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Comment on acp-2021-1050

Anonymous Referee #1

Referee comment on "The representation of winds in the lower troposphere in ECMWF forecasts and reanalyses during the EUREC4A field campaign" by Alessandro Carlo Maria Savazzi et al., Atmos. Chem. Phys. Discuss.,
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Review of "The representation of winds in the lower troposphere in ECMWF forecasts and reanalyses during the EUREC4A field campaign" by Alessandro C.M. Savazzi, Louise Nuijens, Irina Sandu, Geet George, and Peter Bechtold

The paper assesses the representation of lower tropospheric winds in IFS analyses and forecasts compared to numerous observations from radiosondes, dropsondes and Doppler lidar during the EUREC4A field campaign over the tropical ocean in early 2020. The measurements show a diurnal cycle in the boundary-layer, which is captured well by analyses but is too strong in forecasts. On average, forecasts present a bias of up to 1 m/s, which is much reduced in the analysis. Data denial experiments indicate a weak impact of the assimilated EUREC4A data on the wind bias in the analysis, while numerical experiments emphasize the importance of shallow convective transport and suggest a role of deep convection.

The paper builds on a substantial amount of new data resulting from a large observational effort and combined with relevant numerical experiments. The addressed topic is of importance and the novel material will help identify and alleviate model deficiencies. However, the data analysis is too superficial, based on disputable methods and not fully supportive of the conclusions. Furthermore, the narrative tends to present fundamental concepts and new ideas during the course of the paper instead of clearly separating aims, methods and results. Thus, substantial work is required to deepen the analysis, strengthen the interpretation and clarify the presentation. A long list of specific comments is given below to improve the paper.

General comments

- Assessing ERA5 against observations that are assimilated in the reanalysis is questionable. It makes sense to compare the quality of forecasts and analysis to disentangle the origin of errors between initial conditions and model physics but in this case the operational analysis would be more meaningful for consistency.
- Most of the results are based on mean biases only and do not discuss the statistical distribution of model and observations. This is a clear oversimplification and likely obscures a large part of the actual data content.
- The spatial variability is not mentioned and the temporal variability is assessed through the mean diurnal cycle only. This does not support the discussion of small-scale convective processes in Section 6.
- The paper needs restructuring by presenting observations first, then possibly the impact of data assimilation, before discussing the quality of analysis and forecast data.
- Numerous repetitions and inaccuracies make the data and methods often confusing. They must be described once and if relevant only.
- Figures tend to be misleading for negative wind components, partly due to the use of the same color bars as for wind speed. Overall, displaying wind speed and direction would be much easier to interpret.

Specific comments

I. 1 tropical ocean

I. 7 typo: RMSE

I. 17–18 Why?

I. 18–24 This comes too early as neither the ocean nor the Tropics have been mentioned yet.

I. 28–31 Does it reach the lower troposphere?

I. 32–33 Are these observations not assimilated?

I. 39 What is the definition of transient here?

I. 55 “the largest”: ever?

- I. 68–72 Open questions rather than yes/no?
- I. 91 EUREC4A already mentioned several times
- I. 93 rather defines the studied domain
- I. 116 What is the “Boulevard des Tourbillons”?
- I. 120 black square
- I. 121 typo: were
- I. 149 Why these 61 (arbitrary) points? Fig. 1 suggests that only 1 point is taken from ERA5.
- I. 154 horizontal resolution (and already stated several times)
- I. 158–160 either give more details or omit day 4
- I. 163 spell out ERA5?
- I. 167 with more observations and a longer assimilation time window?
- I. 168 Remove “for example” if no other reanalysis is used here
- I. 172 either give more details or omit the operational analysis
- I. 173 ERA5 available hourly

I. 176–177 The question is confusing

I. 177 Why this resolution? It differs from both reanalysis and forecast

I. 179 What is the need of 10-day forecasts to look at day 2?

I. 180–181 Are EUREC4A measurements assimilated in the operational analysis and/or reanalysis otherwise? This is a crucial point for the paper!

I. 185 how is shallow convective momentum transport accounted for in the model?

I. 190 did you run model experiments at different resolutions?

I. 193–194 Why these numbers? What is the vertical resolution and range of the different datasets?

I. 199–214 This paragraph is confusing and largely repeats Section 2. Please clarify and merge.

Fig. 3 What is “wspd”?

I. 219 mid January to mid February

I. 220, 226 Where is the cloud base and top?

I. 235 already stated above

I. 238 refer to dropsondes rather than JOANNE?

I. 240–242 repeats I. 219–220. Again, where are the clouds?

I. 242–243 described below

I. 250–257 This belongs to the Methods

I. 252 instantaneous or daily averaged variable?

I. 260–261 Does it mean the wind direction is wrong?

I. 267–268 show the error distribution?

I. 272–273 It rather suggests the opposite!

I. 276 The title does not seem appropriate as nothing is said about the synoptic situation

I. 278–279 One would expect observations first, possibly combining the different sources

I. 280–281 This is not easy to infer from Fig. 5

I. 282–284 This statement is subjective. For instance, a larger bias in wind speed around 02-08 appears related to the larger wind speed at that time

I. 288–289 This is speculative

I. 296 typo: composites

I. 309–311 similar to I. 278–279: why not show observations?

I. 318–324 move to section 6.1

- I. 330 This is unclear as both u and v show positive bias during daytime

- I. 331–333 This sounds speculative

- I. 336 The phase shift was not mentioned before

- I. 337 It is surprising this is not discussed earlier. Again, where is the cloud layer?

- I. 344–345 This contradicts I. 334 above

- I. 350–351 This fundamental information (1) must be stated earlier and (2) questions the soundness of the above assessment of ERA5 winds

- I. 360–362 What about RMSE, diurnal cycle, etc. discussed in the previous section?

- I. 375 Modeled versus observed?

- I. 376–384 More details on the methods are needed here. A paper “under preparation” is not of much use.

- I. 386 Where is the boundary layer?

- I. 389 Where is the reduction in the large-scale pressure gradient to be seen?

- I. 391 Which forcing?

- I. 397 slow bias

- I. 406–408 the names are not consistent with the legend of Fig. 12 (NoCMT)

- I. 413–414 the mean bias in the zonal wind is strongest at night

- I. 418–426 The paragraph appears to mix shallow and deep convective momentum transport

- I. 429 “what we have called”: is it not the actual operational forecast?

- I. 431–433 please be more explicit

- I. 433–436 Does it matter here?

- I. 438 why the subtropics?

- I. 443–444 Repetition of I. 437–438

- I. 444–445 The clear improvement in u and v barely affects the wind speed. This suggests that the mean bias discussed here is only part of the picture.

- I. 445–448 This appears speculative and contrasts with the above discussion of shallow convective transport in the lower troposphere

- I. 470–471 not explicitly shown in the paper

- I. 474–475 smaller than what?

- I. 476–477 not sufficiently supported

- I. 483–484 reference?

Please label all panels and refer to A, B, C, ... in the text

Displaying all figures at the end of the paper would facilitate the review process