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Hydrology and Earth System Sciences Discussions

Interactive comment on "McMaster Mesonet soil moisture dataset: description and spatio-temporal variability analysis" *by* K. C. Kornelsen and P. Coulibaly

Anonymous Referee #2

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SUMMARY

This discussions manuscript presents a description of the McMaster Mesonet soil moisture dataset, along with a preliminary statistical analysis of the measured spatial and temporal variability in soil-water contents. The dataset consists of four large, densely-instrumented hillslopes at three locations within the Hamilton-Halton watershed in southern Ontario, Canada. Hourly measurements of soil-water content at regular depth intervals for each plot have been collected since 2006 and will continue well into the future. The high temporal resolution and long duration of monitoring at the

different sites allows the authors to present some interesting analysis of soil moisture patterns and dynamics at the hillslope scale, as well as some synthesis across the different sites. Their analysis includes an assessment of how seasonal variability in climate, vegetation density, and also individual rainfall events impact soil moisture variability. Overall the manuscript will be of interest to readers of HESS, since it presents work that is of very good scientific significance, good scientific quality, and fairly good presentation quality.

There are several broad issues I have with the manuscript in its present form, which should be addressed before final publication. In particular, the soil moisture dataset is described as high spatial and temporal resolution, but in my assessment it would be more accurately characterized and analyzed as three very densely instrumented plots / hillslopes with long-term, high-frequency measurements. I am still convinced that this is an incredibly valuable dataset, but presentation, analysis, and comparisons with previous work need to be discussed within the limitations and advantages of the dataset's spatial extent. Additionally, I was disappointed that the dataset is not actually accessible through the link provided in the manuscript. There are a few other areas where the manuscript should be revised, some figures could be greatly improved, and a number of minor technical corrections are needed. Therefore my recommendation is to return the discussions manuscript to the authors for moderate revisions to the text and figures.

GENERAL COMMENTS

The first issue I found with the manuscript is that the authors characterize the dataset as high resolution, which is somewhat misleading. While the hourly measurements and long-term monitoring at the three sites are indeed quite impressive, the spatial coverage is essentially just three densely instrumented hillslopes covering roughly 3000 square meters each. This type of hillslope / small catchment scale measurement network is not extraordinarily novel (see Torres et al., WRR, 1998; Western and Grayson, WRR, 1998; Tromp van Meerveld and McDonnell, WRR, 2006; Salve et al., WRR,

2011, etc.). What sets the present work apart from previously published datasets is the duration of continuous, distributed measurements at a number of different sites within a large watershed. The authors should make an effort to characterize the true nature and limitations of their dataset, but I would also encourage them to further highlight the utility of such a dataset for greater synthesis than many previous datasets allow.

On a related note, it is a major stretch to say that the studied watershed is representative of "Canadian Climate" and rather inappropriate to emphasize this as a novel scientific aspect of the work. The field site in southern Ontario is literally surrounded on three sides by the USA and has a fairly mild climate compared to most of Canada's geographic regions, so the political boarder is a flimsy argument in this particular case. From a scientific standpoint it is certainly permissible to interpret results geographically; the field site could be listed as representative of the Great Lakes Region (note: how the area is characterized by Environment Canada).

The biggest concern I have with the manuscript is the lack of a systematic discussion of scaling issues. The introduction jumps around reviewing different studies conducted at very different scales, from in-situ experiments in small catchments to vast remotely sensed datasets, but only briefly mentions the importance of scale. Similarly, the lengthy discussion at the end of the manuscript includes a comparison of the results from the statistical analysis to other studies without considering why the scale of measurements could be a source of some of the differences. Similarly, the implications of the averaging used in the statistical analysis are also scale related, but this is not discussed at all. The manuscript would benefit from a brief description of the range of scales at which soil moisture can be measured, and then provide some context for why densely monitored hillslopes such as this one could be useful for examining scaling issues.

Another major problem with the manuscript is that it does not provide a description or analysis of how snowfall and snowmelt are treated in the dataset or analysis. The

authors do list the freezing temperatures and snowfall as an important novel aspect of this work, but barely touch on the subject. Assuming the dataset will include the meteorological data described in the manuscript, how do the authors determine when precipitation is falling as snow versus rain? Is snowfall measured directly and how is it converted into Snow Water Equivalent? From looking at Figure 2 it is not at all obvious how snow is treated as part of the precipitation record though clearly snowmelt (i.e., temperature) plays a critical role in the soil moisture dynamics.

The manuscript was submitted as a research article, but the conclusions from the preliminary analysis of the data are not particularly substantial. It is therefore clear that the primary contribution of this article is the dataset itself, which is great. However, it is important that several missing pieces of information / deficiencies are addressed in the revision.

1. I found it strange and disappointing that the web address does not link to the dataset, but rather to the research group's home page.

2. I was shocked to find no mention of any qualitative or quantitative description of the soil profiles at the various sites. There were 36 soil pits each going down to around 100 cm, so surely the authors were able to identify some significant soil horizons, textural contrasts, or rooting depths / densities for each site, all of which would greatly enhance the value of the dataset. Do any of the temporal trends relate to specific horizons or root densities within the soil? Why where the specific depths chosen, but not used entirely consistently throughout all four stations/sites?

3. Are the meterorological measurements used to calculate actual or potential evapotranspiration (ET)? Soil moisture deficits often result in actual ET at rates substantially lower than the potential. If the actual ET values are not available, the dataset should include the entire time-series of information used to calculate potential ET to allow users to calculate ET with alternative approaches (i.e., not just Penman-Monteith).

4. Another issue is the distinction between the two "sites" at Kelso. From looking

at Figure 4 it is very clear that the two sites are so close to each other, that they are effectively the same site, even without comparison to the other two sites. The manuscript does not provide any justification that would warrant separating the two (i.e., soil type, vegetation cover, or topography). This is of relatively minor importance as far as publishing the dataset is concerned, but in terms of analyzing the four "sites" distinguishing between Kelso 1 and 2 is rather arbitrary. Some stations in Kelso 1 are closer to stations in Kelso 2 than they are to their nearest neighbor in Kelso 1.

Several figures could be greatly improved upon, particularly to help substantiate the arguments and conclusions in section 4.

Figure 2 – The x-axis label "day of year" is not particularly helpful seeing as the increment does not correspond to weeks, months, or quarters of a year. Revise to use month/year or make the increment correspond to a meaningful period of time (week, month, quarter, etc.).

Figure 3 – Do the symbols on inset map correspond to similar symbols in Figure 1? If so, note this in the caption.

Figure 4 – This figure is far more difficult to interpret than it should be.

1. The images on the left and right columns show the exact same thing, so there ought to be some compelling reason to include both sets of images in the final manuscript.

2. The easting/northing has no bearing on soil-moisture, so instead orient all three sets of images in the same orientation so that the lowpoint is at the bottom of the image.

3. The axes should be the same scale to avoid distortion.

4. These figures also highlight the illogical numbering system, which ideally would be related to topography or cardinal direction, not in random spiral from the central point outward. I think it would make sense for the numbers to be associated with elevation or in rows form high to low elevation.

Figure 7 – This figure tells an important story, but it is quite difficult to follow.

1. Replace the labels a-d and with the site names K1, K2, OR, GR instead, which would make the interpretation far more intuitive. Similarly, it would be helpful to label "mean" and "stdv" on the left and right columns, respectively to avoid confusion.

2. Consider using a different color scheme for the left and right columns, to avoid confusion. For example distinguish between values of soil water content on the left (red=dry to blue=wet) and variability in soil moisture shown on the right (green=low stdv, pink=high stdv).

3. It would be useful to show the color contour for soil moisture and then use the contour lines to show topography, which would help illustrate topographic impacts (or lack thereof) on soil moisture patterns and variability.

4. Although not critical, it would be more logical to have three rows (one for each depth) and four columns (one for each month shown).

5. What type of interpolation is used in this figure? Linear interpolation? Kriging with a trend? Is topography considered in this interpolation? The images on the left column don't mean as much without a robust form of interpolation that takes topography into account. On the right column it doesn't seem to make any sense to interpolate standard deviations between distinct points. Regardless, some justification of the techniques used to create this figure is needed.

SPECIFIC COMMENTS AND TECHNICAL CORRECTIONS

P14000.L4-7: Considering the location and small extent of the field site, this is a flimsy argument for the novelty of this work.

P14000.L12: Web address does not link to the dataset. The article must provide a direct link to the dataset itself, not a research group's webpage.

P14000.L14-19: Outline of the organizational structure is not necessary for a research

article and could be deleted.

P14002: Order in which figures and tables are referenced in the text is not consistent with their numbering. For example, Figures 2 and 6 referenced together before mention of Fig 3. Similarly, Table 2 is referenced in line 1 on this page, without any prior reference to Table 1 (which is not referenced at all). Check for consistency throughout.

P14002.L13-14: Redundant. Delete "For the sake of clarity. . .article." and revise to: "Throughout this article, a site refers to. . ." Similar phrases can be deleted throughout article.

P14003.L16: What is 5cm soil moisture? Does this refer to an interval, a depth, what?

P14004.L8-9: Potential ET or actual ET? Is this part of the dataset or part of the authors' interpretation of the dataset? This is important to clarify (see general comments).

P14004.L19: Wetting and drying curves traditionally mean something very specific in soil physics. However, the authors are not referring to characteristic curves of water retention and hydraulic conductivity here, but rather the rise and fall in soil-water during wetting and drying. Revise to avoid confusion.

P14004.L23: the differences ARE observed.

P14005.L13: suggested revision ". . .number of stations i (usually 9). . ."

P14006.L3: Second summation, clarify the implications of this. The variance is calculated based on spatial variation, not temporal changes? What are the consequences of different averaging schemes (i.e., over depth instead of across a site)?

P14008.L16-19: This information is repeated from the figure caption and could be deleted.

P14009.L2-4: How is spatial variability for each site assessed in Figure 6? Are variations between sites shown in Figure 6? This isn't really spatial variability, but rather variations between sites.

P14009.L15-29: The results discussed here are not easy for the reader to glean from the figures and took significant interpretation / jumping back and forth between figures, captions, and text for me to verify. Consider editing figures and revised text to enhance ease of interpretation (see general comments above).

P14010.L9-13. Contradictions with Mohantey and Skaggs (2001) are likely related to scaling issues discussed in general comments. The present study is for small "footprints" with complex topography and high measurement density, whereas the impact of topography is likely to be smoothed out for a larger footprint used in remote sensing. This is a good place to highlight why direct field measurements (at the scale of the McMaster Mesonet dataset) are important because they illustrate what remote sensing potentially misses: the importance of small scale topographic features on moisture patterns and redistribution. How might this influence inputs / evaluation of a land surface model? Some thoughtful discussion of this would improve the manuscript.

P14011. L.4: What is "position", clarify as this would usually refer to a location in x,y,z, not a soil-moisture value. Use more accurate/descriptive term.

P14011. L.6: What is meant by ranking? This term is introduced here without a list or table of ranks and then used throughout the discussion without a clear definition. Clarify this terminology and how it is employed here.

P14011. L.14-17: This is not a good sentence, it's too long and I don't know exactly what the authors are trying to say. ". . .and so despite absolute changes in rank, when the variability of these sites is considered the change is not thought to be meaningful.". . .? Revise for clarity.

P14011. L.17-19: What is the point of including this disclaimer? Have the authors proposed a hypothesis or not? Why is this beyond the scope of a preliminary statistical analysis?

P14011. L21: Shift significantly what? Is there a statistically significant shift or is this

just a noticeable shift?

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