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**ESPS Manuscript NO: 21402**

**Manuscript Type: Review**

**Answering reviewers**

We would like to thank the reviewer for his careful reading of the manuscript. The main criticism was that some sections are only a list of examples being put together. We tried to improve these sections of the manuscript but the field of macrophage self renewal is emerging and requires a descriptive job.

1 - We carefully checked the spaces between sentences.

2 – We removed the word aggression from the text and replaced it by injury.

3 – Page 1: we removed the first sentence that was partially redundant with the third sentence of the paragraph.

4 – The verb maturate was replaced by mature.

5 – The word closed was replaced by close.

6 – We agree with the reviewer's comment. The results of the use of $^{89}Sr$ are ambiguous for peritoneal macrophages that represent an heterogeneous population. We removed peritoneum from the sentence to avoid a contradiction with figure 2.

7 – It is true that the results are largely dependent on tools used and it is difficult to provide unambiguous conclusions. That point was discussed in the manuscript. We removed the term "fascinating" and replaced it by great because the use of modified mice has clearly provided important data in the field of macrophage.

8 – We have the feeling that the reviewer overanalyzed the model in which only brain macrophages are of YS origin without contribution of monocytes. There is no contradiction between this part of the text and the figure 2 that summarizes several original papers. It is likely that the explosion of original papers in this field will lead to reappraise some of our present conclusions.

9 – We agree that the section concerning cMaf and MafB was confusing. However, the role of these transcription factors in macrophage proliferation has been clearly described by M. Sieweke. We re-wrote this section to better describe the cooperation between Maf and the other transcription factors.

10 – We did not write that intestinal macrophages are independent of monocyte recruitment. We even wrote the reverse to emphasize the role of microbiota.

Nevertheless, a part of intestinal macrophages is independent of monocytes in newborn mice. We agree with the reviewer concerning microglia and alveolar macrophages. We introduced the sentence of the reviewer at the end of the paragraph because it summarizes accurately the development of alveolar macrophages.
11 – We added a conclusion to the paragraph homeostasis to specify that all the findings obtained with mouse models are not extrapolated to humans.
12 – We modified the title of the section: macrophages in diseases instead of human diseases.
13 – The sentence (page 8) has been re-structured
14 – The words die and egress have been removed; only the deficiency in cell death has been maintained in the manuscript.
15 – We made a new paragraph with myocardial infarction and neurological stroke because these complications of atherosclerosis are examples of injuries.
16 – We reorganized the first paragraph of the section "infectious diseases" to better identify what is CCR2-dependent and improve the reading of this paragraph.
17 – We rephrased the sentence concerning microglia (page 11, line 10).
18 – The interference with monocyte recruitment was in the conclusion to identify potential therapeutic implication of a better knowledge of macrophage self renewal. We re-wrote the sentence.