Dear Editor,

Please find enclosed the revised version of the manuscript entitled "Changes in the simulation of instability indices over the Iberian Peninsula due to the use of 3DVAR data assimilation" by S. J. González-Rojí, S. Carreno-Madinabeitia, J. Sáenz and G. Ibarra-Berastegi, that we resubmit to the journal *Hydrology and Earth System Sciences.* Please note that due to one of the comments of Reviewer#2, we have modified the title and now it is called: "Changes in the simulation of atmospheric instability over the Iberian Peninsula due to the use of 3DVAR data assimilation".

We consider that the previous version of the manuscript successfully addressed the comments by Reviewer#1, as he/she already accepted it for publication.

Regarding Reviewer#2, we consider that all the major points raised by this reviewer have thoroughly been addressed in the current version of the manuscript. Please, note that we disagree with this reviewer when he/she states that we do not want to "dig deeper". In our previous revised version of the manuscript we already included substantial new material such as Figures A1 to A6 (in the previous numbering) to answer his/her major point number one, or the justification of the selection of the nearest point and not the average to name a few without being exhaustive. In this new version, we have again included new panels to existing figures (A1 to A3) and two new figures (A6 and A8 in the current numbering), and substantial new text in order to give even more details about the causes of the differences between both experiments.

Attached to this cover letter you will find the new version of the manuscript, the version with the changes tracked, where all the thorough modifications are highlighted, and a detailed response to the reviewer's comments. Thus, we consider that we have successfully addressed all the points raised by this second reviewer and, as such, we hope that the manuscript can be accepted this time.

Yours faithfully,
Santos J. González-Rojí

Reply to 2nd Review made by the Anonymous Referee #2

Received: 11 November 2020

Reply by the authors is shown in blue, and starts with the symbol >>.

The authors have done a good job in addressing the comments of all reviewers and, as a result, the manuscript has improved considerably. However, I have still have some points that have to be considered before the paper can be accepted for publication.

To clarify, my intension was not to annoy you with my critical review, but rather to improve the scientific quality of the paper and make it more useful for a broader community. This is also an issue for a more or less pure meteorological topic to be published in a journal focusing on "fundamental and applied research that advances the understanding of hydrological systems, their role in providing water for ecosystems and society, and the role of the water cycle in the functioning of the Earth system" as HESS.

>> We never interpreted your comments as annoying and we have carefully re-read the replies that we submitted to your previous review, and we can't understand your paragraph above. It's true that we did not always agree with your suggestions but, in those cases, we always wrote the explicit reasons under our point of view (either target audience, main objective of the paper or availability of verification data, for instance).

>> Regarding the scope of the journal, we think that our study fits in HESS as it focuses on the distribution of instability indices over the Iberian Peninsula, which are variables that can be used to estimate the most unstable regions and where convective precipitation can be triggered if all the ingredients needed are fulfilled. Consequently, it falls in the one of the three points stated in the scopus of the journal, which is "the study of the spatial and temporal characteristics of the global water resources (solid, liquid, and vapour) and related budgets, in all compartments of the Earth system (atmosphere, oceans, estuaries, rivers, lakes, and land masses)."

I accept that according to several replies to my comments and questions, the objective is solely to show that the application of data assimilation improves the simulation of three convective variables in their region. The authors obviously do not want to dig deeper and scrutinize possible reasons for the changes nor do they intend to discuss reasons of the high spatial variability – even if this would be of interest to many readers. With this limitation, I'm not sure whether the paper is suitable to be published in HESS. I leave the decision to the editor.

>> It seems that from our replies, the reviewer inferred that we do not want to "dig deeper". For instance, in the previous version of the manuscript we included substantial new material addressing his/her comments. In the current version we include further results in order to determine why we obtained different values of the parameters in different regions of the Iberian Peninsula (new Figures A6 and A8 in the Appendix, and new panels in Figures A1-A3).

Other points that have to be considered are:

1.) Your answer to my major revision point 1 is not satisfactory. At least a statement about the reasons of the increased reliability (more realistic temperature profile or humidity profile) is required. And reasons **why** the parameters at some locations show a larger difference that at other locations must be given.

>> The observed values of TT, CAPE and CIN are reproduced correctly by the experiment including data assimilation (D) as shown by Figures 2 and 5 for TT, Figures 3 and 6 for CAPE and Figures 4 and 7 for CIN. This is not the case for the experiment without data assimilation (N), as it can be shown in the same Figures, where poor correlations or some seasonal biases can be observed.

>> Additionally, these results agree with previous evaluations of these two WRF experiments. As already stated in the manuscript at the end of section 2.1, these two simulations were fully validated in previous studies of the authors (already cited there). Precipitation, Evaporation, Integrated Water Vapour or Soil Moisture from the experiment with data assimilation are similar (or even better in some cases) than the ones produced by the driving reanalysis when they are compared against observational datasets (both in-situ or gridded observations). The main difference between both experiments is only the data assimilation approach as both of them include the same physics parameterization schemes, so that is the only reason to produce more reliable results.

>> The effect of data assimilation in humidity and temperature in our simulation was already discussed in the paper González-Rojí et al. (2018) (see their Figure 13), and we gave further details to the reviewer in the last reply we wrote. As stated there, data assimilation is important at 12 UTC for moisture, and at 00 and 12 UTC for temperature, and their effects are relevant in the southeastern IP and both Guadalquivir and Ebro basins. 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 both N and D model runs).

>> Regarding why the parameters show larger differences at some locations, we already included the details about it in the previous submitted manuscript (first iteration), particularly after presenting Figures 5-7. We also included further plots in the annexes (Figures A1-A6) to show that the differences observed in the studied indices were mainly due to the differences in the vertical profiles of the atmosphere. So, we consider we already did this by providing a comprehensive explanation.

>> We thought that all this information was clearly added to the manuscript in our resubmitted version, but it seems that it was not. Consequently, we have expanded the information included from previous studies at the end of section 2.1, and we have added a summary about the reasons that cause the differences between experiments in the conclusions. Beyond that, we have added new panels to figures A1, A2 and A3 to be able to discuss differences observed during spring, and

we have calculated the differences of virtual temperature and mixing ratio from both model simulations (D and N) for some particularly interesting seasons of the year and some sounding stations in order to explain in detail some particular results.

2.) My former major revision point 2: Why should one expect a lower model prediction skill without assimilation? It's right that "the impact of data assimilation is not limited to the grid cells close to the location of the soundings.... The changes extend over large areas". But from that you cannot conclude that the model in general performs better when you restrict your evaluation only to points where you assimilated data.

>> We think that the reviewer is talking about two different "problems" in a single statement. On the one side, we have the effect of data assimilation in the prediction skill of the model, and on the other one, the problem of validating the results against assimilated observations.

>> Regarding the first one, the main objective of the data assimilation scheme is to produce more reliable and accurate initial conditions for the regional models. Thus, it is clear that the results from the model should be closer to real measurements than the outputs from a standard run with a regional model without data assimilation. The improvement in the results is obtained as the data assimilation scheme is performed only after considering as first guess the simple forecast from the model, and once the effect of the observations is used to modify the fields of temperature, wind and pressure in order to make them closer to the observations. However, as is clear from the literature (Wang et al., 2008; Bollmeyer et al., 2015; Shen et al., 2016) and our own papers already cited (Figure 13 in González-Rojí et al., 2018), analysis increments do not affect only to the grid point closest to the place where data (such as soundings) are being assimilated. The fact that the optimization of the cost function involves all the domains implies that analysis increments are not only affecting the grid point where data are assimilated. The fact that the assimilation improves the temperature or moisture fields at the beginning also means that the advection of these fields is acting during the six hours from assimilation cycle to the next assimilation cycle. Thus, the improvement imposed by assimilation extends to other areas, and also other fields which are physically related to the assimilated variables. Data assimilation implies that the boundary conditions must be adapted after the data is assimilated, so that the information ingested through the boundaries is also affected. As a result, data assimilation improves variables such as vertically integrated water vapour which are not assimilated but which can be verified against independent data from satellites, as we did in Figures 3 and 4 in González-Rojí et al. (2018) or alternative datasets such as evaporation, which we already tested for the full Iberian Peninsula in Figures 8 and 9 in González-Rojí et al. (2018). These are fields that we have verified in the past against independent sources at grid points different from the ones affected by the sounding and we are sure beyond any doubt (and have already proved it quantitatively) that the improvement is real and not limited to the place where the sounding balloons are released. We have added some sentences making this clear in the paper (lines 104-111 in the current version, end of Introduction).

>> Regarding the validation of the results against assimilated observations, as stated already in our previous reply to the reviewer, it is true that the results could be considered as biased.

However, we can not discard any of these observations when preparing the simulations without performing a damage to the study that we want to perform, particularly when only eight radiosondes are available over the Iberian Peninsula. Thus, in order to get the most accurate results out from the model, it is clear that we should use all the available measurements. Mainly because with such a reduced amount of data, it would make no sense to include for example four radiosondes in the data assimilation scheme, and the remaining four radiosondes for validation. Moreover, as stated already in the previous reply to the reviewer, we do not assimilate directly any of these indices or precipitation, as we assimilate pressure, temperature, humidity and wind.

>> Most of this information is already included in the manuscript, but we have developed further the paragraphs associated with these statements in sections 2.1 and 2.2 in the new version.

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>> Wang, X., Barker, D. M., Snyder, C., & Hamill, T. M. (2008). A Hybrid ETKF–3DVAR Data Assimilation Scheme for the WRF Model. Part II: Real Observation Experiments, *Monthly Weather Review*, *136*(12), 5132-5147.

>> Bollmeyer, C., Keller, J.D., Ohlwein, C., Wahl, S., Crewell, S., Friederichs, P., Hense, A., Keune, J., Kneifel, S., Pscheidt, I., Redl, S. and Steinke, S. (2015), Towards a high-resolution regional reanalysis for the European CORDEX domain. Q.J.R. Meteorol. Soc., 141: 1-15.

>> Feifei Shen, Jinzhong Min, Dongmei Xu (2016) Assimilation of radar radial velocity data with the WRF Hybrid ETKF–3DVAR system for the prediction of Hurricane Ike (2008), Atmospheric Research, 169: 127-138.

3.) Your answer to my former 3rd revision point: even though if I'm not fully convinced, you should at least comment on that point in the paper.

>> We have included in the new version of the paper a few sentences about the fact that ERA-Interim also assimilates these soundings as suggested by the reviewer.

>> According to section 4.3 from Dee et al., (2011), only a few stations are excluded from the data assimilation process, which include near surface wind measurements, surface pressure or relative humidity measurements in high terrain, specific humidity in extremely cold regions or radiosondes below the model surface. Consequently, as in our case, mainly all the radiosonde stations over land must be included in it. Even with that, many studies still use assimilated radiosondes for the validation of their own simulations or even the reanalysis (e.g., Vergados et al., 2014; Simmons et al., 2014; Zhao et al., 2019).

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>> Simmons, A. J., Poli, P., Dee, D. P., Berrisford, P., Hersbach, H., Kobayashi, S., & Peubey, C. (2014). Estimating low-frequency variability and trends in atmospheric temperature using ERA-Interim. *Quarterly Journal of the Royal Meteorological Society*, *140*(679), 329-353.

>> Vergados, P., Mannucci, A. J., & Ao, C. O. (2014). Assessing the performance of GPS radio occultation measurements in retrieving tropospheric humidity in cloudiness: A comparison study with radiosondes, ERA-Interim, and AIRS data sets. *Journal of Geophysical Research: Atmospheres*, *119*(12), 7718-7731.

>> Zhao, Q., Yao, Y., Yao, W., & Zhang, S. (2019). GNSS-derived PWV and comparison with radiosonde and ECMWF ERA-Interim data over mainland China. *Journal of Atmospheric and Solar-Terrestrial Physics*, *182*, 85-92.

4.) Your answer to my former 6nd revision point: Please give a comment also in the manuscript.

>> The information about why we considered the nearest point to the station and not an averaged area was already stated in the reviewed manuscript (beginning of section 2.3.1). However, we have included more details to that paragraph.

5.) Your answer to my former 8nd revision point: The conclusion is intended to help the reader understand why your research should matter to them; it should not simply reiterate your results or the discussion; recommend a specific course or courses of action; critically refer on the relevance of your research, and also refer your work to other references. Try at least to consider a few points of these points. Otherwise rename this section into "Summary".

>> As we already stated in the reply to your comment 1, we have added some sentences to the Conclusions in order to make it suitable and interesting for the readers.

Minor revision points:

1. My former minor revision point 1: The references you have cited also used other indices and considered other regions. Still: Why did you selected these three parameters, why not, e.g., SWEAT, LI (which, according to several papers, has the highest predictive skill) or others? Simply because these were available and other not? A simple statement on that in one sentence is sufficient. (Note that the second paragraph of your answer (5 vs. 30 years) is very speculative).

>> As already stated in the manuscript at the end of section 2.3.1, other indices could be calculated. However, some of them are defined in a similar way (e.g., TT and K are calculated based only on temperature at two pressure levels) or previous studies (Blanchard, 1998; López et al., 2001 - Both included already in the manuscript) showed strong correlations between them (e.g., CAPE and LI). Additionally, our main objective is to evaluate the performance of both WRF simulations, so we needed to compare our results to those obtained from different observational sources as the University of Wyoming radiosounding database (also later against IGRA dataset). In

that database, only Showalter index (S), LI, SWEAT, K, TT, CAPE and CIN are available. In the case of IGRA, the same parameters are provided, with the exception of SWEAT.

>> The R package that we created and that is essential for our study, allows us to calculate directly from the pseudosoundings extracted from the model at each grid point the values of different variables and indices such as CAPE, CIN, TT (already used in this paper), LI, S and K. Based on this set of indices, we decided to choose only three of them calculated by different methodologies and not to include all of them as that would make the paper long and repetitive. Based on what we stated in the previous paragraph and in the manuscript, we decided to focus on CAPE, CIN and TT, which are indices that show interesting but not redundant results.

>> Additionally, the last paragraph of section 2.3.1 was extended and further information is given.

2. My former point 6: CAPE is a measure of convective instability, but not a convective **index**.

>> Our former reply to point 6: We know that CAPE and CIN are measurements of energies available (CAPE) or inhibition (CIN) in the column of the atmosphere. However, even if they represent an amount of energy, they can also be considered as indices of instability in the atmosphere. As already stated in our previous reply, many authors before (such as Tsonis, Djuric and Bohren and Albrecht) have defined them as indices in their books for Atmospheric Thermodynamics.

>> In order to avoid further comments about this, we have modified all the sentences where we refer to CAPE or CIN as instability indices, even if we do not agree with the reviewer here.

3. My former point 10: it still reads "…convective precipitation is usually associated with extreme events…" and this is wrong; change "usually" by "frequently", but also define "extreme events" (do you refer to rainfall, hail, or wind?); "due to high intensity and short duration"; the short duration is related to convective processes and does not make convective precipitation an extreme event per se; change into "due to high intensity over a short duration".

>> As suggested by the reviewer, the sentence was adapted. We hope this time it fits the standards of the reviewer. It can be read now:
"In general, convective precipitation is frequently associated with precipitation extreme events due to high intensity over a short duration"

4. My former point 16: I know, but this is a statement not supported by their research. Please change this reference into a more appropriate, e.g., Graf et al., 2011. Graf M. A., Sprenger M., and R. W. Moore, 2011: Central European tornado environments as viewed from a potential vorticity and Lagrangian perspective. #are#, 101, 31–45, https://doi.org/10.1016/j.atmosres.2011.01.007.

>> We have changed the citation associated with that statement as suggested by the reviewer.

5. My former point 23: Basically, I agree; but please explain here in one clause or sub-clause **why** you want to evaluate that.

>> We have developed further the reasons why the evaluation of the convective field is of interest for the readers in the new version of the manuscript. In that part of the Introduction, we have stated that its importance lies in the fact that the study of these parameters makes it possible to determine with enough precision the regions in which conditions conducive to atmospheric instability can be fulfilled.

6. My former point 47 (and 49): Even though you are not directly referring to convective precipitation here, you are discussing TT in relation to rainfall. If precipitation is dominated by other processes such as lifting associated with positive PV-anomalies, TT doesn't matter.

>> Well, to our understanding we are only pointing out that the strongest values of TT and CAPE (according to point 49 raised by the reviewer) are observed in the same regions where the largest amounts of precipitation are measured in each season. Our intention with those statements is only to show the readers that our results fit the observed patterns of precipitation over the Iberian Peninsula. We do that comparison with the precipitation patterns as we know that, to some extent, unstable conditions in the atmosphere are associated with precipitation.

>> In any case, in order to avoid further problems with those statements, we have deleted them from the manuscript.

7. My former points 50 and 51: Have you included a short statement about this in the manuscript?

>> Most of the details about these results were included already in the manuscript after the first revision, along with the plots included also in the reply to the comments made by reviewer 2. Particularly, they are included in lines 372-388. Regarding the comment 51, also a new paragraph about it was included in previous version (lines 459-467).

8. My former point 52: I'm not really convinced by your reply not to consider the convective situation. It would be much more interesting to see how well CIN is modeled in convective situations and not on days, where CIN has no meaning. Besides, consideration of the relation between CIN and CAPE would make the content of the paper much more interesting for a wider community (also for me as a meteorologist).

>> We agree with the reviewer in the fact that considering the convective situation only can be of interest to meteorologists or forecasters, but that is out of the scope of our paper as our main objective is only to evaluate the performance of both WRF simulations at simulating unstable conditions in the atmosphere. We have clearly stated this goal in the abstract, introduction, and finally again in the conclusions. Generally speaking, we want to see if all the convective situations observed are well captured by the simulations, and if the values produced by the experiments are similar to those measured. In order to do that, we must consider all the data available in our

simulated period, and we cannot restrict the evaluation only to the convective situations. If we followed the reviewer's suggestion, we would only choose the convective situations observed in reality and we would only evaluate the corresponding days in the simulations. However, we would be missing the convective situations from the model that are not observed in reality, an important point to detect problems in the simulations.

>> The reviewer also asks about the relationship between the studied variables. As we showed the reviewer in our previous response, CAPE and TT indices were not related by a simple linear relationship as the R2 was below 0.2 for all the stations and seasons. In this case, we have calculated the relationship between CAPE and CIN in all the stations available and during summer, as the values are larger during this season. Figure 1 shows that the relationship between these two variables is again negligible according to the R2 values obtained (below 0.1 always). Thus, as in the case of TT and CAPE, even if these two variables are related to the thermodynamic conditions in the atmosphere, they are not related by a simple linear relationship because of the multiple phenomenology that might exist in the real atmosphere. Thus, we do not see the point in separating CIN for different values of CAPE. Additionally, CIN is a measurement of inhibition, so even if it is evaluated by itself, it can also provide important information in the analysis, and it must be validated independently and not restricted according to the values of CAPE.

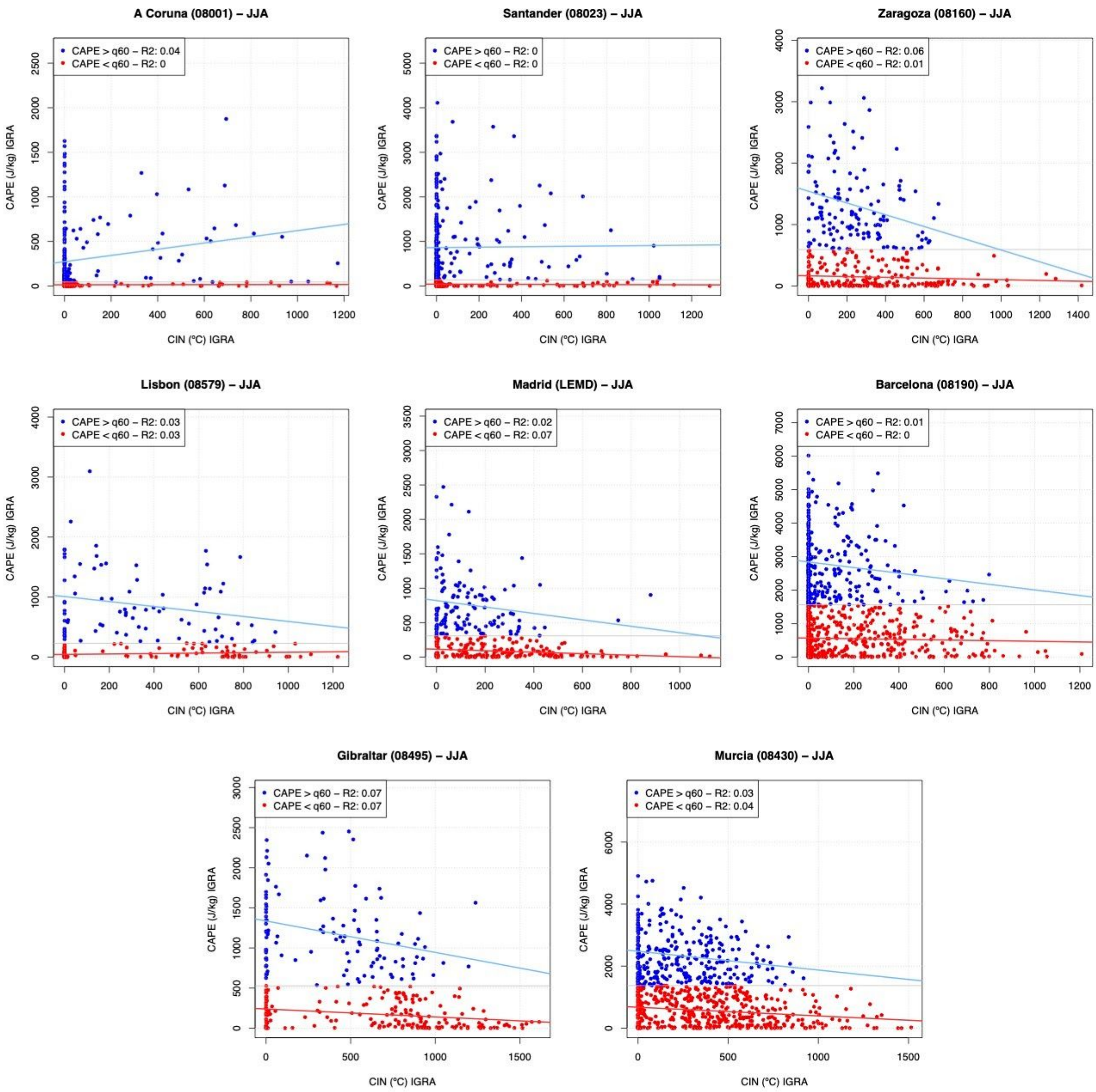

Figure 1 : Scatterplots for the values of CAPE and CIN as included in IGRA for all the stations during summer. The values of CAPE over the 60th percentile are in blue, and the values below that value are in red. The value of the 60th percentile is marked with a grey line, and the linear models are also included with the corresponding colors.

9. There are still some linguistic corrections to be made, but I think these will be fixed by the journal's edit.

>> A detailed revision and edition of the language was conducted in the new version of the manuscript.

10. The introduction broadly describing convective activity across Europe and other features related to convection in general is well written. However, it does not really fit to the main content of the paper focusing on "the performance of two simulations created by using WRF model". At least the topic of data assimilation as the central point of the paper has to be introduced here.

>> A new paragraph about the data assimilation was included in the introduction of the new version of the manuscript.

Further minor points:

1. L18-19: Mean CAPE is always higher at 12 UTC that at 00 UTC; this is not worth mentioning in the abstract.
2. L30: include "…complex topography, **insufficient assimilated observations, and** forecast errors…"
3. L33: delete the words "extreme" as you do not separate among the precipitation intensity.
4. L39: The deep convection…
5. L42: of the atmospheric convection
6. L49: precipitation extreme events convective precipitation
7. L60: most unstable region region with highest instability
8. L67: There is something missing in this sentence (verb and subjective)
9. L86: the mean CAPE
10. L102: extreme events convective situations
11. L115: both wind components **horizontal** wind; geopotential geopotential **height**
12. L125: presents the same prarameterizations relies on the same setup (the parameterizations used are introduced later)
13. L140/163: write out NOAA at first use
14. L167: ...12 UTC, 02 and… 12 UTC, corresponding to 02 and 14 LT, respectively)
15. L168: in L167 12 UTC was 14 and not 13 LT
16. L173-179 is somewhat cumbersome (two almost identical sentences)
17. L186 highlighted highlight
18. L187: This… refer to what?
19. L188: in **our** WRF simulations
20. L196 as we would be taking into account as we take into account (you did that, didn't you?)
21. L197-198: This sentence is unclear. "levels are already measured for a drifting distance"? What is the distance of 4.5 km? How is the drifting related to the cloud cover??
22. L200: "This procedure…" What do you refer to?
23. L204: still, it's not minutes rather more than one hour for an entire profile of the atmosphere
24. L205: "…because of wind"; already discussed above.
25. L222: it **was** not it **is** not
26. L233: "Lifted Condensation Level" "**Lifting** C…" is more appropriate (cf. AMS Glossary)
27. L235 trigger cause
28. L237-238: "provide similar information"; I fully disagree with this statement, see the bunch of literature on the various convective indices quantifying conditional, latent, potential instability, or a combination thereof!
29. L262-264 and elsewhere: Spearman / Persons's correlation **coefficient**
30. L288/L306: worsening decrease (avoid personal assessment)
31. L293: are **most** remarkable in Murcia

32. L295-96 and **elsewhere** (e.g., 299): **in** those stations **at** those stations; **at** the Mediterranean coast; you cannot say in a station; this was already explained in my 1st review.

33. L300 **at/for** Barcelona

34. L308: **S**ection 2.3.1; **of** TT index

35. L332 (and elsewhere): trigger cause

36. L346: and Gibraltar) and Gibraltar

37. L347/48 shorter smaller

38. L363-64: "warmer sounding levels" is strange; Reference the reference

39. L362-64: that's of course not a trajectory! lifted air parcel; the sentence "which produces that the lifted trajectory crosses earlier than D the sounding" is very strange

40. L374: "most active ones"?? You mean highest CAPE values?

41. L399: "…system, so the lifting that can trigger convection can appear" "…system that can trigger convection by orographically induced lifting"

42. L406-407: replace the sentence beginning with "…are originated…" "are a consequence of low dew point temperature mainly due to dry air."

43. L412-13: This sentence is a fragment (no Verb and Subjective); again, you cannot say "in the slope"

44. L417/418 and elsewhere: **on** the Atlantic coast; **on** the western coast

45. L419: that than

46. L438 results **from** TT results **for** TT

47. L440 highly convective events: this is now very confusing. You are investigation mean values, aren't you? Why are you speaking of events? And why "highly"? Did you separate among different intensity classes? I think you did not…

48. L441: "TT and CAPE are indices for atmospheric instability"; No. The one is an index (TT), the other is an integral bulk of convective energy and **not** an index. Besides, CAPE solely estimates latent instability, whereas TT combines conditional (VT), latent, and potential instability.

49. L443-44: "since CAPE and CIN are dependent on the entire profile of the atmosphere.." There are two flaws: you mean the profile below the level of neutral buoyancy and not the whole atmosphere (which is unbounded); and CIN depends only on the layers below the LFC. Furthermore, several studies have shown that – at least in Europe – the Lifted Index has a higher prediction skill than CAPE, even if considers only a parcel lifted to 500 hPa.

50. L446: on the Atlantic coast

51. L449: what do you mean by intensity? When you refer to that, you need to consider CAPE in combination with CIN (as you know, in case of high CIN, instability cannot be released and the intensity is low no matter of CAPE).

52. L451: delete §dynamics"

53. L469: the correlation correlation coefficients according to Pearson and Spearman

54. L470: "…of them" of what?

55. L489: delete "develop"

56. L490: **convective** inhibition

>> We thank the reviewer for highlighting these minor points. They were included in the new version of the manuscript, except some comments:

>> 9: We don’t understand what the reviewer is remarking here, as that is what is already stated in the text. In any case, we have modified it to “the mean of CAPE”.

>> 15: In Lisbon (Portugal), 12 UTC is 12 LT and 13 LT during winter and summer times respectively. In contrast, for the Spanish stations and Gibraltar, 12 UTC is 13 LT and 14 LT during winter and summer times respectively. The fact that the local times stated in the test were chosen during the summer times has been added to those lines, but the local time for Portugal has not been modified as suggested by the reviewer.

>> 20. As replied to the reviewer in the last revision and as stated in the previous line to that highlighted by the reviewer, we are NOT considering the averaged value of several grid points to be compared against radiosonde data. If we do that, with the spatial resolution of 15 km used in our experiments, we would be considering an area of 2025 km2, which is not suitable to be compared against in situ data. Then, that sentence was not modified in the new version.

>> 23. The main point of that sentence is to show that the vertical profile of the atmosphere is not measured instantaneously (as already stated in that line), and not about the exact amount of time needed for it. In any case, we have modified the sentence again.

>> 31: “are most remarkable” as suggested by the reviewer is not what we mean. Consequently, we have changed it to: “These differences are more remarkable in Murcia.” In any case, we think that this kind of editions are adequate for the journal’s style editor after acceptance.