

Dear Editor,

We would like to thank you very much for your work on our manuscript entitled with "Evaluation of a complementary based model for mapping land surface evapotranspiration". As suggested by two referees, the title was changed to "Evaluation of a Priestley-Taylor model for mapping land surface evapotranspiration". We have tried to respond to all comments of two referees and to revise our manuscript accordingly. All revisions were colored in red on the revised manuscript. We also responded to your comments point by point below. We hope our answers clarify all questions and concerns.

We are looking forward to hearing back from you again, and in advance, thank you very much for your further efforts.

Best regards,

Zhigang Sun
on behalf of the co-authors

Both referees agree that there is novelty and interest in the manuscript; however issues relating to the communication, style, methodology, and quality assurance methods in the manuscript have been brought up. These issues must all be addressed before the paper could be considered for publication.

Response: Issues regarding the communication, style, methodology, and quality assurance were addressed in the revised manuscript, and we answered all questions from two referees.

Specifically, please:

- Either provide a much more substantial argument relating the methods used in this study to Complementary Theory than is currently provided, or alter the title, as suggested by Reviewer 1.

Response: As suggested, the title was altered.

- Restructure the manuscript to ensure that the specific research questions are identified very clearly, as suggested by Reviewer 2; and ensure that the methods used to address each question

are explicitly explained, as both reviewers have suggested.

Response: Done. See the responses to Reviewer 1 and Reviewer 2.

- Provide sufficient information about the available data, including footprint data for the flux tower, rationalization for working with small-grid-cell ASTER products (including discussion of the potential confounding role of advection), and a justification for the particular choice of time periods used to evaluate the different kinds of model.

Response: Done. See the responses to Reviewer 1 and Reviewer 2.

- Include a control equation to compare the Venturini Equation with, as suggested by Reviewer 2.

Response: Authors believe that eddy covariance fluxes are the most straightforward to evaluate / validate model estimates.

- Provide detailed information about the estimation of alpha from the land surface characteristics, as suggested by Reviewer 1.

Response: Done. See the responses to Reviewer 1.

- Provide detailed information about the time step used to estimate the fluxes from ASTER and how the ASTER observations were interpreted in light of a daily ET estimate. As reviewer 1 notes, it is not clear whether the authors have, in fact, estimated an instantaneous ET value associated with the timeframe on which the ASTER image was captured.

Response: Done. See the responses to Reviewer 1.

The manuscript as a whole would benefit from further editing as there are several small errors in typing, English language usage, etc. Please ensure that it receives an edit of this nature prior to resubmission.

Response: Typing and English language usages were double-checked by two authors separately. Also, an English native speaker was invited to check English language usages. This could minimize these small errors.

Please note that both the technical and communication issues must be addressed for a revision to be considered further for HESS. That said, it appears that most of the technical issues relate to providing a clearer explanation of methodology, and addressing concerns about advection (potentially this could be addressed by providing a comparison with a lower-resolution thermal product). I hope you will consider providing such a detailed revision, and look forward to seeing

an improved manuscript.

Response: Both the technical and communication issues were addressed. See the responses to Reviewer 1 and Reviewer 2.

A clearer explanation of methodology was conducted. See the responses to Reviewer 2.

A comparison with a lower-resolution thermal product was not included in the revised manuscript. Authors believe that the issue regarding dependence of remote sensing evapotranspiration algorithms on spatial resolution (Gebremichael et al., 2010) is out of the scope of this paper. However, discussions or explains regarding warm advection were addressed in the revised manuscript. See the responses to Reviewer 1.

Reference:

Gebremichael, M., J. Wang, and T. W. Sammis, 2010: Dependence of remote sensing evapotranspiration algorithms on spatial resolution. *Atmospheric Research*, 96(4), 489-495, doi:10.1016/j.atmosres.2009.12.003