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Comment on essd-2020-327

M. Tomas-Burguera (Referee)

Referee comment on "SLOCLIM: a high-resolution daily gridded precipitation and temperature dataset for Slovenia" by Nina Škrk et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2020-327-RC2, 2021

In this paper the authors present a high-resolution (1 x 1 km) dataset of daily precipitation and temperature for Slovenia for the 1950-2018 period. The interest of this dataset is high as it covers a gap in the climatology of Slovenia, a region with both climate and terrain complexity, as mentioned in the manuscript.

The authors used a previously designed methodology by two of them, that has already been tested in Spain and other regions.This methodology is based on estimating daily data of the variable of interest by using GLMs/GLMMs, daily data of the 10 closest weather stations and geographic information, such as latitude, longitude, altitude and distance from the coast. The main advantage of this methodology is that all available data can be used, even if weather stations cover short periods.

The paper is well written and the use of climate terminology is adequate.

I also consider the paper is correctly distributed in their sectioning, and the authors provided different verification tests of the variables, both in time and space.

I would like to make some **general comments** to this paper.

- While the authors used a method based on the spatial structure of climate data, other methods relies on the temporal structure of climate data to estimate missing data.

My first comment is related with this point, as I would like to know the opinion of the authors regarding strengths and drawbacks of the methodology they used compared with

methods based on the temporal structure of the climate data.

- Also regarding the methodology. If I have understood correctly, the authors first estimated missing data of weather stations and in another iteration of the algorithm they estimated climate data at each grid cell of 1 x 1 km. If this is correct, it turns out the authors used estimated data as if they were observed data. Is this correct? If so, what is the possible impact of this on the obtained results in the opinion of the authors?

- Slovenia is a complex territory, with high elevation areas. Unfortunately, only a few number of stations are available above 1000 m. While some of the validation tests are shown in terms of elevation, some discussion regarding its possible impact on the obtained dataset is missing.

- Station network changes all over the period, with an initial increase in the number of stations followed by a constant decrease after the 80's. This constant change on the number of true observations can have a great impact on the quality of obtained results. While validation of the results is presented by altitudes and by months, why the authors did not provide a validation based on decades (for example)? I think it could be really interesting to assess the impact of changes in the station network in the obtained results.

- One of the key points of the used methodology is the estimation of the uncertainty of each estimated data. The authors should provide more information regarding the calculation of the uncertainty and some kind of validation of the estimated uncertainty could also be provided. Maybe a comparison between uncertainty of estimated data and MAE? A temporal perspective of the estimated uncertainty would also be of high interest to evaluate the impact of changes in station network on the uncertainty of obtained data.

I also have some **specific comments**

Lines 81-82. "Although some data series begin before 1950, we decided to limit the research to the years 1950 to 2018, when the station network remained stable over time and space". I don't fully understand this sentence. When the authors said that the station network remained stable over time and space, what exactly do they refer to? In figure 2 it is clear that the number of stations reduced constantly from the maximum number slightly higher than 100 to a number of only 20 in 2018.

- In Figure 9 and figure 11 the authors provided the estimated uncertainty for some derived indices of temperature and rainfall. While in the methodology section the authors mentioned how they estimated the uncertainty of each estimated data of temperature and rainfall, how the authors derived the uncertainty of the indices?

I would like to make some comments/suggestions regarding some of the thresholds used in temperature quality control.

- “For temperature we used also five criteria: (1) internal coherence;” I assume the authors refer to the coherence between Tmax and Tmin when they write “internal coherence”. This should be clarified in the manuscript. Just for curiosity, is this coherence test based on Tmax > Tmin or Tmax ≥ Tmin?

- The following two comments are more a suggestion for successive works or some update of the database than comments to modify this manuscript.

I think the thresholds used to classify a daily data of temperature as an “out of range” data could be enhanced, especially those used for minimum temperature.

While in the introduction, the authors says. “Moreover, temperature ranges from -35 to +40 °C (BertalaniÄ□ et al., 2006) show the very extreme character of the seasons”. Then, in the explanation of the quality control they say: “(3) removal of those days out of range considering maximum temperature (TMAX) ≥ 50 °C or TMAX ≤ −30 °C and minimum temperature (TMIN ≥ 40 °C or TMIN ≤ −35 °C)”

If temperature ranges from -35 to +40ºC, the consideration of Tmin= -35ºC as an out of range value could imply deleting real extreme values. On the other hand, I consider as too high the Tmin>= 40ºC threshold.

- “;(4) removal of all days in a month with a standard deviation equal to zero (suspect repeated values in the series);” With this criterion, the authors only considered repeated values when all the month has the same value. Well, this criterion is as valid as others, but I think this could be easily modified to be more restrictive. Many different thresholds are used in the literature, but when temperature data with decimal precision is provided, 10 consecutive days showing the same temperature are very unlikely. I would suggest the authors to use 7-10 consecutive days as a new threshold.

Table 5.- December is missing.

Figure 1.- As the authors mentioned the complexity of terrain in Slovenia in the manuscript, they could represent a digital elevation model in this figure to help the readers of the manuscript to better understand this complexity. In this figure, the duration of data of each meteorological station could be also represented.