Reviewer’s report

Title: Distribution of Malassezia species on the skin of patients with atopic dermatitis, psoriasias, and healthy volunteers assessed by conventional and molecular identification methods

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Reviewer: Thomas L Dawson

Reviewer’s report:

This paper is interesting and could be a very nice contribution to the field. I see as especially important the comparison of the molecular and culture based speciation. This was interesting and well executed.

It could be accepted without further experimental data, but only with significant revisions to the text. The authors did not adequately review the field, and hence under reported an important issue - that the data relies on culture based sampling. They should call this out more clearly and in more detail review the existing data.

The following comments should all be considered as Compulsory Revisions

1. Is the question posed by the authors well defined?
   The authors present a survey study. The question is relevant and important, and a necessary contribution to the field. It is not the testing of a specific hypothesis.

2. Are the methods appropriate and well described?
   The methods are reasonable, but should be better described as “culture based”. The isolation is all based on culture so it is very important to point out the potential bias introduced by the well documented poor reproducibility of Malassezia primary isolates. There are inherent biases due to the fastidious nature of Malassezia in culture which must be reviewed and accounted for.

3. Are the data sound?
   The data are sound and well reasoned. There needs to be a clearer description of how the isolation technique influences the data and adds inherent difficulties in interpretation. The authors need to add discussion comparing non-culture based isolation techniques (for example Nat Rev Micro 9, 244-253 (2011) or J. Clin. Microbiol. vol. 40 no. 9 3350-335 2002), which indicate that Malassezia are very likely found on every human. Also, it has been previously reported and well supported that M. restricta is found on essentially all humans. The authors need to discuss their work in comparison to this previously published data.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   Yes

5. Are the discussion and conclusions well balanced and adequately supported
by the data?
Yes. The authors do an excellent job of noting the positive and negative aspects of interpreting their data on a technical level. The Conclusions and Discussion, however, could be covered with fewer words and abbreviations and this should improve the readability and clarity.

6. Are limitations of the work clearly stated?
With regard to everything other than the issues associated with cultivation, yes.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
No. The authors failed to reference a significant body of work. There are several key papers, such as Nat Rev Micro 9, 244-253 (2011), J. Clin. Microbiol. 2002 (40) 9 3350-335, PNAS 2007 (104) 47 18730-18735. They also should include previous work in the area of Malassezia resistance in the susceptibility section, such as FEMS Yeast Research 2011 (11) 1, p 80–87.

I found it unnecessary for the authors to state that this “was the first time” this data has been published in Poland. See “Distribution of Malassezia species in patients with atopic dermatitis – quality assessment. El#bieta Rup, Magdalena Skóra, Pawe# Krzy#ciak, Anna B. Macura. Post Dermatol Alergol 2011; XXVIII, 3: 187–190”

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I am a full time employee of the Procter & Gamble Company.