execution. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.5 Robustness Checks

Definition of treatment and outcome. I verify that my results are robust to changes in the measurement of the treatment and the outcomes. In Appendix Table A.6, I show that using height and weight of children as a percentage of the median in columns 1 and 2 does not alter the results. In columns 3 and 4, I also show that - albeit more noisily estimated - my results are largely robust to using the WHO definition of height and weight for age Z-scores. However, the WHO measures are less granular and the distribution is more condensed (scale reaching from -6 to 6, rather than -10 to 10) and may therefore not pick up small changes in height and weight.

In addition, I change the definition of the treatment. So far, I have focused on events with only one casualty to rule out that the effect is driven by damages to the infrastructure. In Appendix Table A.7, I consider all violent incidents irrespective of their severity. Including these events may have two countervailing forces on the size of the coefficient: on the one hand, the coefficient may increase since the level of stress increases with the severity of the event and damages to the infrastructure may exacerbate the adverse the effect of child health. On the other hand, these events may be more likely to trigger miscarriages or lead to internal migration of those that were most affected and therefore underestimate the negative effects on child health. The coefficients in column 1 and 3 are slightly smaller but not significantly different from the coefficients of the main estimation, potentially because both of the aforementioned forces are at play. In columns 2 and 4, I consider a different measure for severity, namely the number of violent incidents during pregnancy. In line with the findings of Camacho (2008), additional violent incidents during pregnancy do not deteriorate child health further.

Sample composition. In a next step, I verify whether districts with very high or low levels of violence are driving the results. This exercise addresses two concerns. First, selection into or out of these districts may contaminate the control group. For instance, the most resourceful mothers are able to move to districts that are known to be very safe. If these children are generally healthier, then we may overestimate the true effect of being exposed to violence during pregnancy. Conversely, while low casualty incidents may not be predictable, in districts with generally high levels of violence they may be correlated with large scale incidents and may thus overestimate the effect. In Appendix Table A.9, I address these points by dropping the 10 districts of Baghdad, which are – as the capital of the country – victim to many of the violent incidents in columns 1 and 5 and by dropping all districts in the Kurdish autonomous region, which belong to the safest in the country in columns 2 and 6. In columns 3 and 7, I also exclude all districts that fall into the 90th percentile of violent incidents between 2006 and 2009. The results are robust to these changes.

Lastly, I repeat the main analysis but exclude sibling pairs that have both been exposed to violence. The interpretation of the coefficient changes from a comparison of sibling pairs where only one child was exposed to violence with sibling pairs where either both or none have been exposed to violence in-utero to a comparison of sibling pairs where one has been exposed to violence with sibling pairs where none have been exposed to violence. The coefficient is robust to these changes and is statistically indistinguishable from the coefficients in the baseline regression.

4 Conclusion

This paper provides novel evidence on the effect of in-utero exposure to violence on child health in the medium-run, focusing on stress as a possible mechanism through which adverse consequences of war are transmitted across generations. Detailed information on the type of severity of violence combined with micro-level data on child health allows me to overcome important empirical challenges in the literature. Specifically, I am able to address concerns about selection into fertility as well as selection into exposure to violence. In addition, I can follow children over the first years of life and can rely on anthropometric outcomes measured by enumerators and a broader set of health outcomes, including cognitive and behavioral skills. Lastly, this paper is the first to show that the type of violence matters for the health outcomes of children.

I show that one single violent incident during pregnancy significantly increases the risk of stuntedness and malnutrition. I also find suggestive evidence that in-utero exposure to violence increases children's likelihood of contracting illnesses, and deteriorates cognitive skills and behavioral outcomes. However, children are able to catch up - but potentially not fully converge - over time. Focusing on exposure to violence with arguably little effect on the infrastructure and access to pre-natal care as well as comparing violent incidents that are more stressful for the civilian population allows me to improve on establishing stress as a plausible mechanism.

My results emphasize that an additional act of violence during pregnancy can still have a strong adverse effect on child health in an environment where violence has remained persistently high over the last 15 years. The paper also uncovers heterogeneous effects of different types of violence, showing that incidents with a low number of casualties but a large psychological impact on the civilian population can be detrimental for child health. Against the backdrop of the large literature on the long-run effects of adverse health outcomes of children, the results highlight the importance policies and interventions that target pregnant women particularly in places where ethnic tensions are high.

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5 Appendix

Table A.1: Severity of Violence by Perpetrator

	mean	min	max	N
Coalition Forces				
Duration (days)	1.48	1	31	1179
Casualties	6.48	1	670	1179
Insurgent Agents				
Duration (days)	1.04	1	16	2826
Casualties	3.58	1	80	2826
Sectarian Agents				
Duration (days)	1.29	1	1077	12874
Casualties	3.11	1	1005	12874
Total				
Duration (days)	1.26	1	1077	16879
Casualties	3.42	1	1005	16879

Based on ESOC data (Iraq Body Count Project and Condra & Shapiro (2012) data set). Summary stats are provided for incidences with one unique and identified perpetrator; events with unknown perpetrator (N=981) or multiple perpetrators (N=602) are not reported.

Table A.2: **Successively introducing controls and fixed effects: Violent incident during pregnancy deteriorates child health**

	Height for Age Z-Score				Weight for Age Z-Score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
violence_{all}	-0.183***	-0.190***	-0.118**	-0.135**	-0.0505	-0.0551	-0.116***	-0.143***
	(0.0600)	(0.0596)	(0.0521)	(0.0588)	(0.0536)	(0.0535)	(0.0420)	(0.0514)
Observations	22,640	22,640	22,640	22,640	22,640	22,640	22,640	22,640
Mothers	10,150	10,150	10,150	10,150	10,150	10,150	10,150	10,150
Districts	118	118	118	118	118	118	118	118
Adj. R-squared	0.002	0.004	0.261	0.277	0.000	0.002	0.298	0.313
Controls		Yes	Yes	Yes		Yes	Yes	Yes
Mother FE			Yes	Yes			Yes	Yes
YoB × district FE				Yes				Yes

	mean (cm)	sd	mean(kg)	sd
Age 0	64.22	15.87	6.98	3.39
Age 1	78.65	24.53	10.44	2.87
Age 2	87.18	14.98	12.66	3.45
Age 3	95.26	20.12	14.62	3.36
Age 4	102.26	20.08	16.41	3.34

The dependent variable is the height for age Z-score in columns 1 to 4, and the weight for age Z-score in columns 5 to 8. Average height and weight by age as well as standard deviations are reported at the bottom of the table. The sample of violent incidents is restricted to those with only one casualty. The treatment is measured as a dummy variable for any violent incident during all of pregnancy. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: MICS questions on illness, cognitive, motor and behavioral skills

Code	Category	Question	Scale	Coverage
CA1		Child had diarrhoea in last 2 weeks	0-1	full sample
CA7		Child ill with cough in last 2 weeks	0-1	full sample
EC8	cognitive	Child identifies at least ten letters of the alphabet	0-1	random sample
EC9	cognitive	Child reads at least four simple, popular words	0-1	random sample
EC10	cognitive	Child knows name and recognizes symbol of all numbers from 1-10	0-1	random sample
EC11	cognitive	Child able to pick up small object with 2 fingers	0-1	random sample
EC12	beahvior	Child sometimes too sick to play	0-1	random sample
EC13	beahvior	Child follows simple directions	0-1	random sample
EC14	beahvior	Child able to do something independently	0-1	random sample
EC15	beahvior	Child gets along well with other children	0-1	random sample
EC16	beahvior	Child kicks, bites or hits other children or adults	0-1	random sample
EC17	beahvior	Child gets distracted easily	0-1	random sample

Table A.4: Correlation among outcomes

		Mean	S.D.	1	2	3	4	5	6	7	8	9	10	11
1	HAZ	-0.57	2.08											
2	WAZ	-0.26	1.84	0.74										
				(0.00)										
3	letters	0.29	0.45	0.03	0.05									
				(0.00)	(0.00)									
4	words	0.12	0.32	0.02	0.05	0.39								
				(0.01)	(0.00)	(0.00)								
5	numbers	0.21	0.41	0.05	0.06	0.31	0.34							
				(0.00)	(0.00)	(0.00)	(0.00)							
6	objects	0.90	0.30	0.01	-0.00	0.06	0.07	0.10						
				(0.14)	(0.87)	(0.00)	(0.00)	(0.00)						
7	sick	0.54	0.50	0.01	0.01	0.03	0.01	-0.05	0.08					
				(0.42)	(0.30)	(0.00)	(0.25)	(0.00)	(0.00)					
8	directions	0.82	0.39	0.00	0.01	0.12	0.07	0.11	0.17	0.05				
				(0.74)	(0.54)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)				
9	indep	0.74	0.44	0.01	-0.01	0.11	0.07	0.11	0.17	0.01	0.43			
				(0.56)	(0.46)	(0.00)	(0.00)	(0.00)	(0.00)	(0.52)	(0.00)			
10	children	0.84	0.37	0.01	0.02	0.06	0.03	0.07	0.10	-0.00	0.17	0.13		
				(0.47)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.60)	(0.00)	(0.00)		
11	kicks	0.26	0.44	-0.02	-0.02	-0.05	0.00	-0.04	-0.03	0.07	-0.10	-0.07	-0.43	
				(0.01)	(0.01)	(0.00)	(0.59)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
12	distract	0.44	0.50	0.00	-0.01	0.01	0.02	-0.04	-0.04	0.14	-0.03	-0.04	-0.06	0.12
				(0.96)	(0.23)	(0.10)	(0.07)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Significance levels in brackets. HAZ = Height for age z-score NCHS; WAZ = Weight for age z-score NCHS; letters = Child identifies at least ten letters of the alphabet; words = Child reads at least four simple, popular words; numbers = Child knows name and recognizes symbol of all numbers from 1-10; objects = Child able to pick up small object with 2 fingers; sick = Child sometimes too sick to play; directions = Child follows simple directions; indep = Child able to do something independently; children = Child gets along well with other children; kicks = Child kicks, bites or hits other children or adults; distract = Child gets distracted easily

Table A.6: **Alternative measures for height and weight**

	Height for Age percent of median (1)	Weight for Age percent of median (2)	Height for Age Z-Score WHO (3)	Weight for Age Z-Score WHO (4)
violence$_{trim1}$	-4.035*	-4.681**	-0.265***	0.0282
	(2.365)	(2.160)	(0.0534)	(0.0318)
violence$_{trim2}$	-0.338	-0.679	-0.116**	0.0136
	(2.195)	(1.965)	(0.0494)	(0.0297)
violence$_{trim3}$	1.104	1.123	0.0281	-0.00729
	(2.443)	(2.531)	(0.0559)	(0.0400)
Observations	22,640	22,640	22,640	22,640
Mothers	10,150	10,150	10,150	10,150
Districts	118	118	118	118
Adj. R-squared	0.296	0.315	0.252	0.299
Controls	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes

The dependent variables are alternative measures for the height and weight of children. In columns 1 and 2, they are expressed as percentages of the median, in columns 3 and 4 they are measured in Z-scores according to the WHO, rather than NHCS in the main specification. The sample of violent incidents is restricted to those with only one casualty. The treatment is measured as a dummy variable for any violent incident during all of pregnancy. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Correlation between Perpetrator and Severity of Attack

	Coalition	Insurgent	Sectarian
Duration (Days)	0.0090	-0.0110	0.0054
	0.2205	0.1347	0.4629
Casualties	0.0609***	0.0043	-0.0284***
	0.0000	0.5597	0.0001

Table A.5 shows pairwise correlation between perpetrator and severity of incident, measured as the duration in days and the number of casualties. P-values are reported under the correlation coefficients.

Table A.7: **Including large scale violent incidents and frequency of violent incidents**

	Height for Age Z-Score		Weight for Age Z-Score	
	(1)	(2)	(3)	(4)
violence_{all}		-0.192***		-0.224***
		(0.0526)		(0.0451)
violence_{trim1}	-0.197***		-0.184***	
	(0.0518)		(0.0425)	
violence_{trim2}	-0.0480		-0.0841**	
	(0.0473)		(0.0395)	
violence_{trim3}	0.0722		0.0437	
	(0.0532)		(0.0458)	
frequency violence_{trim1}		0.00107		0.00135
		(0.00216)		(0.000883)
frequency violence_{trim2}		0.00127		-0.00116
		(0.00336)		(0.00151)
frequency violence_{trim3}		-0.00186		-0.000558
		(0.00219)		(0.00125)
Observations	22,640	22,640	22,640	22,640
Mothers	10,150	10,150	10,150	10,150
Adj. R-squared	0.278	0.278	0.313	0.313
Controls	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes

The dependent variable is the height for age Z-score in columns 1 to 2, and the weight for age Z-score in columns 3 to 4. The sample of violent incidents is expanded to *all* violent incidents, including large scale violent events. In columns 1 and 3, we include a dummy variable for any violent incident occuring in each trimester of the pregnancy. The top row of columns 2 and 4 is a dummy variable for any violent incident during all of pregnancy and includes the number of violent incidents in each trimester separately. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Iraq Household Socio-Economic Survey: Reasons for Migration

Reason	**N**	**share of subsample (%)**	**Share of full sample (%)**
work	230	14.51	0.131
study or complete study	42	2.65	0.024
marriage, divorce or becoming widow	278	17.54	0.158
accompany household	733	46.25	0.419
forced displacement or migration	51	3.22	0.029
back from forced displacement or migration	83	5.24	0.047
displacement for security reason	6	0.38	0.003
conventional armed conflict	23	1.45	0.013
civil conflict	14	0.88	0.008
other	124	7.82	0.071
Observations	1,584		

Table A.8 uses a different data source, namely the Iraq household socio-economic survey of 2012 that asks the question "Did you stay for 6 months continuously in another place?" If yes, "What is the main reason for changing the place of residence?". The answers to the latter question are presented in this table. The full sample consists of more than 175,000 respondents, the relevant sample of migrants within the 2006 to 2009 observation period is 1,584.

Table A.9: **Sample composition: excluding high and low violence districts and mothers with all children treated**

	Height for Age Z-Score				Weight for Age Z-Score			
	excl. Baghdad	excl. Kurdistan	excl. 90th prctle violence	contr. group none treated	excl. Baghdad	excl. Kurdistan	excl. 90th prctle violence	contr. group none treated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
violencetrim1	-0.210***	-0.210***	-0.233***	-0.184***	-0.175***	-0.179***	-0.181***	-0.117**
	(0.0554)	(0.0572)	(0.0571)	(0.0610)	(0.0423)	(0.0435)	(0.0419)	(0.0453)
violencetrim2	-0.0419	-0.0450	-0.0508	0.000613	-0.0461	-0.0560	-0.0557	-0.0304
	(0.0578)	(0.0563)	(0.0568)	(0.0740)	(0.0430)	(0.0396)	(0.0424)	(0.0543)
violencetrim3	0.139**	0.134**	0.118**	0.0988	0.0480	0.0688	0.0403	0.0441
	(0.0582)	(0.0627)	(0.0595)	(0.0741)	(0.0493)	(0.0480)	(0.0487)	(0.0520)
Observations	21269	18808	21381	18593	21269	18808	21381	18593
Districts	108	85	109	118	108	85	109	118
Adj. R-squared	0.257	0.286	0.277	0.252	0.299	0.320	0.314	0.286
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YoB × district FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is the height for age Z-score in columns 1 to 4, and the weight for age Z-score in columns 5 to 8. Columns 1 and 5 exclude the whole of Baghdad, columns 2 and 6 exclude the Kurdish autonomous region and columns 3 and 7 exclude districts in the 90th percentile of violent incidents in the observation period between 2006 and 2009. Columns 4 and 8 only include as a control group mothers where no child has been exposed to violence in-utero. Control variables at the child level include gender, line number and a dummy variable for twins. I also include year of birth dummy variables multiplied with district fixed effects, as well as mother fixed effects. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.