2nd Review „Changes in the simulation of instability indices over the Iberian Peninsula due to the use of 3DVAR data assimilation "
by Santos J. González-Rojí and co-authors submitted to HESS

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.

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.

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

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

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".
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.
5. My former point 23: Basically, I agree; but please explain here in one clause or sub-clause **why** you want to evaluate that.
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.
7. My former points 50 and 51: Have you included a short statement about this in the manuscript?
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 ☺).
9. There are still some linguistic corrections to be made, but I think these will be fixed by the journal's edit.
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.

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