

General Remarks for *García-Valdecasas Ojeda et al.*

In this study, the authors present a new high-resolution dataset over Sierra Nevada in the Iberian Peninsula for the period 1991-2022. The dataset was created with the WRF model, and ERA5 reanalysis data provided the initial and boundary conditions. To test the accuracy of the model, many indices were compared against alternative datasets based on observations and satellite data. The reliable performance of the dataset is highlighted throughout the manuscript. This dataset is valuable for the climate research community working on high mountain environments, but also for other areas such as biology or ecology.

The manuscript follows a logical structure and fits into the scope of Earth System Science Data. However, the authors need to address some comments before it is ready for publication.

Major comments:

Not related to any section:

- After reading the paper, it is unclear if the variables are provided in the original Lambert grid from WRF, or if they have been interpolated to a regular lon lat grid. That should be clearly stated in the text. That is my concern since wind speed is calculated using equation 5, and both components depend on the grid in which they are provided. For a regular lon lat grid, the winds should be rotated from WRF's Lambert grid.

Section 2.2.

- The authors could briefly explain the model sensitivity analysis that led to the selected configuration in WRF. Only the reference is provided in the current version, but the manuscript can improve if more details are provided. For example: Was the analysis based on temperature? Precipitation? Other variables?
- Line 111 mentions the trade-off between suitability and computational resources, but Table 1 provides specific time steps for each domain. WRF can run using an adaptive time step to reduce the simulations' running time. Was the simulation created using that option? If not, is there any reason why the authors did not use it?
- I would consider a 5 km spatial resolution already a convective scale. Thus, I do not see a reason to use the convection parameterization in the first domain of the model. Did the authors test that alternative?

Section 2.3.

- Could the authors provide briefly more details about the interpolation method used in the RegRAIN package? That could ease the reading and understanding of that part of the text.

Sections 2.4.4, 2.4.5, and 2.4.7

- I am not sure if the units of the variables related to precipitation are correctly stated in these two sections. For example, Precipitation is defined between brackets as kg/m², but then in the explanation, it is defined as mm/hour. This also happens in BIO12, BIO13, BIO19, Wet-hour Intensity and Maximum amount of precipitation in the wettest month. Could the authors check these mismatches?
- Is the definition of the Simple Daily Intensity Index correct? It is defined as the mean annual pr when pr >1mm, but I would guess that it should be related to daily values instead of annual values. Please check that.

Section 3.1

- The manuscript would benefit from a short explanation of the pseudo-PDFs. More concretely, why and how they are calculated.
- Lines 317-320: I missed a comment about the shape of the probability distribution function shown by ERA5-Land compared to the other datasets. I would say that it is also different around 20°C.

Section 3.2

- Line 353: Can the authors elaborate more on what they mean by “amount of energy”? That sentence is referred to BIO1, and that is the annual mean temperature, so it is not an energy.
- Figure 4: There is a mismatch between the labels in the Figure and the Caption. UGR-SNGrid is labelled as GFAT-Grid in the figure. The same happens in Figure S7 in the supplementary.

Minor comments:

- References should be listed in chronological order throughout the manuscript (e.g., lines 46,48-49, 68-69, etc).
- Line 76: I would start a new paragraph to explain the structure of the publication.
- Line 110: initial soil moisture conditions or soil moisture initial conditions.
- Line 156: 200 m instead of two hundred.
- Table 2: I printed the PDF and it seems that there is a problem with the formatting of the text related to the coverage of CHIRPS. It appears as bold.
- Table 2: Define t_a in the caption, as it is not explained in the text yet. In this version, only pr, tasmax and tasmin are explained in the caption.
- Line 200: kg*kg⁻¹
- Lines 238-239: “..., and then minimum values for each year are taken AS the minimum...”

- Line 270: I suggest removing the * as the note is in the following line.
- Page 13: Table 3 is defined as Table 1 again
- Line 334: The highest precipitation is found in December, right?
- Line 397 – Caption Fig. 4: It states temperature, but it should be precipitation recorded by the SAIH stations.
- Line 418: BIO5, BIO8 and BIO9, according to the subfigures mentioned in that line.
- Lines 337-338: Something is missing in the sentence. Otherwise, I suggest rewriting it.
- Line 455: With values between 30 and 140 mm.