Reviewer’s report

Title: Female genital schistosomiasis as an evidence of a neglected cause for reproductive ill-health: A retrospective histopathological study from Tanzania.

Version: 1 Date: 3 April 2006

Reviewer: U Petry

Reviewer’s report:

General

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

I. The manuscript needs the following major compulsory revisions of the discussion part:

1. On page 11/first paragraph the authors state that research on the possible impact of genital schistosomiasis is virtually non-existent. I agree that research results are scarce. However we found a weak correlation between Schistosomiasis and HPV in a small prospective study in Southern Tanzania (Petry KU et al., Int J Gynecol. Cancer 2003, 13:505-509). The results of that study should be compared and discussed together with the results of the submitted manuscript’s findings.

2. On page 12/first paragraph the authors state that the majority of schistosomiasis diagnoses was in genital organs, followed by urinary organs. The authors need to clarify that their retrospective histopathological data is highly biased and does not reflect the true prevalence of genital and urinary bilharziosis. Awareness of urinary schistosomiasis is common among health professionals of all kind in Tanzania, which is not the case for genital schistosomiasis. A diagnosis of urinary schistosomiasis is in almost all cases a clinical diagnosis and/or based on urine microscopy but almost never on biopsies. In money-striped health systems in the third world histopathological examinations are performed only in a tiny minority of cases. The authors state that the KCMC performs just 5,000 specimens per year and that this corresponds to 35% of all such examinations in Tanzania. Hence the bias for any kind of epidemiological conclusions based on histology is much stronger than in European countries. The manuscript is very confusing for European readers without a comment addressing this point. I recommend to delete figure1 for the same reasons.

3. On page 12/last paragraph first sentence the authors write that schistosomiasis can “theoretically render the local immune environment”. Although such data for cervical lesions does not exist Raziuddin S et al could demonstrate such changes in urinary bladder (Cancer 1990;65:931)

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. Page 14/first paragraph. The authors quote a publication of two schistosomiasis associated SCC of the uterine cervix in which HPV could not be detected and conclude the existence of an HPV independent mode of cervical carcinogenesis. Given the experience with other cervical cancer studies and HPV-DNA it is much more likely that false negative PCR results and/or insufficient conservation of specimens contributed to this findings as could be demonstrated by J Walboomers (1999 J. Pathol.) and many others (e.g. G. Böhmer et al Am J Obstet Gyn 2003)

Correct for some misspellings (e.g. specmines – page 6, methods sixth line)

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No