

## Reply to Anonymous Referee #2

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.

**Reply:** Authors thank the reviewer for reading the manuscript and taking the time to review it, asking critical questions, which will help us to improve the quality of the manuscript. This comment will be therefore included in the acknowledgements section. We have responded to the referee's request point by point indicating the actions to be taken in the final revision of the manuscript. All our replies are included in blue.

### Major comments:

- (1) 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.

**Reply:** we are sorry for the misunderstanding. The variables are provided according to the original Mercator grid provided by the model. In this regard we will add a clarification in the new version of the manuscript saying that the data are in their original mesh and therefore it has not been necessary to rotate the wind.

- (2) 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?

**Reply:** The sensitivity study aimed to analyze the WRF model performance over Andalusia (southern Spain) using 12 1-year simulations resulting from combining different microphysics and cumulus options in the parent domain (d01) (i.e., the cumulus scheme was only switched off in the inner domain). These experiments were evaluated in terms of precipitation and maximum and minimum temperatures using

different data as reference. Among the different options evaluated in this study in relation to the clusters, the option of using convection off in the parent domain (d01) was also tested, as indicated by the reviewer, but, although this was one of the options that obtained better results, in general, the use of Grell Freitas (GF) convection in d01 seemed to yield more adequate results in all the analyzed variables. Regarding this study, as suggested by the reviewer, we will add more details about the sensitivity study performed in the new version of the manuscript.

On the other hand, we use a fixed time-step following the recommendations of the WRF model developers (6\* $\Delta x$  spatial resolution). This type of timestep is widely used in WRF in convection-permitting simulations and has been shown to show adequate results in the IP. However, it could be an aspect to investigate for future studies, thank you for this suggestion. In any case, this type of time step should be used with caution and as indicated in some studies (e.g., De Morais and Guerrero, 2018) seems to be less advisable than the use of fixed values. In addition, it seems that in long simulations there can be marked differences between the results of using one or the other type of time step, an aspect not solved at least until version 4.2 (see the <https://forum.mmm.ucar.edu/threads/different-results-between-adaptive-time-step-and-constant-dt.9186/>).

Reference:

De Morais, M.V.B., and Guerrero, V.V.U. (2018). Analysis of Computational Performance and Adaptive Time Step for Numerical Weather Prediction Models. *Int. J. Eng. Math. Model*, 2018, 1–8.

- (3) 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.

**Reply:** RegRAIN is a regionalized rain interpolator model based on the Regionalisierte Niederschläge (REGNIE) method (Rauthe et al., 2013). REGNIE is a combination between multiple linear regression (MLE) considering orographical conditions (e.g., latitude and longitude, slope, and elevation) and inverse distance weighting. To do that, a digital elevation model (DEM) is used together with monthly precipitation time series from stations. These details will be included in the new version of the manuscript in Section 2.3 according to the reviewer's suggestion.

Reference:

Rauthe, M., Steiner, H., U., Riediger, A., Mazurkiewicz, A., & Gratzki, A. (2013). A Central European precipitation climatology–Part I: Generation and validation of a high-resolution gridded daily data set (HYRAS). *Meteorologische Zeitschrift*, 22(3), 235–256. <https://doi.org/10.1127/0941-2948/2013/0436>

- (4) 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/m<sup>2</sup>, 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.

**Reply:** Thank you. This was a mistake that will be corrected in the new version of the manuscript. Precipitation variables as reported in ... Protocol is given in  $\text{kg m}^{-2}$ . SDII is the sum of daily precipitation for a given period of time (here we used a year) divided by wet days (days with  $pr > 1 \text{ mm}$ ). We agree with the reviewer and the definition will be changed to:

*“SDII calculates the mean  $pr$  for wet days ( $pr > 1 \text{ mm}$ ) ...”*

- (5) 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  $20^\circ\text{C}$ .

**Reply:** As suggested by the reviewer, and also by the first referee, more detail will be given on how the pseudo-PDFs were made.

- (6) 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

**Reply:** This sentence is according to the definition in other works such as Noce et al. (2020), where BIO1 is defined as “the total amount of energy inputs for the ecosystems in a year” so with energy we wanted to say energy available for ecosystems. We will rewrite this part in the new version of the manuscript to clarify this point.

- (7) 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.

**Reply:** Thank you for pointing out this error, which will be corrected in the new version of the manuscript.

#### **Minor comments:**

- References should be listed in chronological order throughout the manuscript (e.g., lines 46,48-49, 68-69, etc).

**Reply:** According to the in-text citations rules (<https://www.earth-system-science-data.net/submission.html#references>), the order can be based on relevance, as well as chronological or alphabetical listing, depending on the author's preference. Here an alphabetical order is chosen.

- Line 76: I would start a new paragraph to explain the structure of the publication.

**Reply:** as suggested by the reviewer, this change will be made in the new version of the manuscript.

- Line 110: initial soil moisture conditions or soil moisture initial conditions.

**Reply:** In this case we intend to say, initial soil moisture conditions. This will be changed in the new version of the manuscript.

- Line 156: 200 m instead of two hundred.

**Reply:** This aspect will be changed according to the referee's suggestion in the new version of the manuscript.

- 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.

**Reply:** We have checked this text and it is not in bold.

- Table 2: Define  $t_a$  in the caption, as it is not explained in the text yet. In this version, only  $p_r$ ,  $t_{smax}$  and  $t_{smin}$  are explained in the caption.  
**Reply:**  $t_a$  (mean temperature) will be defined in the new version of the manuscript.
- Line 200:  $kg \cdot kg^{-1}$   
**Reply:** This will be fixed according to the referee's suggestion in the new version of the manuscript.
- Lines 238-239: "..., and then minimum values for each year are taken AS the minimum..."  
**Reply:** This will be fixed according to the referee's suggestion in the new version of the manuscript.
- Line 270: I suggest removing the \* as the note is in the following line.  
**Reply:** The asterisk was added because it is a slightly different definition from the conventional one. However, as suggested by the reviewer, the asterisk will be removed.
- Page 13: Table 3 is defined as Table 1 again  
**Reply:** This will be fixed according to the referee's suggestion in the new version of the manuscript.
- Line 334: The highest precipitation is found in December, right?  
**Reply:** right, this will be fixed according to the referee's suggestion in the new version of the manuscript.
- Line 397 – Caption Fig. 4: It states temperature, but it should be precipitation recorded by the SAIH stations.  
**Reply:** right, this will be fixed according to the referee's suggestion in the new version of the manuscript.
- Line 418: BIO5, BIO8 and BIO9, according to the subfigures mentioned in that line.  
**Reply:** right, this will be fixed according to the referee's suggestion in the new version of the manuscript.
- Lines 337-338: Something is missing in the sentence. Otherwise, I suggest rewriting it.  
**Reply:** The sentence will be rewritten according to the reviewer's suggestion.
- Line 455: With values between 30 and 140 mm.  
**Reply:** This will be changed according to the referee's suggestion in the new version of the manuscript.