Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-53-AC3, 2020

Hydrology and Earth System Sciences Discussions

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Interactive comment

Interactive comment on "Changes in the simulation of instability indices over the Iberian Peninsula due to the use of 3DVAR data assimilation" *by* Santos J. González-Rojí et al.

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Received and published: 4 June 2020

Reply by authors starts with the symbol ».

In their study, González-Rojí et al. investigate three different convective parameters obtained from two dynamically downscaled WRF model runs over the Iberian Peninsula. Over a 5-year period, the convective parameters from the WRF runs are quantitatively evaluated with sounding data and spatially investigated for different seasons. In addition, the spatial distribution and variability of the convective parameters is investigated and related to certain precipitation characteristics from the literature. The authors found that WRF runs with 4Dvar assimilation best reflect the convective situation.

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Discussion paper

» We point the reviewer that we have used 3DVAR data assimilation.

Overall, the work is well structured and written with a good balance of text and figures. My main concern is that large parts of the paper are rather descriptive in the sense that mainly the figures are described and not interpreted. Reasons for the discrepancies found between the data sets are not given - although that would be most interesting and would increases the scientific value of the paper. In the current version, the benefit of the work for a larger community remains unclear. In the following you find a list of major and minor points as well as some suggestions for editing.

» Thanks for your comments.

Major revision points:

1.) After reading the paper, more questions arise than answers or new scientific insights are given. This is because the paper mainly describes the figures, but does not provide explanations. Questions are: Why do the assimilation runs perform better compared to the simple WRF downscaling? Since the convective parameters considered depend on both temperature gradient and moisture, hat is better reproduced? On which levels/layers? Depending on the location (sounding station) and the season? Why are the differences between the models greater at some stations than at others (depending on the parameter)? What is the relation between CAPE and TT index?

» The simulation including data assimilation produces more reliable results than the one without it. This conclusion is extracted from the paper after the analysis of the convective indices studied, after comparing the results from both WRF experiments against the ones obtained from Wyoming University (also against IGRA radiosondes as shown in one of the comments posted in the website).

» The differences between WRF experiments are due to the effect of data assimilation in the the vertical profiles of temperature and mixing ratio. The effect of the assimilation is not restricted to the surface, and it is propagated towards the top of the atmosphere and the nearby grid points due to the optimization of the cost function (Barker et al., 2004, 2012). Additionally, as presented in previous studies (already cited in the manuscript in sections 2.1 and 2.2), the effect is also observed in the soil moisture and both surface temperature and moisture. As presented in González-Rojí et al. (2018), data assimilation is important at 12 UTC for moisture, and at 00 and 12 UTC for temperature, and their effects are important in the southeastern IP and both Guadalquivir and Ebro basins (see their Figure 13). This pattern is consistent along the seasons, but its intensity varies seasonally (stronger during summer than in winter). As presented in González-Rojí et al. (2020), the soil moisture content is also different in both simulations as a result of the data assimilation (this variable is not assimilated, and data assimilation is the only difference in the configuration of the model).

» The main objective of our paper is neither to find a relation between the studied convective indices over the IP nor their performance as predictors of heavy rainfall events. We only want to evaluate how well the values of each index are simulated by comparing the results from two different configurations of the model to observational data, and to study the differences in the seasonal patterns due to the use of a data assimilation step in the numerical downscaling phase. There are not many studies analyzing this currently.

» ———————————————————

» González-Rojí, S. J., Sáenz, J., Ibarra-Berastegi, G., & Díaz de Argandoña, J. (2018). Moisture balance over the Iberian Peninsula according to a regional climate model: The impact of 3DVAR data assimilation. Journal of Geophysical Research: Atmospheres, 123(2), 708-729.

» González-Rojí, S. J., Sáenz, J., Díaz de Argandoña, J., & Ibarra-Berastegi, G. (2020). Moisture Recycling over the Iberian Peninsula: The Impact of 3DVAR Data Assimilation. Atmosphere, 11(1), 19.

» Barker, D. M., Huang, W., Guo, Y. R., Bourgeois, A. J., & Xiao, Q. N. (2004). A threedimensional variational data assimilation system for MM5: Implementation and initial results. Monthly Weather Review, 132(4), 897-914.

» Barker, D., Huang, X. Y., Liu, Z., Auligné, T., Zhang, X., Rugg, S., ... & Demirtas, M. (2012). The weather research and forecasting model's community variational/ensemble data assimilation system: WRFDA. Bulletin of the American Meteorological Society, 93(6), 831-843.

2.) The main conclusion of the paper is that the assimilation run performs better compared to the run without assimilation. But is this not to be expected if soundings are assimilated for which the comparison is made afterwards? What would be the result if you left out some of the soundings for the assimilation and made the comparison for these locations?

» The paper supports the idea that the experiment including data assimilation performs better than the one without, similar conclusion to what we have observed for other variables in previous studies by the authors. However, in this case, the main conclusion of the paper is that important differences arise in those patterns only due to data assimilation. The impact of data assimilation is not limited to the grid cells close to the location of the soundings. As shown in the Figures of our paper, the changes extend over large areas of the Iberian Peninsula despite the limited coverage by soundings.

» It is true that the comparison against assimilated soundings can be biased, but we can not discard observations when preparing the simulations without performing a damage to the study that we want to perform. On the other side, as we mentioned before, we are analyzing derived variables not directly assimilated on a regional domain covering places with no observation at all. We are mainly comparing the values of different convective indices after different calculation methods (as the method followed by Wyoming and our method included in the package aiRthermo). Additionally, as an extra way of validating our results, we always compared the values obtained in the patterns over the entire IP with previous studies focusing in the region (or at least covering it even if it is with low resolution data).

3.) Are you sure that ERA-Interim did not originally assimilate the eight soundings you considered? It does not make sense to assimilate any data set twice.

» We did not check every cycle of six hours all the data assimilated by ERA-Interim, as we think it is pointless. We actually assume that some of these radiosondes have very likely already been assimilated in ERA Interim reanalysis. However, that is not a problem for our simulations with WRF as we only used the data from ERA-Interim as boundary conditions for our regional model after the initial run. Since the run which used ERA Interim for initial conditions (January 1st, 2009) corresponded to one year before the period that we started analyzing the output (January 1st, 2010), we can be sure that the interior of the domain is reflecting the variability corresponding to the regional climate model.

» Moreover, the effect of assimilating one station in ERA-Interim, which has a resolution of around 80 km, cannot be comparable to the effect of assimilating a station in a domain with 15 km. Besides that, the original objective of our paper was to compare the quality of WRF simulations and ERA Interim is only used to provide initial and boundary conditions to WRF.

4.) Either there is a general misunderstanding of convection triggering or the formulations are clumsy. Convective instability and sufficient moisture at lower levels are necessary but not sufficient conditions for the development of convective storm. Convection initiation requires additionally a lifting mechanisms that either reduces CIN or lift a parcel to the level of free convection (LFC). High CAPE/TT values neither trigger convection nor can they directly be related to precipitation as written several times throughout the manuscript.

» To some extent, we agree with the reviewer. Convective instability and moisture in low levels of the atmosphere are ingredients necessary to trigger convective storms, and consequently, convective precipitation. However, the final ingredient, which is the lifting, is provided by the instability, forced by orography, the convergence of horizontal moisture fluxes or the breezes in coastal regions. All this information is included already in the second paragraph of the introduction of our paper, so we agree with the reviewer on that.

» In order to avoid misleading ideas by the readers, we have carefully rewritten all the sentences highlighted by the reviewer in the new version of the manuscript.

5.) CIN works only in conjunction with CAPE. In case of zero CAPE, CIN doesn't matter for convective initiation or development. Analyses of the mean values or the spatial distribution of CIN are useful only when considering days with a certain amount of CAPE (or instability in general).

» We agree to some extent with the reviewer on that. However, the objective of this paper is not to evaluate CAPE and CIN only for extreme events as tools to predict extreme convective rainfall. The objective of this paper is to evaluate the ability of WRF simulations (including or not the 3DVAR data assimilation step) to produce reliables values of TT, CAPE and CIN over the Iberian Peninsula, irrespective of whether they produce or not rainfall events.

» As stated already at the end of the Introduction, "the main objective of this paper is to evaluate the performance of two simulations created by using the WRF model at reproducing the atmospheric conditions that can trigger convective precipitation over the IP. To do so, the comparison of pseudo-soundings extracted from the model against real observations will be carried out." At the very end, what we are doing in the paper is to evaluate the Probability Density Functions (PDFs) of the three instability indices obtained in each experiment against the reference values measured by the University of Wyoming (also IGRA in the future version), but not only during extreme events.

» This clarification was added to the new version of the manuscript.

6.) Using only the nearest grid point to a sounding station neglects the horizontal drift of the radiosoundings. A better choice would be to consider the average value of several grid points.

» That is true to some extent. We agree that considering the nearest grid point for the comparison against a sounding is not always the best option. However, this depends on the spatial resolution of the domain of the simulations. Averaging several points can be a good idea when convection-permitting scales are used (below 5-3km), but not when the spatial resolution of the experiments is 15 km (as in our case). If we consider the average of the nearest grid points, we would be taking into account an area of 2025 km2 (45km x 45km), and that is too much for a comparison against station data.

» Additionally, according to recent studies (Xu et al., 2015), most of the vertical levels up to 6 km are already measured for a drifting distance of 7.5 km, independently of a clear or cloudy day (see their Figure 6). As also mentioned by the reviewer, both convective instability and sufficient moisture at lower levels are necessary for developing a convective storm, and these lower levels are already measured below 6km. Thus, taking into account our spatial resolution, we stand by our decision to use the nearest point to the station for comparison against station data.

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» Xu, G., Xi, B., Zhang, W., Cui, C., Dong, X., Liu, Y., and Yan, G. (2015), Comparison of atmospheric profiles between microwave radiometer retrievals and radiosonde soundings, J. Geophys. Res. Atmos., 120, 10,313– 10,323, doi:10.1002/2015JD023438. 7.) No reference is made on the original ERA-Interim fields. Thus it is not possible to assess the added value of the downscaled model runs and the need for higher resolutions of the data.

» The information about the original ERA-Interim fields was also asked by the comment published by a reader. As stated in his reply available online, we used 20 pressure levels downloaded from the MARS repository to feed the WRF model, which are: 5, 10, 20, 30, 50, 70, 100, 150, 200, 250, 300, 400, 500, 600, 700, 800, 900, 925, 950 and 1000 hPa. Our set-up, as stated in the manuscript, uses 51 vertical levels, so there is a relevant increase in the number of vertical levels compared to the data from ERA-Interim. Additionally, the spatial resolution of ERA-Interim is around 80 km, and our domain has 15 km resolution. Thus, we also improve the spatial resolution of the data.

» Taking into account this information already stated in the manuscript, we sincerely consider our simulations provide extra information to the one present in the Reanalysis. Additionally, these two experiments have been already validated against observational datasets (both for stations and grids) in previous studies by the authors, and in some cases, particularly for the experiment including data assimilation, they are able to outperform the driving reanalysis ERA-Interim. All these studies are cited at the end of section 2.1. We have not performed any quantitative analysis of the added value of these simulations since, as we have already stated before, we do not compare the performance of the WRF runs with the original data (see Figures 2 to 8 of the original manuscript, for instance). We are interested in comparing the performance of a run using 3DVAR with a different one which does not use it.

8.) The last section "Conclusions" is only a summary without any (general) conclusions. Tell us what other scientists may learn from your study.

» We do not agree with the reviewer. It includes all the important information extracted from the analysis performed, and it includes details about the comparison of both experiments regarding the indices TT, CAPE and CIN, not only in the location of the radiosondes but also for the entire IP.

9.) A thorough language check is necessary (e.g., "...observations in the stations..." or "obtained in stations" or similar formulations used throughout the manuscript are incorrect/weird).

» A detailed revision and edition of the language has been carried out in the new version of the manuscript.

» The reply to the minor revision points highlighted by the reviewer can be found in the supplementary document.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2020-53/hess-2020-53-AC3-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-53, 2020.

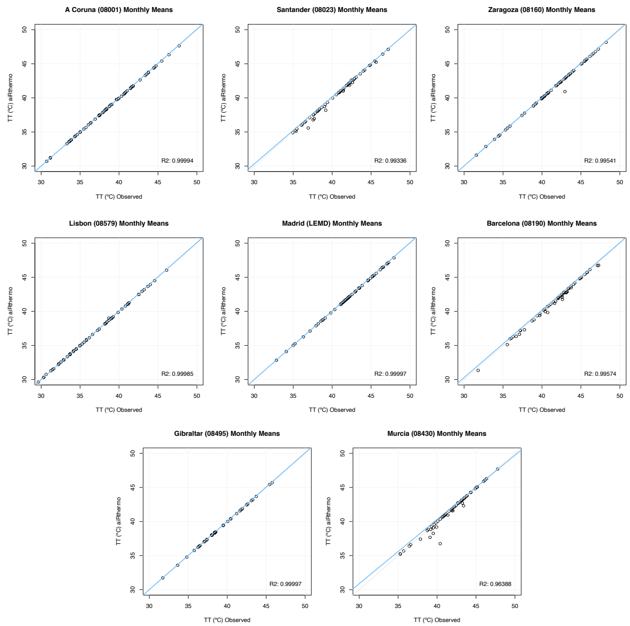

Fig. 1. Scatterplots for the monthly values of the observed values of TT (directly taken from the University of Wyoming) against the monthly values of TT calculated with aiRthermo.

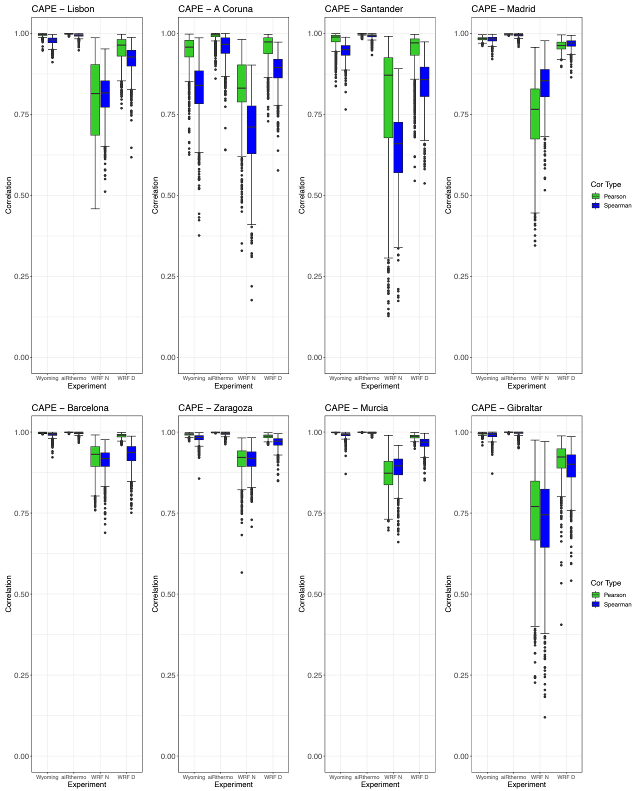

Fig. 2. Box and Whiskers for the correlations obtained during the bootstrap for CAPE and calculated by different methods: Pearson in green, and Spearman in blue. IGRA stations are taken as Reference.

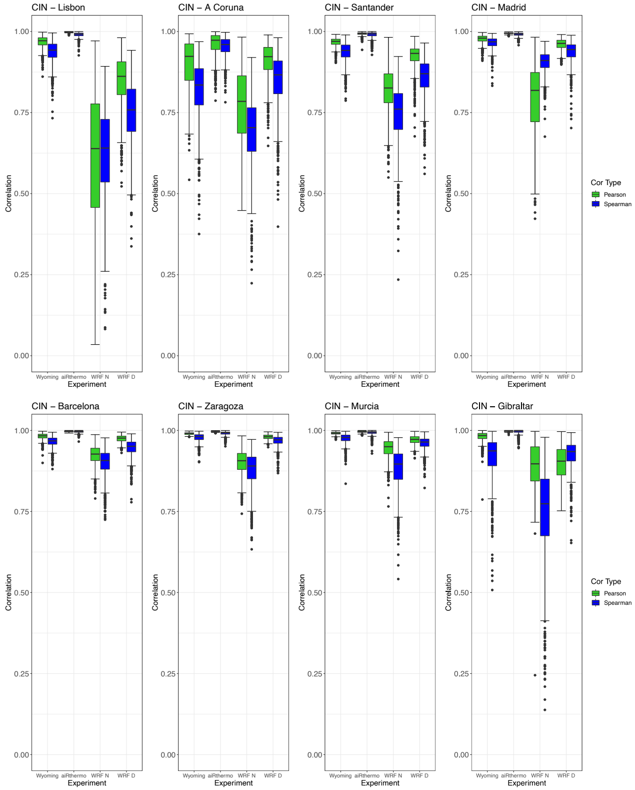

Fig. 3. Same as Figure 2 but for CIN.

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Discussion paper

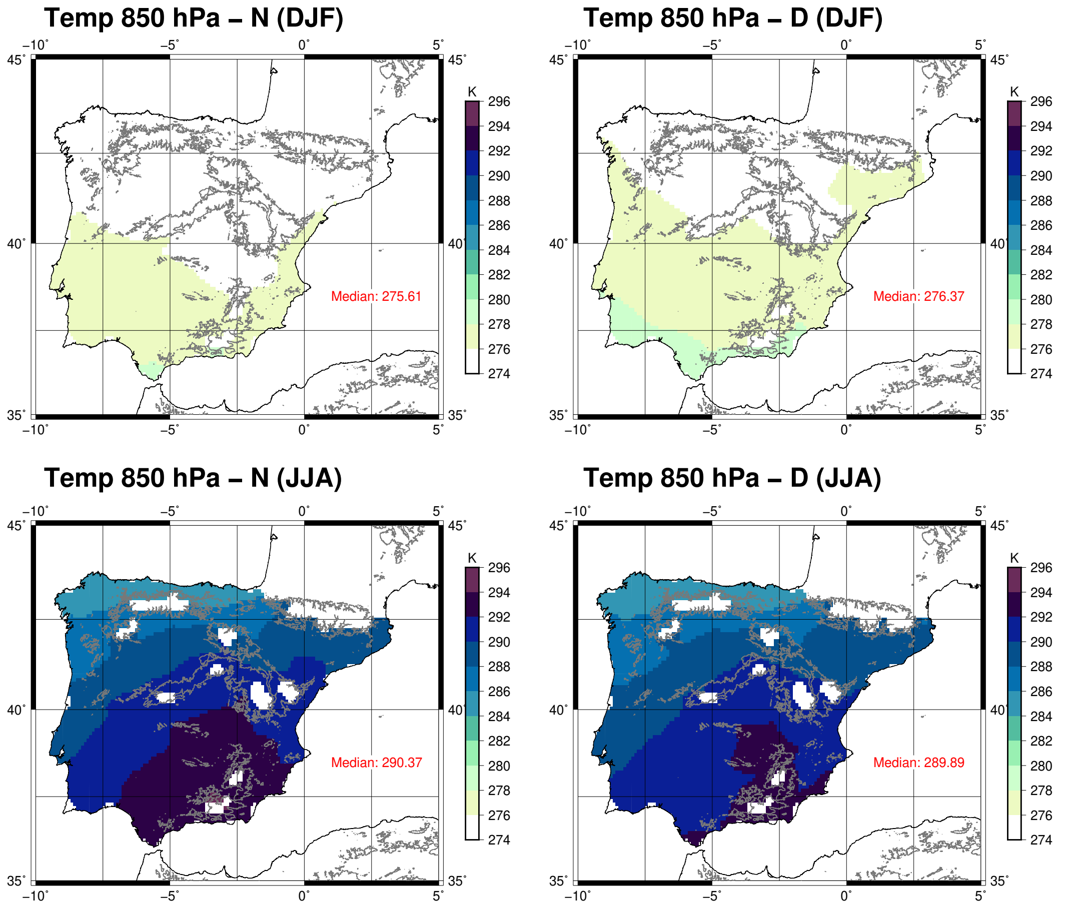

Fig. 4. Spatial distribution of mean Temperature at 850 hPa forSame as figure 4 but for dew point t period 2010-2014 over the IP as computed from N (first column) and D (second column) for winter and summer.

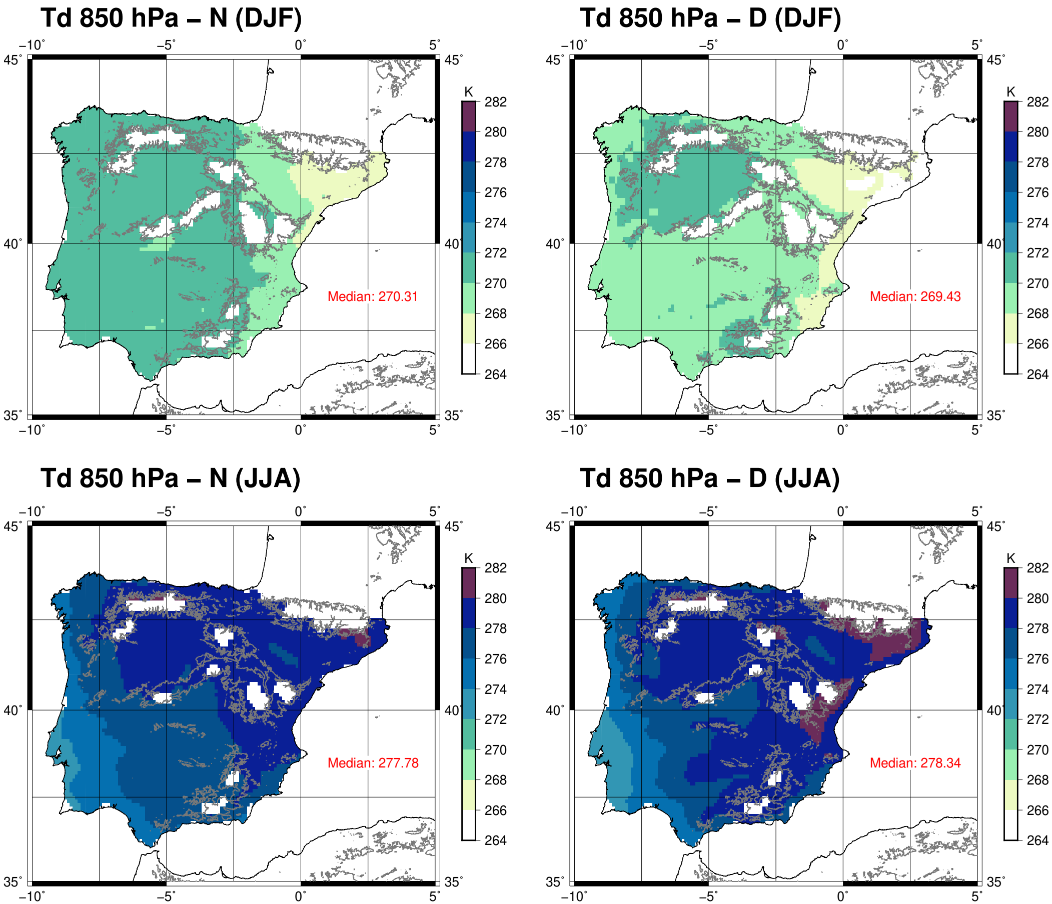

Fig. 5. Same as Figure 4 but for dew point temperature at 850 hPa.

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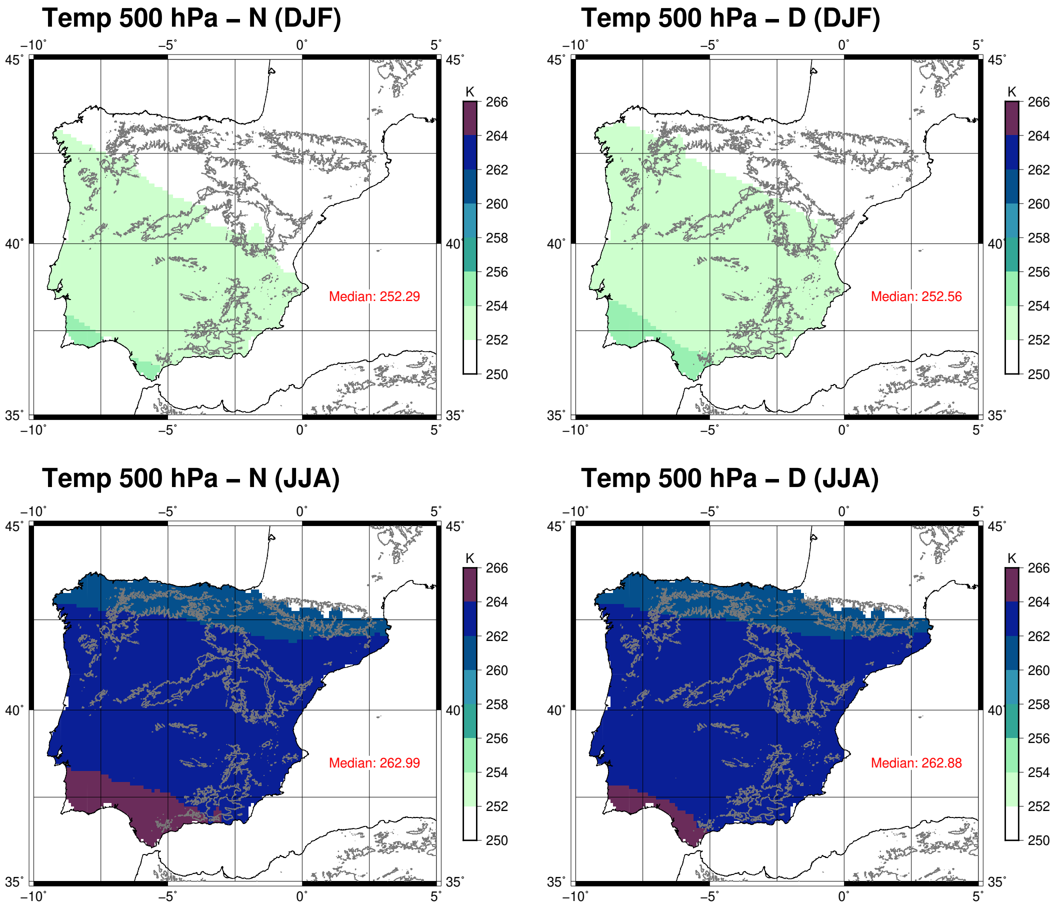

Fig. 6. Same as Figures 4 and 5 but for temperature at 500 hPa.

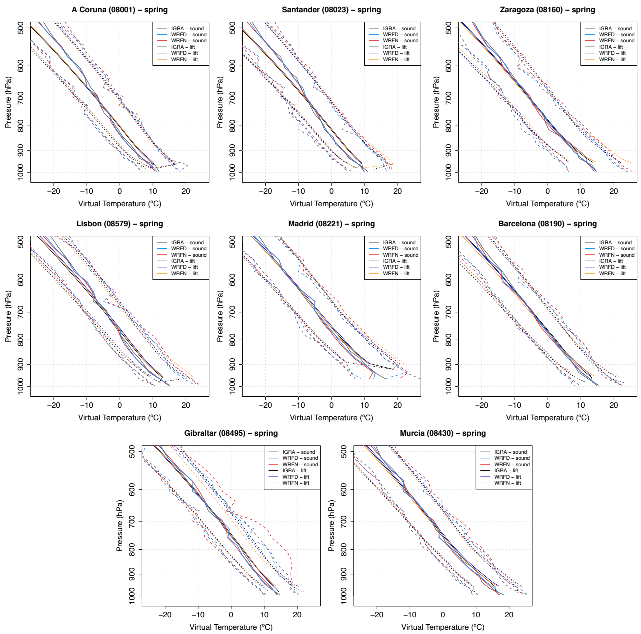

Fig. 7. Vertical profiles of virtual temperature for the sounding levels and for the lifted parcel during spring. The dashed lines represent the 5 and 95 percentiles, and the solid lines the median.

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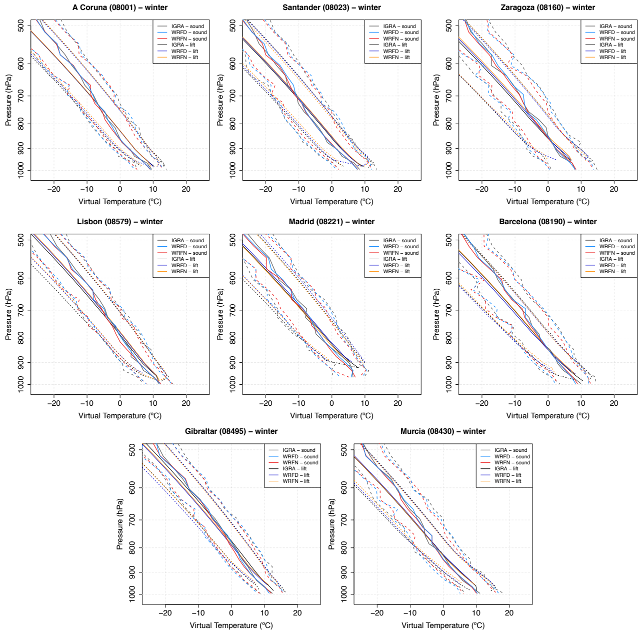

Fig. 8. Same as Figure 7 but for winter.

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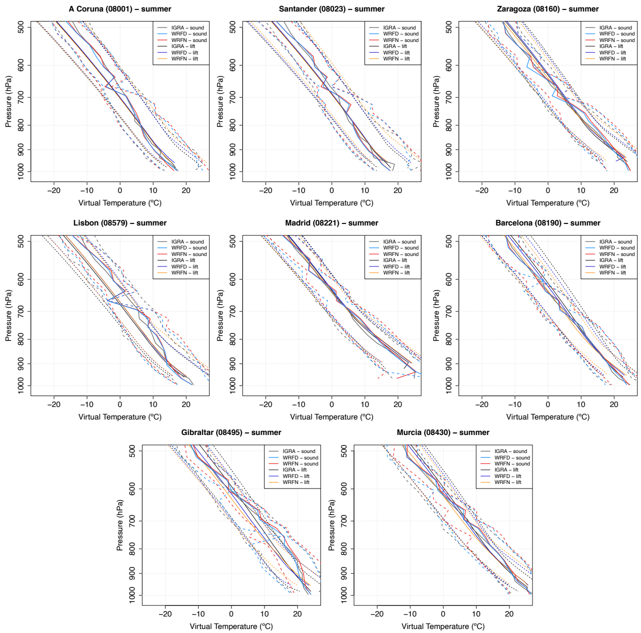

Fig. 9. Same as Figures 7 and 8 but for summer.

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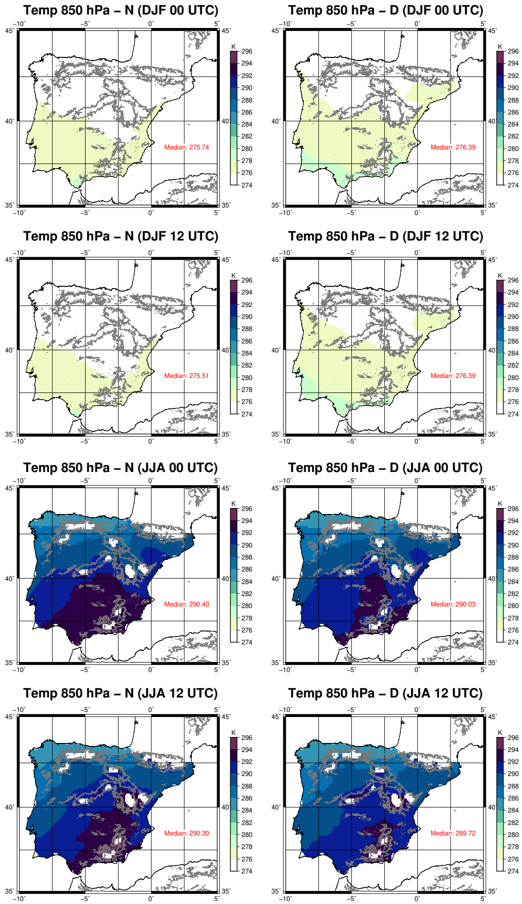

Fig. 10. Spatial distribution of mean Temperature at 850 hPa for period 2010-2014 over the IP as computed from N (first column) and D (second column) for winter and summer at 00 and 12 UTC.

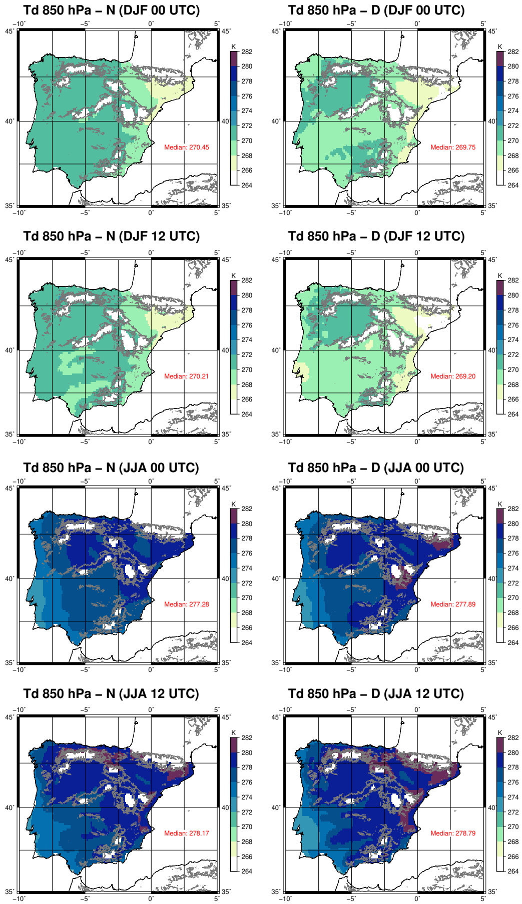

Fig. 11. Same as Figure 10 but for dew point temperature at 850 hPa.

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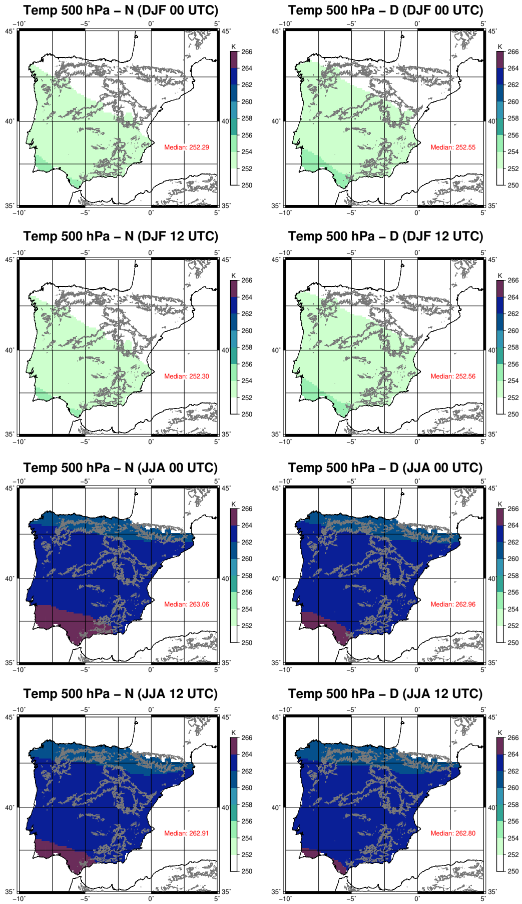

Fig. 12. Same as Figures 10 and 11 but for temperature at 500 hPa.

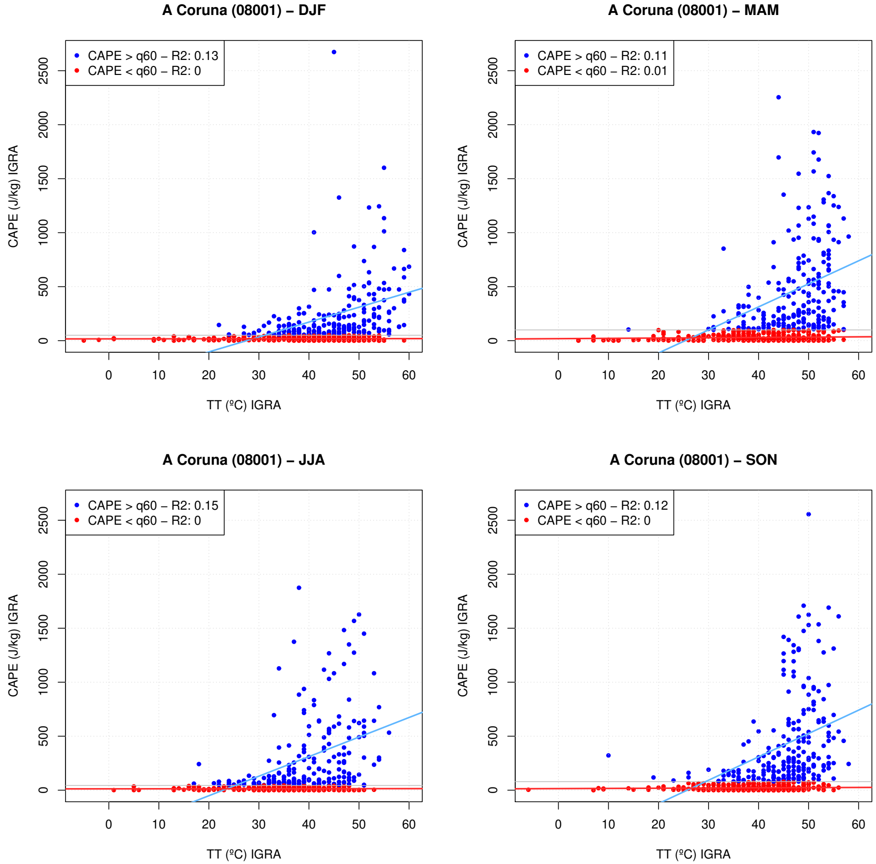

Fig. 13. Scatterplots for the values of CAPE and TT as included in IGRA for A Coruna. The values of CAPE over the 60th percentile are in blue, and the values below that value are in red.

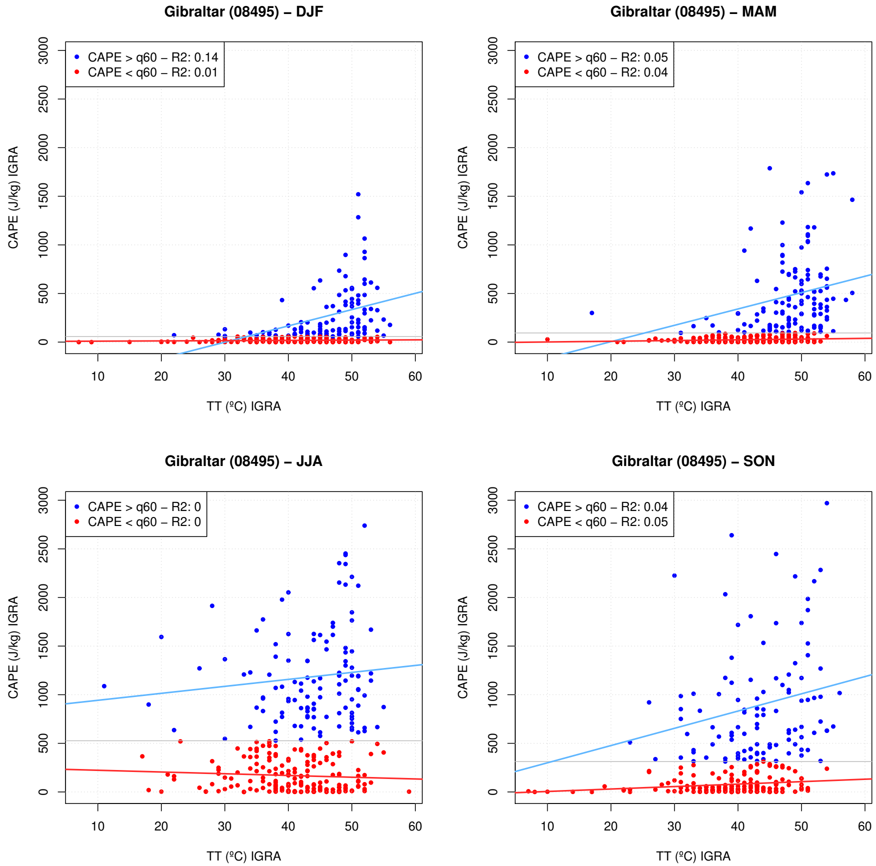

Fig. 14. Same as Figure 13, but for Gibraltar.