

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

E. Nemitz et al.

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General remarks

We thank the anonymous Referees for their careful reading of the manuscript, for the overall positive response, and for their constructive suggestions which have helped to improve the manuscript further. In the following we have responded to the individual more critical points raised by the Referee.

It should be noted that both Referees mainly comment on the comparison of the eddy-covariance results and heat fluxes, while neither of them comment on the comparison of canopy temperatures and the resistances derived here. This indicates that both Referees come from the research community addressing mainly fluxes of momentum,

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heat and (presumably) CO₂. By contrast, as the title indicates, this paper is distinct from more traditional intercomparison papers of micrometeorological instrumentation or flux calculation approaches and focuses on an analysis of the uncertainties in deriving input parameters for gradient flux calculations and SVAT modelling of reactive trace gases such as NH₃. Although we agree that more details were needed throughout the manuscript to interpret differences between measurements, some of the suggestions of the referees would redirect the focus of this publication.

Specific responses

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.

We respond to this general criticism in detail in the individual points raised by this reviewer below.

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.

We agree that more information is needed to fully interpret the results of this intercomparison. For example, a new Table 2 has been added to list the correction procedures as applied, also in response to Referee 2. Again, the Referee is coming back to the individual points they wish to clarify in the more specific remarks below and we address these issues there.

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

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art about these aspects.

We have added more recent references to the introduction, but have mainly addressed this concern in the Discussion section where we have expanded on the discussion of the results in the light of earlier studies.

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

We strongly disagree with the Referee on this point. Just as the referee requests that we add more background information, such as details on the site layout and some of the corrections applied during the data analysis, we believe it is equally important to briefly summarise the key equations used here. While the Section may contain little new information for the experienced micrometeorologist, some of the details of the calculation procedures applied here are worth pointing out. This relates, for example, to our implementation of the gradient technique, which, contrary to most implementations, uses equations that can be applied to any number of heights (Eqs. 8–9), and to the extrapolation of measured fluxes to derive values at the surface ($T(z_0')$, $e(z_0')$) (Eqs. 18–19). Furthermore, the paper is not just aimed at the experienced micrometeorologist, but also at modellers trying to understand the typical uncertainties in model input parameters. Finally, some of the companion papers of the special issue refer to this Section. In our opinion the Section is kept concise and we prefer to retain it as is.

Methods. As indicated 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 these aspects are important to understand better the results and their interpretation. Also the period and duration of experiment is crucial.

The period and duration of the campaign were already stated in Section 3.1 of the original manuscript. An additional figure has now been included as a new Fig. 1, which shows the location of the different setups in relation to the field, together with the wind direction frequency. As explained in the manuscript, the normalised footprint was

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calculated and a data quality flag was generated which indicates for each 15 minute period whether at least 2/3 of the footprint lay within the field itself.

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.

Measurements of NH₃ fluxes at Site 2 were used to estimate the effect of advection of flux measurements in a companion paper, and the performance of this anemometer is therefore of interest. The comparison between the measurements at site 1 and site 2 provides information of the spatial heterogeneity across the field. We agree that in the initial manuscript, this point was not well made and have therefore added the motivation to Section 3.2 and a brief discussion to the end of Section 5.1.

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 the start with the experiments?

We agree that the inclusion of the DWD adds little value to the intercomparison as the canopies are apparently too different. This has been removed. None of the sensors used in this study have been inter-calibrated, although, clearly, the individual groups perform their own calibrations. As clearly stated in the manuscript, the purpose is to compare the results of the setups of the different groups as they would have been used at their own national field sites.

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". These data were not used in the analysis and they are not necessary to understand the others.

We have removed the reference to the other papers and chemical analysers. However,

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we have retained and expanded the description of the wind and temperature gradients as these are now included in the revised manuscript, following the request of Referee 2 below.

Section 3.3 Page 253 line 14: Fig 9a not 8a

Corrected, it is now Fig. 10a.

Section 4.2 Eliminate page 254-255 lines 27-4 .."The eddy covariance...wind sectors"; See general and method comments Section 4.2 Eliminate DWD site description and analysis Section 4.7.

The results from Site 2 have been retained but are discussed with a much clearer objective (see above). The text on the DWD results has been deleted, as far as R_n was concerned. The DWD estimate of St has been retained (as this fed into the Consensus micromet estimate), the site description has been minimised.

The use of maximum turbulent fluxes (from UMIST KH2O) and minimum R_n (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).

This is addressed in detail in the response to Referee 2.

What is CEH Gill R2? Is it the solent 1012RA as described in the table?

Yes it is. This has been clarified in the revised manuscript.

Section 5.1 Page 262 line 20: Table 3 not 2 Page 263 line 4: ..."FAI and UMIST show a reduction amount of scatter".. This is not true looking the graph, the scatter is similar to the other sonic.

We agree that the overall scatter (as quantified by the R^2 value is similar). However, the bulk of the measurements does fall more closely onto the regression line, while R^2 is strongly influenced by a few outliers. This has be reworded in the revised manuscript.

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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.* The discussion and conclusion of the energy balance closure has been changed. See response to Referee 2.

Tables and figures:

Figures 1-6: include letters in the figures if you cite them in the text with letters

Letters had been included on all graphs referred to by the Referee and show on the online versions in BGD of the manuscript. Maybe they did not print correctly?

Figure 4: use the same scale for the four pictures, invert the position of pictures c and d.

Adjusted.

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