

Interactive comment on “Evaluation of seNorge2, a conventional climatological datasets for snow- and hydrological modeling in Norway” by Cristian Lussana et al.

Anonymous Referee #1

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Lussana et al., have produced a “conventional” climatological dataset consisting of variables of daily average air temperature and precipitation sum. The manuscript (hereafter ms) aims to provide a validation for especially precipitation data (temperature data has been validated in another paper) in both directly and indirectly via hydrological and snow modelling. The interpolation method used by Lussana et al., is highly sophisticated and the data produced will no doubt be of high relevance in various applications in Norway and surrounding areas. However, I have some concerns related to the presentation of the dataset i.e. the actual manuscript that will need major revisions before becoming suitable for publishing in ESSD.

C1

Dataset: The data is accessible, but considering the large size of the data (> 30gb) I did not download it to my personal computer. This made think whether the dataset should be organized in smaller junks of data e.g. 1991-2000, 2001-2010 etc as this could be convenient for the users.

Major comments on the manuscript: First of all, the current version of the ms is overly long with plenty of loose statements (starting from the abstract) and needs to be more clearly organized to separate sections. Currently, the text is not proceeding logically. I would suggest following the basic structure of 1. Introduction, 2. Material and Methods, 3. Results, 4. Discussion, 5. Conclusions, with informative subheadings. Moreover, the current ms has 15 figures, which, needless to say, are too many and makes the identification of the main message very difficult. I would advise to keep the figures being critical for the paper in the main text and move rest to the supplementary material.

Throughout the paper, two versions of the seNorge are being compared (first stated in lines 96-97). However, it remains unclear to the reader, which dataset are more accurate in absolute terms. For sure they produce different outputs in hydrological modelling, but which one to “trust”?

To be honest, I don't see the added value of the hydrological/snow modelling applications presented in the paper. If the aim is to show that the new precipitation data is of high quality (i.e. validate it) then this focus should be enough and the validation could be conducted and presented in more in-depth manner. It is a bit disturbing that no clear research question has been raised to address a specific point in e.g. hydrology. In another words, if the precipitation data is good, shouldn't the subsequent analysis steps be also in the terms of climate data? The authors state this by themselves at line 373: “. . . without addressing any particular application.”

I got the impression that the seNorge2 temperature data has been previously validated. Therefore it seem unnecessary to perform this again. In addition, both temp and prec validation have been presented very vaguely and much emphasis has been given for

C2

the hydrological investigations. Moreover, I struggled to find the idea behind the “indirect-validation”, as many of the figures/results show only that there are differences between the older and newer datasets (without clearly addressing which one is “correct”).

Specific comments:

L17-19: This sentence is unclear, please rephrase.

L38-43: Isn't this being said in the previous chapter? Through the ms there are many places with unnecessary repetition.

L47-50: is this part describing the history of seNorge1 needed?

L53-55: Same as chapter one of the intro?

L57-74: Again, I understand that you want to provide info about the past versions of seNorge data, but is such a long paragraph of this manner needed?

L79-84: I got the impression, that the presented data is not ready yet in some parts?

L89-90: why the increase in complexity is important? Usually the simpler the better.

L92-95: too much technical information in the introduction. Please move to material and methods.

L104: Here and throughout the text; the term “conventional” is not particularly exciting. Why wouldn't you just call it gridded data?

L104-110: Are these being calculated from the gridded data or from station observations?

L114-115: why not the entire period, which is significantly longer than 1971-2010?

L118-122: Are these number from entire Fennoscandia or just Norway?

Chapter 2 needs to be divided to clear sub-chapters with their own subheadings, as now the text is difficult to follow.

C3

L157: “. . . fixed in time” is too vague for a reader, please specify or omit.

L162-165: On my opinion, these chapter-endings (here and elsewhere) describing what's coming next are not needed.

2.1. Optimal Interpolation: As OI is not completely new method, could this section been shortened a bit, and leave only parts with own applications and modifications? Apparently, OI has been described in detail in author's previous literature.

L215: LOOCV is a very common way to evaluate statistical predictions; such a detailed description is not needed.

L227: If I understood correctly, Lussana et al., 2017 has only been submitted, and thus not qualify as a reference.

L233 -> What is the spatial domain of the study? This has not been mentioned in the text! You use stations across entire Fenno, but show maps of just Norway? Have you run your interpolation for Fenno domain and then clipped the data to cover Norway? In addition, “blending several regional estimates. . .” is very unclear; how you fitted multiple trend models for different areas and then combined them somehow?

L235: “non-linear parametric function”, does this mean higher order polynomial terms? Please specify. In addition, why have you not considered predictors such as lat-lon or distance to sea in your model, as these are very commonly found from interpolation models, and could improve the analyses also in Norway?

L239-247: Your interpolation approach is a very complicated and a figure summarizing the different steps could be helpful. Please consider adding a figure to the supplementary material.

L248-249: Here, could you a bit more specific what you mean by “spatial consistency test”?

L248: “gross” -> “cross”?

C4

L269-271: Consider omitting this small outlook paragraph.

Chapter 2.3.2 Iterative Optimal Interpolation is very complicated and hard to follow, but I guess it's alright to provide a detailed description of the approach.

Chapter 3: why the evaluation period was limited to 2000-15, as the entire period is substantially longer?

L372: "statics" -> "statistics"

L374-378: From the ms you get an impression that the present study validates both temp and prec data, but it seems that temp data has been validated already in previous studies.

L379-386: consider omitting the entire paragraph.

L388-389: Again, different period for prec validation is given; why not to use the same period for each analysis as this is somewhat confusing?

Figure 2 has not been cited in the text.

L399-401: This seems to be discussion material. The results should be limited to briefly presenting the main results of the study.

How the interpolation uncertainty has been estimated, as currently you present the accuracy of the model at stations' locations?

Should the equations 10 and 11 to be moved to material and methods sections (see my earlier comments of the ms structure).

3.2. Evaluating the precipitation fields using. . . First of all, is this actually evaluation or simply just description of the differences between the two datasets? Why old dataset needs to be evaluated again? Modis data should be described in material and methods, not in results.

L458-459: "The regression lines. . .", consider omitting.

C5

L467: "Less accurate but more precise", what does this mean in this context?

L477-483: this paragraph seems to discussion material.

L 477: I don't understand why you expect "the ratio between prec and runoff to be high"? As this ratio is dependent on multiple factors (e.g. land use, topography, permafrost, etc..) I don't understand the purpose of this examination.

L492-494: Have you really addressed the spatial uncertainty in the estimates?

L501-506: Please move to e.g. Introduction. Chapter 3 seems to be the results section, where the key findings are supposed to be clearly presented.

Please open abbreviation SWE.

On my opinion, this paper is not about snow modelling, so please consider moving the excess details about the snow model (chapter 3.3) to supplementary material. In addition, any description of data or methods should appear in material and methods section with appropriate subheadings.

Chapter 3.4. Again new model and data is introduced for the reader; it feels whether there are too much material for one paper.

Figure 13 needs to be cited before figure 14.

Chapter 3.5. -> 4. Discussion. Consequently, 4. Conclusions -> 5. Conclusions.

L628: do the reported values indicated uncertainty or cross-validation error?

L634: again, what are you referring with "accurate" and "precision". It could be so that I just don't understand, but this could mean a danger for miss-interpretation by other readers as well.

L664: could you provide an example where the areas of low station density are? Conclusion chapter is overly long. I would prefer one (max two) paragraph with the key findings presented in a compact manner.

C6

Figure 1: Add lat-lon information to either one of the plots. Also it might be a good idea to include country names.

Again I'm puzzled with the analysis domain of the study, as here it seems that you use data from entire Fennoscandia. But this would only make sense if you would interpolated across the entire Fenno domain. Still you present the results only for Norway.

Why again different time period 1971-2000, since the analysis period is longer than this? In the caption, can you indicate the number of stations?

Figure 2: Related to previous comments, how have you decided this domain? Has this been clipped from a larger dataset? This figure has not been cited in the text. Moreover, is such a loose example (basically just two random dates) even needed at all? What are the white areas in Norway for precipitation? I guess no data (i.e. no prec), but this has not been said anywhere.

Figure 3: "distribution" -> "Distribution". Please define the meaning of boxplots (whiskers etc). What does the "CV-Analysis" in the y-axel stand for? I would assume predicted values, but again there is no mentioning about it.

Figure 4: I don't understand this figure; is this precipitation over some threshold etc? If so, why not more maps with varying thresholds?

Figure 5: This should be moved to supplementary material, as this is not vital information for the paper.

Figure 6: This partly relates to limited understanding of the purpose of hydrological investigation; what is the main message you want to deliver with this figure? Ok, the two datasets give a different result, but which one is correct? Why not to just present the observed and predicted prec values for the two datasets?

Figure 7: I don't see what this figure tells us about the model performance (although it seems that there are differences among the datasets), as it only gives you an idea

C7

about local hydrological conditions.

Figure 8: Again, there is obviously differences between the two data, but I guess that is all this figure is telling us, right?

Figure 9: These maps don't tell us anything about the quality of the precipitation data (which should be validated in this paper). It gives us an idea about the local hydrological conditions. Due to land use, topography, ground water etc you cannot assume the correlation to be high, especially at non-urban areas.

Figure 10: What do the lines depict, mean/median? Or the shaded polygons? What is the main message of this plot, as it seems that both dataset provide relatively similar correlation (I still don't see why this correlation is important in this context. . .).

Figure 11: What is the value-added of this regional inspection i.e. do they represent some particular different conditions?

Figure 13: With what model has these been simulated with? What do the plots tell us about the quality of seNorge2 precipitation data?

Figure 14: Here and other captions; please open the meaning of abbreviations (here P_{corr}).

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2017-64>, 2017.

C8