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Response to Waja and Motlogeloa (2024): The need for robust research methodology when studying climate and health in developing countries

Significance:

We clarify several areas of misunderstanding raised by Waja and Motlogeloa's (S Afr J Sci. 2024;120(7/8), Art. #18588) critique of our previous publication analysing the associations between drought and acquiring HIV in several countries in sub-Saharan Africa.

We thank Mukhtaar Waja and Ogone Motlogeloa for their critique[1] of our analysis[2] looking at the associations between drought, poverty, sexual behaviours, and HIV acquisition in sub-Saharan Africa. Their review has highlighted several difficulties in investigating the associations between an exposure and an outcome that are separated by many links in a possible causal pathway. Such projects require working in a cross-disciplinary field to combine expertise in both climate science and the epidemiology of HIV, as well as using the best available data.

We used publicly available data from five nationally representative Population-Based HIV Impact Assessment (PHIA) surveys conducted in Eswatini, Lesotho, Tanzania, Uganda, and Zambia during 2016, which included data on 102 081 adults. These are bio-behavioural surveys containing questionnaires and HIV testing. We combined these HIV survey data with gridded precipitation data from the Climate Hazards Group InfraRed Precipitation with Station (CHIRPS) data that compared the rainfall from mid-2014 to mid-2016 with the equivalent rainfall from 1981 to 2016 in each location, defining a drought in this period if the rainfall was less than the 15th percentile compared with all other 2-year periods.

Waja and Motlogeloa argue that we could have instead used the standardised precipitation index (SPI) to define droughts, which is a good suggestion. However, their argument that the World Meteorological Organization (WMO) recommends that the SPI be used for classifying drought events and that it is "an official" drought index is not entirely accurate. The WMO recommends the use of SPI as a benchmark tool for defining drought[3] as it only requires precipitation as input and is, therefore, easy to calculate. The WMO also states in their Handbook of Drought Indicators and Indices, "Just as there is no 'one-size-fits-all' definition of drought, there is no single index or indicator that can account for and be applied to all types of droughts, climate regimes and sectors affected by droughts."[3] There are often alternative options when considering which data to use, with choices made for a variety of reasons. We chose this metric to align with previous research by the Vulnerability Analysis and Mapping Geospatial Analysis Team at the Analysis and Trends Service of the World Food Programme.[4] Additionally, the measure we used is nearly functionally equivalent to a commonly used SPI-based definition of drought as SPI < -1. The PHIA and CHIRPS data sets are all publicly available, so we welcome others to investigate the associations using these data.

It was suggested by Waja and Motlogeloa that the combination of the CHIRPS data could have been made more reliable had we conducted a ground validation of the climate at the study sites. It is not realistic to require that every study should perform ground validation on all data regarding climate measures through independent climatological analyses, as this would be an enormous drain on resources. We suggest that this validation should be done in studies focusing on the attributes of a specific climate data set. Of particular note is that the CHIRPS data set is extensively used and relied upon by a number of international agencies working on early warning and drought monitoring, such as the Famine Early Warning Systems Network, the World Food Programme, the Food and Agriculture Organization, among others.

We agree with Waja and Motlogeloa's statement that poverty can be problematic to define, particularly across multiple countries. Waja and Motlogeloa state that several issues make measuring poverty difficult and that we did not sufficiently explain how these issues were addressed. Unfortunately, they did not state what specific issues they were referring to, so we are unable to address them here. To summarise, we used a relative measure of poverty (wealth quintiles, with these quintiles calculated separately for each survey country) because an absolute measure of poverty was unavailable within this data source. We feel using this measure is justifiable because purchasing power is usually assessed relative to the setting, and so we think our measure is suitable for an analysis including data from five countries.

In their Commentary, Waja and Motlogeloa critique our analysis for not having defined non-drought control periods and, separately, for using cross-sectional data. Unfortunately, non-drought control periods cannot be constructed using cross-sectional surveys as data on the participants are only available for one point in time. We acknowledged the weaknesses of using cross-sectional data in our limitations section. If they were available, we would be keen to perform similar analyses on longitudinal data sets that contained information on important variables such as poverty, food insecurity, sexual risk behaviour, and HIV transmission. Although imperfect, we believe the methods we utilised are useful for raising hypotheses that can be tested in other data sets. The authors also state that we did not consider the lag period between the onset of a drought event and the impacts this would have on things such as crop production, and then on poverty and other factors in our model. Across many areas of southern Africa, the mid-2014 to mid-2016 period stands out as one of the driest 2-year periods on record, containing two consecutive seasons affected by drought (2014–2015 and 2015–2016).[4] Given that populations had already been affected by a first drought, the impacts of the second drought were felt with a very short lag[4], and so we feel our model should be valid. On a related note, Waja and Motlogeloa also suggest that distributed lag non-linear models (DNLMs) would be more appropriate than multivariable logistic regression models, as they would account for the lag between the exposure and outcome. However, in the data we used, we could not attribute a precise enough date to these drought periods because they entailed a major reduction in precipitation over a 2-year period (mid-2014 to mid-2016). This long-term exposure contrasts with climatic events such as heatwaves, floods, or hurricanes, which are acute exposures, and it is easier to define when they happened. DNLMs require exact time-exposure information, which is unavailable here, so these models could not have been used.

The authors also criticise our approach for not accounting for the lag period between an individual acquiring HIV and being tested for HIV, as well as pointing out that an increase in reported cases of HIV could just be due to increased testing. This would be a valid criticism if we were relying on routine HIV testing data collected over time, but we utilised a cross-sectional survey that tested all respondents for HIV regardless of their diagnosis status, and identified individuals who had recently acquired HIV based on additional tests. When using routine clinical testing to assess trends in HIV incidence over time, the lags between people acquiring and testing for HIV can be an issue, as is the reporting of HIV cases reflecting testing patterns. However, these are non-issues for this bio-behavioural survey.

Furthermore, Waja and Motlogeloa repeatedly use the terms “HIV prevalence” and “recent HIV contraction” interchangeably throughout their critique, but these are distinct concepts, and this is crucial to understanding our analysis. The PHIA survey data contained a marker of having recently acquired HIV using the HIV-1 Limiting Antigen Avidity enzyme immunoassay, which is effective at determining whether someone acquired HIV within the few months prior to the test.[5] People who tested positive for prevalent HIV using standard assays could have acquired HIV many years before a drought occurred. However, the people in our study who tested positive for having recently acquired HIV, would have acquired HIV after or during the drought period.

As with all data, these surveys have limitations, which we acknowledged in our manuscript. These include the dependence on self-reported data for determining wealth and sexual behaviour. However, when capturing data on sexual behaviours in particular, there is little alternative but to use self-reporting. The survey staff underwent 2-month training programmes, as well as refresher training sessions, regarding the potential biases and ways that such surveys should be conducted to mitigate these issues; for example, interviewing each survey participant separately from their other household members. Several of the study’s authors were involved in the organisation and collection of PHIA data and advised on the appropriate use and interpretation of these data sets.

These enormous, nationally representative PHIA surveys are widely used by national, regional, and global policymakers to understand both HIV prevalence and HIV incidence in sub-Saharan Africa[6,7], as well as to assess the characteristics and prevalence of risk behaviour within these populations.

To conclude, although Waja and Motlogeloa’s critique of our work contains several areas of misunderstanding, they do also highlight the general limitations of this type of work with the currently available data. Ideally, future studies should use longitudinal data to improve the strength of evidence for the causal pathway between climate events and the epidemiology of HIV, to enable a better understanding of the potential pathways linking the two.

Declarations

AI was not used in the writing of this manuscript. All authors read and approved the final manuscript.

Competing interests

We have no competing interests to declare.

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