Review of manuscript bg-2020-38-manuscript-version5

This is my second review of the manuscript and the third review round in total.
I am not satisfied with the revisions and the response of the authors to my previous comments. The authors decided to rebut most of my major comments and their response contains, in my view, partly insufficient or inappropriate arguments.
In the following I list the remaining issues and requested improvements.

MAJOR COMMENTS

A) The uncertainty of the main result, the average per-bison-emission, is not treated appropriately. In the new manuscript version there is even a new aspect on this issue.
The authors give the results as "mean ± standard deviation" (e.g. abstract, line 249, ...). It is unclear what that means (standard deviation of what dataset?). Also a standard deviation is usually not a useful uncertainty measure. This needs to be clarified.
- In response to my previous comments, the authors have added some text statements about additional uncertainty sources, but at the same time, they apparently have reduced the uncertainty estimation, instead of increasing it.
- line 198: The 17% uncertainty "for longterm sums" adopted from the literature can hardly be used for the present extremely non-homogenous situation and a very limited measurement time of only daytime cases during only about 3 winter weeks (bison present and camera pics available).
- It is also crucial to declare in the manuscript, how many half-hourly per-bison-emission values were available after all quality filters.
- Figure 11A shows that the individual per-bison-emission data have a strongly skewed distribution. Therefore the random-like error cannot be well estimated according to Gaussian statistics rules. However the difference between arithmetic mean and median is an indicator of a large uncertainty.

B) I consider the discussion of the per-bison-emission results in comparison to the literature as still insufficient. In the response to my previous comment 6, the authors state that "Methane flux is related to the animal in question, its body mass, diet, metabolic state, pregnancy / weaning status, and more." But for the literature comparison they just selected "...results that are similar to ours". This is a clearly non-scientific approach. The authors should not just select literature per-animal emission values that are similar to the present study without considering/stating the relevant factors (body mass, diet, etc.) in the referenced studies.
Since the authors compare their results to feedlot studies, it is also not clear whether the present experiment is considered as representative/comparable to a grazed pasture system or rather to a feedlot system. This should be clarified.

MINOR AND LANGUAGE COMMENTS

C) Response to prev. comment 3:
Since the authors introduce their z0 determination in detail in Section 2.4 and because the z0 values are important inputs for the footprint models, it is surly warranted that the authors present some corresponding results in the text (e.g. range of obtained z0 values with/without animals in the footprint).

D) Response to prev. comment 9:
The rebuttal indicate that the authors principally put the use of a 'Conclusions' section into question, because their arguments could be applied to most scientific papers. I do not agree but I leave it to the editor to decide this issue.

E) line 22: "...similar to eddy covariance measurements of methane efflux from a cattle feedlot during winter". It needs to be clarified here, that the mentioned feedlot results were not obtained in this study but that you mean "previously reported eddy covariance measurements ..."

F) line 163: define the meaning of F_{ij} in Eq. 4

G) line 321: It needs to be clarified whether the 36% is a attribution fraction or a relative uncertainty.

H) Figure 10 and 11A: The use of the term "probability" is strongly misleading here. The shown data rather are observed frequencies of occurrence.