

***Interactive comment on “Satellite-based
evapotranspiration and crop coefficient for
irrigated sorghum in the Gezira scheme, Sudan”
by M. A. Bashir et al.***

T. Wagener (Editor)

thorsten@enr.psu.edu

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This is in an interesting article with authors working in a generally data sparsely region. Initial results are interesting, but the current presentation is not sufficiently detailed and it is not clear what the general significance of the results are. Below are more detailed comments for the authors to consider:

o The language of the paper is not of sufficiently high quality for publication in an international journal. A serious revision of the text is required to achieve this.

o Abstract: While the results are presented, the consequence of the results should be discussed. Rather than providing great detail with respect to the values achieved, it

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would be better to discuss what these results mean and how general the result is.

o P3: “Accurate estimate of evapotranspiration is considered as the key factor in water resources management.” - By whom and for what region of the world? I don't think this is generally true.

o P3: “The main advantage of the energy balance \check{E} ” - What are its limitations?

o P3: “ETM+” - Should be defined here, not just in abstract.

o P5: “An overview of some parameters \check{E} ” - Why only some? How many others are there?

o P5: It would be good to describe the actual impact the instrument malfunction has on the image.

o P5: “There are many remote sensing algorithms for estimating the energy balance fluxes on the surface, each algorithm has its own advantages and disadvantages.” - What are the advantages AND disadvantages of SEBAL?

o P5: If SEBAL has been widely applied, please provide some references of examples.

o P8: “intensive gravimetric samples” - What is your definition of intensive? A map of the study region showing where these samples have been taking (spatial distribution) would be helpful. When did irrigation occur in relation to when the images were taken? At what depths were the gravimetric samples taken?

o P9: “Second degree polynomial equations were used in the regression for the 0-60cm depth while linear regression was used for the 60-100 cm depth.” - Any justification for this choice?

o P9: “soil moisture depletion approach (MD)” - Before this sentence the approach was called water balance approach.

o P13: “Owing to low temporal resolution of high spatial resolution image and the cost

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involved with the acquisition make their use unattractive. Therefore, the availability of free of charge daily basis satellites such as NOAA-AVHRR (National Oceanographic and Atmospheric Agency - Advanced Very High Resolution Radiometer) and MODIS (Moderate Resolution Imaging Spectroradiometer) makes them a viable alternative for future estimation of ET.” - If these products are free, why haven't the authors used them? What are the differences between the products?

o Response to point 4 by Kunstmann: The authors state that the chosen method was used by many researchers and reference Tasumi and Allen 2000; and Chemin and Alexandridis 2001. However, neither of these papers is a peer reviewed publication. If the authors want to justify their approach by referencing other studies, then they should refer to peer reviewed work.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 793, 2006.

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