

Interactive comment on “Soil moisture-runoff relation at the catchment scale as observed with coarse resolution microwave remote sensing” by K. Scipal et al.

M. Thyer (Referee)

mark.thyer@newcastle.edu.au

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Overall, I liked this paper. The authors gave an excellent review of the state of the art in using remote sensed soil moisture for runoff prediction. Including a discussion of the current and future remote sensed techniques for measuring soil moisture. They clearly identified their knowledge gap - that limited research has been undertaken to relate macro-scale soil moisture to runoff and highlight that remotely sensed products have been largely ignored by the hydrological community. The approach they use to contribute to this knowledge is somewhat simplistic (which they acknowledge) but shows quite promising results. This will need further development as the new information from the upcoming satellites becomes available. I think their conclusions are somewhat optimistic in relation to reporting $R^2=0.8$, and may have overlooked some crucial points

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that needs further work to clarify. The suggested additional work and corrections are given below in the specific comments.

I should qualify my review by stating that I have only a limited experience with remotely sensed soil moisture. Therefore, my knowledge of the literature in the area is very limited to make judgements whether this represents novel work. Though I do have considerable experience with hydrological modelling, calibration and prediction, which is where my review is focussed.

I am recommending that this paper is suitable for publication if the specific and technical comments below are addressed.

Specific Comments

1) Does the paper address relevant scientific questions within the scope of HESS?

Yes, a relationship between catchment scale runoff and coarse resolution microwave remote sensing data was clearly identified. Some of the parameters were related to catchment characteristics.

2) Does the paper present novel concepts, ideas, tools, or data?

Yes, the relationship between the newly developed, "Global Soil Moisture Archive 1992-2000" and runoff for the several data sets in Africa, seems to be a promising tool that will improve with further development.

3) Are substantial conclusions reached?

Yes, though I think they are somewhat overoptimistic (see below) and should be tempered.

4) Are the scientific methods and assumptions valid and clearly outlined?

The Kalabo station was simply disregarded due to poor results, with no explanation as to possible reasons. From Fig. 1 it seems Kalabo has a very small catchment

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compared to the other stations. Does this indicate that this BWI index is not suitable for smaller catchments - What is the lower limit of its applicability? The authors should elaborate on this further

On page 432, line 10 - do the authors actually mean intra-annual instead of inter-annual. Intra-annual refers to the seasonality, while inter-annual refers to the differences between years.

Also - what is the mean annual cycle could you please elaborate on the definition?

5) Are the results sufficient to support the interpretations and conclusions?

Yes and No. The authors report a very high correlation ($R^2=0.8$), as a major success. However, from the residual plots of Fig 4 and 5 it seems that the large number of low flows in the dry season (over 1/2 to 2/3 of record) dominates the scatter plot and therefore possibly artificially inflate the R^2 value. The predictive ability of the medium and high flows maybe considerably lower than $R^2=0.8$.

I have two possible suggestions to investigate this: A) Plot a time series of residuals (obs-pred)/obs for a dry/wet/very wet year and indicate the ability of the model to predict the high, medium and low flow periods. OR B) Report separate R^2 statistics for the dry and wet seasons.

This is important from a prediction of ungauged basins point of view - which is mentioned by the authors as a future research goal of using remotely sensed soil moisture products. The predictive ability for different flow seasons/regimes needs to be analyzed and discussed.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Is the same time shift applied to each year? This was not clear when its mentioned on page 431, line 3. If so, is this assumption of a constant time shift valid? Again, this is important point for predication of ungauged basins.. Plotting a time series of residuals

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as suggested above would provide valuable diagnostic tool to investigate this.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes. Except the conclusion states that a novel data set "Global Soil Moisture Archive" is presented. That did not seem to be the major emphasis of the paper; rather it was the development of the relationship between this soil moisture data set and runoff.

8) Does the title clearly reflect the contents of the paper?

Yes.

9) Does the abstract provide a concise and complete summary?

I think the R2 values should be tempered, pending the results of the additional analysis suggested in (5) above. The abstract should also state that the soil moisture-runoff relationship was derived using temporally averaged (10 day) data.

10) Is the overall presentation well structured and clear?

Yes

Technical Comments

11) Is the language fluent and precise?

Some minor spelling and grammatical errors throughout the paper.

Pg 418, line 12, "scatterometer" need an "s". Pg 418, line 21, "to some extend" should read "to some extent". Pg 419, line 9, "of use for hydrological models" should read "for use in hydrological modelling". Pg 422, line 17, the sentence "Already the assimilation....." does not make sense in the context of the paragraph and needs to be rephrased. Pg 426, line 11, "....and point midway the field...." should read "...and the point midway between the field...." if I'm interpreting this correctly. Pg 427, line 27, the word "amounts" does not make sense and this needs rephrasing. Pg 429, line

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22, the phrase "in a first step" is not very good grammar. The sentence should be rephrased to read "The first step in this analysis was to visually analyse the scatterometer....". Pg 430, line 14, Delete "of" from the phrase "Despite of this difference". Pg 431, line 8, "were" should be "where" Pg 433, line 21, the sentence "For a quantitative comparison..." does not make sense in parts and needs to be rewritten Pg 433, line 25, "waterlevel" should be "water level"

At times, the language is wordy and the authors need to tighten it prior to publication.

Also in Fig 4,6,7 "Victoria" is incorrectly spelled as "Vicotria"

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Standard journal writing is to define all acronyms the first time they are used. This has not been done in this paper eg. ERS, METOP, ESA etc. Although these may be well-known in the remote sensing field, they may not be as well-known to the general hydrologist - who are also a major audience. Defining the ALL terms would considerably improve the readability.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Fig. 1 is of poor quality and needs to be improved. Fig 2 and 3 is difficult to discern between different years.

14) Are the number and quality of references appropriate?

Seems to be ok - but I don't have a strong background in the area so can't really pass judgement.

15) Is the amount and quality of supplementary material appropriate?

Fine.

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