

# ***Interactive comment on “McMaster Mesonet Soil Moisture Dataset: Description and Spatio-Temporal Variability Analysis” by K. C. Kornelsen and P. Coulibaly***

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Dear Editor and Referee,

We thank Referee #2 for the insightful comments received and will seek to satisfy the referee's comments in the following paragraphs and/or in the revised manuscript.

## **1 General Comments**

*Comment 1: 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, C6554 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.*

The authors agree with the referee that use of the term *high resolution* could be perceived as subjective depending on viewpoint. However, in this context, the term *high resolution* appears appropriate to characterize hourly data recorded continuously at every 25x25m (spatial resolution) grid over 6 years. This is a unique database in Canada. With regards to the 1 million km<sup>2</sup> of the Province of Ontario and the 10 millions km<sup>2</sup> of Canada, this is a very high resolution soil moisture dataset both in space and time resolution.

The authors are preparing an extensive revision to the introduction section of the manuscript to better place the dataset in context with regard to existing database. Use of terms such as *high resolution* will be further specified and followed by a discussion of scaling in context outlined by Blöschl and Sivapalan (1995). This will include explicit consideration of the scaling processes affecting soil moisture distribution (i.e. revision to Pg. 13997 Line 22 – Pg.

13998 Line 26), and more accurate placement of this dataset in the context of other studies (i.e. revision to Pg. 13999 Lines 3-20) including the above referenced studies.

*Comment 2: 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 authors will also revisit this section in the revision. It was not our intent to claim this dataset representative of the ‘Canadian Climate’, rather to point out that the dataset is unique in Canada, specifically in terms of space-time resolution, length and continuous monitoring aspect. Such continuous hourly data are available in other political jurisdictions, but to the authors knowledge none exist in Canada. The authors thank the reviewer for the suggestion to consider the field site as representative of the Great Lakes Region. As the reviewer has pointed out, scaling is an important consideration in this context, where the spatial extent of the dataset limits claims of ‘representativeness’ of any region. We propose a revision to place the dataset in the context of being representative of processes affecting soil moisture distribution at the hillslope scale in the Lake Ontario basin. In this context the longevity and continuous nature of this dataset would allow better characterization of some processes than other dense datasets which do not possess the same longevity of measurement. Seasonal soil moisture variability is an important aspect to be further analyzed using the dataset.

*Comment 3: 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.*

The authors agree with the referee that scaling is an important issue, which was not fully addressed in the original manuscript. As mentioned in the response to Comment 1, we will revise the manuscript to better describe scaling issues throughout the manuscript where relevant. This will include revision to the introduction section, discussion of scaling implications for spatial averaging in the methods section and revision to the Results and Discussions section. Scaling will be discussed in the framework of Bloschl and Sivapalan (1995).

*Comment 4: 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.*

Unfortunately, when the McMaster Mesonet was designed funding was not available for collection of snow depth information and, as a result, only rainfall is measured (Pg. 14004 Line 7). Some SWE information is available independently of the McMaster Mesonet from Environment Canada weather stations, but is limited. Data for winter months are presented herein for the purposes of introducing the dataset, however a thorough evaluation of the impact of snowmelt and freezing temperatures on soil moisture and TDR probe accuracy is necessarily left for future work. This aspect will be addressed in the revision (See response to Referee #1). It is noteworthy that the main objective of this descriptive paper is to introduce the mesonet database. The dataset will be further analyzed and the effect snow and snowmelt will be further investigated.

*Other Comments: 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.*

This link will be updated to the dataset link. ([www.hydrology.mcmaster.ca/mesonet.html](http://www.hydrology.mcmaster.ca/mesonet.html)). Please note: this site provides an overview of the data, but does not at this time provide direct download access to the dataset as sites for networks such as SCAN do. Data can be provided by contacting the authors.

*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 were the specific depths chosen, but not used entirely consistently throughout all four stations/sites?*

The depths were chosen to capture soil moisture dynamics in the hydrologically active layer. Depths were not consistent because of the high clay content in some locations which made digging a 100 cm soil pit difficult and likely would have caused damage to the probes during installation (Pg. 14003 Lines 1-5). The soil is not highly variable in the study sites. Kelso is mostly clay loam with more clay at further depth (around 80-100cm). Similarly topsoil in Dundas Valley (OR and GR) is mostly silt loam with more clay at further depth. In general the soil profile (up to 80cm) is clay loam at Kelso sites, and silt loam in Dundas Valley sites. Around 80-100cm, clay appears dominant in all sites with minor spatial variability. This is

consistent with the local soil maps. Additional soil description will be provided in the revised manuscript.

*3. Are the meteorological 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).*

Grass reference ET, as presented, is calculated internally by the data logger using the Penman-Monteith method. All information collected by the weather stations is available as part of the McMaster Mesonet dataset and can be analyzed by users as needed. Actual ET is not recorded.

*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.*

The distinction between the two Kelso sites results only from the organization of the data loggers within the network. Soil moisture at Kelso 1 and 2 are recorded independently of each other and can be provided to users as such. Therefore, for the purposes of giving an initial description of the data, the authors felt it useful to consider the two datasets as independent in the original manuscript. A failure of the TDR network at Kelso 1 won't affect data collection at Kelso 2. It is also noteworthy that only Kelso site 1 is equipped with both Campbell Scientific probes and Stevens water soil probes for comparison.

*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.).*

The authors will revise.

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

The symbols are consistent between both maps. We will include this note in the caption as the referee has suggested.

*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.*

Both were included to provide better context and visualization of the site for readers and future data users.

*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.*

The easting/northing method provides a standard context for placing the sites within the watershed and also allows for consistency between Figures some of which are more intuitive oriented pointing north. The authors prefer to keep the easting/northing of figures for consistency.*3. The axes should be the same scale to avoid distortion.*

This will be corrected in the final figure.

*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.*

The numbering system of the stations is related to their position in the data logger. Therefore, the numbering as presented is ‘hard wired’. The referee’s point of a different numbering system being more intuitive is well taken, however the authors are concerned that a change in the numbering system that is inconsistent with the raw output of the data logger would provide too much opportunity for human/programming error. Our concern is that re-arrangement of the numbering system has the potential for data to be incorrectly labeled with the incorrect geographical location.

*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.*

The authors will make the suggested change.

*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).*

Points 2 and part of 5 will be addressed together as they are inter-related. The authors would like to acknowledge the referee’s concerns about the confusion between the mean and StDev plots. We agree that interpolating standard deviation between distinct points is problematic. When initially developing this figure we attempted the use of error bars or distinguishing StDev using a color scale on the location points. These methods were difficult to interpret and ultimately rejected. In order to provide a prompt response to the referee’s comments a final solution is not available, however, the authors will address this point in the final manuscript

by revisiting the prior methods or accepting the reviewers recommendation to use a separate color scheme for StDev.

*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.*

Based on the referee suggestion the authors developed a figure with superimposed elevation contours. However, due to the size and design of the subplots this made the figure very difficult to interpret and so the authors prefer to maintain the original formatting.

*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).*

This figure is likely to be revised when the representation of StDev is reconsidered. The authors will seek to arrange the figure in a manner that is both intuitive and provide the best visual interpretation in the final HESS publication.

*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.*

The interpolation method used in all of the figures is the Natural-neighbor scattered interpolation. Since all data points are collected using a handheld GPS they do not exist on a regular grid and the natural-neighbor Tri-Scattered Interpolation function in MATLAB was used as it provides a continuous interpolant in three dimensions at all points. Therefore, topography is not considered in the interpolation. The reason this was not done was because topography is not the only contributing factor to soil moisture distribution. Since there is no quantitative information available about the strength of the relationship between, topography, vegetation and small-scale soil textural differences on the soil moisture distribution and the variability in the importance of these factors with time, a simple method that made no assumptions was deemed most appropriate.

#### *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.*

This will be revised following the referee's recommendations as addressed in the response to Comments 1 and 2.

*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.*

Correction made.

*P14000.L14-19: Outline of the organizational structure is not necessary for a research article and could be deleted.*

These lines will be deleted from the final manuscript.

*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.*

Reference to Table 2 will be corrected to Table 1. Cross reference to Fig 6, will be removed. Further references will be verified.

*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.*

Correction will be made.

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

5 cm soil moisture refers to the depth. This will be corrected in the final manuscript.

*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).*

These lines will be revised as below.

**Additionally, grass reference evapotranspiration is calculated on-line from collected data within the weather station using the Penman-Monteith equation (Campbell Scientific, 1999).**

*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.*

This line will be revised to “**...at all sites in terms of the rate of wetting and drying.**”

*P14004.L23: the differences ARE observed.*

Correction will be made.

*P14005.L13: suggested revision “. . .number of stations  $i$  (usually 9). . .”*

The authors agree and will make the suggested revision.

*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)?*

The averaging scheme is justified, because of the high hourly autocorrelation, especially with high silt and clay content, the temporal variance of soil moisture is minimal in comparison to the spatial variance when considering daily soil moisture. Therefore, the loss of variability in the data is assumed negligible. Full implications of this will be discussed in the revised manuscript.

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

Added information will 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.*

Figure 6 shows the variance as the shaded area. The term variability will be changed to standard deviation.

*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).*

This text will be revised following prior recommendations and corrections suggested by Referee #1.

*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.*

The manuscript discussions will undergo revision as recommended and addressed in prior comments.

*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.*

“Position” will be changed to rank.

*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.*

The term ‘rank’ refers to an observation by Chen (2006) that ‘rank stability’ is a synonymous but likely more appropriate term than ‘temporal stability’. Meaning in time the relative rank or the ordering of the soil moisture stations do not change. This terminology will be clarified in the manuscript and better understanding of it presented in addressing comments from Referee #1 to include ‘classic’ temporal stability figures.

*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.*

Revised as below. A figure (currently referred to as X) is to be included in the revised manuscript following the recommendation of Referee #1 and is included here to clarify the revised statement.

**At the monthly scale, a similar  $\overline{\delta_{ij}}$  may result in some stations changing ranked position. For example, in Fig X. stations 7 and 5 both represent dry stations but have changed rank order as the driest station. Often due to the overlap in variance of  $\overline{\delta_{ij}}$ , especially for mean representative sites, the change in station rank order is not considered meaningful. An example is Station 4 in Fig X., where the station is always representative of the mean or median value but changes rank order.**

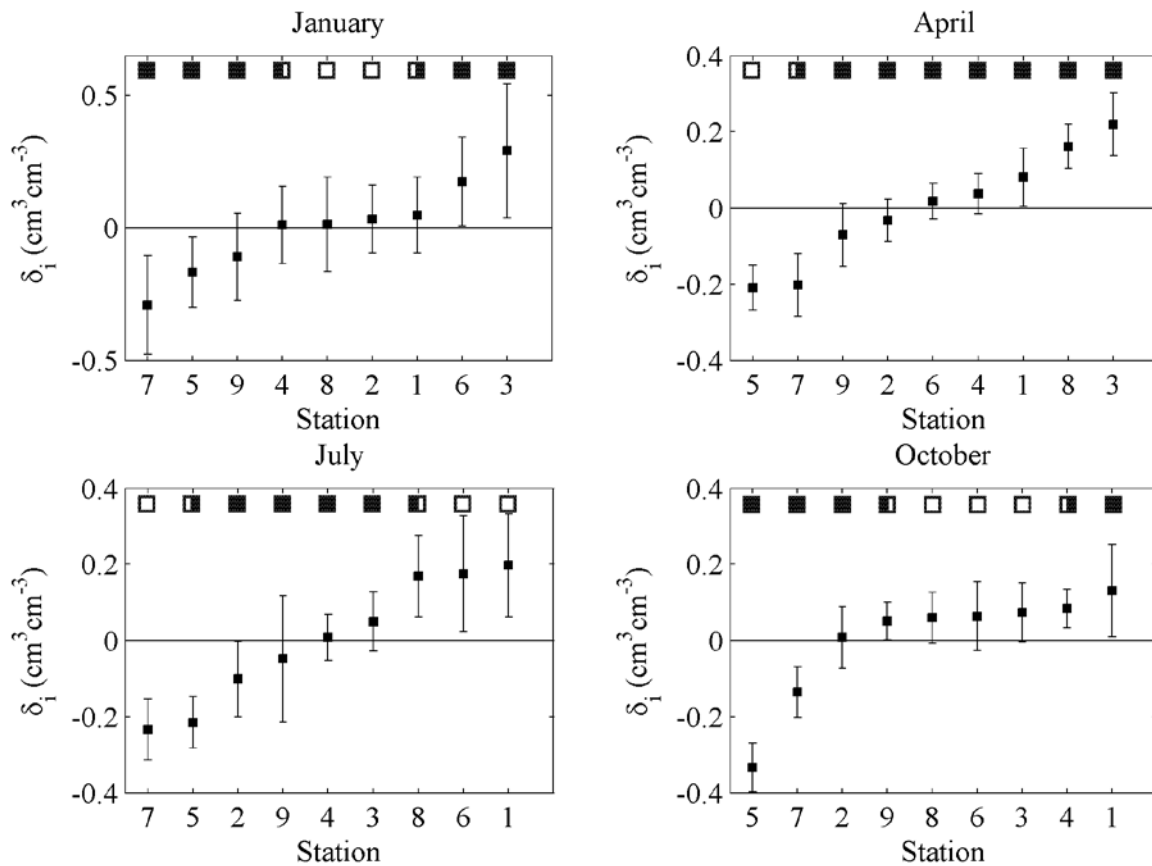


Figure X: Mean relative difference and standard deviation of mean relative difference at GR. The horizontal line shows the site mean soil moisture value. The box at the top of each subfigure shows if the mean relative difference is similar ( $\alpha = 0.05$ ) to its neighbor. A dark box represents dissimilar means and an open box similar means as the station on the respective side.

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?

In the revision a t-test will be used to determine if the means are statistically similar and presented as in Fig X. above. Therefore these lines will be removed.

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

The term significant has been removed. The results and discussion will also be revised to focus on the above figure and a similar figure showing the entire dataset.

**References:**

Bloschl, G., and Sivapalan, M. : Scale issues in hydrological modeling - a review. Hydrol. Processes, 9, 251-290. doi:10.1002/hyp.3360090305, 1995

Campbell-Scientific: On-Line Estimation of Grass Referenc Evapotranspiration with the Campbell Scientific Automated Weather Station: Application Note 4-D. Logan, Utah, 1999

Chen, Y.: Letter to the editor on “Rank stability or temporal stability”, *Soil Sci. Soc. Am. J.*, 70, 306, doi:10.2136/sssaj2005.02901, 2006.