Letters to the Editor

Reporting Studies of Diagnostic Accuracy

To the Editor:

I was very disappointed to see the evaluation by Loveridge et al¹ of criteria for the clinical diagnosis of malaria, especially at a time when increasing efforts are being made to improve the standards of reports of diagnostic accuracy.² This study violates many basic rules for the assessment of diagnostic tests, and the authors are incorrect to draw their conclusions from a study which is fundamentally flawed.

Only 28 subjects were tested with the antigen detection assay. This sample size is grossly underpowered to make any reliable conclusions. Moreover, 23 of the 28 subjects tested were children, making it difficult to extrapolate the findings to adults. Positive predictive value (PPV) is only 1 measure of diagnostic accuracy, and there is doubt that the PPV reported by the authors is accurate given that predictive values can be calculated only when the prevalence of the disease in the population being tested is known. As a minimum, the sensitivity and specificity of the diagnostic test need to be reported, and again the design of this study does not allow these parameters to be calculated with any degree of certainty. By testing subjects with only a clinical diagnosis of malaria, there is no way to know how many of the subjects who were not diagnosed clinically with malaria had positive antigen tests. For all we know, this could be the same proportion as those diagnosed with malaria.

The authors also need to address other issues relating to the performance of the antigen test. Low-level parasitemia in areas of endemic infection may cause positive results with this assay of doubtful clinical significance.³ It would be useful to assess the background positivity of the test in a control group. This is especially important in areas with high rates of human immunodeficiency virus (HIV) infection, as many other infections can mimic malaria.⁴ The authors state that blood smear analysis was impractical, and that this would expose providers to blood from persons infected with HIV. Were they not collecting and testing blood samples anyway for the antigen detection assay?

The authors are to be commended on their willingness to assess their diagnostic criteria for malaria in a difficult setting. However, this does not justify the inadequacies

of the study design and the publication of results that may be misleading.

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References

1. Loveridge BW, Henner JR, Lee FC. Accurate clinical diagnosis of malaria in a postflood epidemic: a field study in Mozambique. *Wilderness Environ Med.* 2003;14:17–19.
2. Bossuyt PM, Reitsma JB, Bruns DE, et al. The STARD statement for reporting studies of diagnostic accuracy: explanation and elaboration. *Clin Chem.* 2003;49:7–18.
3. Moody A. Rapid diagnostic tests for malaria parasites. *Clin Microbiol Rev.* 2002;15:66–78.
4. McDonald LC, Archibald LK, Rheanpumikankit S, et al. Unrecognised *Mycobacterium tuberculosis* bacteraemia among hospital inpatients in less developed countries. *Lancet.* 1999;354:1159–1163.

Response: Reporting Studies of Diagnostic Accuracy

The comments and critique of our study are well taken. First and foremost, the study was never intended to be a prospective randomized controlled trial and was not meant to be archived among the great studies of our time.

It is clear from the title that this is a field study, undertaken in austere conditions. We found among our supplies a box of 28 malaria test kits, and we tried to use these in a way that would give us some feedback to the efficacy of our clinical diagnoses of malaria in an isolated jungle. We were flown into remote areas by helicopter because floodwaters blocked all the roadways. Our supplies were, therefore, quite limited, and carrying microscopes, slides, and so forth was not merely an inconvenience but a complete impossibility.

In response to the study design, had we divided our test groups to include a control, our numbers would have been so small as to preclude any useful statistical power. Therefore, we chose to test the positive predictive value

of our clinical diagnostic criteria in consecutive patients as described in our methodology.

To our knowledge, there are no published reports that attempt to quantify the extent of the Mozambique malaria epidemic in the spring of 2000. We are simply trying to provide real data that will help others appreciate the scope of the epidemic and the challenges of disaster relief efforts. We hope the readers of *Wilderness and Environmental Medicine* will appreciate the limitations

we encountered and take the information presented at face value. These are straight-forward results of an observational study—no more, no less. If they are useful to other humanitarian teams in the future, then we hope our efforts have some merit and are a launching point for further scientific inquiry.

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