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Biogeosciences Discussions

Interactive comment on "Intercomparison and assessment of turbulent and physiological exchange parameters of grassland" *by* E. Nemitz et al.

Anonymous Referee #1

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General comments:

In this paper the authors compare different setup and procedure to calculate fluxes and microclimatic parameters in the same location. The most interesting aspect of the paper is that they deliberately used different instrumentations and data analysis procedures, normally applied by the different groups involved in the project. Even if this procedure increases a lot the variability of measurements and the sources of errors, it permits to understand the uncertainties in the real use of the eddy covariance technique. This is a useful paper, which I recommend you accept for publication but only after some substantial revisions recommended in this review and a complete revision

of the structure of the paper that is very confusing The main limitation of the manuscript lies in the poor description of the experiment. The authors refer to another paper but this reference is not satisfactory because it is necessary to know more about the tower dislocation, instruments setup and calibration/intercalibration to well understand the paper and also to be able to follow some discussion/comments of the authors,. Site 2 and DWD must be omitted from the analysis (see Methods comments)

Specific comments:

Introduction The introduction is not completely exhaustive. The authors need to check the recent literature about fluxes intercomparison to present a more detailed state of art about this aspects.

Section 2 Eliminate this part, it is not necessary, all the procedure are well known

Methods As indicate in the general comments this is the more problematic part of the paper. I would like to see a map with the dislocation of sensors, footprint of the single tower and wind direction frequency. All this aspects are important to understand better the results and their interpretation. Also the period and duration of experiment is crucial. I don't really understand why the authors included in their analysis also site 2 with only one set of instrumentation. As described by the same authors the dataset was exclude from the consensus calculation (that is one of the major issue of the experiment) (Section 4.2), I suggest to exclude it from all the analysis Also DWD site must be omitted in the intercomparison for the different time-resolution of data collection and for the different in site management. Are the sensors intercalibrated before to start with the experiments?

Section 3.2 Eliminate page 250-251 lines 22-4 .."In addition to the eddy..followed the manufacturer's guidelines"; Eliminate page 251 lines 7-11 .."It should be noted..this issue"; This data were not used in the analysis and they are not necessary to understand the others. Section 3.3 Page 253 line 14: Fig 9a not 8a Section 4.2 Eliminate page 254-255 lines 27-4 .."The eddy covariance...wind sectors"; See general and method

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comments Section 4.2 Eliminate DWD site description and analysis Section 4.7 The use of maximum turbulent fluxes (from UMIST KH2O) and minimum Rn (from INRA) to reach the closure of the energy balance seems to me very risky and not in line with the main focus of this paper (intercomparison). What is CEH Gill R2? Is it the solent 1012RA as described in the table? Section 5.1 Page 262 line 20: Table 3 not 2 Page 263 line 4: ..."FAl and UMIST show a reduction amount of scatter".. This is not true looking the graph, the scatter is similar to the other sonic Section 5.2 Page 354 line 27:..."this closure suggests that the Umist..frequency response of the inlet and IRGAs". This is a speculative comment not completely support by the data

Tables and figures:

Figures 1-6: include letters in the figures if you cite them in the text with letters Figure 4: use the same scale for the four pictures, invert the position of pictures c and d

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Interactive comment on Biogeosciences Discuss., 6, 241, 2009.