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Interactive comment on "Intercomparison of fast response commercial gas analysers for nitrous oxide flux measurements under field conditions" *by* Ü. Rannik et al.

Anonymous Referee #1

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General comments:

Rannik et al write an interesting report on the performance of four different fast response N2O analysers in an eddy covariance field campaign over a several months' period. The instruments differed mainly in the sensor noise; the older makes had higher noise than the newer ones, which is good news, as it documents satisfying metrological progress. From their results The Authors defined the flux detection limit, which was in some cases higher than the flux level in usual conditions, i.e. when fertilisation didn't affect the N2O emissions anymore. Interestingly there were periods where the instruments measured very much the same, while in two periods systematic differences were found between two of the sensors and the others. For the latter the Authors speculated

over possible reasons, but were not able to explain the differences.

The article is mostly easy to understand and the field data is very well suited given the objectives of the work. In its current form the manuscript stays a bit too technical, more like a report. Very good work, but, scientifically, not yet exceptionally inspiring. The reason is that the report stays rather descriptive. There are indeed some interesting, advanced, quantitative analyses that go beyond standard - I mean here the method, how the individual N2O sensors contributed to the flux noise - but this is mainly presented but not further discussed. There is little reference to other work, beyond methodology. Some conclusions are mentioned in the discussion, but not summarised. Below I will make some suggestions, on how this work can be improved. Finally, what good scientific yield essentially is, can be debated, and I leave it to the Editor to explain this further in this open discussion.

Some details:

The site description:

Please mention the amount and type of fertiliser.

The measurement setup:

Reading the manuscript, I have got the impression that the experimental setup was generally chosen with great care. I was, however, surprised about the very low measurement height. If I'm right, the reed canarygrass canopy grew very quickly from 0.1 to 1.7 m and then more slowly up to 1.9 m. The measurement height was in the beginning 2.2 m and was raised by only 20 cm when the canopy reached 1.7 m, i.e. to only 50 cm above the canopy. If this is not a typographical error and rather, e.g., 3.4 meter was meant, this would be extremely narrow and should result in the disturbance of the spectra in the high-frequency range. This could, e.g., be the explanation for the observed high frequency noise in sonic temperature power spectra (Figs. 2 and 4). It would be interesting to think further about how this disturbance interferes with the analysis of flux and sensor noise. It was mentioned in the text that the vertical wind speed component (w) was expected not to be affected by noise. I suggest analysing the noise in w as well to show this. And more generally, as total random noise in fluxes and that due to analyser noise is compared in the study, it would probably mean a difference, whether or not the turbulence is disturbed.

Although it is said that the two involved universities were measuring with two separate EC systems (page 11751, line 18) the text never refers to the UEF system, i.e. its sonic anemometer. I guess this means that all 4 analysers were operated with one single sonic anemometer (the UH one). This makes perfect sense for avoiding another source for differences between the sensors. In this case the above mentioned sentence should be rephrased accordingly to not confuse the reader.

And, a bit more specific, what was the reason to run the LGR_CW_QCL with such a low flow rate? To my knowledge the supplier does recommend higher flow rates and a lower operating pressure.

Finally, please mention how and when were the sensors were calibrated (see below).

The data set:

The field data base is very well suited for such analysis. It is relatively long and covers periods with higher and lower flux levels. I think that the Authors have done a very good job when dealing with the limited availability of some of the sensors in some parts of the measurement period and also with the choice to avoid gap filling but confining the analysis only to data where all sensors were producing reasonable data. It became not entirely clear to me, whether the data quality criteria were also used, when the data were selected. In any case it would be desirable to only use the 'good' quality data here.

What I was missing is any reference to the meteorological and soil conditions throughout the measurement campaigns. This could be interesting for others that are also

interested in the control and interpretation of the N2O fluxes. Could you give the time series of soil temperatures and water contents under the measurements, especially during the period prior to and after the fertilisation.

Scientific analysis and discussion:

To begin with: I had the impression that the discussion of the results is the weakest part of the manuscript. A separate section on the scientific conclusions is also missing to take home a message from the work. The following comments will hopefully give The Authors some inspiration to help them increasing the scientific content and impact of this manuscript.

1. Random noise: To opt for separating the total random flux error into the part that is solely caused by the sensor and the rest, is very much reasonable in the context of the manuscript. It's a very strong aspect of the study and I did not read very often about it. The Authors used existing approaches to calculate and partition the random flux error. They mention 'Theoretically, there are several ways to approximate the same error estimate', but I missed a critical discussion on the usefulness of these approaches and a clear explanation for their choice. What is the uncertainty of the different methods / estimates? Wouldn't it be possible to try out more than one and compare the results? This debate can create some interesting scientific discussion. Such analyses are so far rare and exemplifying the methodical aspects and critically discuss them could add value to the article.

2. Why is the uncertainty of the instrumental noise estimate of LGR-CW-QCL so much higher than that of the other sensors (Fig. 5a), while both the total flux noise and the one related to sensor noise are about as certain as with the other sensors? This seems a bit inconsistent to me. For this comparison, a linear scale presentation of Fig. 5 would be better suited.

3. The spectral analysis is straight forward but I wonder why the Authors perform it only at such a small data basis, i.e. a few hours. Please comment. This is probably the

reason, why The Authors give one time constant value for H_2O. This should rather vary with relative humidity as known from earlier work, by the way, even before the work by Mammarella et al. (2009).

4. Flux intercomparison The analysis of the observed systematic differences between the analysers is not entirely satisfactory. For many biogeochemical analyses, such as annual GHG budgets, the systematic uncertainty is even more important than the random noise. There must be many raw and ancillary data that offer more analyses to finally find the reasons for these differences.

As an example the systematic differences between the AR-P-QCL and AR-CW-QCL was discussed in terms of cross-sensitivity with water vapour. One should rather proof than indirectly conclude that the cross-sensitivity of the N_2O spectra with H_2O (and thus the estimated N_2O flux with the H_2O flux) is not /or unlikely to be the cause for observed differences. Which other reasons can explain the differences?

The same applies for the discussion on sensor drift effects. The methods part does not refer to the calibration of the sensors. One could easily investigate the sensor drift with calibrations that were carried out during the course of the campaign. Or, if that wasn't done, one could compare the N_2O concentrations from the 4 sensors with the mean from all measurements. I'm sure this will clarify the issues of sensor drift. Maybe you will even find a sensitivity drift, which could then very easily explain the observed absolute deviations of the flux estimates from the different sensors.

What is the reason for the large noise of fluxes from CS-TDL, when the AR-CW-QCL does not measure any flux (Fig. 6 left panels)? In my experience the absolute flux noise level is either constant, in the best case, or increasing with flux level. A higher absolute noise level at low or zero fluxes is on the contrary rather unusual. This must have a reason; any idea?

5. Comparison with published work. Comparison with literature, not only regarding the methodology used, but also the scientific results, e.g., about noise levels, fluxes

etc., is missing. The discussion and conclusion section does only refer to one single publication, an own one that is cited for the third time in the same context (Mammarella et al., 2010) in this manuscript. I recommend using information from existing literature to define the state of art and describe the progress in this field and highlight this work's contributions.

Finally I'd like to recommend thinking about which general scientific conclusions can be drawn from this study and summarising them in a short, concluding, final section.

Presentation of the material:

The manuscript is with a few exceptions very easy to read and understand. Even leaving out articles does not disturb very much, it is to my knowledge just not correct English. I would also suggest replacing 'multiplication' by 'convolution' (11755, 11 and 16)

Section 2.5 is the one most difficult to understand; in fact it became first clear to me what was actually done, after I read the results part. Probably using 'was' instead of 'can be' would already help a lot (page 11756, lines 12 and 23, page 11757, line 11).

(11756, 16): 'The method evaluates the error in time domain through integration of the auto-covariance and cross-covariance functions of the vertical wind speed and the scalar concentration.' – integration over what? Integration of auto-and cross covariance (w,s) – the part is missing what follows to estimate a single noise value. An equation is needed to complete the text.

(11756, 27) sigma_noise ='the standard deviation of instrumental noise as observed at frequency f' and (11757, 5) What does 'instrumental random noise variance' sigma_noise then mean? Probably not the variance of the noise but the noise expressed as a variance. Not sure of you even mean variance or rather standard deviation.

(11757, 2-4): 'the method developed by Lenschow et al. (2000) and applied to EC fluxes by Mauder et al. (2013) to estimate the flux detection limit due to instrumental noise.' Neither the study of Lenschow et al. (2000) nor the one from Mauder et al. (2013) contains the term 'detection limit'. Please define the term detection limit and show, how it is determined by sigma_noise.

Structure / redundancy The main structure of the manuscript is good with a few exceptions:

1. the introduction to the section 3 (Results) that belongs into the methods description and

2. the repetition of the methodology and the results in the discussion parts that should be avoided.

The presentation the data in tables and graphs is very good, with the exception of Figs 2, 4 and 6, where the single panels are not given letters for reference.

Interactive comment on Biogeosciences Discuss., 11, 11747, 2014.