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Hydrology and Earth System Sciences Discussions

Interactive comment on "Integrating coarse-scale uncertain soil moisture data into a fine-scale hydrological modelling scenario" *by* H. Vernieuwe et al.

Anonymous Referee #1

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Review of the manuscript entitled: Integrating coarse-scale uncertain soil moisture data into a fine-scale hydrological modeling scenario. The authors present an interesting approach on how to integrate coarse-grained soil moisture data obtained from radar remote sensing platforms into fine-scaled soil moisture content data obtained from a hydrological model. The paper is timely as prediction of soil moisture content at a specific scale using measurement methods with measurement windows differing from the scale of interest is becoming increasingly important. The method proposed by the authors is based on a data assimilation approach using a scaling relation between field-averaged soil moisture and its related standard deviation. The paper is

well-written and of interest to the readership of HESS. Figures and tables are clearly presented and support the manuscript very well. I have the following comments and suggestions to offer. A key assumption is based on the fact that the authors postulate that remotely sensed soil moisture are considered to be the truth. As this is not the case, the authors may want to comment on the implications of this assumption. How realistic is this assumption? Page 6032, Line 22. At this point, it is unclear for the reader what is "assimilation". It is recommended to add a sentence to shortly introduce the concept of data assimilation. Line 6 page 6033: For the reader it is not clear at this stage what possibility distributions are and what their difference is with respect to probability density functions (pfds). The authors should reorganize their text and provide a section to explain what these distributions are. It should also be made clear what is the advantage of the fuzzy-based possibility concept as compared with pdf's. Strictly speaking, the fuzzy based concept is not a stochastic concept. Why do the authors prefer to work with this concept? Page 6034, line 1-7 (and page before): Is a coarse scale computational grid (e.g., from a large scale hydrological model or RCM) in combination with remote sensing products on a finer grid also of interest for the authors? This option could also be mentioned, or is the approach specific for small scale computational grid and larger scale remote sensing product? Page 6034, Line 8-21. Such relationships are related with soil hydraulic properties and micro-topography (on the field scale) and also with rainfall variability, topography, land use and vegetation on larger scales. These properties can also be explicitly represented in the hydrological model; the soil properties also on the basis of stochastic techniques. Such an explicit representation in a model should give also the relation between mean properties and variability: why do we need to assume certain function for this relation here? Page 6034, Line 22-29. Also here it is not clear what could be the advantage of combining a classicial probabilistic concept and a possibilistic concept. Page 6035, line 21: I propose to name this section: Deriving possibility distribution of mean field soil water content rather than SAR-Based soil moisture data. Page 6036, line7: It would be useful for the reader to have a brief explanation of the IEM model at this stage. Page

6036: line 10: This should be a separate section as possibility distribution is a key concept and it should not be addressed in a section on data. Page 6036, line 10-12: The difference between probability and possibility distribution is not explained well like this. Also epistemic uncertainty can be expressed in terms of a probability distribution. It is true that a certain pdf-type is assumed then, but I would agree that the triangular function used in fuzzy-based approaches is an equivalent of a pdf, although formally the fuzzy-based possibilities do not have a strict probabilistic meaning. Page 6037, line 8: It would be useful to explain here what interval analysis is and what closed alpha-intervals are. Page 6037, line 17: What is the advantage of using the possibility approach compared to the calculation of a pdf of field averaged soil moisture using IEM in which rms and correlation length are defined as a joint pdf. Using Monte Carlo one could then generate an effective soil water content for the field derived from SAR data using IEM. Page 6037, line 20: This is the only real data part (lines 20-28) in this section. Therefore this section needs to be restructured (see title). Page 6038, line10: As you need data with a high temporal resolution, GPR is probably not the best choice. A better approach will be to use wireless sensor networks for soil moisture. Page 6038, line 17: It is not clear how a field can have the same characteristics as a catchment. What characteristics do the authors refer to? Page 6038, line 17. It is unclear how exactly the relation between mean soil moisture content and standard deviation has been calculated. Was TOPLATS applied for a 5 m x 5 m grid ? Can this model be applied at such a high resolution? As this a land-atmosphere transfer scheme, fluxes in the surface layer are assumed to be 1D with no lateral exchange between columns (this is what I assume TOPLATS does). However, applying TOPLATS on such a high resolution grid violates this basic assumption and it can be expected that the spatial variable evaporative and transpirative fluxes are not calculated correctly. This impacts then also the calculated soil moisture contents. Page 638, line 19-22. The relation between mean soil moisture and its variability could depend on soil texture, but also rainfall variability, and micro-topography. It is not clear whether the relation is not scale dependent und therefore it is not clear if findings on the catchment scale can be used

on the field scale. The authors should comment on that providing also more details on their analysis. Page 6039, line 13: It is not clear to me why the authors included this model for analysis. The data show a clear parabolic-shaped behavior whereas Eq.(3) is a monotonic decreasing function without a maximum. In my understanding this function has been used to either describe the Coefficient of Variance with respect to the mean or only the decreasing part of the observed mean moisture content-standard deviation data. Page 6040, line 17: It would be good to briefly state what the integration method will be used for. Page 6040, line 18: is the disaggregation step similar to downscaling? If so it might be better to use this terminology. Page 6041, line 5: Which method do the authors refer to? Page 6041, line 9: Unclear. Why is it important that we are dealing with closed alpha-cuts and what do the authors mean with "interval analysis"? See also comment above. Page 6042: The section 3.2 is quite long and it presents various analyses. Splitting up this section would help the reader in better understanding what is being done and presented. Basically there is the procedure which consists of a disaggregation step and an update step. Here a flow chart would be helpful. This could be one section in the ms. In addition, there are the various analysis being performed which could also make up several different parts. Page 6042, Line 25-26. Again, I am not sure whether TOPLATS can be applied at this resolution. Could the authors comment on that? Page 6042, Line 26-27. I suggest rewriting: The reference which we will refer to as “truth” Section 3.2. After this section, still some questions, already expressed before remain: What is the advantage of the possibility concept? What is the advantage of disaggregation if the model calculations already provide high-resolution soil moisture distributions, which have a physical basis, but are corrected with a general scaling relationship that is region-dependent and has clear limitations (e.g., neglecting hysteresis)? Alternatives are possible that make better use of the calculated high-resolution soil moisture distributions by the model. Nevertheless, it is an interesting approach that the authors follow. Page 6045, line 10: it would be good to present a flow chart of the integration method. It would be good to provide a short description on how the data assimilation performed? Page 6046, line 1: start a new (sub)section Page 6047, Line 21. “In order to meet this modeling issue”. I suggest reformulating. Page 6048, line 18: The comparison of the two procedures should be reflected in the presentation of the ms. Comments with respect to the conclusions: Results were presented for a synthetic study. This was acknowledged in the abstract, but not in the conclusions. I think that it is important to state this very clearly in the conclusions, and discuss how things could work in the real world. In this context, the authors should also mention that the generated synthetic SAR-data were created with a model that relates the measured brightness temperature and the reality using a model, that later is also used. If we deal with real SAR data, things are more complicated.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6031, 2011.