Author's response to reviews

Title: Human T cell Lymphotrophic Virus Type 1- associated infective dermatitis in KwaZulu Natal, South Africa

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Author's response to reviews: see over
Dear Editor,

BMC Dermatology
31 July 2013

Dear Editor

Re: Revision of “Human T cell lymphotropic virus type 1- associated KwaZulu Natal, South Africa” Ref:

Thank you for the opportunity to revise the manuscript, now entitled: “Human T cell Lymphotropic Virus Type 1- associated infective dermatitis in KwaZulu Natal, South Africa”

The main issues raised by the reviewers were as follows:

1. A better description of the study population was required.
2. Further discussion of the implication of the high proviral load the study describes
3. Improvement in English Grammar style
3. Rearrangement of topics, material and methods
4. Clearer discussion of laboratory data and that of the objectives of the study.

All the above have been addressed and corrections now included in the revised manuscript.

Further, for specific reviewers comments responses follow below in red.

Sincerely yours,
Red Cross Children’s Hospital, Cape Town, South Africa
Reviewer 1: “Human T cell lymphotropic virus type 1- associated KwaZulu Natal, South Africa”
“Another point is the differential diagnosis in the study group: what diagnosis the excluded patients had received, How many had HTLV”?
The patients that did not fulfil the clinical criteria for IDH were excluded very early on during the period of the study. If they did not fulfil the clinical criteria for IDH, they were not analysed further. They would otherwise have been initially considered for enrolment into study because they either exhibited clinical features in keeping with either HIV related seborrhoeic dermatitis or scalp folliculitis suspicious of IDH. As the study shows 9 of the excluded patients were HTLV-1/HV. The remainder therefore were HTLV-1 negative.

An interesting report on a cases series of IDH in South African patients. It is generally well written, and provides the first description of IDH in South Africa.

Thank you

I believe the study population deserves a better characterization: It would be interesting to know

- how many patients are attended in the clinic
A total of approximately 320 patients are seen per week, 1280 per month. Most are secondary and tertiary conditions and IDH makes a very small percentage of this population.

- how many has HTLV infection.
HTLV-1 is not routinely tested for in our setting, but where clinical suspicion of an HTLV-1 associated skin disorder exist than HTLV is tested for. As the current study, 33 of the 60 enrolled participants tested positive for HTLV-1. We had to exclude those patients that tested positive for HTLV-1 but were either HTLV-1/HIV co-infected (n=9) nor did not fulfil the clinical criteria for IDH (n= 6)

- a better description of the study population in table 2, comparing all 60 patients analyzed, regarding to the main findings described for IDH patients
The remaining patients outside those who did no fulfil the clinical diagnosis of IDH, were not further analysed
-What was the prevalence of HIV in the remaining 41 patients?
As the study shows 9 of the remaining 41 patients were HTLV-1/HIV coinfection, but then these were adults patients and were subsequently excluded from further analysis. As for the remaining 32 patients, some were HIV infected and therefore clinically presented with HIV related seborrhoiec dermatitis.

It would be useful to have the same data for all 60 individuals, and to include the differential diagnosis for the whole group: what was diagnosed in the remaining 41 subjects?
See description above

The study included a proviral load measurement, but there is no mention to its results in discussion.
Thank you for drawing our attention to this omission, we have now included this discussion in the revised version of the manuscript

What was authors’ interpretation for the 10.5% PVL found in the study? They should compare this value with those found in literature, and discuss the implications on the potential of HAM/ATLL.
This has been done and the correction effected in the revised manuscript.
Finally, as authors stated in discussion, the crusted nares do not seem to be a good criteria to diagnostic of IDH. Since it was the main differential point in comparison with the reports from other regions, I don’t think it is enough to conclude that IDH in South African patients is different from other places.

The authors accepts this assertion by the reviewer and the statement to this effect has been removed from the manuscript.

Reviewer' 2 : : “Human T cell lymphotropic virus type 1- associated KwaZulu Natal, South Africa”

The data are relevant to the epidemiology of IHL in South Africa, but does not show any new data.
Even though the current study does not show new data, but as far as the authors are aware IDH has not been documented in Africa before, except for a case series of 3 patients reported in Senegal. The current study therefore reports the largest series of IDH cases in Africa, to date, a report of 19 cases. Furthermore, all previous cases reported so far in the literature seem to support a theory that IDH is a condition prevalent only in tropical climate, our study reveal that IDH does occur in non-tropical climate conditons.

Minor Essential revisions
Improvement of English style:
Thank you, this has been effected in the revised manuscript

Arrange topic materials and methods
Has been effected in the revised manuscript

Discuss laboratory data, since results were described in
Has been effected in the revised manuscript

Revise the description objectives of this article.
Has been effected in the revised manuscript

Review divergence for the duration of the research described in the summary (42 months) and results (three years).
Thank you for drawing our attention to this discrepancy, in actual fact, the study period took three years and not 42 months.