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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Comparison of soil moisture fields estimated by catchment modelling and remote sensing: a case study in South Africa" *by* T. Vischel et al.

Anonymous Referee #2

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General comment: The authors compare two independent data sets of estimates of temporally and spatially distributed soil moisture: one that has been obtained by physically-based hydrological modelling (TOPKAPI) and one that is obtained from spaceborne active radar (scatterometer on board ERS) using a simple conceptual model to compute the soil moisture profile in the soil horizon based on the remote sensing based moisture estimation within the first 5 cm of soil. The dynamics of the two estimates of average soil moisture at catchment scale agree quite well, as do the modelled and remotely sensed soil water index at the scatterometer footprint scale. However, an important (systematic?) bias is observable. The authors state themselves that the "paper aims to compare, for the purpose of corroboration, not validation, two different approaches". Hence the apparent agreement between the two estimates cannot serve as a proof that the two approaches provide accurate results. The confidence that one can put in the results obtained with the two approaches is, of course, increased. Unfortunately the authors did not have access to field measurements of soil moisture that could have been used to objectively discuss the capability of the two approaches to provide accurate soil moisture estimates in space and time (though it has to be admitted that there doesn't exist any ground truth of catchment average soil moisture only some indication based on a dense network of soil moisture measurements at variable depths).

I believe that the results presented here are worth being published, subject to some clarifications. The dynamics between the two data sets are very similar and demonstrate the capability of scatterometers to monitor basin averaged soil moisture. The approaches to estimate soil moisture are not innovative themselves since both methods have been presented in separate studies. The scientific merit thus mainly consists in comparing both data sets. Therefore the discussion part should however be emphasized. Several sources of uncertainties indeed remain and are worth being discussed in a more detailed way by the authors of this paper. The biggest concern is associated to the observed bias: possible interpretations of it need to be emphasized and potential ways to correct it would be helpful.

- The authors state that the effects of plant growth and decay are taken into account through the application of seasonally varying sigma_dry and sigma_wet reference values (p. 2278 & 2279). It is of paramount importance to explain how this was done because, as far as I understood, the methodology (eq. 1) that was adopted is based on the assumption that roughness and vegetation cover do not change between subsequent image acquisitions. This assumption is indeed only acceptable if the season is split in various time frames with constant land cover. So it is very important to explain this in a more detailed way. Many uncertainties (and thus mismatches between model and RS estimates of soil moisture) may in fact stem from this. How did you assess the temporal variation of vegetation cover? And how did you perform the seasonal splitting? There is also a contradiction when the authors first say that sigma_dry and sigma_wet are the lowest and highest values of backscatter coefficients determined on the nine-year measurement period and a few lines later they state that both terms are seasonally varying??? Also it is sensible to question the assumption that in each season the lowest backscatter corresponds to zero soil moisture (p. 2278 l. 25)? Would it not be more appropriate to describe this as the minimum observed soil moisture within the observation period and, consequently, instead of using the term “soil moisture” to use something more qualitative as “surface soil moisture index”?

- The paper is clearly unbalanced with the description of the model set-up and calibration outweighing the rest of the paper and particularly the remote sensing based approach to estimate the soil moisture (a ratio of 8:1 in terms of paper pages!!!). Also the results and discussion parts are minor compared to the model description. I would therefore suggest compacting the modelling part and to extent the satellite part (see for instance the previous comment) and the discussion. - The self-confidence of the authors is sometimes a bit annoying. They tend not to cast too many doubts on their assumptions and results. Obviously, there are some major flaws (e.g. the bias, the correction factors of model parameters, the simplistic model to transform surface soil moisture into soil moisture profiles, the Nash values of 0.6 in validation mode etc.) that the authors do not clearly acknowledge or at least critically comment. A more “auto-critical approach” would be appropriate. - The model calibration consists in optimising 4 multiplicative correction factors. This is generally considered to be acceptable if the model is used for discharge predictions (discharge being an integrate response of catchment behaviour). However, if the model is used to provide soil moisture fields, such an approach appears to be more criticisable. In fact the classification of model parameters that is done a priori largely determines the patterns that are simulated. This fact should be acknowledged. Unfortunately the model is calibrated with discharge data only. The capability of the model to predict soil moisture (patterns of soil moisture as well as basin averaged moisture) cannot be assessed with the data at hand. - The authors state that, except for soil conductivity, the model parameters estimated a priori were quite appropriate. This comment does not apply to the Manning roughness value for channel. The multiplicative factor is calibrated at a value of 1.7 and knowing that the plausible values of this parameter range somewhere between 0.03 and 0.07 this value is a clear indication that the a priori value was not appropriate at all. It is possible that the unexpectedly high value of channel roughness tries to compensate another error of the model. This disagreement between estimated and calibrated value should at least be commented. - Some additional computations were necessary to provide a reliable comparison between the two estimates of soil moisture. I found it surprising that the authors chose to apply a simple conceptual infiltration model in order to compute the soil moisture in the whole soil layer. In my opinion, it would be more appropriate to change the structure of the hydrological model in such a way that it would allow to explicitly compute soil moisture in the first 5 cm of soil. The separation between the remote sensing and the modelling approach would be “cleaner”. By adding a simple conceptual model to the remote sensing based approach, the comparison becomes less evident and eventual mismatches become more difficult to interpret as it is unknown whether they stem from the “raw” backscattering signal or the plugged-in conceptual model. The exponential filter adds large uncertainties to the remote sensing approach which is unnecessary. - No real sensitivity analysis is shown in the present paper. There are a high number of degrees of freedom in both approaches, and this fact could facilitate the finding of rather good matches between both data sets after calibration. Of course I trust the authors that the hydrological model calibration was done only on discharge and not on remote sensing estimates of soil moisture (so potentially even better fits could be achieved Ě). The same applies to the calibration of the “remote sensing models”. Considering this, it would be helpful to discuss the influence of hydrological model parameters, the choice of reference backscattering values, the characteristic time lengthĚetc. on the R2 values (expressing the level of agreement between the two data sets). - To me the bias between the two estimates is far from being “slight” (p. 2288). The dynamics are very similar but the order of magnitude is rather different (a bias of more than 20%!). The comments of the authors thus seem to be overly optimistic and the explanation that the authors give for this bias appears to be rather surprising. They focus on the choice of the reference backscatter values without giving any explanation why among the many potential sources of uncertainties this would be the most likely one (considering that seasonally varying reference values were chosen!). It is also not very clear why the choice of these sigma_dry and sigma_values would be uncertain (they are obtained by extracting the min and max values from a time series of backscattering values). It is more the underlying model that is uncertain, especially the assumption that sigma_dry corresponds to zero moisture within any time frame where the vegetation is supposed to be constant between subsequent acquisitions (it was not clear from the previous sections how the authors took into account the vegetation growth and decay). The assumption of a linear relationship between backscattering and soil moisture is also known to be criticisable (in fact it is not linear). Their comment further reveals that the authors seem to trust the hydrological modelling approach more than the remote sensing based approach, without giving any explanation for this. The simplicity of the method that was used to transform surface soil moisture into SWI may also be a possible explanation for the observed bias. In my opinion the bias is an important problem and the authors should give more convincing explanations for it (and ideally give some ideas how this bias could be avoided/corrected). - In the discussion I missed a comparison between the results obtained in this study with those that were obtained with radiometers (and SAR) in the framework of similar studies. Such a discussion would be an important contribution to the ongoing debate between the appropriateness of scatterometers, radiometers and SARs to obtain spatial fields of soil moisture. In general, in the discussion it would be helpful to set the results of your study in a more general context by citing other studies as well.

Specific comments: p. 2275 l. 4 please avoid the term “easily” because it is not that easy to calibrate the probesĚ p. 2275 l. 29 Aubert et al. used field measurements of soil moisture and not remotely sensed estimates p. 2276 l. 5 not sure if I agree with

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this sentence: any assimilation study has to be based on some sort of comparison between soil moisture from remote sensing and models (in order to set up the observation model and to parameterise the error covariance matrices). p. 2277 l. 23 why do you consider the flow data to be uncertain? What are the sources of uncertainty? Did you consider them during the calibration? p. 2280 l. 2 Hortonian overland flow being neglected in an arid catchment appears as a major flaw in the model description. The argument why the authors believe that the model structure is appropriate (p. 2290) should be moved forward (to p. 2280) to avoid confusions. p. 2280 l. 8 it is not clear whether the soil reservoir is split into a surface soil and an underground component. How did you take into account the increased saturated hydraulic conductivity near the soil surface? p. 2283 l. 24 what do you mean by "residual soil moisture theta_s" ? p. 2284 l. 17 how did you do the calibration of the 4 model parameters? Did you use any optimisation software? p.2286 l. 16 why did you not use the more "reliable data" for model calibration? Why is this data more reliable and what are the sources of uncertainties of your discharge data? How did you take these into account (see comment above)? p.2289 l. 20 two exceptions: first estimate of channel roughness was also wrong (see general comment) p. 2291 point 3 I don't agree with this conclusion. The agreement between the two estimates of soil moisture should not be treated as a validation of either one!!! Hence, the match between modelled and remotely sensed SWI cannot be considered as an evidence that the simple conceptual infiltration model allows to compute soil moisture profiles in a satisfactory way.

Technical corrections: Table1: X needs to be defined (is defined only later) Figure2: units should be given when available (e.g. DEM map), what is the meaning of the soil type legend? Figure5: typing error: 2 stations p. 2276 l. 25 on board of p. 2277 l.15 skip the term "unique" p. 2278 l.16 better: soil moisture retrieval methods or methods to retrieve soil moisture p. 2284 l.27 surface n0 (space is missing)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2273, 2007.