

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

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I thank the reviewers for their constructive comments on the paper and the answers provided by the authors.

The authors have clarified some of the misunderstandings and issues raised by the reviewers in their responses. Nevertheless some major concerns of the reviewers have not yet been addressed satisfactorily in the responses and should be addressed more carefully in a revised version of the manuscript. These points are

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1. **Scale:** The paper needs to emphasize more clearly which scales are addressed and especially where the limits of the applied method are. In their response the authors argue that their method is not applicable to areas of size of 25 km², but claim at the other hand that it should provide a useful approach for the validation of satellite flux estimates. They give MODIS data with 1 km² size as a reference here. However I agree with reviewer 1 that it is of high interest if the method is applicable also on larger scales, like for instance thermal infrared data from geostationary satellites. Given the fact, that the Lindenberg observatory is operating a scintillometer with a path of approximately 4 km length, it is an interesting question if this data could be of any value to validate a 4x4 km² satellite pixel for instance. Shortly speaking, it is not clear where the limits of the method are to the authors opinion. I would therefore recommend the authors to carefully investigate the potential and especially limits of their method in a revised version of the manuscript. I could imagine that this could be done with a synthetic experiment, running a SVAT model at different spatial resolutions.
2. **Applicability:** Both reviewers did criticize the application of the method on rather homogeneous targets and criticized that this is an oversimplification of a real application. The authors respond that the testing of the method is only applicable in case of homogeneous areas and it is understood that this is the best way to start with the validation of the approach. Nevertheless, the reader will not know in the end if the suggested method is also applicable for more complex surface conditions. Again, where are the limits of the method? What is driving the limitations? Can authors give a threshold on the maximum degree of complexity where the method is applicable?
3. **Model-data synergies:** Reviewer 1 did criticize that the authors use a sub-optimal approach to balance between the information content from the observations and the model simulations and suggests to use data assimilation as a method to combine the two. The answer of the authors is unsatisfactory to my

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opinion, which might be due to a misunderstanding of the reviewers suggestions. Authors respond that DA can not be used, as a) only a few point like FluxNet stations are available worldwide and b) models often do not support DA. To my understanding, the reviewers intention was to use DA with the used model to get a best estimate of the mesoscale surface fluxes, weighting the uncertainties of the model and observations. It sounds like a durable approach and I highly recommend the authors to pursue or at least discuss DA in a revised version of the manuscript.

As a summary I recommend the authors to submit a revised version of the manuscript under the condition that the reviewers comments are addressed appropriately in the paper. A review of the revised manuscript will be made by the referees.

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