Health-Seeking Amidst Violence: Evidence from the Philippines

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Abstract

That crime and conflict can negatively impact child health is well-documented, but one potentially important mechanism has received little attention: do increases in local violence reduce the utilization of curative and preventative care? Combining a database of violent events with the 2013 Demographic and Health Survey of the Philippines, I exploit within-location variation in violence over time. I find that violence reduces the probability a mother takes her sick child to a health facility, gives birth in a hospital, and vaccinates her children.

Keywords: violence, healthcare utilization, Philippines

JEL Codes: I12; D74

1 Introduction

Violence – whether manifested in the form of criminal activity or civil conflict – is costly (Anderson 1999; Blattman and Miguel 2010), and these costs are not limited to the direct damage to human life and physical infrastructure. For instance, a number of studies have documented that exposure to violence has negative effects on human capital accumulation, and in particular, on child health.[1] Exposure to violence in-utero or in childhood has been shown to negatively impact birth weight (Camacho 2008; Mansour and Rees 2012; Duque 2017; Brown 2018), height-for-age in childhood (Alderman, Hoddinott, and Kinsey 2006; Bundervoet, Verwimp, and Akresh 2009; Akresh, Verwimp, and Bundervoet 2011; Shemyakina 2015; Duque 2017), adult height in later years (Alderman et al. 2006), as well as cognitive and non-cognitive skills (Duque 2017). To explain these effects, the existing literature has emphasized three important mechanisms: maternal stress, poor nutritional intake, and heightened vulnerability to disease.[2]

Though these direct mechanisms are clearly important, this paper explores another way in which exposure to violence can affect a child's health: by increasing the non-monetary costs of healthcare. In particular, individuals who live in a violent environment face a greater risk of encountering violence in their day-to-day life outside the home: shopping, en route to school or work, or on the way to a doctor or health facility. If fear of harm translates into a higher cost of healthcare in an individual's decision problem, increases in violence could have detrimental effects on child health by reducing the utilization of both curative and preventative care. Reductions in any

[1] There is also a related body of research on the effects of conflict on education outcomes (Akresh and De Walque 2008; Chamarbagwala and Morán 2011; Shemyakina 2011; Brown and Velásquez 2017), which I will not be exploring in this paper.

[2] First, maternal stress during pregnancy can hinder fetal development. Secondly, for young children in households that have been displaced or whose sources of income (crops, livestock) have been destroyed by large-scale violence, the quality of nutritional intake falls dramatically. Finally, the displacement of households, along with the destruction of infrastructure, can also make children more vulnerable to disease.

type of healthcare utilization could be damaging, but reductions in curative care in developing country contexts are particularly dangerous. Not seeking treatment for diseases like pneumonia, malaria, and diarrhea, the three leading causes of child mortality (World Health Organization 2017), could mean the difference between life and death.

This paper provides empirical evidence for this hypothesis, which has been proposed in other contexts but for which little evidence currently exists. The idea that security fears generated by heightened violence might lead to temporary changes in behavior has been suggested by studies on the relationship between violence and other non-health-related variables. For example, Chamarbagwala and Morán (2011) and Shemyakina (2011) use security fears as an explanation for their finding that violence negatively affects educational attainment (but do not explicitly test this hypothesis), while Brown and Velásquez (2017) find that other mechanisms (financial ones, specifically) are more important than security fears in explaining the negative effect of violence on educational attainment in their study. Relatedly, Velásquez (2015) emphasizes increased security fears as the main explanation for the finding that violence reduces labor supply for self-employed women. In summary, current evidence directly addressing this hypothesis is sparse, somewhat mixed, and focused on areas outside health.

There is also a related literature that explores how violence can change household production, investment, and labor decisions (Bundervoet 2006; Ibáñez, Muñoz-Mora, and Verwimp 2013; Arias, Ibáñez, and Zambrano 2014; Fernández, Ibáñez, and Peña 2014; Rockmore 2014), but these studies tend to focus on how decisions respond to the increase in income risk caused by violence, and not to the increased fear of victimization. Also of relevance are the studies documenting that exposure to violence can change risk preferences (Voors et al. 2012; Callen et al. 2014; Brown et al. 2017; Jakiela and Ozier 2015), which could result in even larger responses than if violence were only a cost shifter. In short, there are many reasons to hypothesize that violence could alter parents' health-seeking in a way that is detrimental to child health.[3]

The question of whether increases in local violence can affect healthcare utilization is particularly relevant for the Autonomous Region of Muslim Mindanao (ARMM) in the Philippines, a conflict-ridden region that also faces greater health challenges compared to the rest of the country (DHS 2005). This paper explores whether the violence in the ARMM makes individuals less likely to seek preventative and curative care, which could drive or exacerbate their health disadvantages. I acknowledge, of course, that other economic and social factors must also play a large role in explaining the poorer health outcomes in the ARMM compared to the other regions of the Philippines.

Combining a database of recorded violent events with the 2013 Demographic and Health Survey (DHS) of the Philippines, I use location-specific variation in violent events over time to answer this research question. I find that increases in the occurrence of violent events reduce both curative and preventative healthcare utilization among mothers of young children. A one standard deviation increase in violence in a given month decreases the likelihood a mother brings her child to a health facility that month, and if pregnant, the likelihood she delivers at a hospital instead of at home – by approximately 3 to 4 percentage poins for all outcomes. Violence during the first year of a child's life reduces (by 7 percentage points) the probability of that child receiving the recommended vaccinations. My results do not appear to be driven by price shocks, weather shocks, or government investment projects that might be correlated with changes in violence, or by selective migration or mortality.

[3]Violence can also decrease healthcare utilization through mechanisms unrelated to parental behavior: by damaging health infrastructure or causing other disruptions in service provision. Although data limitations make it difficult to separate the supply-side effects from the behavioral responses discussed above, I address this issue in section 4.3.

I find suggestive evidence that a demand-side mechanism (rather than a supply-side mechanism) is driving these results. The proposed demand-side mechanism – that security fears are leading to reductions in the demand for healthcare – closely relates to recent evidence from the Philippines. Berman, Downey, and Felter (2016) find that expanding governance into previously unstable areas significantly improves child weight-for-age. The authors posit that the effects could be due in large part to improvements in security, which is consistent with the argument made in this paper. Though data limitations preclude the analysis of health outcomes in my sample, existing knowledge on the importance of both curative and preventative care suggests this costly avoidance behavior could have lasting negative health effects.

2 Background and Data

2.1 Violence in Mindanao

Mindanao is the second largest island of the Philippines, a predominantly Catholic nation which was colonized by the Spanish in the 16th century. This southern island's proximity to Indonesia and Malaysia brought Islam to the region long before the arrival of the Spanish, who were never able to gain full control of the area. It was not until the end of the Philippine-American War at the beginning of the 20th century that Mindanao was brought under central control. Tensions rose in the 1960's, when central government resettlement policies brought an influx of Christian settlers to the region, resulting in Mindanao having a Christian majority overall, with Muslim majorities in the provinces that now make up the ARMM (Schiavo-Campo and Judd 2005).

Growing resentment toward these resettlement policies and the resulting land disputes, as well as increased logging and mining activities in the area, led to the formation of the first resistance group – the Moro National Liberation Front (MNLF) –

in the late 1960's. Since then, other separatist groups have split off from the MNLF, including the Moro Islamic Liberation Front (MILF) and the more radical Abu Sayyaf, the group responsible for kidnappings and bombings that made international headlines in the 2000's. Other groups operating in the area include Communist separatist groups and clan militias (BBC Oct 8, 2012).[4]

Throughout this time period, different government administrations have attempted to respond to the rebel movement in various ways. In 1989, the ARMM was formed,[5] giving Muslim-majority provinces a degree of self-rule and paving the way for MNLF leaders to take official roles in the government. After the signing of the Final Peace Agreement between the government and the MNLF in 1996, MNLF leader Nur Misuari became governor of the ARMM. This agreement widened the rift between the MNLF and the MILF, which continued to launch attacks against the military. In 2000, an "all-out-war" policy was declared against this breakaway group (Adriano and Perks 2013). Since then, periods of relative calm have been interrupted by acts of terrorism and periods of escalating violence. The period spanned by my data coincides with a new push for peace by the Aquino administration, although criminal activity and violence continued throughout (Whaley 2013). It is this region's history of conflict that motivated the collection of the violence data that I describe in the next section, but the violence that takes place during my sample period is better characterized by smaller-scale violent crime than full-blown civil conflict.

[4] In the spring of 2017, over 3 years after the period analyzed in this paper, militants who had pledged loyalty to yet another extremist group – ISIS – captured the city of Marawi (Solomon and Villamor 2017).

[5] Initially, the ARMM consisted of Maguindanao, Lanao del Sur, Sulu, and Tawi-Tawi. Basilan joined in 2001. (Adriano and Perks 2013)

2.2 Violence Data

To create a measure of violence intensity, I use a list of violent events recorded by the Bangasamoro Conflict Monitoring System (BCMS 2014). Specifically intended to monitor the conflict in the ARMM, the BCMS focuses on the provinces of Maguindanao, Lanao del Sur, Sulu, Basilan, and Tawi-Tawi.[6] The dataset contains a list of violent events that took place from 2011 to 2014, including the event date and location.[7] The data is compiled from crime and conflict records of regional police offices in the ARMM and then supplemented with information from media reports.

The BCMS defines violent conflict as "incidents where two or more parties[8] use violence to settle misunderstandings and grievances and/or defend and expand their individual or collective interests." Therefore, although the ARMM is particularly conflict-ridden due to the presence of rebel groups, the events that are recorded are not limited to those with rebel group involvement. This differentiates this dataset from more commonly used conflict databases like the Armed Conflict Location and Event Data (ACLED) and PRIO Armed Conflict Dataset, which focus on politically-motivated violence, protests, and armed conflict.[9] In the BCMS, around 12% of events from 2011 to 2014 had a rebel group listed as one of the actors involved; over half were civilian-only events. I include events involving non-rebel group actors in my analysis because this measure of violence should better capture the general level of perceived danger in a community, which should be influenced by all types of violence,

[6] Isabela City and Cotabato City are geographically located in the ARMM provinces of Basilan and Maguindanao, respectively, but are not technically part of the ARMM. They are, however, monitored and included in the BCMS and therefore in my analysis.

[7] The province of each event is recorded for all events, municipality for 99%, and barangay, which is the smallest administrative region, for around 80% of all records. For data completeness, I use counts at the municipality-level. For events missing a municipality, I record that event as having occurred in all of the municipalities in the recorded province. My results are also robust to simply dropping events missing a municipality.

[8] This includes incidents with a perpetrator and a victim.

[9] The BCMS is especially useful because the PRIO data is not available for the Philippines, and, since the ACLED was only recently expanded to include the Philippines, ACLED data only dates back to 2016 (which does not overlap with my sample period).

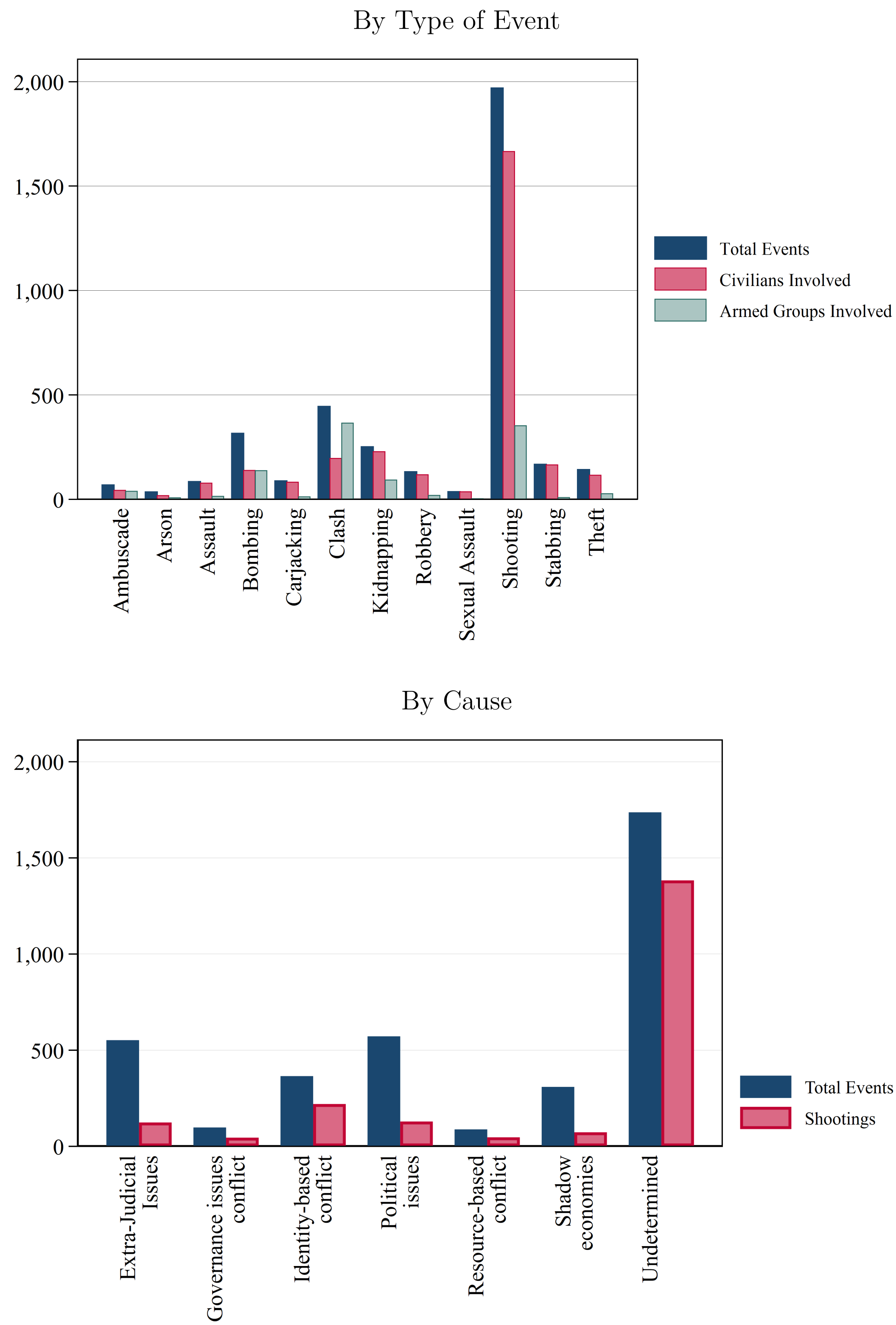
not just rebel-specific events.

I calculate the total number of violent events per municipality-month, excluding certain categories of events which are less likely to have direct effects on an individual's perceived personal safety.[10] The first panel of Figure 1 summarizes the distribution of events by type, for all of the violent events from 2011 to 2014. This figure also separately illustrates the distribution for events where civilians were reported to be involved and for events where armed groups were reported to be involved (these two are not mutually exclusive). Civilians were involved in the vast majority of events, and shootings were by far the most common type of incident.

The second panel of Figure 1 illustrates the distribution of violent events by cause. For just under half of the events, the reason for the violence was undetermined. Among the remainder, the most common causes were political issues, extra-judicial issues, and identity-based conflict, which includes clan-based feuds known as *rido*.

[10] Excluded categories include: threats, extortion, domestic violence, property crime, and swindling, which make up less than 4% of total events and are less likely to affect an individual's fear about going outside due to their either more personal, targeted, or less overtly violent nature.

Figure 1 Number of Violent Events Recorded in the BCMS, 2011-2014



2.3 Healthcare Utilization Data

I obtain information on healthcare utilization from the 2013 Philippines Demographic and Health Survey (DHS), designed to collect data on maternal and child health, fertility, family planning, and other related issues. Between August and October of 2013, the DHS interviewed a stratified random sample of households, representative at the national and regional level, in order to identify eligible women aged 15 to 49. These women were asked about their complete birth histories, followed by more detailed questions about all children born since 2008.

For each of these pregnancies, mothers were asked for detailed information, including the place of delivery and vaccinations their child received after birth. For the most recent pregnancy only, mothers provided information about prenatal visits. In addition, mothers answered questions about the current health of these children: whether they were sick in the last two weeks and if so, whether they were taken to a health facility for consultation or treatment.

The DHS questionnaire also includes detailed information about household and individual demographic characteristics, which I include as controls. Specifically, my main specification controls for child gender, child age, mother's age, and mother's education. In other specifications, I also control for a household wealth score, an indicator for a Muslim mother, household size, the number of children in the household under 5 years old, the number of women in the household aged 15 to 49, an indicator for a female head of household, household head age, and the number of living children born to the mother.

2.4 Other Variables

In all of my specifications, I control for temperature and rainfall, which I obtain from NOAA's Global Historical Climatology Network (GHCN) monthly mean dataset

(Lawrimore et al. 2011). I also use municipality-level characteristics from summaries of the 2010 Philippine Census tabulated by the Philippine Statistics Authority and province-level counts of government workers from Department of Health records (National Epidemiology Center 2011, 2012, 2013). In robustness checks, I control for the occurrence of natural disasters, which I obtain from the EM-DAT International Disaster Database (Guha-Sapir, Below, and Hoyois 2016), ongoing or completed community-based infrastructure projects, which I obtain from the ARMM Social Fund Project (2014), and crop prices, which I obtain from the Philippines Statistic Authority (2016).

2.5 Summary Statistics

Table 1 reports the means and standard deviations of the demographic variables that I use as controls, for all children in the DHS (born to eligible mothers starting in 2008). The first column reports statistics for my sample of interest (the ARMM), and the second column describes provinces outside the ARMM. In the ARMM, which is clearly very different from the rest of the country, mothers are less educated, households are poorer and larger, and fertility is higher.

Differences also exist in the healthcare utilization patterns of this region, as shown in Table 2. Mothers in the ARMM are significantly less likely to take their sick children a health facility. Similarly, ARMM mothers are less likely to give birth in a health facility. The magnitudes of these differences are not trivial. For example, women in the ARMM are more than twice as likely to deliver at home than those living in other regions. Prenatal care seeking is significantly higher outside of the ARMM, as are all vaccination rates.

Because the violence data is only available for the ARMM from 2011 to 2014, I restrict my sample to individuals living in the ARMM. Although this results in small sample sizes and is clearly not representative of the country as a whole, residents of the ARMM certainly form a population of interest, given their lower socioeconomic status, starkly different healthcare utilization patterns, and of course, exposure to violence. When analyzing utilization during pregnancy, during birth, and after birth, I am restricted to children born between 2011 and 2013 due to the availability of the violence data and the timing of the DHS survey, which was completed in 2013. In Table 2, the sample sizes for prenatal visits are slightly smaller than for the delivery variables because these questions are only asked regarding each respondent's most recent birth (in regressions, these samples are further limited by the need for violence data 9 months before birth). For vaccinations, I am interested in whether children have received the recommended vaccinations before their first birthday and therefore restrict to children at least a year old at the time of the survey. Unfortunately, child height and weight are not measured in this survey, and though mothers are asked to report their child's weight at birth (from memory or from a health card), this variable is missing for over half of the children in the ARMM.

In Table 3, I report municipality-level violence, weather, and population statistics for the years 2011-2013 in the ARMM municipalities that were surveyed in the DHS. Municipalities experience on average one violent event per month, but this varies

Table 1 Summary Statistics: Control Variables

	(1)	(2)	(3)
Variable	ARMM	non-ARMM	Difference
Child Age	2.00	2.00	0.0017
	(1.42)	(1.42)	(0.054)
Mother's age	30.3	29.8	0.44
	(6.94)	(7.02)	(0.26)
1(Male)	0.54	0.52	0.021
	(0.50)	(0.50)	(0.019)
1(Mother: No education)	0.084	0.014	0.071***
	(0.28)	(0.12)	(0.0054)
1(Mother: Incomplete primary)	0.23	0.11	0.13***
	(0.42)	(0.31)	(0.012)
1(Mother: Complete primary)	0.12	0.11	0.0080
	(0.32)	(0.31)	(0.012)
1(Mother: Incomplete Secondary)	0.18	0.17	0.0071
	(0.38)	(0.38)	(0.014)
1(Mother: Complete Secondary)	0.24	0.33	-0.082***
	(0.43)	(0.47)	(0.017)
1(Mother: Higher than secondary)	0.14	0.27	-0.13***
	(0.35)	(0.45)	(0.016)
HH Wealth score	-111946.5	-17774.9	-94171.6***
	(71970.5)	(99725.0)	(3651.4)
1(Mother: Muslim)	0.76	0.032	0.73***
	(0.43)	(0.18)	(0.0084)
HH size	6.64	6.42	0.22*
	(2.64)	(2.53)	(0.096)
Number of children 5 and under in HH	2.01	1.75	0.26***
	(0.94)	(0.91)	(0.034)
Number of women aged 15-49 in HH	1.33	1.49	-0.16***
	(0.72)	(0.83)	(0.031)
1(Male household head)	0.95	0.87	0.077***
	(0.22)	(0.33)	(0.012)
Age of HH head	37.3	42.2	-4.86***
	(11.2)	(13.3)	(0.49)
Number of living children born to mother	3.83	3.06	0.77***
	(2.31)	(2.09)	(0.080)
Number of children	*794*	*6422*	*7216*

Notes: *** p<0.01, ** p<0.05, * p<0.1.

Table 2 Summary Statistics: Outcome Variables

	(1)	(2)	(3)
Variable	ARMM	non-ARMM	Difference
Contemporaneous Healthcare Utilization			
1(Went to health facility for illness)	0.13	0.23	-0.11***
	(0.33)	(0.42)	(0.016)
Number of children	*752*	*6199*	*6951*
Pregnancy/Birth Healthcare Utilization			
1(Any prenatal care)	0.72	0.97	-0.25***
	(0.45)	(0.16)	(0.012)
1(Prenatalcare at hospital)	0.12	0.28	-0.16***
	(0.32)	(0.45)	(0.028)
Number of children born 2011-2013 (most recent births only)	*370*	*3099*	*3469*
1(Delivered at home)	0.73	0.29	0.45***
	(0.44)	(0.45)	(0.023)
1(Delivered in hospital)	0.19	0.51	-0.32***
	(0.39)	(0.50)	(0.025)
1(Delivered elsewhere)	0.081	0.21	-0.13***
	(0.27)	(0.41)	(0.020)
Number of children born 2011-2013	*422*	*3386*	*3808*
1(Completed all vaccinations before 1st birthday)	0.26	0.40	-0.14***
	(0.44)	(0.49)	(0.033)
1(Completed Measles vaccination before 1st birthday)	0.47	0.80	-0.33***
	(0.50)	(0.40)	(0.028)
1(Completed BCG vaccination before 1st birthday)	0.62	0.92	-0.30***
	(0.49)	(0.28)	(0.021)
1(Completed Hep B vaccinations before 1st birthday)	0.31	0.46	-0.15***
	(0.46)	(0.50)	(0.033)
1(Completed DPT vaccinations before 1st birthday)	0.48	0.83	-0.36***
	(0.50)	(0.37)	(0.026)
1(Completed Polio vaccinations before 1st birthday)	0.44	0.82	-0.38***
	(0.50)	(0.38)	(0.027)
Number of children born 2011-2012 (currently over 1 year old)	*246*	*2075*	*2321*

Notes: *** p<0.01, ** p<0.05, * p<0.1.

substantially month-to-month and across municipalities. The maximum number of violent events during this time period among DHS municipalities, not shown in the table, was 32. The remainder of this table summarizes municipality characteristics from the 2010 census.

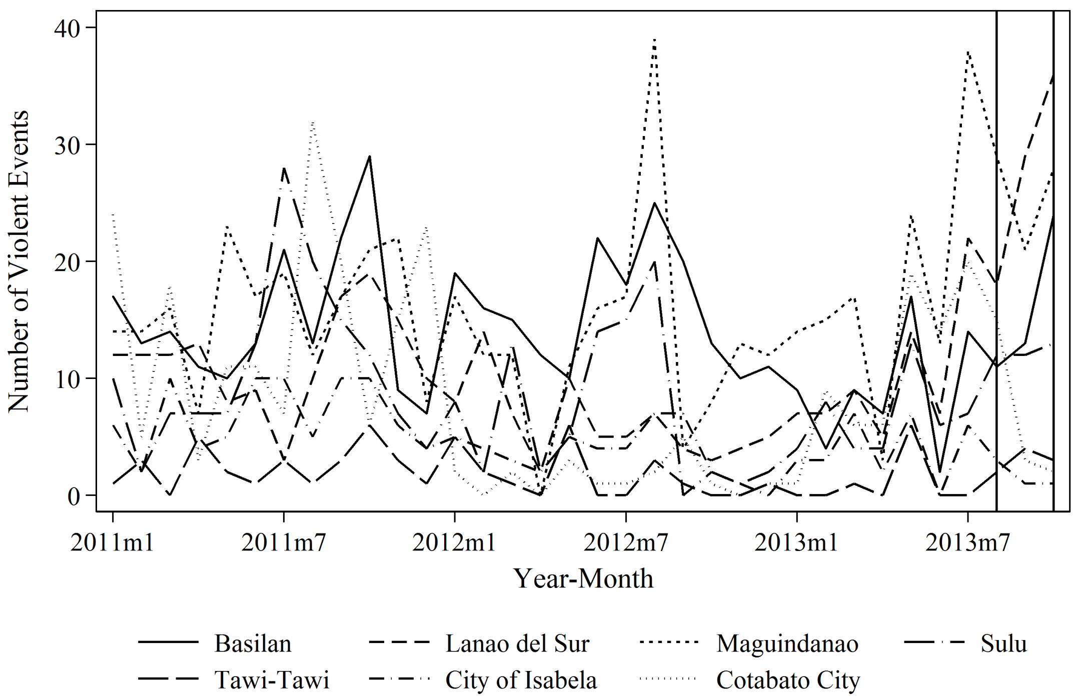
Table 3 Summary Statistics: Municipality-Level Variables for ARMM Municipalities in DHS

Variable Name	Mean (s.d.)
Municipality-month-level variables (2011-2013)	
Number of violent events per month	1.09
	(2.94)
Maximum temperature (degrees Celsius)	34.0
	(0.84)
Total precipitation (mm)	198.8
	(124.5)
Number of municipality-month observations	353
Municipality-level variables (2010 census)	
Proportion of HH w/ electricity	0.57
	(0.25)
Proportion of HH w/ own toilet	0.23
	(0.20)
Proportion of HH that own land	0.44
	(0.20)
Proportion of HH with any household convenience device[1]	0.84
	(0.089)
Proportion of ever-married women married before 20	0.43
	(0.066)
Average fertility	3.44
	(0.73)
Number of municipalities	52

1. Household convenience devices include radios, televisions, CD players, telephones, computers, refrigerators, washing machines, and vehicles.

3 Empirical Strategy

Figure 2 Number of Violent Events Recorded in the BCMS, by Province/City



Notes: Vertical lines represent the first and last month of DHS interviews. Due to their large populations and special administrative status, Isabela City and Cotabato City were plotted separately.

To illustrate the variation in violence over time and across space, Figure 2 plots the number of violent events per month by province, from January 2011 to October 2013, the entire range of births in my sample (births in the DHS for which I have violence data). The two vertical lines represent the beginning and end of the DHS interviews, revealing a relatively short time span for my contemporaneous utilization analysis. There is, however, considerable variation in the trends across provinces even in these three months.

This province breakdown provides a general illustration of the variation in violence intensity, but (as described above) my analysis actually uses a more local measure of violence: the number of violent events in each municipality and month. Municipalities are the second smallest administrative region in the Philippines. In the ARMM, the average municipality is only about 330 square kilometers with a population of about 32,000 people.[11] Given these figures, it seems reasonable to assume that violent events that take place in an individual's municipality of residence are local enough to be relevant to their decision-making.

A key issue in any study of the consequences of violence is the fact that it is not exogenous: areas that experience more violence may also have certain characteristics that drive healthcare utilization. To deal with this, I include location fixed effects and therefore take advantage of location-specific variation in violence over time. For contemporaneous health outcomes, which are only measured over a span of three months, there is limited variation over time, especially at the municipality level. For outcomes that are tied to birth month rather than survey month, I have a longer time frame. For this reason, I run two different specifications for these two sets of outcomes.

For contemporaneous curative health-seeking, I estimate the following empirical specification: for outcome h_{ipjm} of child i, living in municipality j (in province p),

[11] Statistics as of 2007 and 2010, respectively. See http://www.nscb.gov.ph/activestats/psgc/listmun.asp

whose mother was interviewed in survey month m, we have

$$h_{ijpm} = \beta_0 + \beta_1 v_{jpm} + \beta_2 X_i + \beta_3 W_{jpm} + \beta_4 Z_{jp} + \mu_p + \eta_m + \epsilon_{ijpm} \tag{1}$$

where v_{jpm} represents the total number of violent events that took place in municipality j in survey month m. X_i is a vector of individual and household controls described above. Month fixed effects (η_m) control for non-linear trends across the sample. Across the entire sample, there are only three survey months, but within municipalities, this variation is even more limited: in over 90% of municipalities in the DHS, all households were interviewed in the same month. Therefore, including municipality fixed effects would eliminate virtually all of the variation in my independent variable of interest. In order to control for time-invariant location unobservables while still allowing for some variation, I include province fixed effects (μ_p).[12] This means I am still relying on some variation across municipalities (within the same province) to identify my coefficient of interest.[13] To alleviate concerns that unobserved municipality-level characteristics could be driving both violence levels and utilization choices, I also add municipality-level controls, Z_{jp} in some specifications. In addition, because of growing evidence that weather conditions like rainfall and temperature can affect violence (Miguel, Satyanath, and Sergenti 2004; Hsiang, Burke, and Miguel 2013) and the natural links between climate and health, I also include a vector of weather controls W_{jpm}: quadratic functions of the maximum monthly temperature and average monthly rainfall. For this specification and the one described next, I cluster my standard errors at the municipality level; there are 52 municipalities

[12] Although Isabela City and Cotabato City are technically cities and not provinces, they are assigned their own province codes. Because of their large populations, they are treated as provinces in this analysis.

[13] In Appendix section B, I discuss evidence showing that municipality-level health-seeking tendency fixed effects are positively correlated with violence. Therefore, the exclusion of these fixed effects likely results in an underestimation of the magnitude of the negative relationship between curative health-seeking and violence.

in my sample.

Looking at utilization decisions around the time of birth, I am able to estimate a more rigorous specification:

$$h_{ijmy} = \beta_0 + \beta_1 v_{jmy} + \beta_2 X_i + \beta_3 W_{jmy} + \mu_j + \eta_m + \delta_y + \epsilon_{ijmy} \tag{2}$$

where v_{jmy} now represents the total number of violent events that took place in municipality j during a period relative to child i's birth month m and birth year y (a period that depends on the outcome of interest). I sum over the 9 months prior to birth for prenatal visits, just the month of birth for place of delivery, and the 12 months after birth for vaccinations. In this analysis, with more than one year of data, I include birth year fixed effects (δ_y). Because the longer time frame provides me with more within-municipality variation over time, I am also able to include municipality fixed effects μ_j, which control for time-invariant unobservables that may be driving both h_{ijmy} and v_{jmy}.[14]

It should be noted that I am assigning violence based on the mother's current municipality of residence, which may not be where she lived during pregnancy or the year after birth. Unfortunately, specific migration information is not available in the 2013 Philippine DHS,[15] but it is worth noting that the earliest birth in my sample occurred less than 3 years prior to the survey. I discuss this issue in more detail in section 4.2.4. In section 4.2.3, I address other migration-related threats to identification.

[14]These fixed effects make municipality-level controls Z_{jp} unnecessary because these are time-invariant. Within my time frame of interest, the census data is only available for one year, 2010.

[15]The 2013 interview does include a question about the respondent's "type" of residence – city, town, or rural – 5 years ago, but this is not necessarily a good proxy for migration if people move location to another location of the same "type," or if areas are reclassified from rural to urban as cities expand.

4 Results

4.1 Effects of Violence on Healthcare Utilization

Table 4 Effects of Violence on Curative Care Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Visited Health Facility	Visited Health Facility	Visited Health Facility	Visited Health Facility
Number of Violent Events in Municipality During Month of Survey	**-0.010**	**-0.011**	**-0.014**	**-0.025**	**-0.023**	**-0.025**
	(0.0025)***	(0.0030)***	(0.0053)***	(0.0097)**	(0.0098)**	(0.014)*
Observations	752	752	752	320	320	320
Mean of Dependent Variable	0.13	0.13	0.13	0.30	0.30	0.30
Effect of 1 Standard Deviation	-0.02	-0.02	-0.03	-0.05	-0.04	-0.05
Additional Demographic Controls	No	Yes	Yes	No	Yes	Yes
Municipality Controls	No	No	Yes	No	No	Yes
Sample		All children			Children reported sick	
Fixed Effects			Province, Month, Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$).

All regressions include child gender, child age fixed effects, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic).

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

In Table 4, I report the coefficient estimates from equation 1. Here and in all subsequent tables, I also report the implied magnitude of the effect of increasing violence by one standard deviation. This table shows that increases in violence significantly reduce curative care seeking. Columns 1 to 3 report the results for the full sample of children. Column 3 shows that the results are robust to the inclusion of municipality controls, minimizing concerns about the results being driven by municipality-level characteristics correlated with violence. Because I am only able to include province (and not municipality) fixed effects in this specification, it is important to verify that municipality-level observables (which could potentially be related to both violence levels and healthcare utilization) do not appear to be driving the results in columns 1 and 2. Moreover, as I discuss in Appendix section B, where I compare the results of a municipality-fixed-effect and province-fixed-effect specification for the outcomes for which I have more time variation (analyzed below), there is no evidence to suggest that the omission of municipality fixed effects would generate a spurious negative correlation between violence and health-seeking. If anything, they appear to be working in the opposite direction.

In column 3, a one-standard deviation increase in violence in a child's municipality of residence reduces the probability of being taken to a health facility by approximately 3 percentage points, a 23% reduction at the mean. Given that the early diagnosis and treatment of acute respiratory infections (with antibiotics) and diarrhea (with oral rehydration therapy) can literally save lives (Philippines Statistics Authority and IFC International 2014), these results reveal a large cost of violence that has not been explored before.

I show in columns 4 to 6 that the significant negative relationship holds when I restrict to the sample of children reported to have fallen ill. This alleviates concerns that the results in Table 4 are being driven by changes in the number of children reported sick (since the question about health-seeking is only asked of mothers who reported that their children fell ill[16]).

The next two tables report results from equation 2, which does allow for municipality fixed effects and therefore offers more rigorous evidence identified off of within-municipality variation over time. In these specifications, I analyze the link between violence and healthcare utilization around the time of birth and am therefore restricted to the sample of children born between 2011 and 2013 (for whom violence data is also available).

Columns 1 through 4 of Table 5 report regression estimates of the effect of violence during pregnancy on prenatal care decisions, which involves a much smaller sample size than in the previous table.[17] I do not find any significant effects on the probability of having any prenatal visits – the positive coefficients in columns 1 and 2 are small in magnitude and not statistically significant. Column 3 shows that the likelihood of going to the hospital for prenatal care (conditional on having any visit at all) is significantly less than zero, but this estimate is almost one-third smaller and no longer significant after the inclusion of additional demographic controls in column 4. The lack of strong evidence for a large effect on prenatal care is consistent with the findings of Duque (2017), and implies that existing evidence for sizable health effects of in-utero violence exposure must be detecting effects driven by a different mechanism, such as maternal stress or nutrition (which echoes the conclusion, made in Brown (2018), that prenatal care is not a primary mechanism for the birthweight effects of violence).

In Columns 5 to 10 of Table 5, I show that violence during a child's month of birth has an impact on the mother's decision about where to deliver. An increase in violence intensity makes mothers significantly more likely to deliver at home (columns 5 and 6).

[16]The dependent variable in Table 4 is an indicator equal to one if a sick child was taken to a health facility in the 2 weeks prior to the survey. I assign a zero to all children who were not sick during the time period, which is consistent with the use of this variable as an indicator of curative care seeking specifically (and not healthcare utilization in general).

[17]The smaller sample size is due to the violence data restriction as well as the fact that prenatal care questions were only asked about the most recent birth for each mother.

Table 5 Effects of Violence on Pregnancy and Delivery Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Any prenatal care	Any prenatal care	Prenatal care from hospital	Prenatal care from hospital	Delivered at Home	Delivered at Home	Delivered in Hospital	Delivered in Hospital	Delivered Elsewhere	Delivered Elsewhere
Number of Violent Events in Municipality During 9 Months Prior to Birth	**0.0022**	**0.0033**	**-0.0032**	**-0.0023**						
	(0.0028)	(0.0026)	(0.0014)**	(0.0014)						
Number of Violent Events in Municipality During Month of Birth					**0.014**	**0.014**	**-0.013**	**-0.011**	**-0.0011**	**-0.0022**
					(0.0073)*	(0.0070)*	(0.0064)*	(0.0063)*	(0.0037)	(0.0037)
Observations	281	281	207	207	422	422	422	422	422	422
Mean of Dependent Variable	0.74	0.74	0.12	0.12	0.73	0.73	0.19	0.19	0.081	0.081
Effect of 1 Standard Deviation	0.04	0.06	-0.06	-0.04	0.04	0.04	-0.04	-0.03	0.00	-0.01
Additional Demographic Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Fixed Effects	Municipality, Birth Month, Birth Year									

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic).

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

This effect comes from mothers being significantly less likely to deliver in a hospital (columns 7 and 8), which both global and national health policy has emphasized as an important way to improve maternal and child health (Philippines Statistics Authority and IFC International 2014). A one standard deviation increase in violence is associated with a 4 percentage point increase in home delivery and a 3 percentage point decrease in hospital delivery, which translates to a 5% reduction at the mean (for home deliveries) and a 16% reduction at the mean (for hospital deliveries).

Finally, Table 6 shows that violence over the course of the year after a child's birth affects the probability of completing vaccinations. In columns 1 and 2, the dependent variable is an indicator for a child being fully immunized by her first birthday. In the remaining columns, each dependent variable is a dummy equal to 1 if the child received all necessary shots for the particular disease's vaccination schedule, by her first birthday. Columns 1 and 2 reveal that violence reduces the probability of a child being fully immunized, which appears to be driven by significant declines in the measles vaccination rate in particular (columns 3 and 4).

The violence variable used in the previous tables combines various different types of violent events, depicted in Figure 1. In the appendix (Table A1), I look separately at the effects of the four most prevalent categories of violence: shootings, armed clashes, bombings, and kidnappings. Across the three main types of health-seeking that were significantly affected by violence (curative care, delivery, and vaccinations), bombings and shootings appear to have consistently large and statistically significant effects. Kidnappings significantly affect curative care visits only, while clashes do not have significant effects on any of the main health-seeking variables.

In Appendix Table A2, I also report results from specifications that use other definitions of the violence variable – per capita violence (Panel A), violence scaled by municipality area (Panel B), and quarterly instead of monthly violence for the monthly violence specifications (Panel C, in order to address the fact that monthly data is

Table 6 Effects of Violence on Completion of Vaccinations before First Birthday

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All	All	Measles	Measles	BCG	BCG	Hepatitis B	Hepatitis B	DPT	DPT	Polio	Polio
Number of Violent Events in Municipality During Year Following Birth	**-0.0034**	**-0.0031**	**-0.0048**	**-0.0045**	**0.00043**	**0.00030**	**-0.0024**	**-0.0017**	**-0.00040**	**0.00071**	**0.00087**	**0.0018**
	(0.0014)**	(0.0015)**	(0.0017)***	(0.0021)**	(0.0022)	(0.0029)	(0.0017)	(0.0020)	(0.0018)	(0.0023)	(0.0019)	(0.0021)
Observations	239	239	245	245	243	243	246	246	244	244	245	245
Mean of Dependent Variable	0.26	0.26	0.47	0.47	0.62	0.62	0.31	0.31	0.48	0.48	0.44	0.44
Effect of 1 Standard Deviation	-0.07	-0.07	-0.10	-0.10	0.01	0.01	-0.05	-0.04	-0.01	0.02	0.02	0.04
Additional Demographic Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Fixed Effects	Municipality, Birth Month, Birth Year											

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic).

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

noisy). The pattern of results is, on the whole, preserved.[18]

4.2 Threats to Identification

4.2.1 Time-Varying Omitted Variables

My empirical strategy accounts for the potential endogeneity of violence by including location and time fixed effects. While these separately address time-invariant location-specific unobservables and non-linear time trends that affect the entire sample, any omitted time-varying location-specific variables pose a threat to identification.

The existing literature on conflict provides guidance on what types of omitted variables are of particular concern. In particular, a wide body of research on the relationship between income shocks and conflict suggest that both positive and negative income shocks can increase conflict. Positive income shocks may increase conflict by increasing the potential reward from fighting (Collier and Hoeffler 1998; Mitra and Ray 2014; Nunn and Qian 2014). On the other hand, negative income shocks could also increase conflict by decreasing the opportunity cost of violence (Fearon and Laitin 2003; Miguel et al. 2004; Hidalgo et al. 2010; Nielsen et al. 2011; Gwande, Kapur, and Satyanath 2012). Given this, anything that affects the opportunity cost of conflict or the amount to be gained from fighting needs to be considered. In this section, I consider three potential drivers: tropical storms, agricultural price shocks, and government infrastructure programs.

First, tropical storms and major floods are common in the Philippines and can affect both violence and health-seeking. I use the EM-DAT International Disaster

[18] One notable exception is the per capita violence specification for vaccinations, where the coefficients flip signs and lose statistical significance. This could be an indication that the perceived risk that individuals infer from a violent occurrence in their municipality is not inversely proportional to population size, especially over a longer period of time (like the one year period used in the vaccination regressions). Indeed, the strength of the results in Panel B suggest that the geographic size of the municipality might also be an important scaling factor that the per capita results do not take into account (many heavily populated municipalities are actually quite small in terms of area).

Database to obtain a list of storms and floods that took place in the ARMM during my study period. From this database, I am able to record which provinces were hit by disasters and in which months. In Appendix Table A3, I show that controlling for the number of storms during the relevant period leaves my main estimates virtually unchanged.[19]

Agricultural price shocks are another important factor that could affect both violence and health-seeking in either direction. In order to rule out these mechanisms as the explanation for my findings, I obtain, from Philippines Statistic Authority (2016), province-level monthly prices for the most important crop categories in the ARMM according to the 2012 Philippine Agricultural Census:[20] maize, paddy, coconuts, and rootcrops.

In Appendix Table A4, I first control for the price[21] of the crop that accounts for the highest proportion of agricultural land use in each province and repeat my analysis. Next, I control for price shocks by creating a single weighted average of maize, paddy, coconut, and rootcrop prices (using the crop-specific fraction of the total area devoted to these four crops as weights), and multiply this index by the municipality share of adults working in agriculture, to account for variation across municipalities in the relevance of price shocks for income levels. As shown in Appendix Table A4, controlling for each of these price variables does not affect my coefficient estimates.[22]

A third important consideration for this setting is the existence of local development projects. Development projects are potentially important drivers of violence: Crost, Felter, and Johnston (2014), for example, find evidence that a large community-driven development program in the Philippines led to more civil conflict

[19]The "relevant period" is different for each regression. For contemporaneous utilization, the relevant period is the month of survey. For delivery variables, it is the month of birth, and for vaccinations it is the year after birth.

[20]See `https://psa.gov.ph/content/special-report-highlights-2012-census-agriculture-2012-ca`.

[21]standardized using the crop-specific mean and standard deviation

[22]Because prices themselves may be affected by violence, controlling for them as I do in Table A4 could be problematic. I discuss an alternative strategy for ruling out their role as confounders below.

deaths. The authors attribute the increases in violence to attempts by insurgents to sabotage government programs for political gain.

Although the community-driven development program studied in the Crost et al. (2014) study did not cover the ARMM, municipalities in the ARMM did receive substantial government funding during my study period. The ARMM Social Fund Project was designed as an offshoot of the Final Peace Agreement between the MNLF and the Philippine government. Part of the funds for the project were used to support community-based infrastructure projects starting in 2011. These projects included a wide range of health, education, and transportation-related building projects, including the construction of daycare centers, barangay health centers, additional school classrooms, and the repairing of roads.

Similar to agricultural price shocks, the influx of outside funds could have affected both violence and healthcare utilization in either direction. To ensure that my results are not being driven by the effects of these infrastructure projects on both violence and health-seeking, I first control for the number of ongoing or completed health projects in each municipality in each month. I then add controls for the number of education and transportation related projects. As shown in Table A5, the inclusion of these controls do not affect my main conclusions: violence is significantly and negatively related to healthcare utilization.

I employ a second strategy to complement the robustness checks in Tables A4 and A5, to alleviate concerns that prices and government projects are potentially problematic controls (because they themselves may also be affected by violence). In Appendix Table A6, I regress my various price and government program variables (which vary at the province-month or municipality-month level) on the number of violent events in the relevant municipality-month, controlling for month, year, and municipality fixed effects. This table shows that none of these variables appear to have a significant relationship with violence – all coefficients are insignificant and small relative to their means and standard deviations. It is unlikely, therefore, that these variables – rather than violence – are what drive the main results of the paper.

4.2.2 Placebo Tests

The results of Appendix Table A7 provide additional support for the validity of the empirical strategy. For each of the main outcomes of interest, I report the results of two regressions that serve as a simple placebo test. The first (in odd-numbered columns) repeats my main analysis, replacing current violence with future violence: specifically, I use the number of violent events during period $t+5$ (for contemporaneous health-seeking and place of delivery), or $t+2$ (for vaccinations).[23] This regression can be thought of as a placebo test because violent events in the future should not have any effect on health-seeking decisions today. Indeed, in all but one of the odd-numbered columns, future violence shows no significant relationship with health-seeking decisions.

The second set of regressions (in even-numbered columns) includes both current violence and future violence, and helps to alleviate concerns about the significant coefficient in column 3. When current violence is added to the regression (in column 4), the coefficient on future violence becomes smaller and insignificant, while the coefficient on current violence is statistically significant and similar in magnitude to the estimates in Table 4. This suggests that the significant coefficient in column 3 was driven by the positive correlation between current and future violence, and the omission of current violence from that regression. Taken together, these results show that it is indeed *current* violence that is responsible for the effects reported above.

[23] I use violence in $t+5$ for contemporaneous health-seeking and place of delivery because violence is serially correlated. Earlier leads (violence in $t+1$ to $t+3$ for example) are highly correlated with violence in period t – with correlations ranging from 0.7 to 0.9 – and are therefore not appropriate placebos. For vaccinations, where my relevant period is one year instead of one month, including leads further than $t+2$ results in dropping over half of the sample (because the violence data ends in 2014).

4.2.3 Selection

The next robustness check I run addresses the possibility that violence may affect the composition of my sample. For example, violence might drive some people to migrate (even temporarily) or may prevent the surveying of particular households. If the people who are excluded from my sample due to high violence are ones that are more likely to utilize formal healthcare services, this would generate the same pattern of results I describe above – even if there were no causal link between violence and utilization. In order to evaluate the possibility of this type of selection, I check to see whether individuals surveyed during months with higher violence levels are observably different from individuals surveyed during low-violence months. Specifically, I first regress the demographic variables that I use as controls on the number of violent events that took place in the month the individual was surveyed, controlling only for province fixed effects.

For similar reasons, I also check to see whether violence during the month of birth or during the year following birth – the relevant time periods for the delivery and vaccination results – are significantly associated with the control variables I use for these regressions (all the variables used above except child age), controlling for municipality fixed effects. This check does not only address the possibility of endogenous migration, but also selective mortality. In extreme cases, violence around the time of birth could have resulted in maternal or child mortality and consequently changed the composition of the sample I am able to observe.

The results of these regressions are summarized in Table 7. On the whole, these results reveal no meaningful compositional effects of violence. All coefficients are small in magnitude, and only 4 out of 34 are significant at the 10% level (and only one is significant at the 5% level).

4.2.4 Migration

While the previous sub-section addresses concerns about violence-driven migration affecting the sample composition, there is another migration-related identification issue that it does not address: measurement error. As mentioned in section 3, the DHS only provides a mother's current municipality of residence, not the municipality in which she was living during each birth recorded. As a result, there is potentially some measurement error in the violence variables related to place of delivery and vaccination outcomes,[24] because violence in the municipality of residence (which is the variable used in the analysis) may not be the same as the actual violence a mother was exposed to during her child's birth or first year of life, if she was living in a different location. This measurement error is likely non-classical: if mothers who previously lived in violent areas are the ones more likely to have migrated, this would generate a negative correlation between the "true" violence exposure measure and the measurement error.

The consequences of this non-classical measurement error depend on the variance of the measurement error and how it compares to the variance of the true violence variable.[25] If the measurement error variance is smaller than the true violence variance, a negative correlation between the error and true violence will result in attenuation bias that is smaller than in the classical measurement error case. If the opposite is true, however, the attenuation bias will be larger, possibly even resulting in an estimated coefficient with the opposite sign of the true coefficient.

I argue that the former scenario is more likely than the more problematic latter scenario – that is, the measurement error variance is likely smaller than the true violence variance. To support this claim, I draw on data from the 2003 and 2008 Philippine DHS surveys, which (unlike the 2013 survey) asks about the number of

[24] Fortunately, this kind of measurement error is not an issue for the curative care analysis, for which violence in the municipality of residence is the appropriate variable to use.

[25] See Bound et al. (1994) for a framework for non-classical measurement error.

Table 7 Selection Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Child age	Mother's age	Male	Mother completed primary	Wealth score	Muslim	Household size	Children under 5 in household	Adult women in household	Male household head	Household head age	Children born to mother
Number of Violent Events…												
During Month of Survey	**0.0017**	**-0.11**	**-0.0077**	**0.013**	**0.062**	**-0.0016**	**0.038**	**0.017**	**0.030**	**0.000082**	**-0.11**	**-0.0010**
	(0.013)	(0.090)	(0.0066)	(0.010)	(0.031)*	(0.0046)	(0.051)	(0.016)	(0.015)*	(0.0014)	(0.17)	(0.035)
Observations	759	794	794	794	794	794	794	794	794	794	794	794
Dep. Var Mean	2.00	30.3	0.54	0.39	-0.83	0.75	6.64	2.01	1.33	0.95	37.3	3.83
Fixed Effects						Province						
During Month of Birth		**0.11**	**-0.010**	**-0.0050**	**0.011**	**-0.00089**	**-0.028**	**0.0023**	**-0.038**	**0.0015**	**0.23**	**0.036**
		(0.21)	(0.0074)	(0.0079)	(0.011)	(0.0025)	(0.050)	(0.012)	(0.012)***	(0.0042)	(0.17)	(0.037)
Observations		426	426	426	426	426	426	426	426	426	426	426
Dep. Var Mean		29.1	0.53	0.40	-0.82	0.75	6.64	1.98	1.38	0.94	37.2	3.63
Fixed Effects						Municipality						
During Year Following Birth		**0.013**	**-0.00012**	**-0.0015**	**-0.0021**	**-0.00017**	**-0.013**	**-0.00085**	**-0.0038**	**0.00088**	**-0.0029**	**0.011**
		(0.020)	(0.0027)	(0.0020)	(0.0011)*	(0.0021)	(0.017)	(0.0056)	(0.0041)	(0.00086)	(0.027)	(0.0073)
Observations		426	426	426	426	426	426	426	426	426	426	426
Dep. Var Mean		29.1	0.53	0.40	-0.82	0.75	6.64	1.98	1.38	0.94	37.2	3.63
Fixed Effects						Municpality						

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

years a mother has been living in the village, town, or city in which she is being interviewed. Using this information, I am able to determine which births took place in the same location as the current recorded residence. In the ARMM, 93% of the births in 2003 and 95% of the births in 2008 took place in the recorded place of residence. Assuming similar proportions in 2013, this means that the vast majority of births in my sample are actually assigned violence from the correct municipality, implying that the measurement error variance should be relatively small. In short, the measurement error resulting from migration in this sample is likely too small to be generating anything more than attenuation bias.

4.3 Mechanisms

Violence can affect healthcare utilization through demand-side as well as supply-side mechanisms. Increases in local violence could restrict the supply of health services – either by damaging health infrastructure, closing or overcrowding health facilities, or preventing the delivery of vaccines and other necessary supplies to health clinics. On the demand side, security fears may deter mothers from seeking care for their children if increases in local violence translate into a higher cost of healthcare.[26] Though it is difficult to separate the supply-side from the demand-side, this section attempts to shed some light on what mechanisms might be in play.

To investigate the hypothesis that increases in violence change parental health-seeking by affecting demand, I analyze heterogeneity in the effects of violence by mothers' reported frequency of TV and radio use. If increases in violence deter mothers from seeking care, then mothers must be aware of recent violent occurrences. While neighborhood social networks could certainly provide this information, other

[26]It is worth noting that security fears might also lead to reduced health-seeking via reduced income. That is, the insecurity and perceived danger generated by violence might lead to lower income – through decreased labor supply, for example, as suggested by Velásquez (2015)– and that could be what drives the decrease in the demand for healthcare.

important sources of information include television and radio news. In the DHS, mothers are asked about their TV and radio habits, to which they respond "1" if they watch TV (or listen to the radio) at all and "2" if they watch (or listen) at least once a week. In my sample, slightly over 60% of mothers watch television at all, and approximately 60% of these women watch at least once a week. Listening to the radio is slightly less common – around 57% of mothers listen to the radio at all, and about half of these women listen at least once a week.

Table 8 reports the results of regressions that allow for heterogenous effects of violence across mothers with different TV-watching and radio-listening habits. In this table, the signs of the TV interaction coefficients in all of the regressions are consistent with the hypothesis that mothers more likely to be aware of violence (from TV) exhibit larger responses to it, and these coefficients are statistically significant in half of the regressions. In fact, among mothers that report no TV use at all, violence appears to have no significant impact. Mothers' TV use could certainly be endogenous for several reasons, but in the appendix (Table A8), I show that household wealth is not driving these interactions. I also show in the appendix (Table A9) the regression results for home and hospital delivery, restricting to recent births (because the survey question captures current habits, not necessarily the habits a mother had during the birth of her child, and this discrepancy should be less serious for recent births). In Table A9, the interaction coefficients in the home delivery regressions are even larger in magnitude in these specifications (though the opposite is true for hospital births).

On the other hand, the signs on the radio interaction coefficients are all small and insignificant, which is somewhat puzzling, given that radio news is likely to be more local than TV news. It is worth noting, however, that in the regressions restricting to recent births (in Appendix Table A9), we see a significant positive coefficient on the radio interaction, which is consistent with the hypothesis. In general, the findings discussed here are neither necessary nor sufficient to confirm that violence induces a demand-side response, but they do suggest that awareness of the violence matters.

To investigate the importance of a supply-side response, I conduct an indirect test based on the idea that a supply-side violence shock should matter most for provinces with a limited supply to begin with. If the negative effects of violence are primarily due to health facilities closing down or becoming overcrowded, we might expect to see a muted response among individuals who have more alternatives – those who live in areas with more medical workers and who can, for example, visit another doctor if one facility closes. To conduct this test, I use yearly province-level data from the Department of Health to calculate the total number of medical workers (doctors, nurses, physicians, midwives, and barangay health workers) per 10,000 people. In Table 9, I include this variable in my regression specification, along with its interaction with the violence count variable, to test for heterogeneity across provinces with more available options for healthcare. Across all columns of Table 9, the violence coefficients are very similar in magnitude to the coefficients reported in my baseline results. In addition, the interaction terms are not significantly different from zero. In half of the regressions, the signs of the interactions are inconsistent with the hypothesis that greater supply of medical workers mitigates the effects of violence. On the whole, this provides little evidence for a supply-side response.

While the above test is only an indirect one, and does not address all potential supply-side channels (like the disruption of vaccine distribution), the lack of evidence for a supply-side response is consistent with the nature of the violence in the ARMM during my study period. Even though the ARMM is notorious for rebel group violence, which is often large scale and potentially destructive to infrastructure or disruptive to service provision, the time span being studied coincides with a push for peace from the central government. In fact, almost half of the events in the violence dataset during my period of interest are civilian shootings, which are distinct from clashes between armed groups.

In the appendix (Table A1), I break down the main violence variable into different types of events – shootings, armed clashes, kidnappings, and bombings. I run separate regressions for each type of event. While bombings, which are likely to have supply-side effects, do have independent effects on health-seeking, shootings and kidnappings – which are less likely to result in closures or over-crowding – also significantly impact health-seeking. While it is impossible to completely rule out the existence of a supply-side response without higher-frequency, more spatially granular data on the supply of health services, these facts do make it unlikely that over-crowding or closures are the only drivers of the reduced-form results described above.

5 Conclusion

This paper sheds light on the barriers to healthcare utilization by demonstrating that individuals in conflict-ridden regions respond to violence in ways that could be detrimental to their children's health. Mothers are less likely to go to a health facility, either to take a sick child, to deliver a baby, or to vaccinate their children, when they face greater local violence. This effect is strongest among mothers who are more likely to be aware of the violent events (by watching TV), which offers suggestive evidence that the negative effects of violence are coming from the demand side, rather than just restricted supply. This is a clear example of costly avoidance behavior, which is more commonly addressed in fields like environmental economics (Zivin and Neidell 2009; Zivin, Neidell, and Schlenker 2011), but for which we do not have much evidence in the context of crime and conflict.

Due to the data limitations described above,[27] I am unable to directly estimate exactly how much this avoidance behavior affects child health. Nevertheless, given existing evidence on the positive returns to formal sector curative care (Dow et al. 1997; Adhvaryu and Nyshadham 2012, 2015) and the emphasis in health policy on the importance of institutional deliveries and vaccinations as pathways to better health, this avoidance behavior surely has a non-trivial cost. Especially in this setting of endemic conflict, healthcare utilization rates that remain persistently low throughout a child's formative years could result in lasting health and human capital disadvantage.

[27] Child height and weight are not measured. Mothers are asked to report their child's weight at birth (from memory or from a health card), but this variable is missing for over half of the children in the ARMM. In addition, birth weight would only allow me to assess the health impacts of violence during pregnancy (which I did not find had strong effects on prenatal care) and not violence in the month of birth or first year of life.

Table 8 Heterogeneous Effects of Violence, by Mother's Frequency of TV and Radio Use

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Number of Violent Events in Municipality During Relevant Period	**-0.0082**	**-0.012**	**-0.020**	**0.0063**	**-0.0016**	**0.0043**
	(0.0092)	(0.011)	(0.022)	(0.024)	(0.0056)	(0.0045)
Number of Violent Events x Mother's TV Use	**-0.0055**	**-0.0072**	**0.030**	**-0.019**	**-0.00046**	**-0.0047**
	(0.0064)	(0.0065)	(0.0078)***	(0.0092)**	(0.0027)	(0.0022)**
Number of Violent Events x Mother's Radio Use	**0.0023**	**0.0043**	**-0.0047**	**0.0062**	**-0.00079**	**-0.0010**
	(0.0055)	(0.0059)	(0.0077)	(0.0091)	(0.0015)	(0.0015)
Mother's TV Use	**0.029**	**0.032**	**-0.0047**	**-0.013**	**-0.037**	**0.080**
	(0.023)	(0.024)	(0.047)	(0.040)	(0.071)	(0.075)
Mother's Radio Use	**0.00060**	**-0.0042**	**-0.017**	**-0.012**	**0.080**	**0.0028**
	(0.019)	(0.020)	(0.036)	(0.026)	(0.045)*	(0.052)
Observations	748	748	419	419	236	242
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year		Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Mother's TV/Radio Use is equal to 0 for mothers who do not watch/listen at all, 1 for mothers who do watch/listen at all, and 2 for mothers who watch/listen at least once a week.

Table 9 Heterogeneous Effects of Violence, by Medical Workers per Capita

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Number of Violent Events in Municipality During Relevant Period	**-0.0098**	**-0.018**	**0.014**	**-0.013**	**-0.0022**	**-0.0092**
	(0.015)	(0.020)	(0.0073)*	(0.0069)*	(0.0057)	(0.0056)
Medical Workers per 10,000 people (in province)	**-0.42**	**-0.36**	**-0.0015**	**0.0018**	**-0.52**	**-0.34**
	(0.18)**	(0.25)	(0.0039)	(0.0036)	(0.28)*	(0.28)
Number of Violent Events x Medical Workers per 10,000 people (in province)	**-0.000025**	**0.000084**	**0.0000040**	**0.00066**	**-0.0014**	**0.0056**
	(0.00037)	(0.00045)	(0.00059)	(0.00046)	(0.0076)	(0.0070)
Observations	752	752	422	422	239	245
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year		Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

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A Appendix Tables

Table A1 Effects of Different Types of Violence on Health-Seeking

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Panel A						
Number of Shootings in Municipality During Relevant Period	**-0.033**	**-0.046**	**0.015**	**-0.014**	**-0.0080**	**-0.0093**
	(0.016)**	(0.021)**	(0.013)	(0.013)	(0.0029)***	(0.0035)**
Panel B						
Number of Clashes in Municipality During Relevant Period	**-0.040**	**-0.055**	**-0.027**	**0.014**	**0.0016**	**0.0026**
	(0.044)	(0.060)	(0.044)	(0.039)	(0.015)	(0.022)
Panel C						
Number of Kidnappings in Municipality During Relevant Period	**-0.036**	**-0.059**	**-0.037**	**0.040**	**0.030**	**-0.025**
	(0.013)***	(0.021)***	(0.055)	(0.046)	(0.026)	(0.033)
Panel D						
Number of Bombings in Municipality During Relevant Period	**-0.028**	**-0.027**	**0.064**	**-0.046**	**-0.012**	**-0.021**
	(0.010)***	(0.013)**	(0.021)***	(0.017)***	(0.0099)	(0.011)*
Observations	752	752	422	422	239	245
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year		Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$).

Each panel reports the results of a separate regression, each using a different independent variable.

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Table A2 Robustness to Alternate Definitions of Violence Variable

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Panel A						
Number of Violent Events (per 10,000 people) in Municipality During Relevant Period	**-0.091**	**-0.10**	**0.10**	**-0.059**	**0.015**	**0.013**
	(0.049)*	(0.059)*	(0.053)*	(0.042)	(0.028)	(0.038)
Observations	752	752	422	422	239	245
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Effect of 1 Standard Deviation	-0.02	-0.02	0.04	-0.02	0.04	0.03
Panel B						
Number of Violent Events (per 10,000 hectares) in Municipality During Relevant Period	**-0.0093**	**-0.012**	**0.021**	**-0.019**	**-0.0061**	**-0.011**
	(0.0022)***	(0.0037)***	(0.0094)**	(0.0099)*	(0.0024)**	(0.0027)***
Observations	752	752	422	422	239	245
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Effect of 1 Standard Deviation	-0.02	-0.03	0.04	-0.03	-0.08	-0.15
Panel C						
Number of Violent Events in Municipality During Quarter of Survey or Birth	**-0.0052**	**-0.0080**	**0.0082**	**-0.0059**		
	(0.0014)***	(0.0025)***	(0.0042)*	(0.0038)		
Observations	752	752	396	396		
Mean of Dependent Variable	0.13	0.13	0.72	0.19		
Effect of 1 Standard Deviation	-0.04	-0.07	0.06	-0.05		
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year		Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

Each panel reports the results of a separate regression, each using a different independent variable.

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Table A3 Robustness to Natural Disaster Controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Number of Violent Events in Municipality During Relevant Period	**-0.011**	**-0.014**	**0.013**	**-0.011**	**-0.0026**	**-0.0043**
	(0.0030)***	(0.0053)***	(0.0069)*	(0.0062)*	(0.0017)	(0.0022)*
Number of Storms in Province During Relevant Period	**1.78**	**1.42**	**-0.089**	**0.059**	**0.043**	**0.022**
	(0.47)***	(0.54)**	(0.059)	(0.062)	(0.062)	(0.068)
Observations	752	752	422	422	239	245
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Effect of 1 Standard Deviation	-0.02	-0.03	0.04	-0.03	-0.05	-0.09
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year		Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Table A4 Robustness to Price Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Visited Health Facility	Visited Health Facility	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered at Home	Delivered in Hospital	Delivered in Hospital	All Vacc.'s	All Vacc.'s	Measles Vacc.	Measles Vacc.
Number of Violent Events in Municipality During Relevant Period	**-0.011**	**-0.014**	**-0.0089**	**-0.012**	**0.013**	**0.013**	**-0.011**	**-0.011**	**-0.0032**	**-0.0031**	**-0.0048**	**-0.0045**
	(0.0030)***	(0.0051)***	(0.0034)**	(0.0054)**	(0.0069)*	(0.0070)*	(0.0062)*	(0.0063)*	(0.0016)**	(0.0015)**	(0.0022)**	(0.0021)**
Average Province-Level Top Crop Price During Relevant Period	**-0.18**	**-0.11**			**-0.0084**		**-0.025**		**-0.095**		**-0.22**	
	(0.39)	(0.38)			(0.066)		(0.071)		(0.48)		(0.34)	
Average Municipality-Level Price Index During Relevant Period			**0.15**	**0.25**		**-0.035**		**0.040**		**0.037**		**0.100**
			(0.16)	(0.14)*		(0.11)		(0.10)		(0.22)		(0.25)
Observations	752	752	752	752	422	422	422	422	239	239	245	245
Mean of Dependent Variable	0.13	0.13	0.13	0.13	0.73	0.73	0.19	0.19	0.26	0.26	0.47	0.47
Effect of 1 Standard Deviation	-0.02	-0.03	-0.02	-0.03	0.04	0.04	-0.03	-0.03	-0.07	-0.07	-0.10	-0.09
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	No	Yes	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year				Municipality, Birth Month, Birth Year							

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Average Province-Level Top Crop Price is the province-level price of the crop that accounts for the highest proportion of agricultural land use in each province, averaged over the relevant period.

Average Municipality-Level Price Index is the weighted average of maize, paddy, coconut, and rootcrop province-level standardized prices, multiplied by the municipality-level share of adults working in agriculture, averaged over the relevant period.

Table A5 Robustness to Development Project Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Visited Health Facility	Visited Health Facility	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered at Home	Delivered in Hospital	Delivered in Hospital	All Vacc.'s	All Vacc.'s	Measles Vacc.	Measles Vacc.
Number of Violent Events in Municipality During Relevant Period	**-0.010**	**-0.014**	**-0.0091**	**-0.013**	**0.013**	**0.013**	**-0.011**	**-0.011**	**-0.0035**	**-0.0034**	**-0.0043**	**-0.0038**
	(0.0032)***	(0.0054)**	(0.0036)**	(0.0057)**	(0.0070)*	(0.0069)*	(0.0062)*	(0.0062)*	(0.0012)***	(0.0015)**	(0.0022)*	(0.0025)
Number of Health Projects During Relevant Period	**-0.0026**	**-0.0045**	**-0.00062**	**-0.0030**	**-0.0098**	**-0.0099**	**0.027**	**0.028**	**-0.083**	**-0.088**	**0.053**	**0.043**
	(0.014)	(0.012)	(0.014)	(0.013)	(0.020)	(0.020)	(0.038)	(0.039)	(0.062)	(0.061)	(0.11)	(0.10)
Number of Transportation Projects During Relevant Period			**0.0036**	**0.0012**		**-0.00064**		**0.00052**		**-0.025**		**-0.069**
			(0.0043)	(0.0076)		(0.022)		(0.021)		(0.047)		(0.071)
Number of Education Projects During Relevant Period			**-0.0020**	**-0.0029**		**0.0023**		**-0.013**		**0.010**		**-0.00018**
			(0.0096)	(0.015)		(0.023)		(0.024)		(0.072)		(0.065)
Observations	752	752	752	752	422	422	422	422	239	239	245	245
Mean of Dependent Variable	0.13	0.13	0.13	0.13	0.73	0.73	0.19	0.19	0.26	0.26	0.47	0.47
Effect of 1 Standard Deviation	-0.02	-0.03	-0.02	-0.03	0.04	0.04	-0.03	-0.03	-0.07	-0.07	-0.09	-0.08
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	No	Yes	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year				Municipality, Birth Month, Birth Year							

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Table A6 Relationship between Violence and Robustness Check Controls

	(1)	(2)	(3)	(4)	(5)
	Average Province-Level Top Crop Price	Average Municipality-Level Price Index	Number of Health Projects	Number of Transportation Projects	Number of Education Projects
Number of Violent Events in Municipality	**0.0046**	**0.0020**	**-0.026**	**0.0093**	**0.0053**
	(0.0056)	(0.0020)	(0.020)	(0.011)	(0.010)
Observations	4216	4216	4216	4216	4216
Mean of Dependent Variable	-0.28	-0.014	0.27	0.53	0.71
Standard Deviation of Dependent Variable	0.69	0.43	0.81	1.15	1.68
Fixed Effects			Municipality, Month, Year		

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

Analysis is conducted at the municipality-month level.

These regressions restrict to ARMM municipalities in the years 2011-2013 (the years for which I have both DHS birth data and violence measures).

Table A7 Placebo Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Visited Health Facility	Visited Health Facility	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered at Home	Delivered in Hospital	Delivered in Hospital	All Vacc.'s	All Vacc.'s	Measles Vacc.	Measles Vacc.
Number of Violent Events in Municipality in Future Period	**-0.011**	**-0.0061**	**-0.016**	**-0.0084**	**0.00025**	**-0.00083**	**0.00085**	**0.0018**	**0.017**	**0.011**	**0.011**	**0.0029**
	(0.0089)	(0.0100)	(0.0082)*	(0.0100)	(0.0058)	(0.0056)	(0.0068)	(0.0067)	(0.012)	(0.014)	(0.0090)	(0.010)
Number of Violent Events in Municipality During Relevant Period		**-0.0085**		**-0.011**		**0.014**		**-0.011**		**-0.0069**		**-0.0088**
		(0.0041)**		(0.0067)*		(0.0071)*		(0.0064)*		(0.0056)		(0.0050)*
Observations	752	752	752	752	422	422	422	422	157	157	161	161
Mean of Dependent Variable	0.13	0.13	0.13	0.13	0.73	0.73	0.19	0.19	0.26	0.26	0.47	0.47
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	No	Yes	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year				Municipality, Birth Month, Birth Year							

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Future Period refers to the 5th month after the month of survey for contemporaneous utilization, the 5th month after the month of birth for delivery, and the 12 months starting 2 years after birth for vaccinations.

Table A8 Heterogeneous Effects of Violence, Controlling for Household Wealth

	(1)	(2)	(3)	(4)	(5)	(6)
	Visited Health Facility	Visited Health Facility	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Number of Violent Events in Municipality During Relevant Period	**0.0013**	**-0.0029**	**-0.012**	**-0.00091**	**0.0046**	**0.0060**
	(0.020)	(0.020)	(0.025)	(0.025)	(0.010)	(0.0079)
Number of Violent Events x Mother's TV Use	**-0.0087**	**-0.010**	**0.029**	**-0.018**	**-0.0012**	**-0.0049**
	(0.010)	(0.011)	(0.0080)***	(0.0092)*	(0.0026)	(0.0025)*
Number of Violent Events x Mother's Radio Use	**0.00078**	**0.0028**	**-0.0066**	**0.0080**	**-0.0018**	**-0.0013**
	(0.0056)	(0.0059)	(0.0082)	(0.0090)	(0.0026)	(0.0020)
Number of Violent Events x Wealth Score	**0.0076**	**0.0081**	**0.0094**	**-0.0089**	**0.0041**	**0.0011**
	(0.014)	(0.015)	(0.015)	(0.016)	(0.0062)	(0.0035)
Mother's TV Use	**0.031**	**0.034**	**-0.0047**	**-0.013**	**-0.032**	**0.081**
	(0.024)	(0.024)	(0.047)	(0.040)	(0.071)	(0.075)
Mother's Radio Use	**0.0012**	**-0.0036**	**-0.017**	**-0.012**	**0.085**	**0.0043**
	(0.020)	(0.020)	(0.035)	(0.026)	(0.046)*	(0.053)
Wealth Score	**0.027**	**0.017**	**-0.23**	**0.25**	**-0.26**	**-0.23**
	(0.063)	(0.067)	(0.059)***	(0.064)***	(0.16)	(0.11)**
Observations	748	748	419	419	236	242
Mean of Dependent Variable	0.13	0.13	0.73	0.19	0.26	0.47
Additional Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	No	Yes	N/A	N/A	N/A	N/A
Fixed Effects	Province, Month, Year		Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Mother's TV/Radio Use is equal to 0 for mothers who do not watch/listen at all, 1 for mothers who do watch/listen at all, and 2 for mothers who watch/listen at least once a week.

Table A9 Heterogeneous Effects of Violence, Restricting to Recent Births

	(1)	(2)	(3)	(4)
	Delivered at Home	Delivered in Hospital	Delivered at Home	Delivered in Hospital
Number of Violent Events in Municipality During Relevant Period	**-0.16**	**0.032**	**-0.039**	**-0.025**
	(0.11)	(0.13)	(0.042)	(0.038)
Number of Violent Events x Mother's TV Use	**0.052**	**0.0051**	**0.043**	**-0.0051**
	(0.042)	(0.045)	(0.018)**	(0.020)
Number of Violent Events x Mother's Radio Use	**0.12**	**-0.033**	**0.0054**	**0.0072**
	(0.059)*	(0.063)	(0.021)	(0.023)
Mother's TV Use	**0.12**	**-0.079**	**0.0031**	**-0.014**
	(0.14)	(0.15)	(0.055)	(0.051)
Mother's Radio Use	**-0.091**	**0.059**	**-0.044**	**0.0082**
	(0.14)	(0.11)	(0.056)	(0.039)
Observations	126	126	253	253
Mean of Dependent Variable	0.67	0.25	0.70	0.22
Additional Demographic Controls	Yes	Yes	Yes	Yes
Municipality Controls	N/A	N/A	N/A	N/A
Years of Birth	2013	2013	2012, 2013	2012, 2013
Fixed Effects	Municipality, Birth Month, Birth Year			

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$).

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic). Child age fixed effects are included in Visited Health Facility regressions.

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

Mother's TV/Radio Use is equal to 0 for mothers who do not watch/listen at all, 1 for mothers who do watch/listen at all, and 2 for mothers who watch/listen at least once a week.

B Comparing Municipality and Province Fixed Effects Specifications

Table A10 Comparison of Province and Municipality Fixed Effects Specifications

	(1)	(2)	(3)	(4)
	Delivered at Home	Delivered in Hospital	All Vaccinations	Measles Vaccination
Coefficient on Number of Violent Events During Relevant Period:				
With Municipality Fixed Effects	**0.014**	**-0.011**	**-0.0031**	**-0.0045**
	(0.0070)*	(0.0063)*	(0.0015)**	(0.0021)**
With Province Fixed Effects	**0.0079**	**-0.0076**	**-0.00012**	**-0.00087**
	(0.0058)	(0.0046)	(0.0017)	(0.0021)
With Province Fixed Effects and Municipality Controls	**0.012**	**-0.0076**	**-0.00019**	**-0.0014**
	(0.0054)**	(0.0047)	(0.0021)	(0.0024)
Observations	422	422	239	245
Mean of Dependent Variable	0.73	0.19	0.26	0.47
Additional Demographic Controls	Yes	Yes	Yes	Yes

Notes:

Standard errors clustered at the municipality are reported in parentheses ($^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$).

Each number represents the coefficient estimate (on the number of violent events during the relevant period, defined below) from a separate regression.

All regressions include child gender, mother's education (category fixed effects), mother's age (quadratic), maximum temperature during month (quadratic) and average precipitation during month (quadratic).

Additional demographic controls include: wealth score (quadratic), 1(Muslim mother), household size, number of children under 5 in the household, number of women aged 15-49 in the household, 1(female household head), household head age, and number of living children of the mother.

Municipality controls include: proportion of households with electricity, proportion of households with own toilet, proportion of women married before age 20, average fertility, proportion of households with any household convenience device.

Relevant Period refers to the month of survey for contemporaneous utilization, month of birth for delivery, and 12 months after birth for vaccination.

In Table A10, I investigate the role of municipality-level unobservables, which I cannot fully account for in the curative care results in Table 4, but do account for in Tables 5 to 6 using municipality fixed effects. To do this, I repeat the analysis conducted in Tables 5 and 6, but replace the municipality fixed effects with province fixed effects. This sheds light on whether municipality-level unobservables that determine health-seeking are positively or negatively correlated with violence, and will therefore help to evaluate the validity of my curative care results in Table 4 (which do not include municipality fixed effects because of the limited time variation).

Each coefficient in Table A10 comes from a separate regression. For reference, the first row repeats the coefficients from the most rigorous specification reported in Tables 5 and Tables 6 (using demographic controls and municipality fixed effects). The second row reports the results from replacing the municipality fixed effects with province fixed effects, and the third row reports the results with province fixed effects and the municipality controls used in Table 4. In column 1, excluding the municipality fixed effects and replacing them with province fixed effects (row 2) results in a smaller (and statistically insignificant) coefficient relative to row 1. This suggests that omitted municipality characteristics that increase the likelihood of home delivery are negatively correlated with violence – in other words, if we consider higher home delivery to be an indication of lower health-seeking, higher-violence municipalities also have higher unobservable health-seeking tendencies. In the next row, the addition of municipality-level controls brings the coefficient estimate much closer to the original estimate in row 1. These results suggest that, if anything, the exclusion of municipality fixed effects in Table 4 results in a downward bias, and that the municipality-level controls are quite successful (for this particular dependent variable) at capturing the municipality-level characteristics that matter.

The rest of the columns (where the dependent variables are now indicators of increased health-seeking rather than decreased health-seeking) tell a similar story. The coefficients are less negative in row 2 than in row 1, suggesting that the violence is positively correlated with unobserved municipality health-seeking tendencies and that the exclusion of municipality fixed effects biases the coefficients upwards (less negative). Unlike in the home delivery regression, adding municipality level controls does not result in coefficient estimates more similar to row 1, suggesting the upward bias may not be fully addressed by the municipality controls that I include in Table 4. The bottom line, however, is that there is no evidence that the exclusion of municipality fixed effects in my curative care results is generating a spurious negative correlation between violence and health-seeking.