Biogeosciences Discuss., 12, C8493–C8498, 2015
www.biogeosciences-discuss.net/12/C8493/2015/

Biogeosciences
Discussions
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Interactive comment on "Combining two complementary micrometeorological methods to measure CH_4 and N_2O fluxes over pasture" *by* J. Laubach et al.

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Received and published: 15 December 2015

Final Author Response

We are grateful to both anonymous referees for their overwhelmingly positive comments on our manuscript.

Referee 2 recommends "accepting the manuscript as is". We are thrilled by this endorsement, and therefore no specific responses to this referee are required.

Referee 1, by contrast, finds the manuscript somewhat too long and too detailed. We consider this a fair opinion. When describing experimental work which consists of new combinations of approaches, or contains new aspects, authors have to find a balance between, on the one hand, presenting a concise and readable story, and on the other hand, giving enough detail to convince referees and readers that the experimental procedures and data analysis are of high standard. With this manuscript, we have certainly tended to the latter. We highly appreciate the referee's clear guidance which passages should be considered for shortening, and we state below in the replies to specific comments how we intend to follow these suggestions.

Note regarding minor changes to the data:

The algorithm employed by us to gap-fill the CO2 fluxes is contained in the "OzFluxQC" software provided by Peter Isaac et al. Since the submission of our discussion paper, there have been a number of updates of this software package. One of these updates includes a correction that causes minor alterations of the nocturnal CO2 fluxes in our dataset, which in turn leads to minor changes in the CH4 and N2O emission estimates. We have also, during the preparation of a companion paper by Hunt et al., revisited the low-turbulence filtering threshold. For consistency, we would like to use the same version of our dataset in both papers. The revised filtering alters the data selection for both the GGR and NSR method somewhat, and thus indirectly also alters the daily estimates of the GGR (NSR) method on some days (nights). Due to these two changes in the data analysis procedure, many numbers given in our Results section will be changed by a few percent. These changes are, however, small enough that the conclusions are not affected; overall, our two methods and their combination appear rather robust. In particular, the estimate of annual N2O emissions will stay within the uncertainty limits given in the Discussion paper.

Response to Anonymous Referee #1

Each referee comment (in quotation marks) will be followed by our response.

General Comments

BGD

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“As the authors point out in their title the two approaches are complementary: the ïňĄrst is appropriate for the well–developed atmospheric turbulence often experienced in the daytime while the second is applicable on calm nights. However, this could introduce some uncertainty when there is a diurnal cycle in gas emissions as has been observed in other ïňĄeld studies of N_2O production. Nonetheless, the paper should prove useful to other researchers of the target gas emissions. The authors point out that their study was initiated because of the high cost of fast sensors appropriate for eddy covariance measurements of CH_4 and N_2O, most notably for N_2O.” Reply: If we understand the referee’s point correctly, it is that combining two methods which are always applied at different times of day, and thus not compared to each other, carries the risk of misrepresenting the diurnal cycle of gas emissions. This appears to be a comment in passing, rather than a criticism of our approach. We agree that in principle such misrepresentation is possible; however, by applying each method only in the conditions that it is best-suited for, in practice we minimise potential biases for both methods.

“I have some areas of concern. One is the length of the MS. It is well written, but I feel that the great detail in it makes it longer and more discursive than need be. Some examples follow:” Reply: we are happy to shorten the manuscript in some places, see replies to specific comments.

Specific Comments

“p.6: The sampling procedure for the FTIR spectrometer is described. It was found that changes were needed to attain the desired sensitivity and a new system was used. Since the ïňĄrst system was unsatisfactory it should be enough to cut to a short description of the second, without wading through an unnecessary page of detail.” Reply: The second sampling procedure achieved an improvement over the first in terms of precision of the gas mole fractions, but at the cost of halving the data yield. We disagree with the description of the first sampling procedure as “unsatisfactory”. The larger part of the presented dataset (almost 14 months) was collected with the first procedure, and another 6 months with the second. Both procedures thus need to be described.

“p.10: I am not expert in gap-ïňĄlling procedures, but I ïňĄnd it surprising that gap-ïňĄlling was apparently used so freely. Perhaps the authors could quantify just how much.” Reply: We are not quite sure what exactly the words “so freely” refer to. Choice of threshold value, or of threshold variable? Both are discussed in the companion manuscript by Hunt et al. which will be submitted within the next month. At our sites, ca. 45 % of night-time data fell below the low-turbulence threshold, which is similar to many other sites around the world. We add in Section 3.3.1 a half-sentence giving the fraction of nights available for the NSR method.

“p.10, Eq.5: Is it acceptable to use whole-night averages rather than shorter term determinations in this equation and how are “sufïňĄciently calm” nights identiïňĄed?” Reply: If the fluxes of CO_2 and the gas of interest were perfectly correlated in time, then the length of the averaging period would not matter at all. We do not know the true fluxes, but we can take the correlation coefficient between the mole-fraction gradients for guidance. The lower this correlation, the poorer we expect the whole-night estimate to be, which is why we excluded regressions with low Rˆ2. If one tried to apply the NSR method with shorter periods, then the number of points to determine the regression slope would be reduced (increasing the uncertainty), and the random error of the CO_2 flux would propagate into the flux estimate for the gas of interest. So, while theoretically shorter periods would allow to account for out-of-sync drivers of the gas fluxes, in practice such accuracy gain is unlikely to be achieved, because it would be overwhelmed by increased random error. There is also a theoretical argument for high temporal correlation between the gas fluxes: the soil microbial processes that produce (or consume) CO_2, CH_4 and N_2O are all soil-temperature dependent and should thus co-vary. The selection procedure for calm nights is described in the paragraph following the sentence with “sufïňĄciently calm” (last of Section 2.4).

“pp. 11 & 12: To me, the description of the Soil and Vegetation conditions and CO_2

ïňĆuxes contains more detail than is needed and could be shortened." Reply: Yes we are happy to make some cuts here.

"p.13: DeïňĄne non-resolvable gradients. My guess is that they exhibit a change from negative to positive (or vice versa) within the gradient" Reply: These are defined in the preceding sentence. We'll link the two sentences up by inserting the word "such" ("Such non-resolvable mole-fraction differences...").

"pp. 15-17: These pages, which discuss various aspects of the GGR method, are good examples of the highly detailed patches in the MS that I think could be shortened." Reply: Section 3.2.3 is already short, and we would agree to cut another sentence or two. Section 3.2.4. is also relatively short and we use it to show that the footprint of the GGR method contrasts with that of the NSR method described in a later section. Section 3.2.5 will be scrutinised for possible cuts.

"pp. 18-19 (section 3.3.3 on footprints and nocturnal ïňĆuxes): long discussion" Reply: We agree that some minor cuts are possible, but the substance of this section should stay, because it shows that the footprint requirements can be a major caveat of the NSR method. "I have some concern about the use of the term turbulent diffusivity, as in Eq. 1. My understanding is that it should be used in a partial derivative equation rather than a ïňĄnite difference one like Eq. 1 so that $F\chi$ = -K $\partial\chi$ / ∂z , as, for instance, in Thom (1975, Momentum, Mass and Heat Exchange of Plant Communities in "Vegetation and the Atmosphere" Vol.1, Ed. J.L Monteith, pp.57-109, Academic Press, London). This allows for a non-linear form for χ (z) and a non-steady state. I prefer to describe K as used by the authors in Eq.1 as a transport or transfer coefïňĄcient. I haven't gone through the ramiïňĄcations that might arise from using Eq.1 in the present context. It may be that in the end, both Eq.1 and the partial differential equation above give the same answer." Reply: The referee is correct that the vertical diffusivity is defined using the partial derivative with height. Our Eq. 1 is already simplified for the practical application, where measurements can only be taken at discrete heights to approximate $\partial[\chi]$ / ∂z. We will insert a sentence just above the equation to clarify this. There are no further ramifications of our early approximation. If we wrote the partial derivatives, for [χ] in Eq. 1 and for [CO2] in Eq. 3, then the ratio of these two would appear in Eq. 4, and the simplification to finite differences would have to be employed at that stage, with identical result. Technically this is a linearisation; however, Eq. 4 is accurate not only if the profile shapes are linear with height, but also if the profiles shapes deviate from linearity in the same fashion for both gases – which is what similarity theory would predict under the assumption that the sources/sinks for both gases were co-located. Regarding the name of K, our simplification does not change the concept or the units, so it is still correct to call it “diffusivity”. By contrast, the expression “transfer coefficient” is ambiguous. Stull (1988, An Introduction to Boundary-Layer Meteorology) uses it synonymous with diffusivity. Phillips et al. (2007, see manuscript references) use it for K / Δz. In other contexts, it is a dimensionless number (e.g. bulk transfer coefficient). We prefer to avoid such ambiguity.

“pp.23-25: These discussion pages make good points, particularly the opening paragraph on p.25 which recommends combining the GGR and NSR techniques to give long term means since the GGR method yields more data during the day than at night and the NSR method is nocturnal only. The authors point out that their combination optimises data usage.” Reply: Thank you for this affirmative comment. No changes required here.

Technical Comments

“I noticed some typos in the manuscript: p.7, line 4: thermostate for thermostsat” Reply: “thermostat” is the correct spelling, this will be fixed.

“p.7, line26: instationary for non-stationary” Reply: “instationary” is a correct and common technical term in fluid dynamics, no need to change.

“p.28, line 17: contents for content” Reply: This will be corrected.

Interactive comment on Biogeosciences Discuss., 12, 15245, 2015.