Hydrology and Earth System Sciences Discussions

Interactive comment on "A framework to utilize turbulent flux measurements for mesoscale models and remote sensing applications" *by* W. Babel et al.

Anonymous Referee #1

Received and published: 20 July 2011

The manuscript presents a method to combine eddy covariance flux measurements with SVAT modeling to determine flux averages on the 1km scale. This method is described in detail and evaluated using LITFASS 2003 data. Upscaling fluxes is a topical issue and of interest for the scientific community. However I have two main major concerns:

- Theoretical basis: The authors suggest a pragmatic, straightforward method to derive the grid box mean flux without presenting any theoretical basis: The box is subdivided into patches of different land use. Fluxes for one land use class (i.e. target land) are

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obtained from measurements while modeling results are used for the other land use patches. Such an all-or-nothing scheme, putting full weight either on measurements or on modeling results is statistically suboptimal. Using model results to upscale and complete limited observations is a well developed field of research in meteorology named data assimilation. There, it is accepted that both sources of information should be used to obtain an optimal estimate by weighting the data according to the error covariance matrix – all-or-nothing schemes are not state of the art. Furthermore, the manuscript lacks any reference to data assimilation.

Adequacy of evaluation concept: Although the paper provides an extensive evaluation of the methodology based on LITFASS 2003 data, I question this proof of concept for the following reasons: i) The method is only tested for the situation of two land-use classes and the so called target case (Section 3.2.1). More land-use classes or the mixed case (Section 3.2.2) are not considered. ii) The LITFASS 2003 campaign is limited to a short time period (about one month) with very special weather conditions (extremely dry summer of 2003). Thus is generalization of the findings is questionable. The authors even mention the need of significant tests (p. 5186, line 8), but unfortunately do not pursue this idea. iii) The flux differences between the considered land use classes are small.

I was surprised to read the beginning of the third paragraph of the abstract: First it is claimed that application of the method to LITFASS 2003 shows the potential of the approach. Then it is mentioned that the spatial integration would be rejected in this situation due to the small flux differences. I hardly dare to conclude that the benefit of the method is demonstrated for a situation which is not suitable for a practical application of methodology.

Taking this the aforementioned main concerns and the other major comments below into account, I regret not to recommend this paper for further publication.

Further major comments

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Gapfilling: I would recommend to omit any gap filling for this study. Gapfilling is needed to calculate "climatological" budgets, but not for any evaluation purposes – dates with missing values can easily be ignored. Gapfilling results in dangerous similarities between reference data and the data to be validated. This issue is shortly discussed on page 5192, but it can easily be avoided.

1km scale: At various locations in the text, it is mentioned that the method is designed for grid box with 1km edge length. Please explain where does the scale "1 km" come from? Why is the technique not applicable for 500m or 5km grid boxes?

Section 4.3 – evaluation of the threshold: In practical applications the critical point is how well D_obs can be inferred from D_mod. Please investigate this problem in detail. How good is the correlation between D_obs and D_mod?

Balanced summary of the results: Both the discussion and the abstract are biased towards the positive results. Looking at table 2, it becomes obvious that the method is beneficial for two combinations of stations and shows neutral to negative impact for one combination. This two to one outcome needs be reported.

Focusing on upscaling: The paper should be better focused on its core topic: i.e. upscaling. For example, there is no need to discuss the instrumentation of the stations in detail or to explain the SEWAB equation (1)-(5). In addition, the paper will be easier to read if fewer references to other sections are used and certain topics are not separated into different sections: e.g. model evaluation techniques are both discussed in Section 3.1 and Section 4.1; discussion of threshold X (3.2.1 and 4.3). In the light of the quite simple theory, it might be possible to simplify the nomenclature that a list of symbols is no longer needed.

Uncertainties and table 5: I wonder whether the deviations between model results and OBS derived in Section 4.4.1 (or Table 3 and 4) are an estimate for the model uncertainty – instead of the estimate at the end of page 5194. How do the values of 35Wm2 and 50Wm2 "follow from Table 5"? Why is there a need to list the instrument

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uncertainties in Table 5?

Need for calibrated SVAT: Is there really a need for a calibrated SVAT scheme? On page 5192, the MAE between OBS and uncalibrated SVAT is 39Wm2 and between OBS and calibrated SVAT 37Wm2. Is this small improvement of 2W/m2 worth the effort?

Title: The title is to vague, in particular the verb "utilize". It is misleading to mention "mesoscale models" and "remote sensing", because both applications are not presented in the paper. Suggestion: "A method to upscale turbulent flux measurement using SVAT modeling"

Quality flagging and table 6: I can not follow the reasoning to derive the rules of table 7. The underlying theory and assumption need to be outlined in more detail.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5165, 2011.