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

**Anonymous Referee #1**

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General Comments

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.

2. 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

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as this will help explain the simulations and their performance prior to the analysis. This is currently a major limitation of the study.

3. 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.

4. 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).

Specific Comments

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.

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

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.

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?

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

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

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

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

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.

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.

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.

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.

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?

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.

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

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.

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

Page 1982, Line 24. This reviewer is not clear as to the 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.

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.

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'?

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

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

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

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.

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

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.

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

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

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 occured. Note that Vivoni et al (2010) also found hysteresis between mean and variance of soil moisture.

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

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.

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

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.

Technical Comments

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

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

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.

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

Page 1975. Line 2. Please define PDEs.

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

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

ing.

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

Citations;

Vivoni, E.R., Rodriguez, J.C. and Watts, C.J. 2010. On the Spatiotemporal Variability of Soil Moisture and Evapotranspiration in a Mountainous Basin within the North American Monsoon Region.Water Resources Research. 46: W02509, doi:10.1029/2009WR008240.

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

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