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Hydrology and Earth System Sciences Discussions

Interactive comment on "Rainfall retrievals over West Africa using SEVIRI: evaluation with TRMM-PR and monitoring of the daylight time monsoon progression" *by* E. L. A. Wolters et al.

Anonymous Referee #2

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This paper deals with the application of a rainfall estimation technique, originally developped for Europe, to the West African monsoon region. The study further uses TRMM PR data and a subset of raingauges observations to evaluate the satellite derived rainrates. Finally a very crude analysis of the propagation of the rain features is proposed. The monsoon rainfall observations from satellite is a very active field since 40 years now (since GATE) and I think this paper does bring, if any, only very little pieces of interesting and new information.

I encourage the authors to reconsider the objective of such a publication. In the present

form I do not recommend accepting the manuscript.

Below are the detailed comments along three main lines : remote sensing and justification, validation aspects and meteorology.

-Remote sensing aspects and justification While the presentation of the algorithm is clear, its justification in the introduction is not. Opposing the ground network capabilities against the satellite perspective does not bring you any where. It is clearly possible to monitor the monsoon seasonal march with rain gauges over Sahel (Sultan and Janicot, 2003, JCLIM; Janicot et al., 2008, Annales G) so the satellite does not fit in a gap. It complements the existing network and extends the capability of the ground network but not at all scales. Furthermore there are a large amount of products already doing this very well and I am not sure the present retrieval makes it any better than GPCP 1DD for instance.

Moreover there has been extensive work showing that the ultimate, best products for rainfall estimations is actually the merging of satellite and gauges products (see GPCP, TMPA, TAMSAT etc...) The recent intercomparison work of Jobard et al., 2010 IJRS clearly reveals that only the merged products are reaching a high quality level over the WAM.

Finally I think there are many relevant previous papers that should be identified in your manuscript that paved the way for satellite rainfall estimates over Africa that I could not see in the present version. Putting more work on a thorough bibliography effort might be needed. All the mentioned references and references there in might help to do so.

-Validation aspects There the validation is made for instantaneous rainrates (which is not used in the application section of the paper) and not much about the accumulated rain amount which is much important to the users in meteorology and hydrology (at various scales). Also while you focus on summer 2006 only a very small subset of the data from the AMMA project is used and these are strong limitations of the present evaluation/comparison. One product is used as a reference and there is no effort to

further include other estimates in the evaluation. So it is unclear how the new product behaves compared to some other (for instance other geo based algorithms) which is one more strong limit of the present study. It does not allow to see what the new product brings in.

Roca et al 2010 JAMC, proposed a meteorological benchmark to evaluate the satellite products over the monsoon. It uses the errors on the estimates and a variety of ground based datasets and scales to assess the relevance of the satellite products for use by the meteorology community. They furthermore developped and applied the technique during summer 2006 and intercompared various products. I encourage the authors to actually get inspired by such a an approach to really evaluate what their new product is really bringing in to the topic.

-Meteorology I am very surprised of such a very crude analysis of the propagating features being presented here. The authors are spending quite some time to build their case of having a very accurate quantitative rainfall products and it is now used extremely qualitively (visual inspection of the hovmuller diagram). Carbone et al 2002 JCLIM provide a much more refined perspective on this topic that could be of interest to the authors. The conclusions are that Sahelian MCS travel around 50 km/h is known since the inception of tropical meteorology. Furhtermore the authors correctly note that in the literature there is a variety of defition used for MCS yielding to hard to interpret differences in their contribution to rainfall but do not provide any efforts to cope with that and the 27% number is therefore useless and its meaning very hard to put back into perspective.

Overall this part of the paper does not bring in any interesting pieces of information and it is unlikely you need the present rain product to do this.

It is written several times that "MCS are transported through AEW" ; "dynamically forced" etc... I dsiagree with such statements. Please see the statistical study of Fink et al 2003 JGR to get an overview of past work and of results that clearly shows the

limitations of the AEW-squall line or MCS relationships in Sahel. See also Diongue et al 2002 QJRMS for a detailed discussion of a case study analysis that brings in more up to date perspectives on the MCS-wave interactions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6351, 2010.