

Reflection & Reaction



Global warming and malaria: a call for accuracy

For more than a decade, malaria has held a prominent place in speculations on the impacts of global climate change. Mathematical models that “predict” increases in the geographic distribution of malaria vectors and the prevalence of the disease have received wide publicity. Efforts to put the issue into perspective¹⁻⁵ are rarely quoted and have had little influence on the political debate. The model proposed by Frank C Tanser and colleagues⁶ in *The Lancet* and the accompanying Commentary by Simon Hales and Alistair Woodward⁷ are typically misleading examples.

The relation between climate and malaria transmission is complex and varies according to location,² yet Tanser et al base their projections on thresholds derived from a mere 15 African locations. Slight adjustments of values assigned to such thresholds and rules can influence spatial predictions strongly.⁸ The authors invest considerable effort in assessing the sensitivity of their model to climate change scenarios but do not report the internal sensitivities to thresholds and rules. The predictive skill of their model is low (63% sensitivity, 95% CI 61–65%) but they consider projections acceptable if prevalence is projected “to within a month” (presumably ± 1 month?), thereby biasing their model towards success. A model covering an entire year in a parasite-positive site would always be correct, although in such areas it would be relatively insensitive to climate. By contrast, sites in which transmission is seasonal would provide a more reliable test of accuracy, but estimation is more difficult because climate sensitivity is greater. Furthermore, because parasite clearance in communities is not instantaneous,⁹ spot samples of parasitaemia on survey dates are not a suitable indicator of the duration of the transmission season. Lastly, “person/months” are unsuitable as a measure of transmission: an extension of season from 1 to 4 months will have more impact than from 10 to 12 months. According to their model, an extension of transmission from 11 to 12 months results in 10^6 more person/months in a population of 10^6 people, whereas an extension from 1 to

5 months gives the same increase in a population of 250 000.

What Tanser and colleagues have modelled is merely the duration of the transmission season, which they interpret as “heightened transmission” and increased incidence. A greater failing is their reliance on “parasite-ratio studies”. The relations between transmission season and parasite prevalence, and parasite prevalence and clinical disease, are unclear but unlikely to be linear. Moreover, they use 1995 data for human populations, although these are projected to double by 2030. In addition, the proportion living in urban areas—with a specific climate¹⁰ and orders of magnitude less malaria transmission^{11,12}—is projected to rise from 37% to 53%.¹³ For all these reasons, we do not accept the model as a “baseline against which interventions can be planned”.

It is regrettable that many involved in this debate ignore the rich heritage of literature on the subject. For example, in 1937, in his classic textbook,¹⁴ L W Hackett stated: “Everything about malaria is so moulded and altered by local conditions that it becomes a thousand different diseases and epidemiological puzzles. Like chess, it is played with a few pieces, but is capable of an infinite variety of situations”. A pressing question in Hackett’s time was the changing distribution of the disease in Europe. On the role of climate, he wrote: “Certainly, climate lays down the broad lines of malaria distribution . . . Nevertheless, although this is a very simple and plausible explanation . . . even the early malariologists felt that there was something unsatisfactory about it . . . malaria has not so much receded as it has contracted, oftentimes toward the north . . . Thus in Germany it is the northern coast which is still malarious, the south is free . . . There is, therefore, no climatic reason why (malaria) should have abandoned south Germany or the French Riviera”.

We quote Hackett because we feel that the classic components of science—unbiased observation and systematic experimentation—cannot be sidestepped with models that omit many of his chess pieces. Yet Hales and

Woodward⁷ begin by stating: “The present geographical distribution of malaria is explained by a combination of environmental factors (especially climate) and social factors (such as disease-control measures)”. In our opinion, “even the early malariologists” would surely disagree: much of the decline of malaria in Europe took place without control measures during a period when the climate was warming.

The text by Hales and Woodward that follows displays a lack of knowledge. Thus, “Most people at risk of malaria live in areas of stable transmission . . .” is simply wrong. It is true that in many parts of the world malaria is termed “stable” because transmission remains relatively constant from year to year, the disease is endemic, the collective immunity is high, and epidemics are uncommon. However, in many other regions, the disease is endemic but “unstable” because annual transmission varies considerably, and the potential for epidemics is great. Climatic factors, particularly rainfall, are sometimes, but by no means always, relevant.¹⁵

Again, “On the fringes of endemic zones, where transmission is limited by rainfall . . . there are strong seasonal patterns, and occasional major epidemics” is also wrong. In many regions, far from any “fringes”, malaria is endemic, stable, but highly seasonal. For example, in semi-arid regions of Mali, transmission is restricted to the rainy season, from July to September. The same 3 months constituted the transmission season for *Plasmodium falciparum* in Italy before it was eliminated.¹⁶ Paradoxically, in parts of the Sudan, rainfall is restricted to a month at most, but malaria is transmitted throughout the year. Female *Anopheles gambiae* survive drought and heat by resting in dwellings and other sheltered places.¹⁷ Blood feeding and transmission continue, but the mosquitoes do not develop eggs until the rains return. This phenomenon, termed gonotrophic dissociation, is remarkably similar to the winter survival strategy of *Anopheles atroparvus*, the principal vector of malaria in Holland until the mid 20th century.¹⁶

By contrast, malaria is unstable in many regions that normally have abundant rainfall, and epidemics occur during periods of drought. An illustrative example is the catastrophic 1934–35 epidemic in Ceylon (now Sri Lanka), estimated to have killed 100 000 people.¹⁸ Worst hit was the south-western quadrant of the country, where average annual rainfall is greater than 250 cm, and malaria was endemic, but unstable and relatively infrequent. The dominant vector, *Anopheles culicifacies*, breeds along the banks of rivers and tends to be scarce in normal years. In the years 1928–33 there was abundant rainfall, river flow was high, *A. culicifacies* was rare, and the human population was exceptionally malaria-free. However, after failure of two successive monsoons, the drying rivers produced colossal numbers of *A. culicifacies*, and the resulting epidemic was exacerbated by the low collective immunity. In the drier parts of the island, where *A. culicifacies* was dominant but transmission was more stable, immunity protected the population from the worst ravages of the disease.

Hales and Woodward state that “the underlying problem” of the future “extension of seasonality” of malaria is “pollution of the atmosphere”, and call for rich countries to “recognise their obligations to the poorest by substantially reducing fossil-fuel consumption”. We understand public

anxiety about climate change, but are concerned that many of these much-publicised predictions are ill informed and misleading. We urge those involved to pay closer attention to the complexities of this challenging subject.

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Aluminium-containing DTP vaccines

I was interested to read the review by Tom Jefferson and colleagues¹ concerning the possible adverse effects of immunisation with aluminium-containing diphtheria, tetanus, and pertussis (DTP) vaccines. I was surprised that the authors were able to conclude from their review that further research in this field was unnecessary. It would seem to me that this conclusion did not adequately reflect the findings of the limited resource base underpinning the review. The authors criticised the quality of the data they had available to them and yet these data were still deemed sufficient to support such a strong conclusion. In addition, the

authors made no reference to the fact that aluminium-based adjuvants contribute to the recipients systemic body burden of aluminium. We now know that aluminium in adjuvants is dissolved and transported throughout the body, including the brain² and we cannot discount the biological availability of this aluminium. It is a sobering thought that aluminium adjuvants have not had to pass any of the safety trials that would be expected of any drug or treatment. Their application is historical and this should not necessarily be equated with their safety. There is no consensus as to whether it is safe to introduce aluminium in prophylaxis

or otherwise, and until the requisite research is carried out it is misleading to conclude that aluminium adjuvants are safe for all to use.

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