Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2017-82-AC1, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 4.0 License.

Earth System Science Data Discussions

Interactive comment on "31 years of hourly spatially distributed air temperature, humidity, and precipitation amount and phase from Reynolds Critical Zone Observatory" *by* Patrick R. Kormos et al.

Patrick R. Kormos et al.

patrick.kormos@noaa.gov

Received and published: 17 December 2017

Response to comments from Referee 1: 31 years of hourly spatially distributed air temperature, humidity, and precipitation amount and phase from Reynolds Critical Zone Observatory - ESSDD

Line number references are to the original manuscript unless otherwise noted.

General Comments

This paper describes a spatial distributed dataset of temperature, relative humidity, precipitation and precipitation phase that spans the Reynolds Creek watershed at an hourly scale over 31 years at a 10-meter resolution. This dataset will be very valuable to the earth surface modelling community to test and develop spatial modelling tools and provides insights into the climate/elevation/land use dynamics of this region. This is an important contribution that fits well with the scope of the ESSD special issue on "Hydrometeorological data from mountain and alpine research catchments".

Overall the paper is well written and well organized. The largest criticism of it is that it is lacking important details on how/why the methodologies used were implemented or justified. This information is critical for any potential users to properly evaluate the usefulness of this data to their particular interest. The lack of an error analysis of the data infilling and spatial distribution procedures needs to be rectified before publication. I would recommend that major revisions be made prior to its acceptance.

We thank the referee for a thorough review. We have added details on the methodologies below as suggested. Although an error analysis would be a significant advancement for this dataset and the field, it is beyond the scope of this paper. We plan on conducting an in-depth error analysis using this dataset in an additional paper.

Specific areas of clarification/corrections required or recommended are identified in the order they appear in the paper hereafter:

1. Pg. 1 Line 13-15: Please justify the selected water year. Why does the dataset end in 2014? Is data collection still occurring and if so why was this data not included? With changes in climate recent years are of great interest. Will this dataset be updated in the future on an ongoing basis?

We have included the justification for the selected water years in the additional sentences inserted in line 15 as follows: "Relative humidity data were available at 3 locations in RCEW starting in 1983, which is necessary for distribution of humidity and precipitation phase variables. Although data collection is ongoing at RCEW, the dataset

ends at water year 2014 due to the timing of research funding. Updated weather station data for these and other data are available at http://reynoldscreekczo.org/rcewdata/ and the dataset will be updated to include additional weather variables and water years as funding allows."

2. Pg1 Line 18-20 This sentence is unnecessary in my opinion.

We have removed this sentence as suggested.

3. Pg. 2 Line 1-4 Windspeed and incoming radiation observations are mentioned here. Why were these not distributed as well. These variables are critical for modelling and inclusions of these would greatly increase the value of this dataset. Justification for why these are not included is required. Is this underlying point data available publicly elsewhere? If it is a link to the source would be beneficial. If not publicly available why not and who could be contacted if someone still wanted access.

Wind speed and radiation observations are measured and available at many stations in RCEW. The manuscript presented here only deals with temperature, precipitation, humidity, and phase, as indicated by the title. The inclusion of radiation and wind speed involve significantly more time and effort, both in quality control of measurement data and methods for distribution. Future publications will include these datasets on the same modeling grid. We are currently working on developing the incoming solar radiation dataset at the top of the canopy. The underlying point data are available publicly at http://reynoldscreekczo.org/rcewdata/ as described in the response to comment 1, which also addresses additional variables as suggested.

4. Pg. 4 Figure 3 I cannot discern the bottom uncertainty bound for precipitation in December and January, update figure as appropriate.

Thank you for your attention to detail. We have fixed this figure as suggested.

5. Page 4 Line 14-15 Is this the most up to date description of the spatial data available? Have there been any change in the basin in the last 16 years (land use or climate

change driven) that would alter this interpretation. Rather unclear from the WRR article where this data is as no doubt data archives have changed in the last 16 years. Can a better link be provided to this data? Is this the same data as the link to on page 11 line 13 (that link address does not work for me).

Mark's maps are currently the most comprehensive and detailed maps available, but some localized changes may have occurred with disturbance and woody encroachment. The extent of vegetation type changes are uncertain, but ongoing field and remote-sensing surveys may help to document recent shifts. I've changed the address to ftp://ftp.nwrc.ars.usda.gov/reynolds-creek-datasets/archived-2001-water-resources-research-versions.zip, which works better.

6. Page 4 Line 20 Should the Hanson et al 2001 reference not be the Hanson et al 2004 reference here? Removal of Hanson et al 2001 from bibliography should also then occur.

We have changed this sentence as follows to include both citations:

"Precipitation is measured with a dual Belfort style gauge system described by Hanson et al. (2001, 2004).

7. Page 4 line 20-page 5 line 2: Does this mean you used the 1.8 correction factor determined by Hanson et al 2004? This is rather simplistic if that is the case considering the significant work on undercatch corrections since then.... please explain or justify what is done here more clearly.

This is the best method for correcting for undercatch in the absence of wind data, which is the case for most of the early precipitation measurements. The use of this method allowed for consistent application of wind correction from WY 1984-2014.

8. Page 5 line 9: This is sentence is redundant in light of preceding paragraph.

We have removed this sentence and combined these paragraphs.

9. Page 5 line 9-14: The explanation of data infilling via multiple linear regression needs significant expansion. More justification of the approach and methodology is needed. Does this approach account for elevation difference explicitly in any way? There will be errors associated with regression infilling so information on magnitude of these errors is needed. This is critical to evaluate the uncertainty of the generated dataset. How is this approach better than simply doing the de-trended kriging, described later, on the unfilled dataset? One would have expected that as the de-trended kriging is able to impose physically realistic constraints on the interpolation, unlike an unconstrained statistical regression, that it would return better results. Please explain/justify why this infilling approach was taken.

We do not think that simply doing the detrended kriging would do a better or a worse job of filling these data gaps. However, the software that generates the detrended kriging images is significantly faster if the point vector data is continuous. When the program encounters a data gap, it must recalculate the station weights. For this reason we have chosen to fill reasonable gaps during the quality control stage of the data set preparation. We have included this reasoning in the text as follows on page 5, line 11:

"We chose to fill these gaps during the quality control stage of the data set preparation, opposed to filling them with the detrended kriging algorithm, for computational efficiency reasons. Recalculation of station weights, the most computationally expensive part of the spatial distribution process, must be performed every time the detrended kriging software encounters a missing value."

10. Page 5 Line 15-16: Please explain in greater detail what is meant by "1) the degree of topographic and vegetation sheltering and 2) the spatial arrangement of measurement locations"

We have changed this sentence as follows:

"... on 1) the degree of wind sheltering due to topography and vegetation sheltering and . . .."

11. Page 5 line 22-23: More explanation of how RH is distributed is needed. Is RH directly interpolated and bounded to 0-100% regardless of air temperature changes? If this is the approach taken the physical relationship between air temperature and water vapor holding capacity has been ignored. Was conversion of RH to water vapor pressure accounting for air temperature at the point stations and interpolation of this water vapor pressure considered? Not enough information is provided to understand/justify what was done. Overall the de-trended kriging approach needs to have an assessment of error/uncertainty. How well does it capture the spatial patterns away from the stations. A piece-wise station removal approach may be an appropriate to understand the errors of this approach. Very hard to determine how good this dataset is otherwise.

RH was directly distributed and bounded to 0-100%. There is no difference between calculating vapor pressure or dew point temperature at a station with measured temperature, and distributing relative humidity. Dew point temperature would have to be bounded by distributed air temperature and vapor pressure would have to be bounded by saturated vapor pressure. Any difference in rounding errors would fall within the errors associated with the HMP humidity sensors, which is plus or minus 9% around 100%.

12. Page 5 Line 31: Why 7 percent? A very specific value to not have a justification.

This was merely an exercise to illustrate the spatial resolution of the data. There is no hard and fast way to describe a rain-snow transition zone. It was just a value that made the figure look good.

13. Page 6 line 4-6: Typo? Bottom bound of phase delineation -5C or -0.5C? Justify the value of this dewpoint approach considering all of the papers since 2013 that have demonstrated better precipitation phase delineation with a wet-bulb temperature. This precipitation phase delineation will be greatly influenced by how RH is distributed (see previous point). Justify the inclusion of precipitation phase, a calculated variable with significant uncertainty, when the objective of this paper is to describe spatial distribution

of OBSERVED data.

Thank you for catching this typo. We have changed -5C to -0.5C as suggested.

We agree that this is a variable with significant uncertainty. We used the best available method to provide these data to a broad field of modelers including ecological modelers who may need precipitation phase as a model forcing variable. Other methods that use wet bulb temperature are cumbersome and time consuming and, in the end, fit an empirical relationship to highly scattered data points, who's origins and subjective in nature (using noisy snow depth data to determine precipitation phase). Although we did attempt to go through this exercise, the number of weather stations involved, the time commitment needed, and the very small difference between using wet bulb temperature and the dew point temperature led to the decision to use the dew point temperature method.

14. Page 6 line 12: "Precipitation data do not account for wind redistribution of snow." Is an unnecessary sentence.

We think this is a necessary sentence for the clarity of the dataset, especially for non-specialists. We have left it, but will remove it at the request of the editor.

15. Page 7 Figure 5 and Page 8 Figure 6: Panels f show a mixed phase category. From the text mixed phase is divided in to rain or snow linearly so why is the proportion of mixed as snow not communicated?

This is just the way we decided to present the data in the bar chart.

16. Page 8 line 1: Please include the link to the data source here.

We have included the link to the data source here as suggested.

17. Page 9 Line 14: Link to spatial data doesn't work for me. Same as reference to Seyfried 2001 on page 4 lines 14-15? Please combine this information to reduce redundancy.

Please see response to number 5 above. We have changed the link. We will leave this link on pages 9 and 4, but will remove one of them at the request of the editor.

Interactive comment on Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2017-82, 2017.

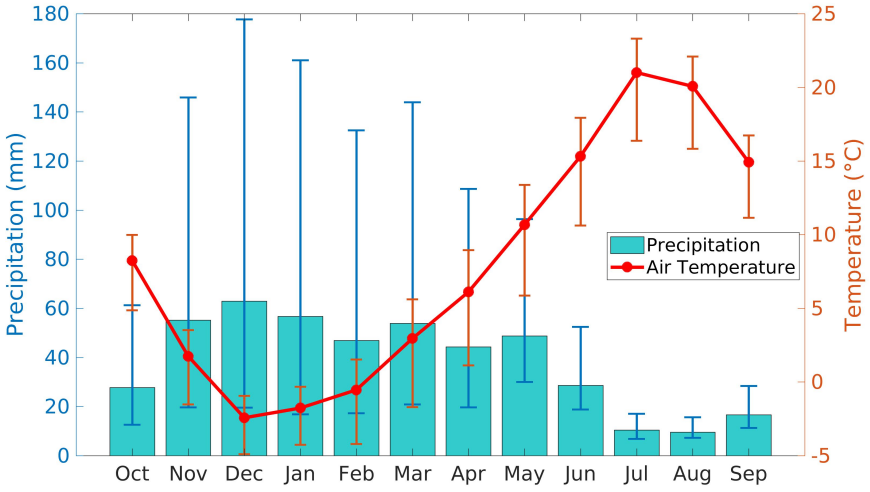

Fig. 1. Figure 3 Revised