We thank the referees for their comments. We would like to answer their comments and explain and indicate the changes that were made to the manuscript.

Reply to referee comments

Anonymous Referee #1:

1. *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?*

We partly agree with the reviewer that we assume that the remotely sensed soil moisture is 'unbiased', however, the retrieved soil moisture is not a single accurate value that equals the 'truth', but is uncertain information represented by means of a possibility distribution that can be regarded as a set of intervals in which the field-average soil moisture is expected to lie with a given possibility level.

Furthermore, it is not known whether the remote sensing data or the modelled data cover the truth. In fact, the information of both sources can be (partially) overlapping or in total conflict, and does not give information about the true value itself. However, as our method is a first attempt to integrate possibilistic coarse-scale information with deterministic fine-scale information, an appropriate fusion technique that takes into account the amount of overlap or conflict in both types of information has not been incorporated. Furthermore, if both types of information are in total conflict, this would indicate that for each pixel, two conflicting values should be taken into account in the further calculations, which would complicate further model calculations.

In the paper, we assume that the radar-based observations are 'unbiased', *i.e.*, the actual field average soil moisture is covered by the possibility distribution of the retrieved soil moisture. Generally, data assimilation experiments also assume non-biased observations. However, if observations would be biased, a bias-correction algorithm would be needed (e.g. De Lannoy et al., 2007b). Yet, such technique would require a lot of additional research when applied in a possibilistic framework.

2. *Page 6032, Line22. 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.*

We changed this sentence as follows: "Assimilation of soil moisture observations, which boils down to objectively combining soil moisture observations with the model results at the same time step in order to produce a `best' soil moisture estimate, can improve the predictive capability of hydrological models."

3. *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?

On page 6033, line 6 a first intuitive explanation of the meaning of a possibility distribution of roughness parameters was already given ("reflecting the possible soil roughness parameters values for a particular roughness class or tillage state"). We now also added a reference to the relevant section where the reader can find more information about possibility distributions. We also added the reason why Verhoest et al. decided to work with a priori information in the soil moisture retrieval algorithm: Furthermore, it is unfeasible to measure soil roughness at each bare soil field when radar remote sensing is to be applied at the catchment or regional scale (Verhoest et al., 2007). Therefore, Verhoest et al. (2007) suggest using *a priori* roughness information based on the known tillage state of the field. For each tillage type, a possibility distribution (see Section 2) of roughness parameters, reflecting the possible values of soil roughness parameters is then used in the soil moisture retrieval algorithm." The difference between possibility and probability distributions is further explained in Section 2.1.

4. *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?*

Our method is not applicable when a coarse-scale computational grid is used in combination with small-scale remote sensing data. In this case, a method needs to be sought that combines several small-scale possibility distributions of soil moisture content to update the soil moisture value in the coarse grid pixel by a valid composition of these small-scale possibility distributions.

5. *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?*

We agree that the model could allow for calculating the relation between the field-average soil moisture (at that instant of time) and the variance at each time step, which will be a function of high-scale variabilities in soil properties and forcings. However, it would be more appropriate to use an independently derived relationship, obtained from field experiments (e.g. GPR-based), in order to circumvent that model errors or model inefficiencies are captured in the relationship. Unfortunately, as these were not available we decided to set up a synthetic case study in which a model-based scaling relationship is used which was identified separately from the twin experiment itself.

An explicit (analytical) function modeling the relationship between the mean soil moisture and corresponding variance is needed for the framework suggested as it requires that for all possible soil moisture content values of a certain possibility degree the corresponding (expected) soil moisture variability is calculated. Because of the range of average soil moisture values, one cannot rely on the model predictions at the moment of assimilation as the model only provides one field-average soil moisture content value.

6. *Page 6034, Line22-29. Also here it is not clear what could be the advantage of combining a classicial probabilistic concept and a possibilistic concept.*

The method presented in this paper combines a classical probabilistic concept and a possibilistic concept because of the fact that we have soil moisture measurements represented by possibility distributions while the within-field soil moisture variability is expressed as a probability distribution. As further explained in our answer to question 10, the type of uncertainty (epistemic) that we are dealing with does not allow working with a probabilistic framework.

7. *Page 6035, line21: I propose to name this section: Deriving possibility distribution of mean field soil water content rather than SAR-Based soil moisture data.*

The name of that section has been changed to: Possibility distributions of field-averaged soil moisture content.

8. *Page 6036, line7: It would be useful for the reader to have a brief explanation of the IEM model at this stage.*

We have added some sentences describing the IEM without introducing formulas. Additional references to papers that discuss and apply this model are added.

"Several models have been proposed to relate soil moisture to the backscatter signal, ranging from purely empirical relationships to physically-based models. In this study, the Integral Equation Model (IEM) for small and medium roughness, developed by Fung [1994, pp. 63–64], is applied. This model, which only simulates the single scattering component of the backscattering process, has already been applied successfully in several remote sensing studies [Altese et al., 1996; Alvarez-Mozos et al., 2005, 2006; Hoeben and Troch, 2000; Mancini et al., 1999]. It is only valid for surfaces with a single-scale roughness having small to moderate surface RMS heights ($ks < 2$, with k the wave number ($k = 2\pi/\lambda$ being the wavelength) and s the RMS height). The autocorrelation function is considered to be isotropic and is represented by an exponential function. Besides the roughness parameters, the model uses the dielectric constant of the soil to compute the backscattering value.

After applying the inverse IEM, the obtained dielectric constant is converted into volumetric soil moisture using the dielectric mixing model [Dobson et al., 1985]. If the latter results in soil moisture values larger than saturation, then the soil is considered to be saturated, whereas if the retrieved moisture value obtained is smaller than the residual moisture content, it is replaced by the latter value. This operation, in accordance with Verhoest et al. [2007], is performed in order to ensure that only soil moisture values are retrieved that are physically possible."

9. *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.*

We restructured Section 2 into

- Section 2.1. SAR-based soil moisture estimation
- Section 2.2. Possibility distributions
- Section 2.3. Identification of the SAR-based possibility distributions

10. *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.*

We do not agree with the referee who states that epistemic uncertainty can be expressed in terms of a probability distribution. As explained in the paper by Dubois, 2006, uncertainties can be subdivided into two classes: the uncertainties due to the variability of a quantity (e.g. the distribution of precise (measurable) roughness parameter values within a field or the surface runoff quantities within a catchment), and uncertainties about quantities which are deterministic but ill-known because they pertain in the future (e.g. the average roughness parameters of a specific field at the end of next winter or the river discharge due to the next extreme rainfall event) or because of a lack of knowledge (we don't know (e.g. immeasurable) the precise average roughness parameter of a field or the exact river discharge at the outlet of a catchment). Returning back to our problem at hand, the average rms height and correlation length of an agricultural field are deterministic values. Their uncertainty is not subject to variability, the values are what they are, yet, we lack knowledge and good measurement instruments to determine them. The little knowledge that we have on these parameters is not rich enough to allow the use of probability distributions. Possibility theory on the other hand, can handle this type of uncertain information.

11. *Page 6037, line 8: It would be useful to explain here what interval analysis is and what closed alpha-intervals are.*

Closed alpha-intervals contain all of their endpoints.

An explanation on interval analysis has been added as follows:

"Next, interval analysis or computation is applied to identify the corresponding alpha-cut of the possibility distribution of the output variable, which is the interval determined by the minimum and maximum output value obtained through application of *f* on the alpha-cuts of the input variables.

12. *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.*

Please note that we don't state that there is an 'advantage' of working with possibility distributions. We use possibility theory as this theory is capable of working with epistemic uncertainty.
Working with a probability density function of roughness parameters implies that the uncertainty we are dealing with originates from variability. We would then be able to measure the precise roughness parameters at different locations and determine a histogram/probability distribution which would then be propagated through the IEM. Yet, as the problem is that roughness parameters are difficult to measure, we do not have the knowledge to identify a joint probability density function for these parameters.

13. *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).*

We restructured Section 2 (see our answer to question 9)

14. *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.*

Depending on the application, GPR is certainly a good choice for providing representative, high-resolution soil surface moisture maps that can be compared to radar remote sensing data products, and hence, used to properly calibrate remote sensing retrieval algorithms. Although GPR time-lapse measurements may be time-consuming, time-lapse data may not be required in a hydrological context once remote sensing algorithms have been properly calibrated. GPR is not significantly affected by the sub-meter variability of soil moisture, which may be significant, and provide a characterisation depth that is similar to remote sensing radars (which varies with soil moisture). In contrast, although sensor networks are useful for providing data with a high temporal resolution (e.g. Bogena et al., 2007, 2010), they suffer from a lack of spatial representativeness (in the three dimensions and with respect to the characterisation scale) and are cumbersome to install. In addition, by their intrusive nature, sensors are expected to influence water flow in their own vicinity. Basically, GPR and sensor networks provide quite different products and depending on the application, one or the other tool may be preferred.

15. *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?*

We agree with the reviewer that this sentence is unfortunately written and has been removed from the paper.

16. *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.*

The application of TOPLATS at a 5 m resolution may indeed introduce some errors with respect to the modelling of lateral fluxes. However, this model accounts for topographically induced fluxed through the topindex. In our modelling exercise, spatial variable evaporative and transpirative fluxes that induce significantly different soil moisture values within the field are not expected as (1) only a fairly small field is considered, and (2) continuous bare soil conditions are assumed.
We are not aware of other studies applying TOPLATS at a similar high spatial resolution. However, CLM2.0, an alternative model, with similar concepts as TOPLATS, has been applied successfully at high spatial resolutions (*cfr.* De Lannoy et al., 2007a,b). Compared to CLM2.0, TOPLATS accounts for topography through the topindex. Given the fact that in our experiment changes in soil moisture are mainly induced by topography and not by other processes (such as soil moisture extraction through vegetation), we believe that TOPLATS is suitable for the experimental setup used in the paper.

17. *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.*

The relationship derived is completely based on the model results of the field studied. This relationship may be scale dependent (as shown with real data in Famiglietti et al. (2008)) and catchment characteristics may indeed influence the relationship. However, this is out of the scope of the paper, as a theoretical experiment is performed which applies a relationship that has been derived for the same boundary conditions. If other boundary conditions would apply, or when a rescaling between other scales has to be performed, a new relationship between average soil moisture and spatial variance should be established.

18. *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.

Equation (3):

has been introduced by Famiglietti et al, 2008 to relate the mean soil moisture content with its standard deviation and is not a monotonic decreasing function (see Figure (1) for K1= 0.5942 and K2=2.9230, and examples in the paper of Famiglietti et al., 2008).

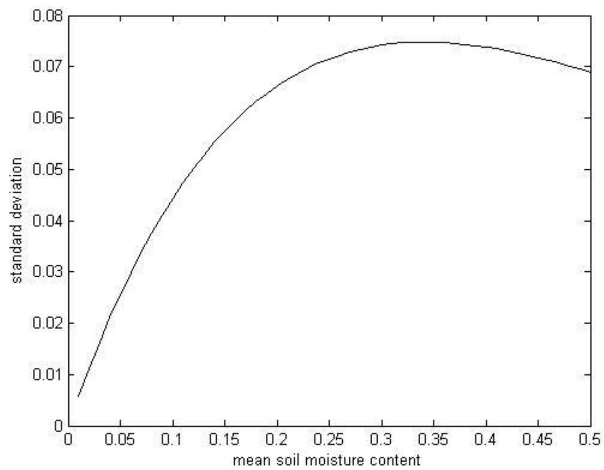

Figure 1: Plot of Equation (3) with K1=0.5942 and K2=2.9230

The decreasing function described in that paper, however, is the one that relates the field-averaged soil moisture content with its coefficient of variation (CV) (see Figure(2) for a plot of this function with K2=0.5942 and K2 = 2.9230):

(1)

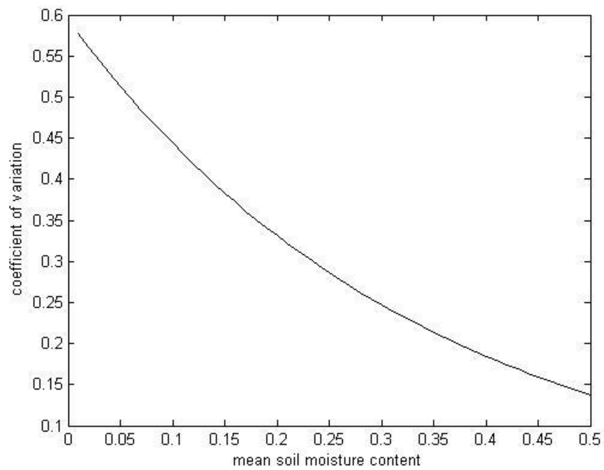

Figure 2: Plot of Equation (1) with K1=0.5942 and K2=2.9230

19. *Page 6040, line 17: It would be good to briefly state what the integration method will be used for.*

A short introduction to the method has been added. "The integration method integrates the coarse-scale SAR measurements into the fine-scale distributed model and is subdivided into two steps. The first step is called the disaggregation step and disaggregates the coarse-scale possibility distribution of field-averaged soil moisture content into a bundle of cumulative normal distribution functions. The second step, the update step, first establishes the empirical distribution function of soil moisture content values, uses the information of the bundle to update this distribution and updates the modelled soil moisture content values."

20. *Page 6040, line 18: is the disaggregation step similar to downscaling? If so it might be better to use this terminology.*

The disaggregation step is not a downscaling of the coarse-scale soil moisture data. It disaggregates, it breaks down, the possibility distribution of field-averaged soil moisture content values into a bundle of cumulative normal distribution functions, each having their own possibility degree. There is no assignment of soil moisture content values to the finer grid yet.

21. *Page 6041, line 5: Which method do the authors refer to?*

We changed "this method" to "this disaggregation step"

22. *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.*

Closed alpha-cuts are necessary in order to be able to calculate a minimum and maximum of the output interval. When the input intervals are open intervals, so is the output interval and a minimum and maximum can hence not be calculated.

23. *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.*

We split up section 3.2. into three subsections:

- 3.2.1 Twin experiment set up
- 3.2.2 Update step using possibility degrees in the bundle
- 3.2.3 Update step using central cumulative distribution function

However, we don't see how a flowchart could help here as the integration method only consists of two steps: first, a disaggregation of the possibility distribution using the scaling relationship and second, an update of the modelled soil moisture content values using the bundle (using all information in the bundle or only the central cumulative distribution function).

24. *Page 6042, Line 25-26. Again, I am not sure whether TOPLATS can be applied at this resolution. Could the authors comment on that?*

See our answer to question 16.

25. *Page 6042, Line 26-27. I suggest rewriting: The reference which we will refer to as "truth"*

We changed this to: The reference, which will be referred to as the truth. (We kept the passive writing style of the manuscript).

26. *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.*

We hope that we have satisfactorily answered the question w.r.t. the usage of possibility distributions previously (see our answers to questions 3, 6 and 10).
In general the purpose of data-assimilation is to update the model state (albeit in a distributed fine-scale, coarse-scale, conceptual,… model) whenever measurements become available, just because of the fact that model forecasts are never perfect. Since it is not

unlikely that, as in the setup of this paper, a difference in scale between the observations (coarse scale) and the model (fine scale) exists, the disaggregation step is needed to take this together with the uncertainty types, into account. To this end, an independent scaling relationship could best be used. However, we do agree that this scaling relationship is not optimal. Although it has been determined for the field considered, it is a synthetically-based relationship because of the scarcity of independent measurements and it neglects factors such as the soil moisture history and hysteresis. These limitations of the relationship could be overcome by, instead of fitting a simple function, determining a model that takes into account a time window of forcing variables.

27. *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?*

As the integration method just consists of two steps (a disaggregation step, explained in algorithm 1, and an update step), we don't see how a flowchart could improve the understandability of this paper.

In order to, hopefully, clarify the data assimilation performed the following line was changed:

In this way, a new empirical cdf is obtained (the red cdf in Figure 8) according to which the modelled soil moisture content values are updated such that the former wettest (driest) pixels receive the new wettest (driest) soil moisture content values (similar to pdf-matching).

28. *Page 6046, line 1: start anew (sub)section*

A new subsection has been added: 3.2.3 Update step using central cumulative distribution function

29. *Page 6047, Line 21. "In order to meet this modeling issue". I suggest reformulating.*

This has been changed to: Therefore, a method has been introduced

30. *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.*

In the abstract the following lines have been added: "The method is subdivided in two steps. The first step, the disaggregation step employs a scaling relationship between field-averaged soil moisture content data and its corresponding standard deviation. In the second step, the soil moisture content values are updated using two alternative methods."

In the conclusions we changed: … a unique scaling relationship was fitted to soil moisture data to: … a unique scaling relationship was fitted to **synthetically obtained** soil moisture data

We furthermore added in the last paragraph of the Conclusions:
"This relationship was furthermore identified on the basis of a synthetically generated data set. It would be more appropriate to use an independently derived relationship, obtained from field experiments (e.g. GPR-based), in order to circumvent that model-errors or model-inefficiencies are captured in the relationship."

In a real-world situation, the same methodology as presented in the manuscript can be used. Instead of a modelled backscatter value (taken from the truth, which we know in the case study), one would have a SAR measurement of backscatter, which will be converted into a possibility distribution of soil moisture content using the joint possibility distribution of rms height and correlation length. However, the true soil moisture content would not be known. The reviewer comments on the usage of the brightness temperature, however, in contrast to radiometry, the brightness temperature is not used in SAR remote sensing.
We added: "In a real-world situation, however, field-averaged backscatter values would be obtained from the SAR and converted into a possibility distribution of soil moisture content values."

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

Dr. Teuling (Referee)

31. *The manuscript by Vernieuwe et al. discusses the integration of course scale surface soil moisture data with a high resolution land surface model. It is timely, clearly written and illustrated, and deals with one of the key challenges in hydrological modeling and data assimilation, namely the integration of different (and uncertain) products at different scales. This makes the manuscript potentially of interest also to hydrologists not directly working on soil moisture.*

We thank dr Teuling for his encouraging comments.

32. **General comments**
My main criticism concerns the use of a single scaling relationship between the field scale mean soil moisture content and its variability. The authors make extensive use of a suite of analysis tools (to such an extent that it even becomes hard sometimes to follow the main line of argumentation), implicitly suggesting that the analysis is completely objective. However, every chain of arguments is as strong as its weakest link, and I believe the weakest link in this case is formed by the first link, namely in the generation of the model runs used to derive the scaling relationship between field scale mean soil

moisture content and its variability. As also pointed out by the authors based on a literature review of studies on real-world soil moisture variability and model simulations of this variability, the scaling relationship is non-unique and subject to hysteresis. It should be stressed that this hysteresis is a fundamental result of the model structure, and does not result from noise. In addition, in the real world the spatial variability of soil moisture is not only simply governed by topography, but by a complex interaction between past weather conditions, topography, vegetation patterns, and soil characteristics that is not, or at best only partly, accounted for in the model simulations since only topography was varied. The effective result is that there is no relationship between mean soil moisture and its variability, but only upper and lower envelopes. The actual value moves between these envelopes set mostly by soil properties (see e.g. Salvucci, 1998) mainly as a function of past climate (see e.g. Teuling et al., 2007). Thus, any function could be generated between these envelopes based on the arbitrary choice of climate conditions during the model run. This will not only impact the variability, but most likely also the spatial pattern. While the authors discuss the limitations of the method at the end of the paper, in particular also the use of a single scaling relationship and the fact that soil moisture patterns are dynamical, it would make the paper stronger if ideas are presented and discussed on how, at least potentially, to work around this fundamental problem. In summary, the authors should either acknowledge that the scaling relationship has been chosen somewhat arbitrary based on selected climate conditions (which also limits the applicability to this climate range-while data assimilation is mostly relevant during extreme conditions where model uncertainty is largest), or provide better arguments why their approach is physically justified.

We agree that the generation of the model runs in order to derive the scaling relationship between field scale mean soil moisture content and its variability is a weak link. The best option would be to measure soil moisture continuously (e.g. through sensor networks) or with a high temporal frequency (e.g. through a multitude of GPR measurements) such that the full range of soil moisture conditions is covered. Unfortunately, we did not possess such dataset and therefore, we had to search for an alternative and acceptable solution. Searching for such solution can furthermore be motivated by the fact that, if our technique would be used in other applications, the necessary data from field observations are most likely not going to be available either. Modeling the relationship then is a valid alternative.

Similar to the other reviewer, dr. Teuling also comments on the fact that hysteresis plays an important role and furthermore, that the spatial structure of soil moisture is governed by past weather conditions, topography, vegetation patterns, and soil characteristics. We definitely agree with this, but still want to relax this slightly: since we only considered one field with fairly homogeneous soil characteristics which was continuously bare soil, the structure of the soil moisture patterns is mainly driven by topography and past weather conditions (which also can be considered as spatially homogeneous for the field under condition). In order to overcome these limitations of the relationship, one could consider to determine a model that tries to predict the scaling relationship, taking into account a time window of forcing variables. However, to the best of our knowledge, such research has not been performed yet. If it would be available, then the function used in our paper can be changed with the one derived from such model.

In the paper we added some additional text that tries to highlight the shortcoming:
"Ivanov et al. (2010) hypothesize the existence of an attractor in the phase space of the hydrological system, explaining the existence of hysteresis in this relationship. As interactions between past weather conditions, topography, vegetation patterns and soil characteristics actually govern the spatial structure of soil moisture, one can argue that no unique relationship exists between mean soil moisture and its variability, but rather that the relationship moves between an upper and lower envelope set by mostly by soil properties (Salvucci, 1998) mainly as a function of past climate (Teuling et al., 2007). However, modeling this behaviour is not straightforward. Still, in the remainder of this paper, as to simplify the method presented hereafter, we decided to ignore the different factors and processes underlying the non-uniqueness of this scaling relationship and to introduce a unique relationship which is fitted to the data (Fig. 3). Of course, if a model would be available that describes the scaling relationship as a function of past weather conditions, topography, vegetation patterns and soil characteristics, one could use it instead of the simplified unique relationship applied in this paper."

In the conclusions we stressed this once more:

"An essential part of the methodology presented in this paper concerns the scaling relationship used. In this paper, a simplified unique relationship is fitted to modelled results, neglecting hysteresis issues and the impact of climate variables, topography, vegetation and soil characteristics on the soil moisture pattern. However, if the dependence of the scaling relationship on hysteresis and external variables could be modelled, then one could use this modelled relationship rather than the simplified relationship suggested in this paper."

Specific comments
Title: "Course-scale soil moisture data" suggest spatial scales of a SMOS-pixel and not a scale orders of magnitudes smaller. It would be better to be more specific about the scale of application. I would suggest to use "Field-scale" rather than "course-scale", since this is the scale of interest in the study.

We thank the reviewer for his suggestion, however we prefer not to change the title of the manuscript. In the example used to demonstrate our technique, we indeed restricted ourselves to the field scale. However, the methodology remains valid if remote sensing observations are available from a scale that is coarser then the scale used to model. Changing the title may lead to a confusion as if this technique would only be applicable at the field scale.

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