Reviewer’s report

Title: Characterization of disease-specific cellular abundance profiles of chronic inflammatory skin conditions from deconvolution of biopsy samples

Version: 0 Date: 21 Feb 2019

Reviewer: Raghunath Chatterjee

Reviewer’s report:

The present manuscript entitled "Characterization of disease-specific cellular abundance profiles of chronic inflammatory skin conditions from deconvolution of biopsy samples" by Garza et. al. developed a signature matrix from 22 reference cell types and used it to quantify the cellular abundance in psoriasis and atopic dermatitis.

Authors should address the following comments:

1. The main contribution of this manuscript is the development of DerM22, however the matrix is not provided as a supplementary material. Authors should provide the matrix for the scientific community.

2. Fig.1 A: Flow cytometry data for immune infiltrates in lesional skin of psoriasis showed 6 data sets. Are these different study samples? Two clusters are formed for these data sets, one comprising 2 datasets and another 4 datasets? One dataset overlap with the HG-U133 and another with the HG-U133 Plus 2.0. Are they different in terms of disease severity?

2. Fig 1C: Authors interpreted that the keratinocytes identified using CIBERSORT are correctly identified as they showed good overlap with the Epidermal SCs, TAs and keratinocytes. However, I can see that the CD8+ cells also showed good overlap with different other cell types. Authors should justify this observation.

3. In basal and suprabasal epidermal sublayers, 12% adipocytes (10%) and adipose stem cells (2%) is very surprising. What is the proportion of CD8+ T-cells? There are reports that the CD8+ T-cells are abundant in epidermis, while CD4+ T-cells in the dermis. But I do not see such abundance in the observed results. Authors should discuss about this differences with reference to the published literatures.

4. Fig 3E is very difficult to read. Authors stated that there is a significant increase/decrease of some cell types with disease severity. They used Wilcoxon rank sum test, and without adjustment for multiple hypothesis testing. They should present the corrected P-values. How many samples for each category were considered? Looking into the variances in Fig 1, I have doubt about the significant differences for low abundant cell types after adjustment.
5. The validation part of the observed results is limited, it is just based on some IHC and limited
flow cytometry data. Author should mention the limitation of their results in the discussion.

6. There are some typos, e.g., "overestimated in health and disease"; page 12, line 318: "the
layer-specific composition of the skin described in Fig.",. Authors should correct these typos.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an
additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further
assessment in your comments to the editors.

I am able to assess the statistics

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