Hydrol. Earth Syst. Sci. Discuss., 11, C1073–C1086, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C1073/2014/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Characterizing coarse-resolution watershed soil moisture heterogeneity using fine-scale simulations" *by* W. J. Riley and C. Shen

W. J. Riley and C. Shen

wjriley@lbl.gov

Received and published: 23 April 2014

We thank both reviewers for their time and effort to review our paper. Below we address each of the reviewer concerns separately, with their text in italics and our responses in normal font.

Reviewer 1

This is an interesting study that attempts to use a distributed hydrological model to explore the relationships between the mean soil moisture state in a coarse resolution model and the higher moments of soil moisture obtained from a finer resolution model.

This is a very nice idea and a fruitful avenue to pursue.

We thank the reviewer for the positive comments on our manuscript.

It would be useful for the authors to present details on the model setup, forcing, parameterization, initialization and calibration and validation with respect to the observations for the fine resolution (220 m) case as applied to the Clinton River Watershed as this will help explain the simulations and their performance prior to the analysis. This is currently a major limitation of the study.

The text in the original manuscript was brief mainly because these details are available in (Shen et al., 2013a) and therefore we want to avoid repetition. However, to address the reviewer's comment, we have now added text to Section 2.2 to expand on these details:

"As described in (Shen et al., 2013a), to create a PAWS+CLM model for the Clinton River watershed, daily weather data were obtained from the National Climatic Data Center [NCDC, 2010]. We obtained 30 m resolution National Elevation Dataset (NED) to generate average cell elevation and lowland storage bottom elevation. The 30 m resolution IFMAP 2001 land use and land cover data [MDNR, 2010] were aggregated to provide land use information. Three dominant land use types (PFTs) were modeled in each horizontal cell. The soil color data is extracted from the global dataset [GSDT, 2000]. We obtained the spatial distribution of lateral conductivities of the unconfined aquifer (glacial drift) by interpolating well records from the WELLOGIC database [GWIM, 2006; Oztan, 2011; Simard, 2007] using Kriging. The bedrock has very low permeability as it is composed of shale and some limestone. The model was calibrated using USGS gaging station, 04165500 (Clinton River at Mt. Clemens) using a parallel version of the differential evolution algorithm".

This reviewer is concerned with the overuse of non peer-reviewed presentations at conferences or submitted manuscripts as reference sources (Maxwell et al. 2012, Niu et al. 2013, Niu and Phanikumar, 2012, Niu et al. 2011, Shen et al. 2013a, Shen et *al 2013b) in particular since these involve the model being applied here. These are suggested to be removed or more published sources used.*

As recommended by the reviewer, we have removed citations to non-peer reviewed articles.

It would be useful to sharpen the focus of the study. The use of the surrogate models is not deemed by this reviewer as an important contribution, while the exploration of the underlying physical controls on the relation between soil moisture moments is (i.e. explanations related to the inundation of riparian areas, linkage to the mean ET and elevation gradient). Expanding this part (instead of suggesting it as future work) would make this manuscript a worthy contribution that will be cited well (after demonstrating the model performs well).

We appreciate the reviewer's comment in this regard, and agree that more work needs to be done on explaining underlying controls between mean and higher-order moments. However, the development, testing, and application of ROMs is also an important result of our work and we wish to demonstrate its potential usefulness for application in models. Our conclusion that the relationship between moisture variance and mean is controlled by the combination of gradient and ET is a first step in what we envision will be future work generating generalizable relationships across many watersheds.

Page 1969, Line 4. The authors should consider the work of Vivoni et al. (2010, WRR) as a better citation for the surface energy budget, see citation below.

Done, as suggested.

Page 1969, Line 9. The work of Wood et al. (2011) advocated modeling on the order of 100 m, not 10 m2.

Fixed, as suggested.

Page 1973, Line 2. The work of Lawrence and Hornberger (2007) would be useful to

add to this discussion since it touches on what the controls of soil moisture variability could be under different mean states.

Added, as suggested.

Page 1973, Line 10-16. While the previous literature review is useful, it does not seem to be well linked to the downscaling hypothesis introduced here. The first sentence here is exactly what the literature review addresses and has been shown previously to be case. What is the novel hypothesis here? Clearly it is this downscaling hypothesis, but we do not know what it is in sufficient detail to tie it back to the literature review. Please define or explain the downscaling hypothesis in relation to the prior work. Why is a model needed to test this hypothesis?

We have changed the first sentence of this paragraph to reflect one of our goals, which is to 'build on previous studies' to develop a downscaling method for watershed-scale models. The value of using a model, compared to observations alone, to test this hypothesis is that we can have continuous and spatially explicit estimates of states and fluxes, and since we know the mechanisms included in the model, we can attribute patterns to individual processes. We have added a comment to this effect in this section.

Page 1974, Line 4. Why is the Clinton River Watershed a good place to test the hypothesis introduced above? An explanation would be useful.

We have added text to the Methods section describing the value of using this watershed for our study:

"This watershed is well suited for our study because of its varied topography and subsurface properties, heterogeneity of surface and subsurface lateral exchanges, and heterogeneity in vegetation. The basin has rugged hills on the highlands of the west and flat, low-lying plains toward the east."

To be clearer, we also added this sentence: "This contrast in topography, as shown later, impacts large-scale groundwater flow and the differences between hilly and flat terrain soil moisture dynamics."

Page 1975, Line 11-18. This material is distracting from the main topic of the manuscript.

These sentences are in the paper because we wanted to indicate that the model has been applied in several watersheds with some success, and would like to leave these citations in the paper for that reason.

Page 1975, Line 25. Is this really the first attempt? How about the literature cited (Li and Rodell, 2013, Manfreda et al. 2007, etc)?

We meant "our" first attempt, and have changed this sentence to clarify that distinction.

Page 1976, Line 2. Please add Figure 10d from Shen et al. 2013c to Fig 1 so the reader can directly compare differences.

Since we already have so many figures with so many subpanels, we choose to leave this figure out, particularly since it has been published recently.

Page 1976, Line 8. An explanation of why the 'fine-resolution' value of 220 m was selected would be useful here or previously. It should be noted that 220 m grid cells would considered coarse relative to the available elevation and land use data (30 m) and a description of the aggregation from 30 m to 220 would be useful. Further, 220 m would be coarse relative to the approach advocated by Wood et al. (2011) cited earlier in the manuscript.

We agree that hyper-resolution simulations should be further pursued, but for this paper we needed to consider a balance between resolution and computational resources. To address the reviewer's concern, we added this sentence to the paper: "Although this is still coarser than the hyper-resolution called for in [Wood 2011] and proof-of-concept work in Kollet et al. [2011], it provides substantial resolution of topographic and landuse variation across a horizontal 256×280 grid.".

In addition, a description of spatial aggregation is provided in the new sentence we added: "We obtained 30 m resolution National Elevation Dataset (NED) to generate average cell elevation and lowland storage bottom elevation. The 30 m resolution IFMAP 2001 land use land cover data [MDNR, 2010] was aggregated to provide land use information."

Finally, we added a sentence to Section 4 indicating that hyper-resolution simulations are the next reasonable steps in investigations of spatial structure and scaling properties.

Page 1977, Line 2. This is a limitation of the work in that only a small portion (the wet end) of the relation between spatial variability and mean state will be explored and its related to the humid climate of the site.

As we discuss, this portion of the mean moisture range dominates (>80 percent) the coarse-resolution gridcells. While that may not be true for all watersheds, it justifies our focus on it here. We added text to clarify this argument in Section 2.4.

Page 1977, Line 14-25. This material is not relevant to this study. Please focus on the comparison of the 220 m resolution model run with respect to the available observations in the Clinton watershed as this serves as the basis for the soil moisture datasets to be analyzed. Please show a subset of the available model-observations for the period of interest at 220 m, including streamflow, MODIS ET and water table depths.

This material is relevant to this study in that it demonstrates that the model is able to realistically predict many of the hydrologically important responses relevant to soil moisture dynamics. Since these comparisons have all been presented in previous publications, we will not present figures here.

Page 1978, Line 10. Since only the non-frozen conditions will be used in this study, the authors could likely exclude the discussion of the frozen soil effects and model-data *mismatch. Please discuss how this site-scale simulation was setup and parameterized and the type, number and arrangement of soil moisture sensors used. How do the authors account for the scale mismatch between the 220 m pixel and the site sensors, if at all? What performance metrics are revealed by the comparison for the non-frozen period? It appears that the mean soil moisture state is captured well but not the temporal variability or the recession characteristics. The authors should comment on this and its impact on the reliability of the model for the purposes of this study.*

The site simulation was taken directly from the simulations we used here, just extracted from the corresponding cell, and is shown to demonstrate that the model captures the basic dynamic soil moisture hydrological response for that site. It is still interesting to see the frozen dynamics being simulated well in one year and not so well in another year. It is indeed true that soil moisture can vary substantially in a short distance and the 200 m-cell-average value is not expected to fully agree with an in-situ moisture probe. Even if the model can be run at meter-scale resolution, our knowledge of the subsurface properties is simply not good enough to allow us to predict meter-scale dynamics perfectly.

This comparison does show imperfect model performance (as is the case of all models). However, we contend this level of inaccuracy is not going to have significant implications on the reliability of our conclusions. To address this reviewer concern, we added this sentence to the paper: "These mismatches may be attributed to differences between grid average moisture of a 220 m cell and the site-specific moisture measured by the probe or local variation and uncertainty in subsurface properties."

Page 1978, Line 20. Is temporal aggregation performed from the simulations up to the daily scale? Or is the model a daily model? Would temporal aggregation affect the estimation of the soil moisture moments?

The model is run at an hourly time step, but results were aggregated to a daily time step for our analyses here. This is described in the Methods section. We expect

that temporally resolving moisture at an hourly time step would not change the basic patterns discussed here, but do not demonstrate that in this paper.

Page 1979, Line 17. It is interesting that the authors related the appearance of the convex-upward shape to a terrain properties - drainage density. Can they indicate what the physical linkages between these could be? Later, a nice example is provided on the flood wave inundation along riparian zones. Are these two issues related? I find this interesting and novel and it would be useful to explore in more detail.

We believe the large topographic variation in this region is responsible for these features, and have added the following sentences to Section 3.3 to clarify this point:

"Higher drainage density corresponds to larger topographic variation, and this region connects upland hills and lowland plains and is characterized by a sharp decline in elevation. As a result it is also a transition zone over which the distance to the water table decreases strongly. Therefore the 7040 m cells in these regions all included large variations in soil moisture, and they shift from high to low water table regimes seasonally."

Page 1980, Line 6-14. This discussion seems to be misplaced.

We removed this paragraph, as suggested.

Page 1980, Line 21. Which observations are referred to in 'i.e. a smaller range in than in the observations'? There do not seem to be observations of soil moisture (other than the 1 station) in this study. The authors might be referring to the difference between the polynomial fit and the model-based estimate, but the latter is not an observation.

We refer to the observations in Famiglietti et al. (2008), and modified this sentence to clarify this point.

Page 1981, Line 21. Which observations? Do you mean Famgilietti et al. (2008) or these model-based estimates?

Yes, we are referring to the Famiglietti observations, and have modified this sentence to clarify this point.

Page 1982, Line 24. This reviewer is not clear as to what Fig. 8 is showing. What are the bins supposed to represent? Are these bins of fine resolution pixels within each coarse resolution pixels? One would expect the dry bin (1) to always occupy the low mu-theta relative to other bins as they would have low counts for high mu-theta. That does not seem to be always the case. A fuller explanation would be useful.

Yes, they are bins (i.e., proportions) of fine-resolution pixels within each coarse-resolution gridcells. The distributions are sometimes counter-intuitive in the way the reviewer indicates, and as we discuss in the text.

Page 1983, Line 1-8. Given that this reviewer did not understand the figure, it was not possible to follow this discussion or the parts not shown in the figure.

We added text to this section to try and clarify the patterns represented in Figure 8.

Page 1983, Line 17-18. What evidence is there for the role of porosity and flat terrain on controlling this behavior? Why is this referred to as 'criticality'?

We are referring to the saturation limit in this sentence, and the word 'criticality' is meant to indicate the rapid change observed. We have re-written this sentence to remove that word and clarify the concept.

Page 1983, Line 18-19. This belongs in the future works section.

As suggested, we have moved this idea to the Future Work section.

Page 1983-1984, Lines 21-8. This paragraph is not really needed, nor is Figure A2. This is introductory material.

We prefer to leave this paragraph in as a point for discussion.

Page 1984, Line 9-14. This is repetitive material.

This sentence refers to the fraction of gridcells with linear relationships between moisture mean and variance after the peak in this relationship, and is therefore not repetitive.

Page 1985, Line 2-5. It could be argued that the complex model used here actually helps to highlight the important controls by explicitly accounting for all the factors involved, as opposed to remote sensing observations where the controls may not be directly relatable to underlying physical properties of the system.

We removed this sentence as suggested.

Page 1985, Line 7-8. This belongs in the future works section.

We have moved this idea to the Future Works section, as suggested.

Page 1985, Line 17. A more effective method to show Fig. 9 is through scatterplots and 1:1 lines in each coarse resolution pixel with goodness of fit measures. The same comment holds for Figure 10 and A3.

We show the transient results, and report the R2 value for the scatterplot, to illustrate the dynamic nature of the moments. In this way both metrics are represented on the same figure.

Page 1986, Line 23. What is the link to greenhouse gas budgets and this study?

CO_2, N_2O, and CH_4 emissions from terrestrial systems all depend on soil moisture. We have added a phrase to clarify this point.

Page 1986, Line 24. The model was not convincingly tested in this study at the resolution of interest (220 m).

As we indicated in Section 2.2, the model has been tested in this watershed in several studies, although we acknowledge in the manuscript a paucity of soil moisture measurements available for comparison.

Page 1987, Line 1. The analysis here should have revealed hysteresis, if it occurred, for example in Fig. 5. It apparently does not occur and the surrogate approaches would not be able to capture them, if they occurred. Note that Vivoni et al (2010) also found hysteresis between mean and variance of soil moisture.

We have amended this sentence to reflect this idea, and added the Vivoni et al. (2010) citation.

Page 1987, Line 3-12. These discussion points are somewhat obvious and need not be stated.

We prefer to leave them as they summarize necessary next steps.

Page 1987, Line 13. This is a good place to describe the limitation of not modeling at 30 m resolution given that the topographic and land cover data are available at this higher resolution.

Modeling this basin at 30 m is quite computationally challenging – the computational burden and memory load would increase by a factor of more than 50 over our finest grid (220 m). This is currently impractical but our on-going efforts and new work on computational infrastructure are trying to address this issue. In fact, besides some proof-of-concept work or applications in very small basins (<10 km2), we have not seen modeling work at 30 m resolution for watersheds >1000 km2. Many operational models for large-scale simulations are trying to achieve 1 km resolutionâĂŤso called hyperresolution [Wood 2011]. Also, as indicated above, many subsurface properties (soil and groundwater aquifers) are not available at this resolution (with reasonable accuracy).

Page 1987, Line 18-26. This portion is not well supported by the study and might be too premature to discuss in a publication.

We wish to include this text, because it is appropriate for the Future Work discussion and we only indicate that we believe our approach would be useful in this regard.

Page 1988, Line 12-16. What is learned from this exercise? The surrogate models can only be developed by running the full simulation (220 m) within each coarse resolution area (7040 m). They are model-specific (i.e. tuned to the physical processes in this model and the current setup) for the specific catchment region over non-frozen periods. What is their utility once derived? There is clearly no universal fit.

This close correspondence between the surrogate and fine-resolution model predictions argues that these types of reduced order models can be used to inform heterogeneity at scales below those explicitly represented at coarse resolution. It also argues that the surrogates can be effectively applied to understand controls on spatial heterogeneity of soil moisture (e.g., relationships between variance and mean). We have added two sentences to the Summary and Conclusions section to clarify these points.

Page 1969, Line 14. Please use the acronym as tRIBS.

Fixed, as suggested.

Page 1970. Line 21. The word mean or average is required to describe the term mu-theta.

Fixed, as suggested.

Page 1973, Line 10. Formally, the term mu-theta does not have higher order moments, it is theta that has higher order moments. Some clarification is needed here.

Fixed, as suggested.

Page 1974, Line 1. ROM has not been defined yet.

Fixed, as suggested.

Page 1975. Line 2. Please define PDEs.

Fixed, as suggested.

Page 1978. Line 16. Can you mention which year (4 through 8) is linked to 2003?

That year should be 2006. We have corrected the text.

Page 1978. Line 24. Usually, figures need to be introduced in the order of the numbering.

We removed the references to the figures from this sentence, as it was not needed. The figures are now mentioned in the text in the order they are numbered.

Page 1980. Line 19. The equation shown is not a simple exponential, suggest to remove the term exponential.

We have clarified this point: it is an 'exponential function', which matches the wording from the original Famiglietti et al. (2008) paper.

Reviewer 2

Some sentences are too long, such as "We applied a watershed scale hydrological model (PAWS+CLM) that has been previously tested in several watersheds and developed simple, relatively accurate (R2 of 0.7–0.8) reduced order models for the relationship between mean and higher-order moments of near-surface soil moisture during the nonfrozen periods over five years." It is not easy to understand.

We changed this sentence by dividing it into two sentences.

P1968, L2, "than" is redundant.

"than" is appropriate for this sentence.

P1968, L3, two "and" is used, which makes confusion.

We corrected this confusion, as suggested.

P1984, L5, "were stressed" may be "were unstressed".

We clarified this sentence, as suggested.

The figures can be reorganized and make the topic focus on the relations between

$\mu\theta$ and $\sigma\theta^2, s\theta$, and kθ. Therefore, Fig.2, Fig.3, Fig.8, Fig.A2 are redundant and the related discussion can be rewrote. Fig. A1 and Fig. A3 should be kept.

We are comfortable with the focus given to those relationships under the current paper structure.

Is "C1+C2gET" used to surrogate the relation between $\mu\theta$ and variance? How about the relations between $\mu\theta$ and sθ and kθ? And are C1 and C2 consistent or different for different gridcells?

We focused on the first relationship in this paper. The relationship of the slope between $\mu\theta$ and variance was established using the fact that C1 and C2 vary by coarse-resolution gridcells, as is discussed in the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 1967, 2014.