Author’s response to reviewss

Title: Resistance and Resilience to Experimental Gingivitis: A Systematic Scoping Review

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Author’s response to reviews:

Dear reviewer, Dear Dr. Tatacis,

Thank you very much for your expert review of our paper. We were pleased to read your good comments and the revisions that we were suggested to address. Your comments have resulted in an amended and improved manuscript, which we resubmit herewith.

1. We have adjusted the abstract and more information on the immunological biomarkers is included. You were right that we originally addressed only microbiology, this was narrow focused and not reflecting all results.

We added in the Abstract the following extra information (in Italics and underlined):

“For immunological biomarkers, it was challenging to retrieve a robust pattern of changes across multiple studies. IL-1β and IL-6 in saliva and in gingival crevicular fluid increased during induction phase and returned in the resolution phase below baseline values. The biochemical parameters cystatin-SN, cystatin-S and lactoferrin in saliva were increased at the end of induction phase, however also here no clear pattern emerged based on all available studies.”
2. We have elaborated on our reasoning for excluding split mouth studies in the methods section and reiterated this in the discussion.

In the Methods section we added the following sentences (in Italics and underlined):

“Studies on mouth rinses, placebo control groups, animals, surgical treatments, antibiotics, probiotics, those lacking a healthy control population, cross-sectional designs, any form of intervention during the induction phase, and split mouth experimental design were excluded. The latter type studies were excluded due to risk of transfer of fluids and biomarkers from one part of the oral cavity to another. Moreover, systemic responses are likely to be lower in a split mouth study design than in full-mouth EG due to the smaller gingival surface area of inflammation; this may then affect the inflammatory profile of plasma and GCF.

3. With regard to your comment about splitting findings from GCF and saliva, we fully agree with you. We have now reported and rewritten the findings from blood/plasma/biopsies, GCF and saliva separately. This explains the many track changes in the document, in particular in the Results section. These changes are now not reiterated here in this letter, but can be seen in the version with track-changes. We thank you for your feedback and hope that we have addressed all the comments as preferred.

4. Your comment: “When reviewing/reporting on studies involving smokers there is no comment on the ascertainment of smoking status (a biochemically based determination of smoking status is preferred over the more common self-reporting method)” is indeed important. We have addressed this first in the Results and later in the Discussion section as follows:

In the Results section: “Five studies compared smokers to non-smokers in exposure to the EG trial (10, 12, 13, 23, 29); smoking status was always based on self-report and not biochemically assessed.”

In the Discussion section: “However, this suggestion needs to be interpreted with care since it was found only in one study with a limited number of smokers (n=11) and non-smokers (n=14). Moreover, we acknowledge that when reviewing/reporting on studies involving smokers biochemical determination of smoking status would be ideal rather than the commonly applied self-reporting method.”
We hope we have addressed all comments sufficiently and hope that the paper is now suitable for publication.

Sincerely,

Charifa Zemouri and Bruno Loos on behalf of all authors.