Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-38-RC1, 2020

Interactive comment on "Methane efflux from an American bison herd" *by* Paul C. Stoy et al.

Anonymous Referee #1

Received and published: 21 March 2020

GENERAL COMMENTS:

In this paper, Stoy et al. estimate bison enteric emission using the eddy-covariance method, coupled with a footprint model and a cattle location method. This type of approach is under development (Felber et al., 2015; Coates et al., 2017; Dumortier et al., 2017; Prajati et al., 2018; ... all cited in the document) and has the advantages of providing an estimate in the field, integrating the animal to animal variability, having great temporal resolution and the potential to be automated. The current bison herd is small but growing and the application of such a method is especially interesting on a wild species on which the classical methods by metabolic chamber or on-animal tracers are undoubtedly more complicated to apply. Therefore the scientific interest of the paper is proven. However, this method faces technical and methodological difficulties that limit its accuracy. The choice and accuracy of the footprint model, the technical difficulty of automatic tracking of livestock location, the best way to calculate

a turbulent flux in non-stationary conditions, and the best way to determine a flux per individual based on turbulent flux and contribution to the footprint are still insufficiently investigated.

However, the paper does not present any significant advances on these points. Geolocation is carried out by manual analysis of images in the visible range, resulting in a restricted dataset of about 170 half hours, making it impossible to study a seasonal or even diurnal evolution of the emissions, a footprint model is arbitrarily chosen and is not compared to other available models and the difficulties related to non-stationarity are not addressed. The paper traces its path, in a pragmatic way admittedly, relying on choices made by other authors and not yet consolidated. An analysis of the dependence of flux on ustar in the absence of bison is proposed, with the aim of identifying a possible filtering criterion for low turbulence, but it is inconclusive in my opinion because of the low magnitude of the fluxes, both of CH_4 and CO_2. So there is little methodological input. A positive point from this point of view is the sensitivity analysis of the estimation of flux per individual to the precision of geolocation/precision of the footprint model. Some parts are however difficult to follow (e.g. smoothing of positions, see note below).

Some parts of the paper don't seem very useful to me. I am thinking in particular of the justification for the fact that the methane emissions measured do come from livestock (low background flux, i.e. from the soil/plant continuum). This is an essential part of the method, but it seems quite obvious to me for an ecosystem of this type in the winter conditions encountered. The observation of the absence of CH_4 flux when the bison are removed from the pasture seems to me sufficiently meaningful and I don't see the point of presenting the absence of dependence of the CH_4 flux on abiotic variables (radiation, temperature) to substantiate this observation. I was also hampered by some speculative passages (e.g. mechanisms of flux dependencies to u*, role of excreta, possible diurnal variability) and the perspectives are certainly well written but already known by the community.

Remains the main message that, despite the large uncertainties in the enteric emission per individual, the enteric flux is lower than that of other types of ruminants. It is stated in the introduction that since bison have a grazing behavior that favors nutrient-rich species they may have lower enteric emissions but in this study fodder is provided and is not characterized precisely, neither in terms of quantity nor in terms of quality. The reader therefore has no leads to circumstantiate this result.

I therefore feel that this article is premature and that the critical mass of original and useful information for the community is not reached at this stage. I encourage the authors to expand their dataset to allow for a statistically robust analysis of the quality of the footprint model, of the diurnal flux variability, to investigate methodological limitations in more detail and to propose explanations for low bison enteric emissions. Because I think the topic deserves a new and more robust submission when the above comments will be addressed, I also added below my specific comments, hoping it will help the authors to improve their analysis.

I would also like to point out that the shape of the paper is good, the writing is fluent, the references appropriate and the figures clear.

SPECIFIC COMMENTS:

L20: The uncertainty of 14 gCH4 day-1 bison-1 mentioned in the abstract without any additional comment is, as clearly explained in L194, only including spatial uncertainty (and I have some concerns on this point, see below) and uncertainty due to the flux summation. Information on the huge dispersion on your <f> estimates (standard deviation of 61 gCH4 day-1 bison-1 !) is not even mentioned in the abstract, which is misleading.

L28-70: Nice introduction.

L78: The composition of the herd is not specified. Age distribution could strongly influence CH4 emissions.

L118: One or two additional lines on spectral corrections would be useful. Lateral separation, reference cospectrum, magnitude of the correction factor.

L146-147: Too little information is given on the visual geolocation of bison based on the cameras. All we know is the position of the cameras and "manually attributing bison locations to squares in a 20m grid". How can you assign a distance to the mast with cameras that have no high-angle views and no distance markers in the different azimuths? Are the images from the different cameras combined to triangulate the positions of each individual? And how are the 6 positions averaged at half an hour? The authors propose an uncertainty of 20m on this estimate, which seems small in the absence of details on how to proceed.

L151-155: The paper is not self-standing on the point of "2D Tikhonov regularization". More information is needed so that the reader can understand the concept without having to read the reference assiduously. I do not master this technique but when I see that this spatial smoothing results in redistributing 3 individuals from the group at (x=40-80m,y=80-100m) to a distant group on the example of fig 4, I wonder about its relevance to simulate possible errors of location or footprint function.

L142: The approach used to determine <f> gives an estimate per half hour. However, the half hours with low contribution to the footprint will show a large dispersion, as this term is used in the denominator in eq. 3. Did the authors try to determine <f> rather by flux regression vs. contribution to the footprint?

L148-150: I don't think that shifting everything by 1 grid-square cell in each cardinal direction can simulate a systematic error of the footprint model. Proceeding in this way, the impact on the estimation of the mean of the half-hourly <f> will be smoothed. I would understand better if it was systematically shifted by 1 grid-square farther/closer with respect to the mast (modify r in polar coordinates).

L184-187: It is not clear whether Fig. 10 only shows the locations for the 173 half-hourly periods where CH4 fluxes are also available or whether it is the 444 half-hourly periods with camera tracking. The first option seems less misleading to me.

L184-187: The forage was not brought in the direction of the prevailing winds. As a result, cattle are often on the sides of the footprint. Is Hsieh's model reliable under these conditions? You suggest that the analytical models were validated for this type of exercise (L249) but it was not the Hsieh model.

L196: You should explain how you combine the spatial uncertainty and the uncertainty due to long-term methane flux sums (but annual sums in Deventer et al., 2019, what is the logic behind using it here?)

L204: The Hogan's publication is 17 years old already. You should rely on more recent literature. Also, the 60 kgCH4 per year per animal for range cattle is an average over very contrasted cattle nature. It would be useful to be more precise.

L238-241: not convinced by the statistical reliability of this assertion. Since it seems to be the case for you too, Figure 13 should be removed.

Fig 5: Is it really necessary to show (and use in statistical analyses) both SW and Rnet?

TECHNICAL CORRECTIONS:

L191-193: the range 36-44 is repeated twice. Probably a typo?

L196: gCH4 bison-1 day-1 instead of gCH4 m-2 day-1 !!!

L211: 'negative' instead of 'positive'

L494: something is wrong in this sentence.

Fig 6: For better readability, the tower should be the origin of the spatial scale. Also in fig 10.

Fig 7: add ticks for the x scale.

Fig 9: ustar should be in m s-1

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-38, 2020.