

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.

T. Vischel et al.

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1 Overview

The authors would like to thank the anonymous referees for their careful review of the paper. The insightful, thorough and searching comments, particularly those of Referee # 2, directed us to re-examine the fundamentals of the TOPKAPI model. Anonymous Referee #1 and Referee #2 (hereafter referred to as respectively R#1 and R#2) raised several points in the paper that required clarification.

We have considered the comments of the Referees with care and modified some parts

of the paper accordingly. In particular, the discussion in Section 5 has been extended to better discuss the results and address the concerns raised by the Referees. An important result of this post-analysis of the paper, was that some inconsistencies were identified in the Digital Elevation Model (DEM) treatment. This has led us to make some modifications in the DEM processing procedure and in the model formulation. The results obtained with the modifications are similar to those obtained in the initial submitted version of the paper but technical improvements in the model parameterization have been made.

In responding to the referees' comments, the changes to the model and their effect on the results are first outlined. Thereafter we respond in detail, starting with R#2. Note that the minor technical changes suggested by the referees are not detailed here but they have been taken into account in the revised version of the paper.

2 Outline of modifications in the hydrological modelling

Following the comments of the referees, we identified incorrect values of the channel and hill slopes computed from the DEM. After some investigation, the cause was found to be the result of applying flow routing in 4 directions (D4). Filling the sinks, which is part of the DEM treatment to get the correct hydrological flow routing, results in a strong smoothing of the relief detail because of the constraints enforced by the limitations of the 4D routing procedure.

The DEM we utilised at 1 km pixel scale had a vertical precision (resolution) of 1 metre, so that many channel slopes calculated automatically in the flat areas were computed as zero. Because this was physically unrealistic, the calibrated values of other parameters had to be changed to compensate.

To retain the detail of the relief variability, an 8-direction (NSEW and diagonal pixels)

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or D8 flow routing procedure was employed as being more suitable. Because the TOPKAPI model was initially designed with D4 routing, some modifications have been made in the model formulation as applied here.

DEM preprocessing:

1. The first step was to obtain the flow directions, which were routinely computed in D8 by the GIS software after infilling the sinks.
2. We note that D8 slopes calculated by many GIS software packages do not always make sense in the context of TOPKAPI. As a result, we used a separate calculation procedure to obtain the slopes of the hill-slopes and the channels; soil and overland reservoirs were distinguished from the slopes of the channel, each using D8:
 - the slopes of the soil and overland reservoirs were computed according to a neighbourhood function (as commonly calculated in the GIS) more representative of the mean slope within the cell and thus more representative of the transfers inside the cell (in and over the soil).
 - The slopes used to transfer the flows in the drainage channel network were computed from cell to cell in a down-stream direction.

This distinction has now been made in the paper.

Modifications in the TOPKAPI model: the channel length expressed as X_c can now be made equal to X (the characteristic length of the cell) or to $\sqrt{2}X$ (the length of the diagonal of the cell). This change influences the flow routing and the flow partitioning between the soil/overland and the channel (new partitioning ratio of WX_c/X^2 instead of WX/X^2 in the original version of the model specification).

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Consequence on the results: globally the results remain very nearly the same. There is a slight degradation of the model performance in modelling the discharge; the Nash values lie in the range (0.600, 0.788) instead of (0.634, 0.826). However the values of the calibrated parameters have changed for the better: $\text{facL}=1$ instead of $\text{facL}=1.1$ and especially $\text{facKs}=60$ instead of 100.

3 Response to the Referees' comments

The comments of referees are (*“quoted in italic ”*) in the text, partly or entirely when not too long, and discussed by the authors.

3.1 Comments specific to R#2

General comments

“...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.”

These important general comments will be answered in response to the more detailed questions that follow.

Determination and seasonality of the lowest and highest backscatter coefficients

“The authors state that the effects [. . .] state that both terms are seasonally varying???”

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We agree that the explanation of the retrieval technique was somewhat misleading. We extended the text and corrected one sentence to: “To account for effects of roughness and heterogeneous land cover, seasonally varying minimum and maximum backscatter curves are determined based on the nine-year measurement period 1992-2000.” For a more detailed discussion the reader is referred to Wagner (1999abc). We also made new references to papers that discussed the quality of the ERS soil moisture data over other study areas.

*“Also it is sensible [. . .]
use something more qualitative as “surface soil moisture index?””*

We added a discussion about the interpretation of the absolute soil moisture values provided by the ERS scatterometer. As discussed, the absolute values are indeed deemed less reliable than the relative values. However, an increasing number of independent validation studies demonstrates the quality of the retrieved surface values which is why we prefer not to use the more weak term “surface soil moisture index”. Please note, that for the scatterometer based estimate of the profile soil moisture content we use the term “Soil Water Index” to express that a rather simple infiltration model is used.

Lack of criticism of the model performance

*“The self-confidence of the authors [. . .]
“auto-critical approach” would be appropriate.”*

The text has been revised where appropriate. A deeper discussion has been added in the revised version of the paper in order to address the admonition of R#2. We hope that these comments reflect a more critical approach. Most of the changes are discussed in relation to the comments, detailed below.

The general organisation of the manuscript

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*“The paper is [. . .]
(see for instance the previous comment) and the discussion.”*

The set-up and calibration of the TOPKAPI model in South Africa is an original contribution that the authors wish to highlight in the paper. By contrast, the description of the methodology used to create the remotely sensed surface soil moisture index has already been the subject of several publications that are referenced in the text. The perceived lack of balance is thus unavoidable but it is now somewhat reduced by a more detailed discussion of the results in the last section (see next comments).

The methodology used to calibrate the hydrological model

*“The model calibration consists [. . .]
cannot be assessed with the data at hand.”*

It is true that the spatial a priori distribution of the parameter values follow a pattern consistent with the relatively static physical properties of the catchment. That is precisely the charm of the TOPKAPI model. The global adjustment of the spatially variable parameters, without adjusting their relative values, is designed to discount the relatively poor information content of the few series of the more highly temporally variable hydrological variables, such as rainfall, streamflow and evapotranspiration.

Thus the referee’s comment points out the difficulties that face all hydrologists concerning the limited availability of data, especially on a regional-size catchment such as the Liebenbergsvlei, studied here. The data available on the Liebenbergsvlei are: the discharge at three flow stations, the rainfall measured by tipping-bucket rain gages (an exceptionally dense network in Africa), and regional estimates of evapotranspiration. Such a monitoring network is quite good for a regional scale catchment.

The challenge is to deal with the data availability and the knowledge of the hydrological processes that control the response of the hydrological systems. In the modelling process, the available data and the knowledge of the hydrological processes have

to be combined to estimate the components of the water cycle that are not directly measured, as for example soil moisture. This is exactly what we have tried to do in our study.

Again, we emphasise that the focus is on soil moisture. As mentioned by the authors and noticed by R#2 (*“it has to be admitted that there doesn’t exist any ground truth of catchment average soil moisture”*), no ground-based soil water probe measurements are available on the catchment. According to the data available, the only way to constrain the model was to use the flow data and to adjust the main parameters that are known to mostly influence the simulations of discharge (i.e. the four calibrated parameters). It is then assumed that, if the processes modelled by TOPKAPI are representative of the main processes that occur in the catchment, then constraining TOPKAPI only by the flow data would give a realistic representation of the other components of the water balance at the catchment scale, and in particular soil moisture.

The authors are keenly conscious that the availability of soil moisture probes should have been useful in this study to verify, according to ground measurement, the relevance of soil moisture modelling. It is worth noting that, motivated to a large extent by the soil moisture project in which the present work is integrated, a national network of soil moisture probes is planned to be implemented in South Africa with a particular refinement expected over the Liebenbergsvlei. This network will be used for future works, to address some of the issues that the available data are still not able to cover.

The disagreement of the parameter values estimated a priori and the values calibrated

“The authors state that, except [. . .] disagreement between estimated and calibrated value should at least be commented.”

True. However, in the opinion of the authors, the value of 1.7 for the channel roughness factor is partly due to the precision of the available DEM. The vertical precision of the

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DEM is 1 metre and the catchment is relatively flat, particularly close to the drainage network in the northern (lower) part of the catchment. Long river sections, characterized by zero slopes, had to be artificially corrected according to the precision of the DEM. It is however worth noting that the channel roughness only influences the timing of the discharge in the channel and consequently does not interact with the processes on hillslopes (overland and soil processes) that determine the soil moisture. One of the improvements that we have achieved in the model is the timing of the river flows (a more appropriate DEM would be useful here) but improving the modelling of the timing in the channel will not change the results associated with the soil moisture estimation. Due to the constraint of the limited space available in a single paper, this has not been discussed in depth; however we have added a comment on this aspect in the discussion.

The use of a conceptual infiltration model applied to the remotely sensed data in order to make the comparison with the model simulations.

*“Some additional computations were necessary [. . .]
filter adds large uncertainties to the remote sensing approach which is unnecessary.”*

The referee has touched upon an important limitation of the original TOPKAPI model - we (and it appears so is Todini) are working on a surface layer component of the model, but this was not available for the paper. Also, it is true that the use of the exponential filter to infer the soil water content adds supplementary uncertainties in the comparison process. However, as studies by Ceballos et al. (2005), Pellarin et al. (2006) and others have shown, the quality of SWI is surprisingly good considering the simplicity of the method. Also we would like to point out that the comparison of SWI with models will continue to an important issue because - like TOPKAPI - the large majority of models do not have a surface layer that can be directly compared to remotely sensed surface values. Thus, the SWI method is expected to facilitate the scientific exchange between the modelling and remote sensing communities.

Again, we note that the explicit representation of the surface soil moisture in TOPKAPI (as identified by the authors at the end of the paper) is an essential improvement of the model; this will be complement, but be independent of the conceptual infiltration model applied to the remotely sensed data. As pointed out in the discussion in the paper, such modifications in the model raise a number of questions that require future investigation.

Model sensitivity analyses

*“No real sensitivity analysis [. . .]
the R^2 values (expressing the level of agreement between the two data sets).”*

To allay the fears of the referee, it is worth emphasizing that in our opinion, the remarkable outcome of this work is that two **completely independent** (remotely sensed and hydrologically back-calculated) estimates of SWI matched so well; this is what drove us to publish these results. The question of a sensitivity analysis is indeed important for the discussion. In the revised version, we have added a Figure and a Table showing a sensitivity analysis of the two calibrated parameters that influence the discharge and the soil moisture simulations: L the soil depth and K_s the soil permeability (n_o and n_c were shown to not influence the soil moisture, as they only affect timing). This sensitivity analysis shows that the values of the parameters influence the discharge simulation: K_s mainly influences the volume of runoff, while L influences the values of the main peak of the discharge. The modelled soil moisture is also influenced by these parameters. In particular, the soil depth logically controls the variability of the mean soil moisture signal. Both L and K_s can influence the bias, but whatever the value of the parameters, the correlation between the modelled and remotely sensed soil moisture remains fair (R^2 higher than 0.6). This suggests that even if there is an uncertainty in the parameter values, the findings of the study remain the same.

No sensitivity analysis was carried out on the remotely sensed soil moisture because the soil moisture product is used in its finalized form.

About the models and data uncertainties

“To me the bias between the two estimates is far [. . .] explanations for it (and ideally give some ideas how this bias could be avoided/corrected).”

As pointed out by R#2, there are several potential sources of uncertainties in both estimates that can explain the bias. This aspect is now more critically discussed in the revised version of the paper by separating the uncertainties associated with the hydrological model (modelled processes, parameter values, calibration,8230;) and the uncertainties associated with the remote sensing of soil moisture. Overall one should note that models, remote sensing and in-situ data provide in general more reliable information about soil moisture trends than about absolute (area-representative) soil moisture values. Therefore, absolute values often do not match which is why in data assimilation studies of soil moisture, matching methods such as the cdf-matching techniques are commonly applied.

With respect to the comment that the relationship between soil moisture and backscatter is not linear, it should be noted that, indeed, well-known models such as the IEM bare soil backscatter model of Fung do not show a linear behaviour. But there is mounting evidence that the treatment of surface roughness phenomena in backscatter modelling has fundamental flaws which is why the functional behaviour of IEM also has to be very critically viewed. In fact, most empirical studies showed that a linear model gives good fit between backscatter expressed in decibels and soil moisture which is in contradiction to theoretical predictions.

Comparison of the results with similar studies realized with other sensors

“In the discussion I missed a comparison [. . .] results of your study in a more general context by citing other studies as well.”

We agree that such a comparison with other studies using different sensors/techniques

is highly interesting, but it is outside the scope of this study. Also, because such a comparison (e.g. with AMSR-E soil moisture data) is not made within this study, we do not believe a discussion would be appropriate in this paper. Such discussion can for example be found in Crow et al. (2007) or:

Wagner, W., V. Naeimi, K. Scipal, R. de Jeu, J. Martínez-Fernández (2007) Soil moisture from operational meteorological satellites, Hydrogeology Journal, 15, 121-131, doi 10.1007/s10040-006-0104-6.

Specific comments

“p. 2275 l. 4 please avoid the term "easily" because it is not that easy to calibrate the probes”

Done

“p. 2275 l. 29 Aubert et al. used field measurements of soil moisture and not remotely sensed estimates”

The reference has been removed.

“p. 2276 l. 5 not sure if I agree with 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).”

The sentence: “However, very few studies in the literature compare the estimations of soil moisture from remote sensing with the estimations from hydrological models (Biftu and Gang, 2001; Parajka et al., 2006).”

Has been slightly modified to: “However, very few studies in the literature detail a comparison between the independent estimations of soil moisture from remote sensing with those from hydrological models (Biftu and Gang, 2001; Parajka et al., 2006).”

“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?”

River discharges data were estimated by using depth/discharge rate curve. The measurement of water level in the river is accurate; however the use of the rating curve to extrapolate high flows is understood to lead to uncertainties in the measurements. As far as uncertainties specific to the flow data are concerned, we were worried about inconsistencies in the flows recorded at station 1; this point is dealt with under the response to R#2 - p.2286 l. 16, later in this section.

Work on uncertainty propagation has yet to be carried out. As a first South African (semi-arid region) application of the TOPKAPI model, it was assumed that the estimated discharges were sufficiently accurate to be used as target values in the calibration procedure. The paper aims to give the first results of the comparison between modelled and remotely sensed soil moisture, the issue of quantifying the model and data uncertainty will be done in further work.

Note however that by writing: “The quality of the flow data stations 1 and 2 has improved since 2002, but the recent flow data are not considered since the dense rain gauge network was no longer operational after the year 2002.” the authors actually meant: “The quality of the flow data at stations 1 and 2 (in terms of data availability) has improved since 2002, but the recent flow data were not used because the dense rain gauge network was no longer operational after the year 2002.” The sentence has been changed in the revised version of the manuscript.

“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.”

In this paragraph, the model is described independently of its application on the Liebenbergsvlei catchment. It is thought that that suitability of the TOPKAPI hypothesis and

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especially the absence of Hortonian processes might be the subject of debate, that's why it has been reported. However to avoid possible confusion, a note has been added at the end of the paragraph "Model assumptions" to warn the reader that the absence of modelling Hortonian processes in the case of the Liebenbergsvlei catchment will be justified later in the paper.

The following sentence has been added in the revised version of the paper:

"The absence in the TOPKAPI model of an explicit representation of infiltration-excess runoff processes (Hortonian processes) might be of concern for a semi-arid catchment like the Liebenbergsvlei. However, as discussed later (see Section 5) recent field experiments have shown that such an assumption is in fact realistic on the Liebenbergsvlei."

"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?"

In the version used in the present study, the TOKAPI model does not display a stratification of the soil. The soil layer is considered as a lumped entity where the hydraulic conductivity is dependent on the average soil moisture integrated through the entire soil layer.

"p. 2283 l. 24 what do you mean by "residual soil moisture theta-s" ?"

This is a mistake. It should read "residual soil moisture θ_r ". This has been changed in the revised version.

"p. 2284 l. 17 how did you do the calibration of the 4 model parameters? Did you use any optimisation software?"

No specific optimisation software was used; the parameter space was discretized, the objective functions plotted and manually, systematically explored. The set of parameters giving the best values of the objective functions was then retained.

“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)?”

In our opinion, the measurements made at the station 1 for the season are not reliable since some major peaks evident in station 2 are obviously missing at (the downstream) station 1 during season 1. Therefore the model was calibrated using the data of station 2 in season 1 that are reliable. These data at station 2 in season 1 were chosen for calibrating the model because there were no external flows entering the catchment from Lesotho during this season that would have influenced the calibration procedure. The flows in season 2 at both stations, although affected by the inflows from the inter-basin transfer, were judged to be reliable during the period chosen and therefore used for validation after the effects of the inflows were accounted for.

“p.2289 l. 20 two exceptions: first estimate of channel roughness was also wrong (see general comment).”

This has now been added and commented in the text.

“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.”

The sentence: “iii The remotely sensed SWI comes from a conceptual infiltration model (Wagner et al., 1999c) whose parameters seem to be quite suitable for the study area.”

has been replaced by

“iii The remotely sensed SWI comes from a conceptual infiltration model (Wagner et al., 1999c) whose parameters produce estimates consistent with what were found using a more physically based (distributed catchment modelling) approach.”

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3.2 Answer to the comments specific to R#1

General Comments

“The authors should further emphasise the benefit for using a distributed hydrological model which allows to represent the horizontal flow that tends to redistribute soil moisture, how this distribution can be compared to the one deriving from the scatterometer measurements and how these measurements can be assimilated in the model in distributed form.”

The benefit of using a hydrological model explicitly representing horizontal transfers has been emphasized in the revised version of the paper. The following sentence was added in the conclusion: “The study shows the benefit of using a distributed hydrological model that is able to explicitly represent the horizontal transfers in the soil in order to spatially redistribute modelled soil moisture.”

“Not all the results may be fully convincing (such as for instance the low values in the Nash-Sutcliffe coefficients, which should be further discussed)”

This aspect is now discussed in the last section of the revised paper.

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