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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.

Anonymous Referee #1

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Review of "31 years of hourly spatially distributed air temperature, humidity, and precipitation amount and phase from Reynolds Critical Zone Observatory" by : P. Kormos, D. Marks, M. Seyfried, S. Havens, A. Hedrick, K. Lohse, and M. Sandusky

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.

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

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?

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

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.

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

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).

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.

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.

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

Page 5 line 9-14: The explanation of data infilling via multiple linear regression needs signficiant 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.

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”

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.

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

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.

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

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?

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

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.

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