Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2021-2-RC1, 2021

Comment on wcd-2021-2

Dominik Jacques (Referee)

Referee comment on "The impact of GPS and high-resolution radiosonde nudging on the simulation of heavy precipitation during HyMeX IOP6" by Alberto Caldas-Alvarez et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-2-RC1, 2021

This manuscript examines forecasting experiments where radiosonde and GPS delay observations are assimilated before a significant precipitation event. The main goal being pursued is to establish whether increased model and/or observation resolution can bring significant improvements to the forecasts. Various combination of model resolutions and observations are tested. The performance of these forecast is mostly assessed from the resulting precipitation compared against observations. The overall conclusion is that the assimilation of operational radiosonde data is important but assimilating extra â□□high-resolutionâ□□ observations is not. Deficiencies in modeled moist processes and lack of vertical information in GPS observations are given as factors that could explain the results obtained. With their heterogeneous distributions and difficult statistical properties, â□□physicalâ□□ state variables such as moisture and precipitation remain challenging to data assimilation and verification. As such, this manuscript takes place in the context of an active topic of research. While the experiments and analyses presented are not fundamentally novel, they contribute to a better understanding of data assimilation for moist processes. The topic is interesting and within the scope of the Weather and Climate Dynamics journal. The manuscript is well organized and generally easy to follow. The in-depth examination of the meteorological impacts (i.e. changes in moisture) brought by the assimilation process is interesting. Perhaps the area that needs the most improvement is the description of results related to figure 5. As discussed in major comment 1 below, the description of certain scores is missing or unclear. There is also a labeling error in figure 5. Only one precipitation case is presented in this study. On the one hand, this allows for an in depth analysis of the factors contributing to this precipitation event. On the other hand, this imposes a strong limitation on the generalization of conclusions drawn from the various analyses. Luck (good or bad) cannot be ruled out of the many factors influencing the forecasts. Interestingly, the analysis reveals that the assimilation of one radiosonde in the operational network has a significant impact on the forecasts being performed. One can wonder if the conclusions of the manuscript would have been different had this radiosonde been part of the extra â□□high resolutionâ□□ observations being tested. The examination of only one precipitation event should not prevent the publication of this manuscript. However, the limitations that come from this should be emphasized in the

concluding statements. Special care should be taken with respect to the modelâ□□s treatment of moist processes (section 5a) which seem to be supported by other studies but which may only be applicable to this one case. Because modifications are required to figure 5 and the associated discussion, it is recommended that the manuscript be accepted after major revisions. Â Major comment 1 Figure 5 is problematic as it presents results that are not consistent with the verification metrics presented in sections 2.4. Addressing this issue is important as this figure is the basis for most of the discussion later in the manuscript. For example, it is not clear what the â□□percentilesâ□□ presented in this figure are or where they come from. Section 2.4 gives a good summary of the verification metrics used in the rest of the study. The percentiles appearing in figure 5 should be introduced there. Also, there appears to be a labeling mistake for the y-axes of panel c. The Fraction Skill Score is in the range [0,1] but the y-axis of panel c) goes from 5 to 35. Because of this, most of the discussion on lines ~350-380 is difficult to follow and/or interpret. It is believed that this part of the text and figure 5 should be reworked before publication of the manuscript. Still on the topic of verification, the use of â□□anomaly correlationâ□□ (section 2.4.1) for the verification of precipitation in a day-to-day forecasting context is unusual and somewhat confusing. If the concept of anomalies makes sense in a climatological context, it is more difficult to apply in a weather context. In my understanding, the â□□anomaliesâ□□ should refer to some departure from a preferred mode for the model solution. Because the mode of high-dimensional pdfs are generally difficult to estimate, they are often replaced by the average of a large number of such solutions. Many seasons are averaged for climate forecasts, many ensemble members may be averaged for ensemble forecasts. In the present case, for a single weather event in a deterministic context it does not seem possible to know the â□□normalâ□□ mode about which the anomalies could be estimated. In particular, the daily average precipitation for one case cannot be thought of as â□□normalâ□□ baseline against which anomalies can be estimated. That said, the correlation coefficient between two fields can be used in the context of verification. To avoid the confusion that arise from the concept of anomalies, it is suggested that correlations be estimated from the fields themselves. Just remove the \overbar{mod} and \overbar{obs} from eq. 5. The results previously obtained will be unchanged since the Pearsonsâ□□s correlation coefficient is invariant to such offsets by constant values. As a final note, one should remember that due to its non-linear response, Pearson's correlation coefficient is difficult to interpret in the context of verification. This problem is discussed in the appendix of https://doi.org/10.1175/MWR-D-18-0118.1. Â Minor comments: Table 1 Table 1 summarizes the description of the different experiments performed in this study. The current titles for the panels of this table make its interpretation difficult. It is believed that small adjustments to the labeling would help. The figure â□□suggested_changes_to_table1.pdfâ□□ joined to this review presents suggestions for changes. Â Description of results Most description of results repeat a lot of information that can be read from the figures. This makes these description quite lengthy and somewhat difficult to read. For example, the beginning of section 4.2 is especially hard to follow. The paragraph ~445-450 also repeats a lot of information accessible in the table being discussed. It is suggested that the description of results be shortened or summarized wherever possible. Â References to supplementary material Often figures found in the supplementary material will be referred to alongside the other figures. For example on line 377 we find "... western side of the Alps (Figs. 4b, S1b ans S2b)." If the

supplementary material will not be immediately available to the readers of the manuscript it is suggested that the supplementary figures not be referred to directly. If these figures are necessary to the comprehension of the text, they should be included in the manuscript. Following are minor comments in the order that they appear in the manuscript: Â line 133: Because of image compression, the red squares in figure 1b look a lot like circles. Â Equation 3: Out of curiosity, what is the value of "s" being used? Does it change with the resolution of the model or the observations being assimilated? Should it? Â Lines 242, 312, 355, 448 and others: The abbreviation "ca." is not very common. It is suggested that ~ or â□□ be used. Writing â□□approximatelyâ□□ would not burden the text too much either. Â Figure 3 In figure 4d) we can clearly see artifacts caused by the inflow through the model boundaries. Visibly, it takes some time for the modelâ□□s parametrizations to generate precipitation from the inflow through the boundaries. Presumably, some of the microphysical species being modeled are initialized at zero at the boundaries. While this does not seem to affect the main areas of interests for this study, this illustrates the difficulties associated with such high resolution forecasts. This phenomenon would probably be worth mentioning. Â Line 294 Being from North-America, all locations listed on this line except the Rhone valley were unknown to me. Perhaps adding letters or arrows could help readers from abroad to locate these places more easily? Â Line 334 : "no dynamic impacts" In the Canadian system, the assimilation of radar-inferred precipitation through latent heat nudging is shown (see paper references above) to reduce RMSE for upper-level winds by a few percent on average over a two-month verification period (~110 forecasts). One would not expect to be able to observe such a small signal on the model dynamics for only one precipitation event. Â line 338 "diverse impact for the different resolutions" Please quantify, maybe omit? Â line 348 typo: MSWP -> MSWEP Â line 363: The blending of the different precipitation products certainly explains part of the smoothness of the satellite-based products. Large differences in sampling volumes should also be mentioned as a factor contributing to the observed differences. Â Line 421 Altitude-based corrections can sometimes be significant, especially in mountainous terrain where the difference between the model terrain and observation height can be large. Do we know if this is the case here? Â Figure 7 The black line for GPS is difficult to distinguish in this figure. Maybe use a thicker/dashed linestyle? Â Line 532 In other instances of the text, the great heterogeneity of the moisture field is mentioned as a source of complications. It seems reasonable to assume that this likely explains why high moisture content was measured by only one sounding. Â Section 4.3.1 The box plots shown in figure 10 show no obvious differences that would be statistically different between the various experiments. Since this section is quite detailed and the manuscript already long, it is suggested that this section be moved to the supplementary materials. If it is believed that the section should remain in the manuscript, lines ~560-575 should be reworked to improve readability.

Please also note the supplement to this comment:
https://wcd.copernicus.org/preprints/wcd-2021-2/wcd-2021-2-RC1-supplement.pdf