

Interactive comment on “Earth observation Water Cycle Multi-Mission Observation Strategy (WACMOS)” by Z. Su et al.

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This paper reports on a program of work, the WAter Cycle Multi-mission Strategy project, launched by ESA, with the GEWEX collaboration. The aim of this program is to develop optimized satellite-derived products to describe the water cycle, taking into account the synergies of the large number of now available Earth observation datasets. The parameters to be examined are the evapotranspiration, the soil moisture, the cloud cover, and the water vapour: for each one, the paper describes the adopted retrieval methodology and briefly present some results.

Global long term datasets of key variables of the global water cycle are still not available with the required accuracy for model evaluation or for the analysis of their inter-annual

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variability. A large range of satellite observations is now available but their use is not always optimized. The objective of this paper is to propose new multi-satellite datasets of key water cycle parameters, which is a very interesting and challenging subject that fits well with the topics covered by the HESS journal.

The introduction and the background sections insist on the links of the WACMOS program with the other international efforts within GEWEX, on the importance of the development of global products over long time periods, and on the benefit of the synergetic use of multi-satellite observations.

Although the study refers at several occasions to international programs, it seems to ignore the collaborative works that are going on. For instance, the LandFlux activity, launched by GEWEX Radiation Panel is currently evaluating the global turbulent fluxes published by different groups worldwide. The WACMOS products do not seem to be part of this initial comparisons (e.g., Jimenez et al., JGR, 2011; doi:10.1029/2010JD014545, Mueller et al, GRL, 201, doi:10.1029/2010GL046230) and the present study does not refer to it. This study also fails to acknowledge the cloud assessment work performed by Stubenrauch et al. within the GEWEX community. No reference is done either to the GPCP effort, when analysing the precipitation occurrence and intensity.

The program insists on the global nature of the water cycle. However, the capacity of the methodologies is not proved at large scales for all products. For instance, the results for the evapotranspiration should be presented at least at continental scale in order to be convincing. By the same token, evaluation of the cloud products at one station only is clearly not sufficient to validate the method.

This study makes use of a large number of satellite observations, but the way these diverse measurements are exploited is questionable, especially for soil moisture retrieval. Both passive and active satellite observations are used to retrieve the soil moisture. Depending on the environments, the method consists in using one retrieval, or the av-

eraged value of both retrievals. Does this juxtaposition/averaging really benefit from the synergy between the measurements? Both active and passive measurements are sensitive to the soil moisture as well as to other parameters such as vegetation cover, soil texture or roughness. Using the two information jointly could certainly help solve this under-constraint problem, but that would require developing a coupled algorithm that does merge the two pieces of information (Aires et al., JGR, 2006; JGR, 2011). The very simple solution suggested by this paper has to be thoroughly evaluated to be convincing. As presented in the paper, it is clearly not satisfying (the comparison with in situ measurements in Figure 14 even seems to show that the combination of products is worse than one of the product alone).

In conclusion, this study is not up to the expectation it initially raises. It presents some work in progress, based on already published work or on the merging of already published data sets, without any convincing evaluation. It does not provide a thorough analysis of new scientific results. One could support the idea of an overview paper that would report on finalized products described elsewhere, but this is not the case either, as none of these products have been carefully assessed yet. Before publications, more work has to be done, to convince the reader of the added value of these multi-mission products.

Some detailed comments:

- Several references are not in the list (e.g. Timmermans et al., 2010; Hollweg, 2005).
- P. 11. By saying "most current algorithms" the author seems to neglect another "school" of remote-sensing based evapotranspiration algorithms based on modified Penman-Monteith/Prisley-Taylor approaches. Although it is certainly true that for the moment there are no grounds to establish that one methodology is superior to the other, most of the published global estimates currently come from this

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alternative approach (e.g., same journal, Miralles et al., 2011, doi:10.5194/hess-15-453-2011).

- P. 12. Being evapotranspiration an official MODIS product, would have not been of interest to at least try a comparison of the MODIS fluxes derived for Figure 2 with the MODIS product
- P. 15. Why is the US surface model used as a reference? Why not using the ECMWF model in an ESA project? Any reasons?
- P. 16. The explanation of errors in ASCAT in terms of sand dunes is not very convincing: the patterns do not correspond at all with the main sand dunes. . .
- P.19 Does the author mean that no "simultaneous" global products of net radiation, ground heat flux or sensible heat flux exist? There are certainly global products of net radiation, such as the NASA/GEWX SRB, or the ISCCP-FD-SRF.
- Figure 2 is of very poor quality (the colour scales have to be changed). The square patterns on A and D have to be explained and the caption needs to be more explicit. The main text mentions fluxes scaled up to daily values, but the fluxes presented at the figure seems to be the instantaneous values at the satellite overpass.
- Table 1. The ET part seems confusing. What do HR, LR stand for? Is it not AATSR, MERIS, MODIS GC coverage and MSG MD? They seem to be mixed up. The 25% reported uncertainty, is it based on preliminary validation efforts or a target precision? ET over oceans does not seem discussed at all in the article, but mentioned in the table. Is it also a WACMOS product?
- Figure 3. The correlation coefficient on figure 3 should be checked. It seems very high given the plots, especially for LE.

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- Figure 6. On which time period are these maps calculated? How are explained the various spatial structures on these maps? For instance, the large changes along the Amazon River for both ASCAT and AMSR, or the large gradient in the Arabic Peninsula for ASCAT?
- Figure 7. Is this map fixed, regardless of the season?
- Figure 8. It seems that the combined product is worse than the AMSR one. Is it really the average of the two individual products? Could you provide the rms error? On this example the ASCAT info appears to bring very little information. Why using it then?
- Figure 10. Could you elaborate on the variable that is presented? Why would it be negative?

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