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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Assessment of vertical air motion among reanalyses and qualitative comparison with direct VHF radar measurements over the two tropical stations" *by* Kizhathur Narasimhan Uma et al.

Anonymous Referee #2

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I like the premise of this paper very much: observational evaluation of reanalysis vertical velocities is much needed, perhaps especially within the broader Asian monsoon region. This is an important topic and I would like to see the paper published in the end. However, the presentation contains some significant gaps and unclear reasoning that should be addressed.

My main reservation is that the comparison as presented is almost entirely descriptive, with little analysis of the causes of biases or how they might inform further improvement of the reanalysis products (see also comment D below). It would be very helpful – if not

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essential – to include more interpretation of both the differences among the reanalyses and the biases relative to observations. For example, the introduction indicates that section 4 includes both a discussion and a summary of the results, but section 4 itself includes little discussion, only summary. Some questions to consider:

- Can we understand anything about what the reanalyses are doing wrong (or right) from the observational validation or reanalysis-only intercomparison results?
- Do the differences indicate major problems or can they be largely understood in terms of spatiotemporal sampling? For example, the narrow column observed by the radar relative to the reanalysis grid scale – do comparisons of reanalyses with grid scales spanning a factor ten offer any context here? In a related question, does resolution of the nearby topography come into play in any obvious ways?
- Are there any clues as to how different types of data assimilation (3D-Var vs IAU vs 4D-Var) influence biases in vertical velocities? What about details of the model physics, such as convective or boundary layer scheme?
- How robust are the results between the two sites? Does this have any implications for which conclusions, if any, can be generalized?

I appreciate the authors' attention to earlier editorial comments. I have included an annotated manuscript with some additional (optional) suggestions, which also references the specific comments below.

General comments and figures

A (Sect. 2) The descriptions of the reanalyses in section 2 should include indications of how vertical velocities are computed in each reanalysis, whether these

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estimates are impacted by data assimilation (i.e. forecast versus analysis versus IAU in the case of MERRA-2), and whether they represent time-average or instantaneous estimates. It would also be helpful to give some basic information about the model vertical coordinates, as I think vertical pressure velocities are usually estimated in these coordinates and then interpolated to the pressure grid. To the extent that these procedures are the same across multiple reanalyses the description could be consolidated, emphasizing differences in each subsection. Any other aspects of the model / data assimilation that might aid interpretation of the differences should also be included here.

B (Method) Some of the specifics of the analysis are difficult to follow. For example, in l.287 it is indicated that directional tendencies are reported for a given height for every month when either the radar or reanalysis data exceeded ± 1 cm s^{-1}. This screening is based on the monthly mean, correct? Then, a few lines later, it is indicated that the directional tendency is calculated only for absolute magnitudes greater than 0.1 cm s^{-1}. Is this now referring to the daily data? Are the results sensitive to these thresholds, especially in terms of the reanalysis evaluation?

C (Sect. 3, last two paragraphs) The directional tendency results for ERA5 are difficult to understand. The lack of strong updrafts or downdrafts at Gadanki seems to contradict the results shown in Fig. 3 (where ERA5 seems to have relatively strong vertical velocities and it is ERA-Interim more than ERA5 that looks like the outlier) and Fig. 6 (which shows a pretty robust seasonal cycle with many monthly averages well above the threshold). The results for Kototabang are likewise preplexing in the context of Fig. 3 and Fig. 7. How do you reconcile the directional tendency results in Fig. 9 with the profiles shown in these earlier figures?

D (Sect. 4, l.339-340) I like this thought, but more needs to be done to really provide a useful platform for improving the reanalyses. For one, it is not clear whether it is the 'methodology for calculating w' that lies behind the identified biases, as opposed to, e.g., different diurnal or day-to-day variations in convective occurrence (see comment 20), interactions between subgrid physical parameterizations and the large-scale flow, crude representation of local topography or land surface conditions, even just differences in spatial sampling area. This last is even suggested as the main reason for the differences in l.195-196, and it is not clear how reanalyses can address this beyond moving to finer and finer horizontal resolutions, which they are already doing. I recognize that it would be a monumental task to try to diagnose all of these and do not ask for a exhaustive investigation, but some investigation and discussion would be warranted. For example, operating on the hypothesis that the differences are mainly related to averaging, you could try imposing 'subgrid fluctuations' on the reanalysis products. What scale of fluctuation would need to be imposed to bring the reanalysis products in line with the observations? Is there any relationship between this value and the reanalysis grid size? Do the results make physical sense, or do they suggest that other factors must contribute?

E (Data availability) The data citations are incomplete. Both NASA (for MERRA-2) and the NCAR RDA (for all other reanalyses) have assigned doi numbers to the datasets used in this paper. These doi values should be used in data citations (input data doi at https://citation.crosscite.org/ for citation details) to help the data providers track the impact of their investment. Dates of access should also be included (since reanalyses occasionally undergo reprocessing to fix errors), along with the specific variables and resolutions used (to ensure reproducibility).

F (Figure 3) Is it possible that the vertical profiles for MERRA-2 at Kototabang have been inverted somehow? The differences between these and ERA5/ERA-Interim are pretty striking, perhaps especially the downward shift of the maximum during May–June in MERRA-2 relative to the upward shift of the maximum in ERA5 and ERA-Interim (not to mention the radar profiles). I know that the orientation of the vertical coordinate may differ (top-to-bottom, bottom-to-top) in data files released by these different reanalysis producers. Please double-check this for MERRA-2, and perhaps also for NCEP-2.

G (Figure 8) Could Fig. 8 be made more effective building off of the presentation in Fig. 3, using difference plots relative to a particular reanalysis-based benchmark? I agree that the current presentation could help in terms of explicitly comparing quantitative biases across different reanalyses, but it is very difficult to pick out details of the individual profiles in the current figure. Another option might be to consolidate some months with similar profiles (it looks like the canonical seasons might work, but warm/cold/transition seasons could also work) and then split the Gadanki and Kototabang profiles (results for the two sites do not seem to share that much in common in the vertical distributions).

H (Figure 9) The caption says this figure shows a comparison between the radars and various reanalysis products. Where are the directional tendencies based on the radar data? It is difficult to evaluate the reanalyses without this information. Please excuse me if I am missing something really basic about the presentation.

Specific comments on text

1. (l.33) Perhaps also mention the role of subsidence and adiabatic warming in the formation of stable inversion layers?
2. (l.36,38) I think the use of ‘control’ here rather overstates the case, especially in l.36. It would be enough to delete ‘controlling’; the second use in l.38 is ok on its own.
3. (l.49) Please clarify what is meant by ‘global estimates’ and its relationship to ‘direct measurements’ versus ‘indirect estimates’; there is also an extra comma here.

4. (l.57) Maybe mention here that this source of uncertainty is particularly important for reanalyses, where assimilation increments in horizontal winds may be comparable to this. It might also be helpful to rephrase the sentence to emphasize this assimilation adjustment rather than 'error'.
5. (l.65) The connection of the above discussion to reanalysis estimates of vertical velocity should be made more explicit (i.e., why do these concerns apply to reanalysis products specifically)
6. (l.66) Suggest rewriting this sentence: 'reanalyses involve many approximations and assimilation-related adjustments, and are not error-free'
7. (l.84) Technically a reanalysis vertical profile is also a column over a single location, just one with a broader footprint. Here it would be helpful to specify how the area of the column differs between the radar, the finest-grid reanalysis (0.25°, right?) and the coarsest-grid reanalysis (2.5°).
8. (l.86) Phrasing needs care here: a number of studies have evaluated vertical motion across reanalyses (in the context of trajectories, wave activity, large-scale motion, etc.), so the primary novelty of this work is the evaluation against radar observations.
9. (l.88) This point is a little repetitive.
10. (l.119) Rephrase: 'Quality control metadata for the EAR measurements are available online'
11. (l.140) How long is 'long'?
12. (l.146) How is this 21 km upper limit identified in the reanalysis profiles, and approximately what pressure level does this correspond to? More generally, many of the results are presented in altitude coordinates. Are heights computed from

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the pressure levels assuming a constant temperature, from geopotential outputs from each reanalysis, another approach? Are data linearly interpolated to a common height grid? Could this have any influence on the comparisons in this paper (e.g., the reanalysis-derived updraft maxima being located lower than those observed by the radar)?

13. (l.152) Citation year for Hoffmann et al. should be 2019

14. (l.153) The ERA5 paper is now in early online release (https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.3803)

15. (l.165) NCEP-2 (as denoted here) was undertaken as a cooperation between NCEP and the Department of Energy (DOE); care should be taken in this text to acknowledge this and distinguish it from the NCEP-NCAR Reanalysis 1

16. (l.171) The paragraph mentions only the original model resolution, but from this description the data are taken from the 2.5° data grid

17. (l.178) This information is not provided for the other reanalyses nor is it clear how it relates to the estimation of vertical velocity. I suggest that the authors try to provide a consistent and concise set of information for each reanalysis in this section, focusing on the points relevant to the data used in this paper.

18. (l.186) for dry air

19. (l.188) daily mean is evaluated for 00–24 UTC or shifted to match local solar time? I guess it shouldn't matter much as long as this is consistent between the radar and reanalysis

20. (l.195) It looks like convective days in the reanalysis products are defined based on the screening from the radar data. How consistently do the reanalyses identify convective versus non-convective days based on measurements at the

radar site? Wouldn't this screening also be sensitive to the differences in grid size? Some sensitivity testing would be helpful here, perhaps using precipitation thresholds as well as w.

21. (l.198) The meaning of 'global' here is not clear — should this be 'qualitative' to set it against 'quantitative' differences that might result from differences in sampling area (but see also comment 20)?
22. (l.206) ERA5 should be written without the hyphen (as it is now in much of the manuscript; thank you)
23. (l.216) It is not clear from the text whether these 'significant variations' are in the seasonal cycle, in the diurnal cycle, or both (since the previous sentence discusses seasonal variations in the diurnal cycle). Some additional clarification would be helpful
24. (l.220) The temporal treatment is another source of potential differences in vertical velocities between the observational data (time averages over at least one hour) and reanalyses (usually instantaneous outputs, I think – please check – and usually only four times per day). Naively, it seems like this might offset some of the smoothing effect of the spatial sampling difference, but it should be mentioned and discussed either way.
25. (l.231) Could this comparison be sensitive to the definition of 'convective' days? This may be especially relevant for Gadanki where the reanalyses may even have different diurnal cycles of convection. For example, Bechtold et al. (2014) reported that changes to the forecast model between ERA-Interim and ERA5 resulted in substantially improved representation of the diurnal cycle of convection over land.
26. (l.249) Does this result generalize to the observational validation — i.e. do the

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EAR results for Kototabang also support this conclusion? This information could be added to Fig. 7 and related discussion.

27. (l.272) Some additional care might be needed in the presentation here, to distinguish when the values being quoted are absolute magnitudes of w (as here) versus when they are biases relative to a particular benchmark (as earlier in the paragraph).

28. (l.278) This presentation ('underestimates'/'overestimates') is a little strange, since it seems to imply an evaluation of ERA-Interim against multiple benchmarks as opposed to an intercomparison among (presumably) equally uncertain reanalysis-based products. A more specific and less judgemental phrasing might help, something like: 'XX shows smaller positive values than YY and larger positive values than ZZ', or 'XX and YY both show downdrafts, but with larger amplitudes in XX', or maybe using stronger/weaker updrafts/downdrafts if you prefer.

29. (l.292) Should delete the space between 10 and % (also elsewhere in this and following paragraphs).

30. (l.293) Is this ratio indicating that 10% of all 12 UTC values exceed 0.1 cm s^{-1} or that 10% of all positive values have magnitudes grater than 0.1 cm s^{-1}? If the first, it is enough to remove 'of updrafts'; if the second, some additional text to clarify is necessary.

31. (l.301) Suggest to be more explicit: 'The fraction of downdrafts decreases above ...'

32. (l.311) Is this 'reaches a maximum' specifically referring to the increase above 17 km or to both the increase below 6 km and abover 17 km? If the latter, 'a maximum' should be changed to 'maxima'.

33. (l.330) Here, do you mean 'the location of the peak w differs between radar and reanalysis data, as it also does over Gadanki' or just that 'the vertical location of the w maximum over Kototabang is different from that over Gadanki'?

34. (l.331) Again, suggest revising this presentation style to be clearer and more objective (see also comment 28).

35. (l.338) These examples are a little strange. It is true that the behavior of physical parameterizations in the reanalyses (used to generate diabatic heating and convective mass fluxes) may be impacted by large-scale convergence / divergence (and hence by the same factors used to compute w), though the feedbacks between w and model physics are two-way, complex, and pretty different from how they are implied to be here. Or perhaps the authors refer to diagnosed diabatic heating (e.g., Yanai et al., 1973) and vertical motion along Lagrangian trajectory pathways? Note that the latter should be distinguished from 'convection', which is included in some transport models but I think usually based on vertical stability rather than w.

36. (Fig. 5 caption) Should this reference be to Fig. 4?

References

Bechtold, P., Semane, N., Lopez, P., Chaboureau, J.-P., Beljaars, A., and Bormann, N.: Representing equilibrium and nonequilibrium convection in large-scale models, J. Atmos. Sci., 71, 734–753, doi:10.1175/JAS-D-13-0163.1, 2014.

Yanai, M., Esbensen, S., and Chu, J.-H.: Determination of bulk properties of tropical cloud clusters from large-scale heat and moisture budgets, J. Atmos. Sci., 30, 611–627, 1973.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2020-18/acp-2020-18-RC2-supplement.pdf

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