Weather Shocks and Violence Against Women in Sub-Saharan Africa

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19th April 2015

Abstract

We use variation in rainfall to study how extreme shocks to income affect intimate partner violence in Sub-Saharan Africa. We find that women experiencing a recent drought are more likely to have been abused during the last year. Though these weather shocks happen randomly, their spatial correlation likely causes bias in the cross-sectional analysis. Using repeated surveys and controlling for area fixed effects, we find that women in the same location are more likely to report abuse in surveys following a drought. Duration analysis shows that drought-induced income shocks do not affect women's overall risk of being abused for the first time in their marriage, but in cases where the household members' income is affected asymmetrically, the shocks give higher risk of abuse.

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§Acknowledgments: The paper has benefited from comments by seminar participants in Bergen, Gothenburg, Oslo and Växjö. The project is part of the research activities at the Centre for the Study of Equality, Social Organization, and Performance (ESOP) at the Department of Economics, University of Oslo. ESOP is supported by the Research Council of Norway.

Introduction

The countries of Sub-Saharan Africa stand out with the highest levels of violence against women in the world (WHO, 2005; Devries et al., 2013). Why that is the case is an intriguing question, and the region has been plagued with other factors that are commonly thought to affect violence, such as conflicts and poor institutional quality. As Sub-Saharan Africa is also the world's poorest region, a plausible hypothesis is that violence is so widespread there because households are poor (Cools and Kotsadam, 2014). We seek to contribute to this understanding by using one of the most common sources of income variation in the developing world, namely variation in rainfall (e.g. Burke et al., 2013; Harari and La Ferrara, 2013).

The strategy of using rainfall variation as a proxy for income shocks has several advantages. The main advantage is in terms of identification. As poor areas are different from rich areas on many dimensions, such as e.g. poor institutions and poor schools, it is hard to disentangle the causal effects of contextual poverty from the effects of these other factors. Similarly, at the individual level it is hard to distinguish the effects of poverty on violence from factors that may cause both poverty and violence such as low impulsive control or self-esteem. By using deviations in rainfall relative to what is normally observed in that location, we are exploiting an exogenous source of variation in income. Whether a rainfall shock occurs one year or the next in a particular location is random, and we are able to exploit this random variation using a fixed effects framework. Rainfall is relevant for African households due to the largely agrarian structure of the economies (Burke et al., 2013) and low levels of irrigation (Miguel et al., 2004). Another advantage of using rainfall shocks is in terms of availability at the local level. Even if economic growth at the local level would have been random, there is no systematic collection of any income data at such a fine level. The main disadvantage of using rainfall shocks lies perhaps in the ability to interpret the effects as stemming solely from income shocks. While rainfall shocks likely affect income, they may also affect other things such as conflicts or the mood of people directly. We are open to the possibility that other channels may also partly explain the relationship between rainfall shocks and violence and our reduced form results do not allow for a distinction of different mechanisms. Nonetheless, the reduced form effects of extreme rainfall variability on domestic violence are interesting on their own, especially as global climate change is predicted to increase flooding and droughts.

The idea of using rainfall shocks to identify the effects of income shocks on violence against women is not without precedence. Miguel (2005) uses local rainfall variation to identify the effects of income shocks on witch killings of elderly women in a district in rural Tanzania. He finds that there are twice as many murders of this kind in the years following weather shocks as compared to normal years. Sekhri and Storeygard (2013) analyze the effects of rainfall in India and find a positive effect of droughts on both domestic violence and murder of spouses, but no statistically significant effect of floods. Our study contributes to the literature by analyzing the effects of weather shocks on a broad set of domestic violence variables in a large sample of African countries with different climate and cultures. In addition to being more general, this also enables us to to test for impact heterogeneity across individuals as well as across communities and to link the effects to the relative income effects within households.

We combine data on precipitation from the ERA-Interim project with the best available data on domestic violence, namely the Demographic and Health Surveys (DHS). In recent years, the DHS surveys include questions on domestic violence, and they also entail GPS coordinates at the level of the primary sampling unit. This enables us to connect experiences of abuse to rainfall shocks. Our total sample size consists of more than 100,000 women in 14 countries. Despite being the best available large scale data set on domestic violence, DHS is still limited in time as the respondents exposed to the domestic violence module are surveyed between 2003-2011. Furthermore, we have repeated observations on domestic violence for six countries only. We therefore analyze the data using several different strategies with their corresponding advantages and disadvantages.

We start by using variation in weather shocks in the total cross-sectional sample, and analyze whether rainfall shocks are correlated with the risk of having been abused last year. This strategy uses a large sample but has serious drawbacks in terms of internal validity. Since rainfall is determined by large weather systems, places that are close to each other are likely to be affected by shocks at the same time. As places close in space are likely to share many characteristics including culture and institutions, areas that experience a shock are likely to be different from areas that do not.

As it is not possible to control for area fixed effects in this wider sample for this variable, we proceed to use two other strategies that enable us to control for grid level fixed effects. The second strategy consists of using a limited sample from six countries for which we have repeated surveys. Even though the surveys are not longitudinal in the sense that each household is followed over time, it is a repeated cross section whereby we have observations from two time periods at the grid level. Aggregating the data to the grid level and measuring the effects of droughts and floods on the change in the probability of women ever having been abused essentially controls for all time invariant confounders. This strategy is thereby stronger in terms of identification, but the drawbacks are a limited sample size and that the two samples in the same areas but different years might be different.

The third strategy instead exploits the fact that women in the DHS surveys are asked how many years after marriage they were abused the first time. Since we also know how long they have been married, we can combine our weather data with the timing of first abuse and see whether weather shocks affect the chances of being abused. We deem this strategy to be strongest in terms of internal validity as we are exploiting a time series within the same sample. These last two analyses are useful in analyzing the determinants for the extensive margin of abuse, i.e. to determine what factors contribute to husbands crossing the important boundary it is to start becoming abusive towards their wives. However, they do not capture the intensity nor likelihood of re-occurrence of violence within a relationship.

Using the first cross-sectional strategy, we find that women who lived in areas where a drought or flood occurred during the last or second to last rainy seasons are more likely to have been abused last year. These results should however not be interpreted as causal, due to the low number of shocks for very few years in combination with strong spatial correlation across areas. We furthermore find the rainfall shocks to be correlated with religious affiliation and with the respondents' fathers having beaten their mothers. While a statistically significant correlation is still observed after controlling for observables, there is no way we can be certain that there is no unobservable factor correlated with both the rainfall shocks and violence.

Moving on to the strategy where we use repeated survey rounds in the same place, we also find a statistically significant effect whereby weather shocks are correlated with a higher risk of abuse. We are more confident in interpreting these estimates causally, as the repeated cross sectional design allows for controlling away all time invariant factors at the grid level. There is still a risk that the shocks are correlated with some unobserved time variant factor affecting violence. We deem this possibility unlikely as the shocks are not correlated with any observable characteristics, nor are the results affected by controlling for changes in other covariates including changes in the levels of violence of the women's fathers against their mothers.

Finally, when linking the rainfall shocks to the timing of first marital abuse we essentially get a long time series of both shocks and violence. Employing a duration analysis, we can investigate whether rainfall shocks cause a higher likelihood of being abused for the first time. Since the time period for this duration analysis is longer, we can control for grid level fixed effects and still retain a large sample. We find that drought-induced income schocks do not affect women's overall risk of being abused for the first time in their marriage, but in cases where the household members' income is affected asymmetrically, the shocks give higher risk of abuse.

As weather shocks reduce incomes if occurring during the rainy season when crops are sensitive to water, we believe these reduced form results to be driven by negative income shocks. We show that droughts affect wealth and we further control for temperature which is the other main channel. Erratic incomes of poor households, often without insurance, are of course negative for many reasons and we add violence against women to that list. The study also contributes to an understanding of weather shocks and health, an understanding that is unfortunately increasingly important due to climate change with more droughts and floods in the years to come.

Theories on the effect of weather on violence

Floods and droughts cause negative income shocks. The first part of this section reviews the literature with respect to the possible links between income and violence. The effect going from droughts and floods to violence may, however, also work through other channels. In particular, bad weather has been linked directly to frustration and aggression (i.e. not via income) as well as to eco-migration. The second part of this section reviews the literature with respect to these other mechanisms. Finally, it is likely that the effects of weather shocks differ from one place to another and for different people and at the end of this section, we briefly review the literature suggesting heterogeneous effects and derive a number of hypotheses that are testable using our data.

Income and violence

Empirical studies document a strong negative correlation between economic development and domestic violence at the country level (Duflo, 2012; Doepke and Tertilt, 2009) and the positive association between poverty and crime is one of the most robust findings in criminology (Benson et al., 2003). Poorer women within countries are generally more exposed to violence than are women with higher incomes (Benton and Craib, 2011; Jewkes, 2002; True, 2012).

A few studies have investigated the relationship between rainfall and crime. Mehlum et al. (2006) find that low rainfall in 19th century Germany increased crime and they show that the effect runs via increased grain prices lowering real wages. Two previous economics papers have analyzed the effects of rainfall on violence against women directly. Both interpret the findings as going through income. Sekhri and Storeygard (2013) analyze the effects of rainfall in India and find a positive effect of droughts on both domestic violence and dowry deaths but no statistically significant effect of floods. They interpret the findings using a theory where men exert violence on their wives in order to extract resources from in-laws. Dowry killings further enable the man to remarry and receive dowry payments from a new family. They document that dowry killings in India lead to more dowry payments. Miguel (2005) uses local rainfall variation to identify the effects of income shocks on murder of “witches” in a district in rural Tanzania. He finds twice as many witch murders in years following droughts or floods. He further shows that extreme rainfall leads to large income drops at the local level, with somewhat larger effects of floods, and interprets the results as negative income shocks leading to the murdering of old women.

Male and female income

One of the most popular sociological theories of domestic violence, the resource theory, suggests that men with few other resources may use violence to maintain dominance within the family (Goode, 1971; Vyas and Watts, 2009). Similar predictions are obtained in economic bargaining theories of domestic violence. These models focus on women’s outside options, which is usually considered to be the utility level in case of divorce (e.g. Lundberg and Pollak, 1996; Pollak, 2005; Eswaran and Malhotra, 2011). Improved outside options through increased individual income should then reduce violence within marriage – all else equal (Farmer and Tiefenthaler, 1997).

Several studies highlight the importance of changes to relative position between the spouses in explaining domestic violence. According to the backlash theory, men may use violence to counteract the increased power gained by women and in order to reinstate their dominance (e.g. Eswaran and Malhotra, 2011). Hjort and Villanger (2011) find that domestic violence increases as women become employed in flower farms, in a randomized field experiment in Ethiopia. Similarly, in status inconsistency theories, where atypical roles threaten male identity (Hornung et al., 1981), women having more resources than men could lead to increased violence. In bargaining models where violence is instrumental or extractive, increased female income may lead to more violence as there are more resources to extract (e.g. Bloch and Rao, 2002).

Frustration and cognitive mechanisms

Poverty leads to frustration which gives rise to mental processes related to escape or aggression. The frustration-aggression hypothesis is the most common psychological theory explaining the link between poverty and aggression (Barlett and Anderson, 2013). Negative *shocks* also trigger frustration as frustration arises when reality turns out to be worse than expected (Munyo and Rossi, 2013). An unanticipated negative drop in household income thereby leads to frustration and frustration in turn is thought to lead to aggression. Similar predictions are found in the so called negative affect theory proposed by Berkowitz (1983) which stipulates that aversive events increases aggression. Experiments with animals show that unexpected negative shocks cause frustration leading to aggressive behavior (see Munyo and Rossi (2013) for an overview). Consistent with such a framework, Card and Dahl (2011) find that upset losses in American football, i.e. losses where the team was expected to win, lead to more violence against women. Similarly, Munyo and Rossi (2013) use soccer games as natural experiments and combine this with property crime data from Montevideo, Uruguay. They find that frustration leads to a spike in violent crime (robbery) one hour after the game.

Having insufficient income to meet the needs of one's family is also stressful, and stress is thought to influence the degree of domestic violence (Jewkes, 2002).[1] In addition, anxiety lowers the ability of people to exert self-control. Mani et al. (2013) find that poverty reduces cognitive capacity in a laboratory

[1] As poverty also leads to stress, Barlett and Anderson (2013) argue that The General Aggression Model (GAM) is a better psychological framework for understanding the effects. GAM argues for an interaction between personality and situational input variables that affect internal emotional state that then lead to either aggressive or non-aggressive behavior. As we are primarily interested in the reduced form effects of weather on violence, without specifying whether the internal mechanism is via e.g. frustration or stress, we remain agnostic about what psychological theory best explain the relationship and we are content with showing that there are many possibilities via which income may be psychologically related to violence.

study as well as in a field study. Poverty also requires demanding mental processes as the poor must face harder tradeoffs and try to keep expenses down. As the human cognitive system is limited these poverty induced mental processes compete with other necessary processes for attention. In the field study they exploit that Indian sugarcane farmers experience cycles of poverty whereby they are poorest just before harvest and richest just after. They find that the same farmer has lower cognitive ability when poor as compared to when being relatively richer. As cognitive capacities such as self-control are consistently argued to be important in the psychology of violence (e.g. DeWall et al., 2011) there is a plausible link between income shocks and violence against women at the individual level.

Poor areas

In addition to the effects of individual level poverty, there is an argument to be made that poor areas cause more violence. Benson et al. (2003) argue that there is more violence in poor communities for cultural and institutional reasons. They build on social disorganization theory which predicts poor areas to have weaker social bonds between individuals leading to less social control and more social isolation. Hence, even if the acceptance of violence is not higher in such areas, people are less likely to intervene and the levels of abuse are argued to be higher. The husbands thereby gain a type of impunity, which is even higher if acceptance rates are also higher in poorer areas. Indeed, Benson et al. (2003) argue that poor areas also have higher acceptance rates.[2] Poverty at the contextual level also reduces the quality of social institutions such as the local police, which may further aggravate problems of violence (Uthman et al., 2009).

Weather shocks are perfectly correlated within a community. Therefore our results show the combined effect of negative income shocks to the community as well as to the individual. The fact that they are shocks, as opposed to more stable characteristics, also gives rise to potentially additional mechanisms. Barlett and Anderson (2013) argue that aggregate economic downturns make people fearful, frustrated, stressed and hostile and they find that indicators of poor economic times in randomly assigned news clips cause aggression related emotions. This suggests that aggregate economic shifts influence aggression at the individual level, perhaps in addition to the effects of more constant levels of

[2] Uthman et al. (2009) find that individuals living in poorer areas have higher acceptance of domestic violence in a study of 17 African countries between 2003 and 2007.

poverty.

The relationship between rainfall and group conflict is consistently interpreted as going via income. Miguel et al., (2004) find that lower rainfall growth led to more conflict in SSA and also documents that economic growth is lower when rainfall growth is lower. Bohlken and Sergenti (2010) use a similar approach and show that negative rainfall shocks increase Muslim-Hindu riots in India. The mechanism leading rainfall shocks to conflict in the economics literature goes via income in the sense that lower incomes lower the opportunity cost of violence and reduces the state's capacity to defend itself (Dell et al., 2013).

Other mechanisms

Weather shocks may affect violence through other channels. The most commonly studied weather induced effect on violence links temperature directly to aggression. Ranson (2014) finds crime to be a function of temperature over 50 years in 300 U.S. counties. Jacob et al. (2007) find that higher temperature increases violent crimes and that increased rainfall reduces it. The authors interpret the results as weather having a direct and immediate effect on crime. High temperature is consistently associated with violence in the psychological literature, where the heat hypothesis states that hot temperature increases aggression by increasing hostility feelings and thoughts (Anderson, 2001). The simplest version of the theory is that people become cranky at uncomfortable temperatures. The result that heat leads to violence has been found in cross sectional and panel data studies, in experiments, and in historical analyzes (see Anderson and DeLisi (2011) for an overview).[3]

Floods and droughts may also cause both temporary and permanent migration, which may increase violence. Anderson and DeLisi (2011) review the effects of weather changes on violence via Eco-migration. When Hurricane Katrina hit the US in 2005, it flooded about 80 percent of New Orleans, causing over a million people to emigrate. Initially, Houston received most migrants and had a huge increase in homicides the following months.[4] Also cooling is

[3] There is some evidence that the relation between weather and violence is biological, at least regarding temperature (Dell et al., 2013). Temperature affects serotonin neurotransmission in the brain which in turn affects aggression (Tiihonen et al., 1997) and recent molecular genetics studies have found gene-environment interactions whereby temperature affects neuro and hormonal pathways in the areas of thermoregulation and emotion regulation in the brain.

[4] Similarly, during the US Dust Bowl in the 1930s, where prolonged drought lead to envi-

related to violence. The little ice age between 1300 and 1850 was a period of much war and violence that Fagan (2000) attributes to the climate change via its effects on food shortages. Anderson and DeLisi (2011) argue that all types of rapid climate change in terms of cooling, heating, floods, and droughts, are likely to spur violence because it threatens human resources. As bad weather in terms of rainfall may also affect migration and people's mood directly, just as temperature, we keep the possibility of these causal pathways open.

Data

Survey data

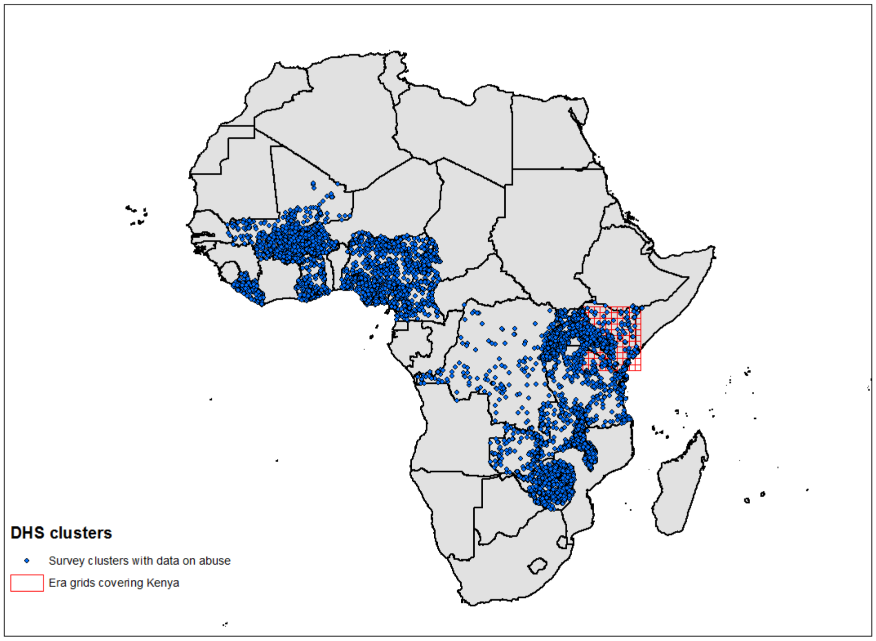
We use data from the Demographic and Health Surveys (DHS), which provide standardized surveys across years and countries. DHS include GPS coordinates at the DHS cluster level, a cluster being one or several geographically close villages, or a neighborhood in an urban area.

Dependent variables

At the end of the 1990s, a standardized module was developed with questions about the respondents' experience with domestic violence. We combine all surveys carried out in Sub-Saharan Africa (SSA) that contain information on experience with wife-beating and GPS coordinates into one data set. The total sample size becomes 104,334 women in the age span 15-49 years, from 14 countries, over the years 2003-2011, living in 25,027 survey clusters. The distribution of the sample across SSA is shown in Figure 1.

ronmental disaster around Oklahoma over 2 million people left the area and reports of violence are widespread. Whether these are effects on violence of weather via income, migration, or direct frustration is of course difficult to disentangle.

Figure 1: Distribution of DHS clusters across SSA and illustration of grid cells in Kenya



These data are collected in the domestic violence module, implying that not all women are selected to answer these questions. Domestic violence is measured using a modified Conflict Tactics Scale (CTS), which has several advantages compared to many other datasets on violence (see Kishor (2005) for an extensive overview). A characteristic of CTS is that it uses several different questions regarding specific acts of violence. In this way the measure is less likely to be polluted by different understandings of what constitutes violence. CTS is also argued to reduce under reporting, as it gives respondents multiple opportunities to disclose their experiences of violence (Kishor, 2005; La Mattina, 2013).

The interviewers who use the domestic violence module are trained specifically to handle the sensitive questions of domestic violence, and they follow a strict protocol ensuring privacy. In particular, the interviewers are instructed to check all the surroundings within hearing distance for the presence of others. Only children young enough to not understand the questions are allowed to be present. The interviews are not allowed to proceed if privacy is not ensured, and the interview is terminated if someone enters the zone (DHS 2011, Interviewer's manual for the domestic violence module).

The care with which data is collected inspires confidence that the problem of under reporting is as low as possible. Furthermore, the high reported prevalence of violence across the region suggests that a considerable degree of women are willing to report violence. Likewise, the high acceptance of wife-beating in the region documented in Cools and Kotsadam (2014) suggests that social acceptability bias in reporting may be of less importance than in other settings. Palermo et al. (2014) use 24 DHS surveys to provide bounds for other sources of violence data such as health systems data or police records. They found that only 40 percent of the women having experienced domestic violence in the DHS surveys had reported this to someone and that only 7 percent had reported it to a formal source. Hence, while we may worry about under reporting, we are still likely to have one of the best sources of data.

Only women who are currently or have ever lived with a partner are selected to answer the questions about experience with intimate partner violence. The module includes questions about both emotional and physical (including sexual) violence. Our focus in this paper lies with physical violence. Violence is recorded as having occurred for women who answer that they have had a partner doing one of the following to them: Pushing, shaking, slapping, throwing something, or twisting an arm, striking with a fist or something that could cause injury, kicking or dragging, attempting to strangle or burn, threatening with a knife, gun, or other type of weapon, and attacking with a knife, gun, or other type of weapon, physically forcing intercourse or any other sexual acts, or forcing her to perform sexual acts with threats or in any other way. Using this definition, 26 % of the women in our total sample were subject to abuse during the last twelve months and 32 % have ever been victims of intimate partner violence.

All women stating that that they experienced abuse from their last partner are also asked when this first happened. The question asked in the module places the first violent episode in relation to either the year of marriage or the year of cohabitation of the *last* partner. A different question asks the women when they *first* married or entered cohabitation (married for short). We therefore had to limit the sample to women who only married once, and who were still married at time of interview. For consistency, we also removed women who were subjected to violence by their husbands before marrying, or reported a later occurrence than the time between marriage and the survey would allow for. Another worry is that the interviewed women misreport the timing of the first violent episode, which is likely to be a larger problem if the first occurrence happened a long time before the survey. We therefore follow Baird et al. (2010) and reduce the sample to women who have been married ten years or less. The women thus enter the sample when they marry, and exit the sample when they are beaten for the first time or in the year of interview, with the year of interview being up to 10 years after marriage.

The Kaplan-Meier hazard rates are shown in Figure 2 for women who have been married 10 years or less, and women who have been married more than 10 years. The observed rates strengthen our suspicion that women who have been married for a long time tend to forget violent episodes that occurred early in marriage, and they also seem to remember the timing of the abuse with less precision as they heap the reporting of violent episodes on five and ten years more than women who have been married for a shorter period. For women married 10 years or less, the risk of experiencing violence for the first time is the greatest after 1-2 years in marriage, and then falls sharply. 23 % of the women married 10 years or less experience violence in the period between their wedding and the survey. The Kaplan-Meier failure rate is 31.5 %, which is the expected share who would experience violence if staying married for ten years.

Figure 2: Yearly risk of first violent episode

Other variables from the DHS

The DHS include extensive information on individuals' background characteristics. We use this information partly to control for observables and to test our identifying assumptions but we also take advantage of the information to test for heterogeneous treatment effects both across individuals and contexts. A first variable that is present for a sub sample of 85,996 women asks whether their mothers were abused by their fathers. We use this variable partly as a placebo test as it should not plausibly be affected by recent weather shocks yet be highly associated with abuse. 25 % of the women answer affirmatively on the question and these women are 117 % more likely to have been abused by their partners (the share increases from 23 to 50 percentage points).

We also add control variables for education, age, and religious affiliation. The educational attainment of women and their partners is measured by years of education and their highest level of educational attainment. On average, the women in our largest sample have 4.5 years of education. We create four indicator variables for the highest attained level of education: No formal education (37 %), elementary (40 %), secondary (20 %) and post secondary education (3 %). The women in the same sample are 30 years old on average and we create seven indicator variables for 5 year intervals ranging from 15-19 years to 45-49 years. Furthermore we create indicator variables for being Christian (41 % of the sample) and Muslim (31 %).

Theory predicts that employment and acceptance may be important moderators for the effects of rainfall shocks on abuse, and we use information on these factors to investigate heterogeneous treatment effects across women. To measure employment, we use several different measures. The first measure is *employed last year* which equals one if the woman was working during the last 12 months; 70 percent of the women responded affirmatively. Women who are not working may be engaged in child care, household production, or backyard farming. In addition, we know in which sectors the individuals are employed and we create a variable for working in agriculture (35 % of the women).[5] The women are also asked if their partners were working the last 12 months and in what type of occupation. We use this information to create an indicator variable which equals one for the 46 % of the women having a partner working in agriculture. Acceptance of wife beating is operationalized by creating an indi-

[5]The different sectors, or type of work, are categorized as: professional, clerical, sales, agricultural self employed, agricultural employed, domestic, service, skilled manual, and unskilled manual.

cator variable that equals one if the person agrees that a husband is justified in beating his wife in any of the five following situations: She goes out without telling him, she neglects the children, she argues with him, she refuses to have sex with him, or she burns the food. 50 % of the women in our largest sample agree that husbands are justified in beating their wife in at least one of these situations.

As we want to investigate whether there are different effects of weather shocks in different areas, we analyze the effects in different samples. First of all, we know that 73 % of our largest sample of women live in rural areas. We also create contextual variables based on individual level data by aggregating the information on parental abuse and the acceptance of wife-beating into averages at the DHS cluster level, excluding the individual's own observation. This method (also known as jackknifing) ensures that the individuals' own characteristics are not conflated with those of the surrounding community.

Weather data and construction of weather shocks

The data on total precipitation which we use comes from the ERA-Interim project, and is produced by the European Centre for Medium-Range Weather Forecasts. The data is recorded twice daily on a 0.75 x 0.75 degrees grid level from January 1 1979 to December 31 2011. The overwhelming majority of data comes from satellites, yet data from radiosondes, pilot balloons, aircrafts, wind profilers, ships, drifting buoys, and land stations is also used (Dee et al., 2011). The NCO program produced by Zender (2008) was used to produce monthly aggregates.

Our interest lies in violence that stems from income shocks created by extreme weather, and we therefore need to specify relevant seasons when agricultural yields are affected by rainfall. As the African continent generally does not enjoy abundant and reliable rainfall, adjusting crops, planting and harvest times to the rainy season is crucial to maximize agricultural production (Liebmann et al., 2012; Ati et al., 2002). If we assume that farmers make adjustments that are more or less optimal over time, we should therefore expect that rainfall during the rainy season when rainfall is plentiful matters the most for yields. We follow the method of Liebmann et al. (2012) closely in constructing rainy seasons that are consistent for Africa as a whole. First, we identified the rainy season in each grid cell as a continuous sequence of calendar months with average monthly rainfall above the yearly average. If there were multiple sequences, we chose the sequence during which the sum of monthly levels of precipitation was the largest.

There are studies which use absolute measures of droughts and floods, such as the number of reported deaths (Neumayer and Plumper, 2007), and in theory it should also be possible to use an absolute measure of rainfall in the analysis. An absolute analysis would ideally be able to identify shocks that were similar in terms of biological or economic impact. However, such an analysis would be less likely to be causal, as the effect of living in a drought-prone area would be indistinguishable from the effect of the drought itself. More flood-prone areas are for instance located closer to the coast, and were therefore for instance more affected by slave trade which is known to affect levels of income and trust between people living there today (Nunn, 2008; Nunn and Wantchekon, 2011). Furthermore, the effects of a given absolute shock, e.g. in terms of number of reported deaths, are obviously affected by the institutions and levels of income in the area and not only by exogenous factors.

To construct our relative measures of droughts and floods, we created a gamma distribution of rainfall during the rainy seasons between 1979 and 2011 for each grid cell. The gamma distributions were used because the distributions tend to be right-skewed, and we therefore wanted a model with full flexibility (Burke et al., 2013).

In the cross-section, we study rainfall shocks during the season which ended the last year before the survey and the season before that, as we believe these are the relevant income shocks for violence during the last year before the survey. In the repeated cross-section, we study changes to the risk of ever having been subjected to violence. We thus use the largest number of seasons which we can use whilst being certain that women in the first survey are unaffected by the shocks affecting them later. Lastly in the duration analysis, study both the effects of droughts and floods which occurred during rainy seasons which ended during the same 12 months in which violence was recorded, and weather events which happened the year before the violent episode. The first effect will contain some that were affected after the bad yield had been harvested and the income shock was a fact, whereas others may have experienced violence before harvest, perhaps in anticipation of the crop failure. The latter captures effects during a complete year of exposure to a failed harvest.

Rainfall and household wealth

Using the sample where we have two surveys, we also make attempts at testing directly whether rainfall has an impact on wealth in the household. Wealth is an imperfect proxy for yearly income, in particular because it is more stable across time. However, this should bias results towards 0, thus any negative effect of a drought on the wealth index can be regarded as a lower bound on its effect on income. Our measure of household wealth is based on the wealth index provided in the DHS. The wealth index is a standardized measure of assets and services for households in a given survey, such as type of flooring, water supply, electricity, and the ownership of durable goods such as a radio or a refrigerator.[6]

We would also like to check that the effect of rainfall on wealth is found across the larger sample which we use for the duration analysis. Where we only have one survey with violence data, we thus extend the sample with prior surveys which have the wealth index yet lack violence data.

Empirical strategies

Cross sectional analysis

In analyzing the effects of weather on violence against women, we perform several different types of analyzes. First, we ask whether having experienced a drought or a flood during a nearby rainy season is related to having been abused the last year. Since we know the time of interview and as we have panel data on precipitation, the estimation will be of the following form:

$$AbusedLastYear_{i,g,t} = \beta_1 D_{g,t-1} + \beta_2 D_{g,t-2} + \beta_3 F_{g,t-1} + \beta_2 F_{g,t-2} + \alpha_s + X_i + e_{i,g,t} \tag{1}$$

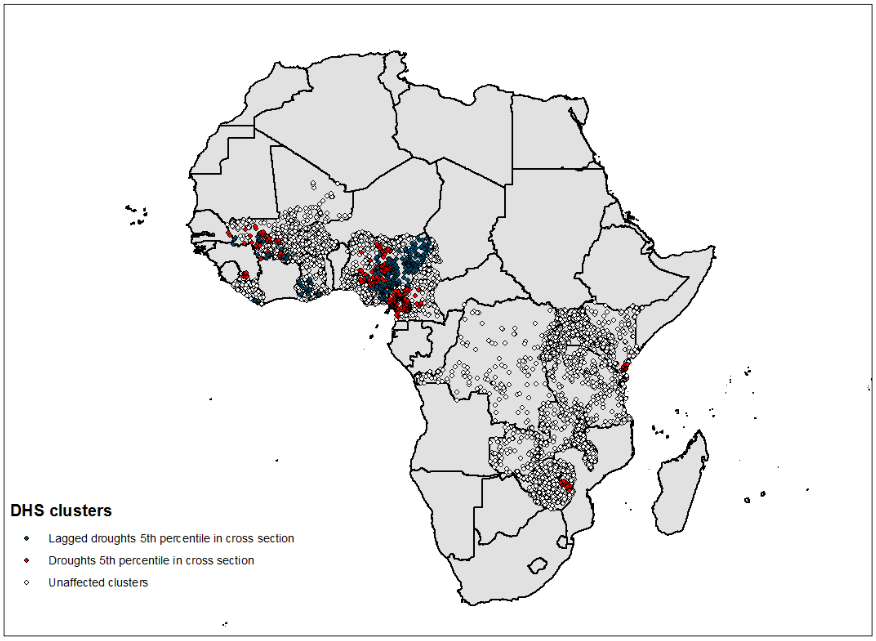
where the dependent variable is a variable for whether a woman i living in grid cell g was abused in the year before the time of interview t. $D_{g,t-1}$ and $D_{g,t-2}$ are negative shocks to precipitation in grid g below a predetermined percentile in the grid-specific gamma distribution during the last and next-to-last completed rainy seasons before the month of interview respectively. $F_{g,t-1}$ and $F_{g,t-2}$ are the corresponding positive precipitation shocks. We include survey fixed effects α_s, which control both for the year of survey as well as the country, and as such, we are only comparing the effects of shocks within a

[6] The assets are connected to an underlying regional specific wealth score and using principal components analysis they are assigned a weight that is used to calculate the overall score. The score is then standardised within the survey and each household is then assigned a relative position. See Rutstein and Johnson (2004) for an extensive description of the wealth index.

given country and year. X_i is a vector of individual level control variables that are plausibly unaffected by recent weather shocks (age, education, and religious affiliation). As the shocks occur at the grid level, we cluster the standard errors at this level.

As the same percentile cutoff is used in each grid cell, all cells have the same probability of experiencing a shock in a given year (Burke et al., 2013). It is only because the shocks occur at different times that some areas will experience shocks and some will not. Nonetheless, the results may still be driven by some unobservable that is correlated with both the shocks and violence. This is due to the relatively short time window of our data and that there is spatial correlation in rainfall across areas. This implies that a shock in one place is correlated with shocks in places nearby and places nearby are likely to be similar in terms of other characteristics, some of which may be correlated with abuse and therefore causing omitted variable bias in our estimates. As we see in Figure 3, the geographical location of the clusters who experienced a drought during either the last or the next-to-last rainy season are concentrated in a few regions only. We investigate this potential problem in a number of ways. First, we perform a placebo test of whether the woman's father beat her mother is systematically linked to the rainfall shocks. We then also control for this variable as well as for other factors that correlate with violence but ought not to be correlated with recent rainfall shocks, in particular age, religious affiliation, and education, and we assess whether the results change due to this inclusion. Finding a similar effect with these controls make the results more credible, yet they are still unreliable especially if these observable characteristics differ across areas. In particular, there is no way of knowing whether some omitted variable is biasing the results and we cannot be certain of a causal interpretation of these results.

Figure 3: Distribution of recent droughts in the cross section



Since we only have data on each individual observed during one year, we are not able to control for area fixed effects in investigating the probability of having been beaten the last year using the total sample. Controlling for area fixed effects requires that we have several observations from the same areas and is greatly beneficial in terms of identification as it controls for all time invariant factors that may differ across areas. In what follows, we pursue two strategies whereby we can control for grid level fixed effects.

First differences using the repeated cross section

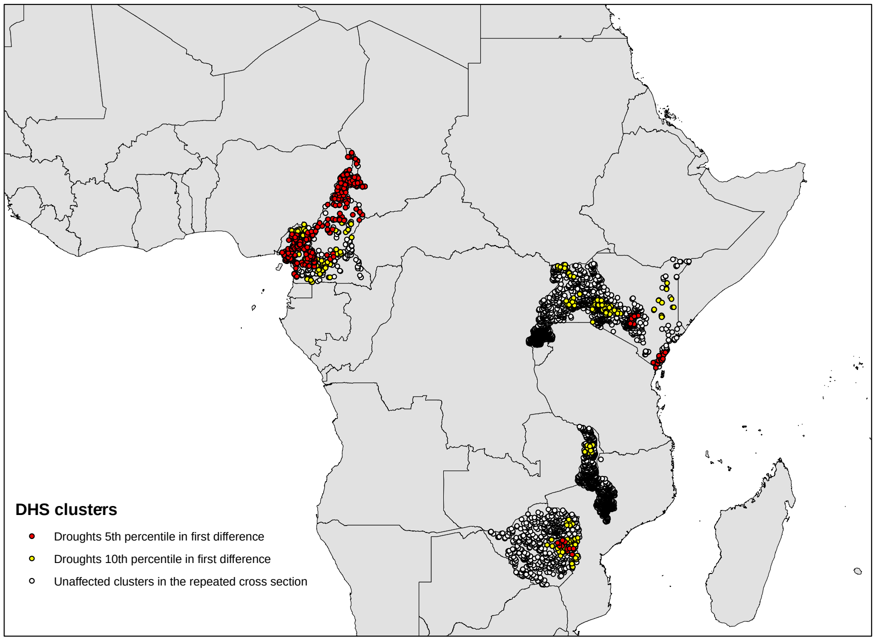
Our first strategy for controlling for area specific factors uses the fact that we have a repeated cross section of surveys in six of our DHS countries: Cameroon, Kenya, Malawi, Rwanda, Uganda, and Zimbabwe. We link the average level of violence in each grid cell for each survey with weather shocks that have occurred the last four rainy seasons, as there are four to six years between the survey rounds. The difference in physical violence between the survey rounds within each grid cell is then regressed on the difference in weather shocks. We estimate the following specification for our 258 grid cells that have two observations over time:

$$
\begin{aligned}
&\overline{Ever\ Abused}_{g,r_2} - \overline{Ever\ Abused}_{g,r_1} = \\
&\qquad \beta_1 \left[\max\left(D_{g,r_2-3}, ..., D_{g,r_2}\right) - \max\left(D_{g,r_1-3}, ..., D_{g,r_1}\right)\right] + \\
&\qquad\quad \beta_2 \left[\max\left(F_{g,r_2-3}, ..., F_{g,r_2}\right) - \max\left(F_{g,r_1-3}, ..., F_{g,r_1}\right)\right] + \alpha_c + e_g \qquad (2)
\end{aligned}
$$

Where the dependent variable is the difference between the second and first surveys in the grid cell average probability of ever having been abused and r_1 and r_2 denote the first and second round, respectively. The weather variables indicate whether there has been at least one weather shock in the grid cell during the last four rainy seasons before the first woman was interviewed in the grid cell that round. We control for country fixed effects α_c so that only differences within a country are compared. In some specifications we also include controls for differences in other important determinants of violence, namely composition differences in age, religious affiliation, education and parental abuse.

The distribution of droughts in this sample is shown in Figure 4 and we see that there is little spread in shocks using the 5th percentile of the gamma distribution. Apart from a few clusters in Kenya and Zimbabwe experiencing droughts at this severity, mostly observations in Cameroon are affected. Using the 10th percentile, we see that that there is a bit more spread in the distribution of the droughts, but there is still no drought in Rwanda even for this definition of a drought. The main advantage of this specification is that it allows us to use grid fixed effects and we are therefore not worried about time invariant omitted variables at the grid level. Thereby the low spread of the shocks is of less concern in this analysis than in the previous one for internal validity. An issue with internal validity that is perhaps more worrisome is that we have to take it on comparison of observables that the populations interviewed in the two different surveys are similar, which is why we again control for parental abuse and other differences in composition. We would also like to limit the sample to only include individuals that have always lived in the same place as weather-induced migration may make the composition in the treatment and comparison grids different. This information is, however, not available in all surveys and the sample reduces to only seventy grid cells once we do this. A different issue is of course whether these results are generalizable beyond Cameroon where so much of the variation stems from. A further issue is that the sample is much reduced and we may also still worry that our weather shocks have affected areas with a different time trend in abuse than the areas that are not affected.

Figure 4: Distribution of droughts in the repeated cross section



Duration analyses of first time abuse

In the violence module of DHS surveys, women who had experienced violence from their partners were also asked how long it took after they got married or started living together until the first violent episode occurred in the partnership. This information provides a time series as the interviewed women are married in different years, which we can use to control for unobserved omitted variables that differ across space and time. To that end, we employ duration analysis to study whether the time from marriage until the first violent episode is affected by rainfall shocks.

The duration models are specified in the following way:

$$\log\left(\frac{First\,violence_{i,g,t}}{1-First\,violence_{i,g,t}}\right)=\beta_1\bar{D}_{g,t}+\beta_2\bar{F}_{g,t}+\alpha_g+\alpha_t+\alpha_a,$$

Where the dependent variable is the relative risk that woman i in grid cell g is abused for the first time in year t of her marriage, $\bar{D}_{g,t}$ and $\bar{F}_{g,t}$ equal 1 if year t of marriage is affected by a previous drought or flood, α_g are grid cell fixed effects, and α_t are piecewise constants by year of marriage. The piecewise constants ensure full flexibility in the baseline rate, which is preferred as we do not have any prior expectation about the form of the time-dependence in the process (Blossfeld et al, 2012). As the sample is restricted to only include older women if they married at a late age, we included 5-year age of marrying groups, α_a, to make sure that the period effects we are studying are not confounded with age or cohort effects.

We used two different specifications of drought- and flood-affected marriage years. As in the previous strategies, both regard a year with rainfall during the rainy season at a level below that which occurs with some probability p that year or the previous year as drought-affected, and flood-affected if rainfall exceeds the corresponding probability $1-p$. The first specification models these shocks as permanent changes in the relationship, so that all later years are affected. The variable can then be specified as:

$$\bar{D}_{g,t} = \max\left(D_{g,m-1}, \ldots, D_{g,t}\right) \text{ and similarly for floods}$$

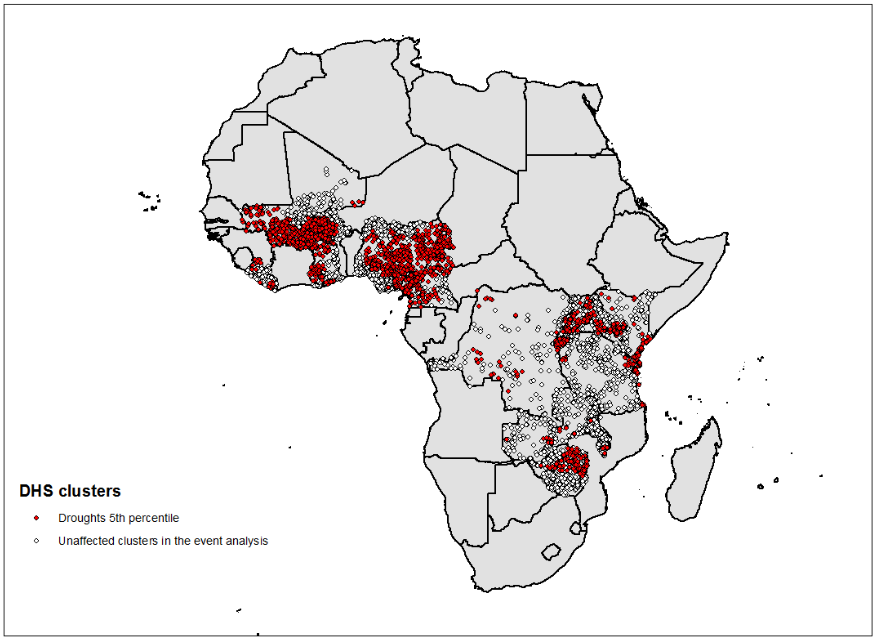
Where m is year of marriage and $D_{g,t}$ is a drought occurrence in grid cell g in year t. The second specification models these shocks as transitory, affecting violence only those two years, and then returning to a non-affected state. The indicator of drought and flood affection then becomes:

$$\bar{D}_{g,t} = \max\left(D_{g,t-1}, D_{g,t}\right) \text{ and similarly for floods.}$$

The first effect is perhaps a more correct representation if income shocks raise the level of conflict in the relationship, and tensions remain higher thus becoming an impelling factor for violence. The latter effect is to be expected if droughts create an immediate frustration which triggers violence in the relationship. Some women experienced two shocks or more during their marriage. Two years with less income than expected may have a stronger effect than one, especially if they occur close in time. The second shock was included as an additional variable in both models.

This sample consists of 36,770 women in 922 different grid cells, sampled in 20 surveys in 14 countries, and entered marriage between 1992 and 2011. In total we have 182,659 events, i.e. marriage years. The distribution of droughts in this sample is shown in Figure 5. At the 5 % drought level, 8 862 women were affected by at least one drought while still in the sample and 2,949 by two droughts, which equals 24 % and 8 % of the total sampled women. Similarly, 9 065 and 2 411 women were affected by at least one and two floods.

Figure 5: Distribution of droughts in the duration sample



It is important to note that whether a drought and flood occurs one year or the next in a specific place is essentially random (e.g. Dell et al., 2013). The control group in this analysis consists of marriage years in the same grid cell that were unaffected by the shock, which are experienced by the drought-affected woman herself earlier in time, other women who married earlier and therefore had more marriage experience before the shock, and/or women who married later and were therefore unaffected by the shock. How these groups are divided is random. This is different from the repeated cross section analysis, as we are here able to exploit the exact timing of the weather incident in relation to the violent episode. We therefore no longer worry about different trends in affected areas or compositional changes between rounds of studies.

The repeated cross-section and duration models both analyze factors that contribute to violence against women who have not previously experienced violence in their relationship. They are thus useful in revealing what might make husbands crossing that important boundary it is to start becoming abusive towards their wives. However, they do not capture the intensity nor likelihood of re-occurrence of violence within a relationship. If previous abstention from violence is an inhibiting factor, the results may be viewed as a lower bound on the total effect of rainfall shocks on violence.

Along these lines of reasoning, we may think about shocks as explaining the timing of the first occurrence of a violent episode, but that other factors - some of which are unobservable - determine who is subjected to violence and who is not. In our baseline duration analysis, we include many women who never experience violence during the first ten years of marriage, regardless of weather shocks. Their inclusion leads to an underestimation of the effect of a drought on violence in marriages where violence occurs. Furthermore, it could be the case that violence increase the propensity for divorce, in which case the women are not in our sample which leads to an underestimation. An opposite selection effect would be present if women marry more violence-prone men due to a drought than they would have done otherwise. For violent marriages, the effect of a drought on first occurrence can be better estimated using individual fixed effects. Specifically, we use the case-crossover method described by e.g. Greenland (1996) and Allison and Christakis (2006) in which each woman who experience a drought is compared to herself at a time when she is not drought-affected. We run the regression:

$$\log\left(\frac{First\,violence_{i,g,t}}{1-First\,violence_{i,g,t}}\right)=\beta_1\bar{D}_{g,t}+\beta_2\bar{F}_{g,t}+\alpha_i+\alpha_t$$

Where $\bar{D}_{g,t}$ is the transitory shock. A necessary condition for this model is that there is no time trend (across the marriage years at risk) in the probability of being subjected to weather shocks. We check that this is the case in our sample.

We were also interested in how the effect of a weather shock is conditioned upon factors at the household and the community levels. Poverty is one such dimension. Using the wealth index, we looked at how droughts affect violence in poorer vs. richer households and we also split the sample into poorer and richer neighborhoods using the jackknife method. In a final section, we deviate from the unified household model by considering households where the relative incomes are likely to be affected by the weather shocks. Here, we exploit information about sector of employment of both partners. We split the sample into women who are working in agriculture and women who are not in agriculture. For women working in agriculture, we look at the effect of droughts when the partner is not working in agriculture (so that her relative income drops) and when the partner is also working in agriculture (in which case both are negatively affected). Similarly for women who do not work in agriculture, we consider the drought effect when the husband is not working in that sector (in which case the household income might be less affected) and contrast it with marriages to husbands who are farmers (where an increase in the wife's relative income is plausible).

First Stage Analysis

In this analysis, we regress the wealth index on the rainfall shocks to provide further evidence that there is a channel linking rainfall and violence which go through income. Also here, we apply a first differences strategy with rainfall shocks the four last years before each survey. We conduct this analysis first on the sample where we have two violence surveys and then add five countries where we only have two surveys with wealth index: Burkina Faso, Ghana, Mali, Nigeria, and Tanzania. In addition to the mean wealth index in each grid cell, we also conduct analyses on the fraction that belong two the bottom and two bottom quintiles. We also here include country-specific dummies.

Results

First stage

We first conducted the analysis of whether rainfall during the last four seasons affected wealth in the sample where we have two surveys with a violence module. Results are shown in Table 1. There are no significant effects of any of the shocks at any severity for any of the wealth variables.

Table 1: Wealth effects of rainfall in six country sample

Panel A:

	(1)	(2)	(3)	(4)
Outcome		Δ Wealth Index		
Shock percentile	2.5 %	5 %	10 %	15 %
Δ Drought	-0.0225	-0.00574	-0.00598	-0.0121
	(0.0507)	(0.0445)	(0.0383)	(0.0373)
Δ Flood	-0.0159	-0.0436	-0.0172	0.0467
	(0.0310)	(0.0322)	(0.0452)	(0.0699)
N	258	258	258	258
R-squared	0.374	0.376	0.373	0.380

Panel B:

	(5)	(6)	(7)	(8)
Outcome	Δ Fraction poor (bottom 20 %)			
Shock percentile	2.5 %	5 %	10 %	15 %
Δ Drought	0.00249	-0.00464	-0.0112	-0.00738
	(0.0292)	(0.0238)	(0.0178)	(0.0143)
Δ Flood	-0.00289	0.0146	-0.00197	-0.00450
	(0.0181)	(0.0175)	(0.0151)	(0.0147)
N	258	258	258	258
R-squared	0.014	0.017	0.016	0.015

Panel C:

	(9)	(10)	(11)	(12)
Outcome	Δ Fraction poor (bottom 40 %)			
Shock percentile	2.5 %	5 %	10 %	15 %
Δ Drought	-0.00838	-0.00610	0.00221	-0.0122
	(0.0254)	(0.0213)	(0.0177)	(0.0171)
Δ Flood	-0.00955	0.0242	0.00150	0.0133
	(0.0195)	(0.0241)	(0.0213)	(0.0193)
N	258	258	258	258
R-squared	0.024	0.028	0.022	0.027

Robust standard errors. All regressions include country fixed effects.
* significant at 10 %, ** significant at 5 %, *** significant at 1 %.

We suspect that this might be because the sample is small and/or because the wealth index does not change much. A second analysis thus uses a larger sample of 11 countries is shown in Table 2. From column 1, we see that there is a significant, negative effect of a 2.5 % drought on the wealth index: the drought reduces the average wealth level by 10 % of a survey-wide standard deviation. Since the distribution of grid-cell means is much narrower than the distribution of household wealth levels, this corresponds to 20 % of a standard deviation reduction in means. As expected, the effect is steadily declining with less severe droughts in columns 2-4. The effect of floods seems to be more jumpy. In theory, rainfall should increase income up until a point after which it falls sharply. As it moves rapidly from being a positive to a negative effect, inaccuracies in measurement may lead to a highly inaccurate estimate of the effect. This is different from droughts, during which less rain is always worse and any drought should be worse than normal rainfall. We caution strongly against interpreting the coefficient of floods in the reduced form as we do not know whether it is a positive or negative income shock. Rather, we include floods as a control variable which allows us to compare droughts to situations with normal rainfall.

Columns 5-8 give evidence that droughts lead to an increase in the incidence of relative poverty. It is significant at the 5 % level for droughts of a severity which we expect occur every 10 and 20 years (columns 6-7), and at the 10 % level for 40-year-droughts (column 5). Droughts at these levels result in increases of 2.4-3.2 percentage points in the likelihood of belonging to the 20 % poorest part of the population. This corresponds to a 9 - 12 % increase in the grid-cell mean headcount ratio. The impact on the ratio of moderately poor (bottom 40 % of the population) is not as large and not significant (columns 9-12).

To sum up, droughts seem to affect the general level of wealth in society, but not the wealth for households little below the median. Droughts of a severity that occurs every 10 years or more seldom lead to increases in the fraction of households that belong to the bottom 20 % of the wealth distribution.

Table 2: Wealth effects of rainfall in eleven country sample

Panel A:				
	(1)	(2)	(3)	(4)
Outcome	Δ Wealth Index			
Shock percentile	2.5 %	5 %	10 %	15 %
Δ Drought	-0.102***	-0.0622	-0.0565*	-0.0241
	(0.0392)	(0.0383)	(0.0290)	(0.0299)
Δ Flood	-0.0182	0.0161	-0.0636*	-0.00597
	(0.0357)	(0.0312)	(0.0361)	(0.0392)
N	615	615	615	615
R-squared	0.134	0.126	0.134	0.119
Panel B:				
	(5)	(6)	(7)	(8)
Outcome	Δ Fraction poor (bottom 20 %)			
Shock percentile	2.5 %	5 %	10 %	15 %
Δ Drought	0.0254*	0.0316**	0.0239**	0.00762
	(0.0151)	(0.0128)	(0.0105)	(0.0106)
Δ Flood	-0.00894	-0.00345	0.0191	0.0127
	(0.0139)	(0.0139)	(0.0132)	(0.0124)
N	615	615	615	615
R-squared	0.013	0.019	0.020	0.010
Panel C:				
	(9)	(10)	(11)	(12)
Outcome	Δ Fraction poor (bottom 40 %)			
Shock percentile	2.5 %	5 %	10 %	15 %
Δ Drought	0.0150	0.0140	0.0219	0.00771
	(0.0164)	(0.0164)	(0.0134)	(0.0138)
Δ Flood	0.00520	0.00357	0.0275	0.0254
	(0.0172)	(0.0195)	(0.0190)	(0.0178)
N	615	615	615	615
R-squared	0.015	0.015	0.025	0.019

Robust standard errors. All regressions include country fixed effects.
* significant at 10 %, ** significant at 5 %, *** significant at 1 %.

Cross sectional analysis

In Table 3, we present OLS regressions of how the reported incidence of abuse last year is affected by incidences of severe drought and flood during the last and next-to-last rainy seasons before the month of interview. The outcome variable in column 1 is whether the respondent was subject to domestic violence during the last 12 months before the interview and the shock variables are droughts and floods defined as events below the 5th percentile or above the 95th percentile in the precipitation distribution respectively. Women living in an area experiencing a drought during the last rainy season are 2.9 percentage points more likely to have been victims of domestic violence during the same year (the estimate is significant at the 10 % level). A drought during the rainy season the year before seems to have a larger impact, and is correlated with 5 percentage points higher abuse levels (significant at the 5 % level).

As discussed in the empirical strategy, we may worry that these results are driven by some factor that is correlated with both the shocks and violence. In column 2, we do a first inspection of this by regressing a variable for whether the woman's father abused her mother. This variable is highly correlated with abuse but should reasonably not be influenced by rainfall shocks in the last years. We see that both types of shocks last year are statistically significantly correlated with parental abuse. Hence, our concern for omitted variable bias in this setting seems warranted. In column 3, we control for parental abuse and we note that the results are similar, and if anything a bit stronger, as compared to column 1. In particular, the standard errors are somewhat smaller which we should expect as parental abuse accounts for a large part of the variation in current abuse. In column 4, we add additional controls for age, education, and religious affiliation and we note that the results do not change much. The results presented in columns 5-7 are based on different levels of the shock indicator ranging from the 2.5th percentile (and 97.5th) of the gamma distribution to the 15th (85th) percentile. We see that the results for the droughts are stable across different cutoffs while the flood correlation decreases with less severe floodings. This is consistent with the notion that less severe floods may have a positive effect on agricultural production.

Although the statistically significant results remaining after controlling for various background characteristics is consistent with there actually being an impact of rainfall shocks on domestic violence, there is unfortunately no way to assess to what extent this conditioning eliminates the problem of omitted variable bias in the estimates. That is, we are still reluctant to drawing causal conclusions from this analysis as the weather shocks may be correlated with other variables that are not controlled for and that also correlate with violence. In what follows, we instead use specifications that allow for spatial controls.

Table 3: Cross sectional analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Abused	FBM	Abused				
Shock percentile	5 %				2.5 %	10 %	15 %
Drought t-1	0.029*	0.038**	0.030**	0.027**	0.031	0.026***	0.018**
	(0.015)	(0.016)	(0.012)	(0.012)	(0.019)	(0.0090)	(0.0079)
Flood t-1	0.00074	0.093***	0.077**	0.069**	0.075*	0.038	0.024
	(0.033)	(0.026)	(0.035)	(0.035)	(0.039)	(0.025)	(0.019)
Drought t-2	0.050**	0.030	0.042**	0.035*	0.055***	0.020*	0.027***
	(0.022)	(0.019)	(0.020)	(0.018)	(0.017)	(0.012)	(0.011)
Flood t-2	0.052**	0.038	0.054**	0.032	0.035	-0.0055	0.0075
	(0.024)	(0.023)	(0.022)	(0.022)	(0.035)	(0.015)	(0.0094)
FBM			0.18***	0.16***	0.16***	0.16***	0.16***
			(0.0060)	(0.0053)	(0.0053)	(0.0053)	(0.0052)
Other controls	No	No	No	Yes	Yes	Yes	Yes
N	104334	85996	85996	80642	80642	80642	80642

Repeated cross section

In Table 4, we present the results of estimating Equation (2) for our 258 repeated grid cells. The dependent variable is now the difference between the second and first surveys in the grid cell average probability of ever having been abused, and column 1 shows that having had at least one drought at the 5 % level before the survey is correlated with 6.4 percentage points higher probability of ever having been abused. In column 2, we run the same regression on our placebo variable for parental abuse and note that we are not able to predict changes in parental abuse with rainfall shocks between the periods. In column 3, we control for changes in parental abuse and note similar results as compared to in column 1 even though the sample is 29 percent smaller due to missing observations on parental abuse. Column 4 includes controls for changes in the composition of people with respect to education, age and religious affiliation, and we see that the results are robust to such an inclusion. Columns 5-7 show results for varying intensities of the shocks, and we see that more severe droughts cause more abuse while column 7 shows that there is no statistically significant effect of droughts at the 15th percentlile level, i.e. a magnitude which is expected every 7 years.

We are more confident in interpreting these results causally as we now control for spatial fixed effects. Nonetheless, due to several caveats with using repeated cross-sections, we proceed to a third type of analysis where we use information on the timing of the first occurrence of abuse.

Table 4: Repeated cross sectional analysis of ever been abused

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Δ Ever abused	FBM			Δ Ever abused		
Shock percentile	5	5	5	5	2.5	10	15
Δ Drought	0.064***	0.026	0.077***	0.077***	0.100***	0.046***	0.028
	(0.021)	(0.030)	(0.017)	(0.019)	(0.017)	(0.016)	(0.019)
Δ Flood	0.00032	-0.018	0.0019	0.0039	0.0062	0.0046	-0.0013
	(0.020)	(0.022)	(0.021)	(0.021)	(0.022)	(0.018)	(0.014)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FBM	No	No	Yes	Yes	Yes	Yes	Yes
Other compositional controls	No	No	No	Yes	Yes	Yes	Yes
N	258	200	200	200	200	200	200
R-Squared	0.30	0.032	0.20	0.27	0.29	0.25	0.23

Duration analysis

In the final strategy, we instead use information on when the women were abused for the first time in the relationship. By using the DHS survey questions on how many years it took after marriage or cohabitation before women who experienced violence were abused for the first time, we conduct a duration analysis which allows us to control for grid level fixed effects. The analysis thus compares shock-affected marriage years with marriage years that have not been affected by a shock in the same place, and we have controlled for differences in risk of violence based on marriage duration and age composition at time of marriage.

Table 5 shows the odds ratios of a duration analysis where we treat the shocks as permanent so that all marriage years after the couple has experienced a rainfall shock are treated as affected. We see that the effect of a drought is positive but mostly insignificant across different cut-offs. However, we see from column 2 that the effects of droughts are rather different when we study a first and a second incidence of a drought year in a relationship. The risk is not affected by a first drought, but a second drought raises the risk by 17 %. The effects of a drought is somewhat larger yet still insignificant in rural areas, where a 5 % drought shock increases the risk by 8 % as seen in column 3.

Table 5: Duration analysis. Permanent shocks

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	First case of abuse					
Shock percentile		5 %		2.5 %	10 %	15 %
Sample	Full	Full	Rural	Full	Full	Full
Drought	1.03	1.00	1.08	1.04	1.08*	1.03
	(0.051)	(0.051)	(0.062)	(0.073)	(0.043)	(0.039)
Flood	1.06	1.07	1.07	1.16***	1.07*	1.10**
	(0.044)	(0.046)	(0.049)	(0.064)	(0.041)	(0.043)
2 Droughts		1.17*				
		(0.11)				
2 Floods		0.95				
		(0.093)				
N	178119	178119	123851	178119	178119	178119

In Table 6, we instead model the shocks as transitory so that each shock is only allowed to have an effect during the two following years. We find no effect of a single drought shock at the 5 percent level using the transitory measure. Using two shocks we see that the results for the second drought is also large in the transitory analysis at 22 %, which is significant at the 10 % level.

Table 6: Duration analysis. Transitory shocks

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	First case of abuse					
Shock percentile		5 %		2.5 %	10 %	15 %
Sample	Full	Full	Rural	Full	Full	Full
Drought	0.98	0.93	0.98	0.94	1.07	1.04
	(0.056)	(0.059)	(0.067)	(0.077)	(0.046)	(0.041)
Flood	1.06	1.07	1.07	1.08	1.06	1.07*
	(0.049)	(0.053)	(0.053)	(0.069)	(0.039)	(0.037)
2 Droughts		1.22*				
		(0.13)				
2 Floods		0.91				
		(0.096)				
N	178119	178119	123851	178119	178119	178119

All these models assume proportionality, i.e. a weather shock has the same relative effect on the risk a first violent episode regardless of when it occurs in the marriage. This assumption is tested using likelihood-ratio tests where the models are compared to extended versions which include year of marriage-specific weather effects. None of the tests significantly rejected the proportionality assumption.

In Table 7 we instead estimate the effects of a transitory model with individual fixed effects. As expected, this increases the size of the drought coefficient. A drought shock at the 5 % level is now associated with an increase in risk of violence of 23 % but this effect is not statistically significant and at the 10 % level an increase of 21 % which is significant. Together, these results suggest that drought shocks might not determine whether a woman experiences violence in her marriage or not, but it increases the risk of a marriage turning violent for marriages where violence would eventually have occurred anyway.

Table 7: Transitory shocks with individual fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	
Outcome	First case of abuse						
Shock percentile		5 %		2.5 %	10 %	15 %	10 %
Sample	Full	Full	Rural	Full	Full	Full	Full
Drought	1.23	1.02	1.14	1.11	1.21**	1.07	0.95
	(0.16)	(0.14)	(0.16)	(0.21)	(0.11)	(0.085)	(0.091)
Flood	1.09	0.98	1.12	1.11	1.04	1.07	0.79**
	(0.11)	(0.099)	(0.12)	(0.15)	(0.092)	(0.078)	(0.076)
2 Droughts		3.68***					3.87***
		(0.65)					(0.50)
2 Floods		3.14***					4.04***
		(0.63)					(0.41)
N	24852	24852	18161	24852	24852	24852	24852

Heterogeneous treatment effects

Using our largest possible sample that also allows for credible causal inference, we move on to test whether there are different effects in different areas as well as for different types of women. At the core of our interest in weather shocks lies the question of how poverty relates to violence. Poor people are likely to be more vulnerable to drought-induced income shocks, and individual and communal poverty may be both enabling factors for violence and weaken social constraints which inhibit violence. In the first and fourth columns of Table 8, we see that women in households that are poor according to the wealth index are not at greater risk of violence. However, a drought shock leads to significantly more violence for women in poorer households than for other women. We also split the sample into communities with high and low levels of wealth using the jackknife method. From columns 2-3 and 4-5, we see that droughts have the most severe effect on violence towards women in poor households living in poor communities.

Table 8: Treatment effects in different areas based on poverty

	(1)	(2)	(3)	(4)	(5)	(6)
Type of shock		Permanent 5 %			Transitory 5 %	
Sample	All	Rich areas	Poor areas	All	Rich areas	Poor areas
Drought	0.948	0.941	0.931	0.913	0.889	0.916
	(0.0555)	(0.0803)	(0.0794)	(0.0630)	(0.0972)	(0.0791)
Drought*Poor	1.244***	1.114	1.369***	1.195*	0.883	1.400***
	(0.102)	(0.139)	(0.159)	(0.118)	(0.152)	(0.172)
Flood	1.048	1.143*	0.945	1.052	1.234***	0.865*
	(0.0535)	(0.0820)	(0.0708)	(0.0596)	(0.0931)	(0.0714)
Flood*Poor	1.032	0.975	1.093	1.013	0.899	1.157
	(0.0671)	(0.0823)	(0.112)	(0.0744)	(0.0856)	(0.141)
Poor	0.969	1.048	0.886**	0.989	1.079*	0.900*
	(0.0370)	(0.0519)	(0.0547)	(0.0342)	(0.0476)	(0.0493)
N	178119	93971	84148	178119	93971	84148

In Table 9, we investigate possible heterogeneous effects across different types of women. In column 1 we investigate the effects for women accepting versus women that do not accept wife beating and we see that there is no difference in the effects of the shocks for these two groups. In column 2 we compare women who are working and women who are not working, and we find statistically significantly larger effects of both droughts and floods for working women.

In column 3, we instead interact the weather shocks with a variable for women working in agriculture. These women are likely to be most negatively affected in terms of income by the shocks and we do indeed find a larger and statistically significant effect of droughts for these women. In column 4, the heterogeneity variable is whether the woman's partner works in agriculture. These partners are then more likely to have been negatively affected by the shocks in terms of income. We see a positive and statistically significantly different effect of droughts on violence against women whose partners work in agriculture as compared to for women whose partners do not work in agriculture. The results in columns 3 and 4 indicate that the effects of droughts are larger if either the woman or her partner is more likely to be economically affected by the droughts.

To further disentangle the effects via women's *relative* income, we restrict the sample to only include women who work in agriculture in column 5 and again interact the effects of droughts with a variable for whether the partner works in agriculture. The baseline drought effect is now large and highly statistically significant. It implies that there is a large effect of droughts for women who work in agriculture but have partners not working in agriculture. This result supports an interpretation whereby women becoming poorer relative to their partners are more at risk of abuse. The interaction term, which implies a situation where both become poorer, has a coefficient far below unity but not statistically significantly different from the baseline. Taken together, the terms cancel each others out which means that violence does not increase due to droughts if both work in agriculture. In column 6, we instead limit the sample to women not working in agriculture and that thereby experience a smaller drop in personal income due to the shock. The interaction term is still having a partner employed in agriculture and the positive interaction term implies that women that are less affected themselves are more at risk of abuse if their partners are more affected by the drought. This interaction term is, however, only statistically significant at the 10 % level. In all, these results suggest that women becoming poorer are more at risk of abuse and that shocks that affect the households asymmetrically are more dangerous for the women in terms of risk of abuse. The results thereby support both the relative resource theory whereby men compensate for loss in resources by violent means and the dependency theory of marital abuse whereby women who become relatively poorer than their partners become more at risk of abuse.

Table 9: Heterogeneous treatment effects across different types of women

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	All	All	Women in agriculture	Women not in agriculture
Heterogeneity variable	Women accepting	Women working	Women in agriculture	Partner in agriculture	Partner in agriculture	Partner in agriculture
Shock percentile	5	5	5	5	5	5
Drought	1.053	0.811**	0.951	0.983	1.388***	0.932
	(0.0741)	(0.0771)	(0.0570)	(0.0642)	(0.169)	(0.0733)
drought*variable	0.970	1.410***	1.277***	1.185**	0.809	1.260*
	(0.0763)	(0.135)	(0.102)	(0.0999)	(0.107)	(0.164)
Flood	1.049	0.931	1.030	1.030	0.968	1.044
	(0.0572)	(0.0584)	(0.0542)	(0.0525)	(0.0933)	(0.0633)
flood*variable	1.021	1.230***	1.082	1.064	1.131	1.046
	(0.0582)	(0.0815)	(0.0750)	(0.0675)	(0.117)	(0.100)
variable	1.582***	1.061	1.117***	0.954	0.928	0.917
	(0.0576)	(0.0396)	(0.0479)	(0.0398)	(0.0575)	(0.0514)
N	177330	177818	176313	161690	48412	107713

A caveat to these heterogeneity results is that the variables are measured at the time of interview while the shocks occur at an earlier stage. Hence, there may be a risk of reversed causality such that women who are abused become more likely to work in agriculture, or perhaps more likely there is a third variable such as gender ideology that make women work more in agriculture after negative weather shocks *and* make the shocks more likely to lead to abuse.

Conclusion

We use variation in rainfall to study how extreme shocks to income affect intimate partner violence in Sub-Saharan Africa. We find that drought-induced income shocks do not in general lead to an increase in the risk of a first violent episode occurring in a union. However, droughts lead to higher risk of first abuse in unions where only the woman and not her husband works in agriculture. This indicates that partnerships are more likely to turn violent if the woman's relative economic power within her household is reduced. We also show that cross-sectional analysis leads to biased results in this case due to spatial correlation in weather conditions. Though this could be resolved using repeated surveys, there is too little variation for such an approach to be credible.

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