Author's response to reviews

Title: A new 500kb haplotype associated with high CD8+ T-lymphocyte numbers predicts a less severe expression of hereditary hemochromatosis

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Author's response to reviews: see over
Porto, 25 September, 2008

From: Eugénia Cruz

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Dear Editor of the BMC-Medical Genetics,

Please find enclosed the manuscript "MS: 8672449692115360 - A new 500kb haplotype associated with high CD8+ T-lymphocyte numbers predicts a less severe expression of hereditary hemochromatosis" which my colleagues and I changed very slightly accordingly to the additional comments of one of the reviewers.

We would like to thank for the careful revision of the manuscript. The response to reviewers’ is indicated below (in blue).

Hoping to have the paper accepted after having answered to the referees’ comments.

Sincerely Yours,

Eugénia Cruz.
Reviewer: Jill Waalen

Reviewer's report:

The revisions made in response to reviewers' comments are acceptable and have improved the manuscript.

No more changes are needed.

Reviewer: K Sigvard Olsson

Reviewer's report:

1. A minor detail, concerning the concept “ethnically different”. It seems as if the authors want to remain in their opinion that descendants to Europeans in Vancouver are ethnically different from those in northern Portugal. “ethnically different” still appears in the abstract, page 4, introduction, page 6, page 16, page 19 even if the authors now discuss the matter. We remove the expression ethnically different from the manuscript. The reason why we use the expression of ethnically heterogeneous population was because besides the 6 patients European descendant, the other 4 were: two Mennonites, one from Indian extraction (mixed English and North American Indian extraction) and one of unknown heritage. We agree with the fact that they are probably all descendants from Europeans.

2. I maintain the view that one should be careful to characterize the patients from the appearance of symptoms because several of them are unspecific ,ref 10,11. However, it now appears from the corrections of page 8, that the two groups with or without symptoms seem to differ with regard to the degree of iron loading, which is the relevant point. The method used by the authors, TBIS, is also the best available. The corrections lead to an improvement.

No more changes are needed.

The message is strengthened when the two patient series are grouped together, page 18, all patients with severe iron overload (TBIS>5g) were homozygous for AAT. I appreciate that the authors now discuss the issue which of the factors that is the modifier.

No more changes are needed.