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Trends Parasitol. Author manuscript; available in PMC 2011 September 15.

Published in final edited form as:
Trends Parasitol. 2005 February ; 21(2): 52–53. doi:10.1016/j.pt.2004.11.007.

Climate variability and malaria epidemics in the highlands of East Africa

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Abstract

Malaria epidemics in the highlands of East Africa garner significant research attention, due, in part, to their proposed sensitivity to climate change. In a recent article, Zhou *et al.* claim that increases in climate variance, rather than simple increases in climate mean values, have had an important role in the resurgence of malaria epidemics in the East African highlands since the early 1980s. If proven, this would be an interesting result but we believe that the methods used do not test the hypothesis suggested.

In their analyses, Zhou *et al.* [1] first aggregate meteorological time-series for two arbitrary, almost equal time periods (1978–1988, n=11; and 1989–1998, n=10) and show that there was no significant change in climate averages between these periods at seven sites above 2000 m across East Africa (19/21 *t* tests for three climate variables). Correct application of a multiple *t* test is likely to reduce the number of 'significant' differences to less than the two recorded, thus supporting the authors' contention. We note that this result corroborates previous studies showing no significant climate trends at seven other sites across East Africa where malaria epidemic resurgences had been reported in the past two decades [2-4], and does so with data from meteorological records rather than climate surfaces (thus obviating suggestions that climate surfaces do not adequately represent local climate changes) [5]. The authors then state that, despite the lack of significant changes in climate mean values, there were, nevertheless, significant differences between average annual variance between these periods. However, only seven of 21 *t* tests show increases in climate variability at the 95% confidence level, and correction for multiple *t* testing would similarly reduce the number of 'significant' results, in this case refuting the authors' contention.

No *t* tests were performed on any of the malaria data, so that the significance or otherwise of the climate results cannot be compared with changes in malaria, significant or otherwise. Changes in malaria incidence are of particular concern because there was no standardization for population increases during the observation period. The authors' statement [1] that: 'N_t was not adjusted for annual human population growth rates because the number of hospitals generally increases in proportion to the human population size increase, and thus the human population size that each hospital has served remains similar during the study period', is not supported by the article cited [3] which actually says the opposite: 'The situation at specific sites cannot be inferred from these national data, but they do provide evidence of regional

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decline in per capita health service provision coincident with many of the malaria resurgences'. Malaria case numbers recorded at individual facilities cannot be used to investigate epidemic trends without accounting for the confounding influence of population growth [6] (in fact, populations have doubled in the past 20 years around most of the sites investigated [3,6,7]). This will be further confused by changes in the health service to population ratio [8], so at the very least, malaria outpatients should be calibrated against total outpatients. Moreover, the 'outpatient' data they presented for Kericho were, in fact, inpatients [4], as has been subsequently corrected [9].

Zhou *et al.* [1] then state that they have modeled the malaria 'outpatient' case number time series at each site as an additive function of seasonal autoregression, climate variability and an error term. However, their model as implemented does not correspond to the statements made about it. First, the climate variability (g-term) has no explicit variance measure(s) because it is a linear function of average monthly temperature minima and maxima, total rainfall and their interaction term averaged over several temporal lags (months) whose cross-correlation with the malaria data gave significant coefficients. Thus, an increase in the diurnal temperature range, but not in actual climate variability, could have an effect on malaria incidence. Second, we also question why the sum or average of the lagged climate variables is taken rather than enabling each lag to have its own regression coefficient in a fully dynamic specification or specifying a static linear regression model. Third, although a huge literature exists on the appropriate statistical tests for selecting such time-series models (i.e. Akaike's information criterion and/or the Bayesian information criterion) that elegantly balance the goals of model accuracy with model simplicity and penalize for overfitting [10,11], none was applied here, so the model (with its ~ten parameters) is difficult to evaluate objectively, despite claims of 'outstanding fits'. Fourth, tests for model misspecification are not reported, raising the potentially confounding issues of spurious correlation if some of the variables are random walks but do not cointegrate [2], and unreliable inference if the residual error is not white noise. Finally, finding that climate (rather than climate variability) has a significant association with past malaria cases is hardly a new [12] or disputed [13,14] phenomenon.

Statistical results ultimately require biologically plausible explanations but Zhou *et al.* [1] make this difficult by using a complicated, two-stage fitting procedure unique to each time series. Such specificity also hinders application of the results to other areas, a prerequisite for the malaria early warning systems [13,14] they advocate [1].

There are several other important misrepresentations, among which we include: (i) the paper by Githeko and Ndegwa [15], which provides no evidence of a geographical expansion of malaria epidemics in East Africa; it simply makes an unsubstantiated assertion in its introduction, without reference to any data; (ii) The authors' statement that, 'Drug resistance can only aggravate malaria-induced morbidity and mortality; it cannot initiate an epidemic' [1], suggests a misunderstanding of the effects of drug resistance on the population dynamics of malaria. Briefly, any change, including drug resistance in the parasites, that enables a progressive increase in the basic R_0 values of any disease is likely to result in recurrence of whatever epidemiological behavior was shown before the drugs were first used; in some circumstances, this will indeed involve periodic outbreaks. Just such an increase in the tendency for epidemic behavior was detected by windowed Fourier analysis of the malaria data from the Kericho tea estates – behavior that was not paralleled by any obvious change in either temperature or rainfall data from the same site, or a time series of El Niño Southern Oscillation index data [16]; (iii) When transformed through a model of climate suitability for malaria (and thus testing climate interaction, which the authors deem so important), the vast majority of the African continent has shown no significant trend in malaria suitability during the past century [17,18], and projections for the next century show

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very mixed results [19]; (iv) The online resources cited by Zhou *et al.* [1] do not have the meteorological data available for independent testing.

In sum, although we think that there is some interest in the suggestion that increased variability in climate might have epidemiological significance, the research presented does not adequately test this idea.

Acknowledgments

S.I.H. is funded by a Research Career Development Fellowship from the Wellcome Trust (#069045) and is affiliated to the Kenya Medical Research Institute (KEMRI)/Wellcome Trust Collaborative Programme, PO Box 43640, 00100 Nairobi GPO, Kenya. S.E.R. is an NERC Senior Research Fellow. R.W.S. is a Wellcome Trust Senior Research Fellow (#058992) and acknowledges the support of KEMRI. He is also affiliated to the Centre for Tropical Medicine, Nuffield Department of Clinical Medicine, University of Oxford and John Radcliffe Hospital, UK. We thank Scott Goetz and Abdisalan Noor for discussions.

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