

Interactive comment on “Estimating Radar Precipitation in Cold Climates: The role of Air Temperature within a Nonparametric Framework” by Kuganesan Sivasubramaniam et al.

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review of HESS-2018-0351 “Estimating Radar Precipitation in Cold Climates: The role of Air Temperature within a Nonparametric Framework,” which is essentially a re-review of hess-2017-662 “Should radar precipitation depend on incident air temperature? A new estimation algorithm for cold climates.” Since this is a new submission, I will review it as such.

General Comments:

This is an interesting paper that should give us some improved insight into using

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weather radar to estimate rain and snow fall. This is very relevant in higher latitudes and in mountain environments where snow is important. To date, there has been limited work in using weather radar for snowfall estimation in a hydrological context. The methods presented herein could be used in many locations. However, the writing is unclear and I got lost at times. I suggest that the authors revisit their objectives and ensure that the paper addresses these. Also, the Discussion is essentially missing as the work is not put into context of the few other relevant studies. Below I outline restructuring and a problem with the Methods/Data.

Equation 4 uses air temperature and relative humidity to estimate the phase of the precipitation from Koistinen et al. (2004) and used by Saltikoff et al. (2015) for Finland. However, air temperature at the gauge is used, and this is not correct. Fassnacht et al. (1999; 2001) lapsed the air temperature up to the radar measurement height. There can be 5 to 10 degrees Celsius difference between the temperature at the height (2 m above the ground) and the radar height (1 km stated on page 8 line 15). At minimum this should be discussed?

What about a split sample approach of calibration and evaluation for the partial weights and k-nn approach? Also, the partial weight for radar precipitation was shown to vary from 0.4 to 1 (Figure 2), so why was a single average (mean) used in the k-nn prediction model. Is this approach not robust enough to have a different partial weight, or perhaps a gridded partial weight? It is stated that there is no spatial pattern in the partial weights, but an interpolated residual type approach could be used (e.g., Fassnacht et al., 2003 among others).

The paper does need restructuring and rewriting. At present I get lost in where I am in the text, regardless of the “foreshadowing” sentences that appear at the end of various sections. 1) At the end of the Introduction, the paper should tell the reader specific objectives that were investigated or research questions that were answered.

2) Some of the material in the Background is repeated from the Introduction. For exam-

ple, the three paragraphs in section 2.1 (Radar precipitation estimation in cold climates) mostly in the Introduction. Either reduce the Introduction or merge the Background with the Introduction to remove the repetition. I suggest the latter and to consider adding sections to the Introduction (e.g., 1.1. Weather radar use for hydrology, 1.2. Radar precipitation estimation in cold climates, 1.3. Nonparametric Radar rainfall estimates).

3) There are methods presented in the Study Area and Data section. These two sections 3. Methods and 4. Study Area and Data need to be revisited to put all the methods together. I suggest a brief section first on Study Area, then a section on Data and Methods, describing the data first, then the methods used.

4) The Results and Discussion are combined and the Discussion is thus limited. I recommend that the Results and Discussion sections should be presented separately, or that the Discussion be much more in depth. There are only three citations in the entire Results and Discussion section, while numerous useful citations are presented in the Introduction and Background sections. There is no Discussion that put this work into context; the Results and Discussion only presents how do these results compare to the findings of Fassnacht et al. (1999), Koistinen et al. (2004) and Saltikoff et al. (2015).

5) At the end of the Summary and Conclusions, it is stated that “[w]hile this study uses data for one weather radar in arriving at its conclusions, preliminary analysis suggests the problems noted here to be generic.” If there are additional “preliminary” results from some should be presented. This statement is important but is hanging.

6) In various locations throughout the text, sentences are added that foreshadow the next or subsequent sections. These are not necessary and should be removed.

The meshing of meteorological data with the radar data on a 1 x 1 km grid, including the interpolation of the station data is confusing. This is the four paragraphs on page 8 line 23 through page 9 line 21. This section needs to be rewritten, as it is unclear what is done. Perhaps a table could be added that describes the four datasets (T, RH,

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wind, and radar). From the text, I assume that temperature and RH data have been gridded at a 1-km resolution from the station data: T using the Optimal Interpolation in a Bayesian setting (Lussana et al., 2016) and RH using the nearest neighbor. What is the Lussana et al. (2016) method? While wind data are available, they are downscaled from a 10 km numerical model dataset. What numerical model dataset is used? This section needs to provide more details - it does not have to be much longer, the methods just need to be clarified. Also, the gauge precipitation (Pgauge) data are used as point measurements meshed with the gridded data; this would also be included in the aforementioned table. These data are revisited in section 4.1.

The font size is too small in most figures and the text is often grey. This makes the figures difficult to read. This should be addressed throughout.

Specific Comments:

Page 1, line 1: I suggest saying “In colder climates ...”

p1, l1 and l2, p2 l1, etc.: be consistent with “form of precipitation” and “state of precipitation.” I suggest calling it “phase of precipitation” throughout the text.

p1, l5: “estimate” or “adjust”?

p1, l11: usually “catch error” is called “undercatch”

p1, l15: do you mean gauge air “temperature” or temperature at the radar measurement height?

p1 l15 and subsequently: to be more specific, use “warmer” than instead of “above” when referring to air temperatures. “Above” implies an altitude above the ground, which is typically associated with a colder air temperature. p2, l25: use “colder than” instead of “below,” etc.

p1, l15-16: the end of the sentence “which indicates that the partial dependence of precipitation on air temperature is most important for colder climates alone” is unclear.

Please reword.

p1, l22: should “2010b” be “2010a?” Check this, as (Villarini and Krajewski, 2010a) has not appeared yet.

p2, l13: why “Conventionally?” Use another word so that the reader does not confuse radar types, such as “the original way” (i.e., conventional), Doppler, dual-polar, multi-wavelength.

p2, l18: since Canada is mentioned here (Crozier et al., 1991) could be add to the citation list on line 20

p2, l25: add an “s” to “quarter”

p3, l2: “different temperatures cause different shapes of crystals.” For solid precipitation, i.e., snow, the degree of super-saturation also affects the crystal shape.

p3, l5: what is meant by “multiple snow types?” Does this imply shapes? If so, state this explicitly.

p3, l8: in many cases the correlation between probability of snow and temperature is an “S’ shaped structure,” (see Fassnacht et al., 2001 for a summary illustration), but a simpler linear relation has also been used (e.g., Fassnacht et al., 2013).

p3, l9-10: be specific with “[t]he dielectric property of solid particles (ice) is very different from liquid particles (water).” “Very different” is vague.

p3, l12: reverse the order of the Hasan et al. (2016) references. You present 2016b before 2016a.

p3, l13: change the word “Historical”

p3, l25-29: delete the sentences in the rest of the paragraph starting with “[t]he rest of the paper is structured as follows.” You do not need to tell what the sections of the paper are, that reads like the end of a thesis. Instead, give us specific objectives to

investigate or research questions that are answered.

p3, l28: The results should be presented, then there should be a separate Discussion section.

p4, l27: there has also been some work on phase discrimination using multiple radar wavelengths (e.g., Al-Sakka et al., 2013).

p4 l29 to p5 l3: This paragraph can be reduced to 1-2 sentences, as this information is generally known.

p5, l12-13: please reconsider “nonparametric approaches ... weakness is that the method is sensitive to outliers.” I am not sure that this is correct. Parametric approaches tend to be sensitive to outliers.

p5, l18-20: these two foreshadowing equations are not necessary.

p7, l4: “classification of precipitation phase at gauge level” is good, but don’t we need the phase of precipitation at the radar height to select the appropriate radar Z-R equation? Although this is what Koistinen et al. (2004) and Saltikoff et al. (2015), it doesn’t necessarily make it correct.

Figure 1: a) I assume that the “length of the observations” is the number of hours with precipitation? b) I also assume that the hypsometry curve is cumulative % of stations below the specified elevation. Please be specific. c) the font size is small and difficult to read. Enlarge and also don’t use grey. d) are the red names local cities? Are they important? If so, move them so they are legible.

p7, l12-13: What is the “accumulated hourly radar precipitation rate product?” Is this accumulated from sub-hourly to yield an hourly total, or is the hourly data added?

p7, l14: tell us how many gauges in the “a relatively dense network of precipitation gauges.”

p8, l26: instead of “are” use past tense through the methods.

p8, l34: reword the last sentence “However, we used data from all available precipitation gauges for this study.” Perhaps state something about the total number of gauge hours of data used (likely in the order of 100,000 gauge-hours).

p9, l2: provide a sources for the “gridded hourly temperature and wind speed dataset”

p9, l6: delete “[m]ore details on the procedure adopted for catch correction are provided in the next sub-section.”

p9, l10: change “resulted” to “resulting”

p9, l29: how little is “intensities below 0.1 mmh-1 contributes little?”

p10, l2-8: this is background. It could be moved to earlier in the text, as this is the methods/data section. Tell us what was done. This sentence could also be deleted.

p10, l4: the word “Nordic” is not necessary here, as it could also be relevant in southern environments

p10, l4-5: the end of the sentence is redundant “due to large catch errors for snow.” In could state that it is “due to high wind conditions.” It is wind that causes undercatch, not “large catch errors”

p10, l7: “Wolff et al., 2015” is not in the citation list

p10, l9 or previous: what type of precipitation gauge and what type of shield are used? This is very important information to assess the degree of undercatch and the error associated with the undercatch correction.

p10, l9-17: throughout this paragraph it is “undercatch” correction. This should be consistent, as there are other errors.

p10, l13-14: “It was found that correlation between the corrected precipitation by using measured wind speed data (15-gauge locations) and gridded data are over 0.97...” Does this mean the correlation undercatch correction using gauge wind speed versus

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the downscaled gridded wind speed?

p10, l14: there are only 15-gauge locations with wind speed measurements. How are the other 53 precipitation gauges corrected for undercatch? From my comment above (p10, l13-14), I assume that the downscaled gridded wind speed was used to correct for undercatch at all 68 precipitation gauges. This is not clear.

p10, l19-22: delete this paragraph. We know what you are going to do Figure 2: add tick marks to the y-axis.

p10, l23 through p11: Are the “Partial weight of predictors” constant over time, i.e., is there a specific value for station that does not change?

Table 1: As this is a summary of Figure 2, this table could be converted to two horizontal box and whisker plots on Figure 1. The reader doesn’t really care about the specific numbers, just the range.

p12, l1 and elsewhere: the word “prediction” implies that this is for the future. I suggest using “estimation” throughout.

Figure 3: the caption is confusing; break into two sentences. Also, are these “length of the data (circle size)” the same as in Figure 1? If so, then don’t display again here, unless this is relevant later?

p13, l4-7: these three sentence could be reduced to a small histogram of RMSE reductions that is added to figure 3.

p12 to 15: why was a single average (mean) partial weight ($\beta_P = 0.68$) used in the k-nn prediction model, when it was shown (Figure 2 and Table 1) that the partial weight varies from 0.4 to 1?

p15 section 5.3: all this text except for the last sentence is background or methods and should be moved to an appropriate location earlier in the paper.

p15, l15: does this “still significant” has a statistical meaning? If so, explain how. If not,

don't use the word significant.

p15, section 5.4: what is the range of the "Temperature Classes?" You only discuss $T > 10^{\circ}\text{C}$. What about $T < 10^{\circ}\text{C}$? The point of this section is unclear.

Figure 6: what are the dots above the solid and mixed phase?

p18, section 5.6: these statements seem to be hanging. Can you present specifics?

p18, l5: what does "the use of temperature as an additional predictor variable is having consistent impact" mean? The words "consistent impact" are not clear

References

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