

## ***Interactive comment on “Data impact studies with the AROME WMED reanalysis of the HyMeX SOP1” by Nadia Fourrié et al.***

### **Anonymous Referee #2**

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#### General comments

This study comprehends an interesting analysis of the impact of assimilating 4 different observation systems in the AROME-WMED reanalysis for the Autumn 2012, period of the first HyMeX SOP.

The main results of this publication include the good performance of the GNSS-ZTD Data Assimilation (3DVar), the improvement gained over the Iberian Peninsula due to the assimilation of the Spanish radar network and the weak impact of assimilating the wind profiles and Lidar measurements. Also noteworthy, is the weak but positive impact of GNSS Data Assimilation on wind correction given this measurement system provides information on the integrated atmospheric moisture column.

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Even though I consider this manuscript has potential for publication, it must undergo major revisions to have a sufficient quality.

There are two overall problems. First, the composition and structure need to be substantially improved as the main guideline of the paper is not clearly shown. Second, an overarching conclusion encompassing the findings for each of the observations systems is needed.

Regarding the composition of the paper, the introduction/justification for this particular manuscript is not highlighted. It is clear that it has been produced in the framework of the AROME-WMED second reanalyses. But the reason why these observations impact studies were carried out is not said nor in the abstract, or in the introduction. The abstract would be more attractive if at least it was mentioned why these four observation types were selected. To this end it would be useful using the sentence in the first paragraphs of the conclusions “Previous studies such as Duffourg and Ducrocq (2011) Ricard et al. (2012) or Bresson et al. (2012), have shown the interest of an accurate description of the low-level moist flow feeding mesoscale convective systems. In this study the impact of various data set related to humidity and wind on the forecast quality from this comprehensive reanalysis is investigated over the 2-month period”.

Second, the methodology of the experiments is chaotically explained. Throughout the manuscript different denominations are used for the same simulation. Hence, I would advise sticking to one nomenclature for the simulations, First Guesses (FG), Analysis (AN) and the different data denial experiments. In section 2, the logic order of presenting the experiments would be, first a brief description of the experiments, then the model, then observations and finally the main concept of the data denial experiments and its nomenclature.

Regarding the conclusions, an overarching statement as to what would be the best(s) observation system(s) to use in future Data Assimilation experiments or operations is needed. This is crucial as only rarely there is such a high availability of different

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observations. In most of the cases efforts and resources must be concentrated and this paper would be helpful in providing some guidance for decision making. For instance, in case of having to choose between one of the four observation systems, which one would bring more added value? In the assimilation procedure should some of these observations be given more weight than others? Is the temporal frequency of any of these playing a special role?

### Specific comments

Title - I would strongly advice including the word assimilation in the title. “Data Assimilation Impact studies with AROME-WMED reanalysis of the HyMeX SOP1” Abstract (L01-05) – In addition to the missing appropriate justification of this paper (see general comments), the introduction is too specific on the terminology of HyMeX. Using acronyms such as OSEs, AROME and HYMEX might be familiar to readers in the community but not necessarily to a broader audience. I would suggest starting the abstract mentioning the global topics of the paper: Data assimilation, the four observation systems and the validation of first guesses/analyses and forecast range.

L51-52 - What is discussed in the paper is really the impact of 4 observation systems (GNSS, LIDAR, Wind Profiler, Spanish radars) not “the many observation data sets which were assimilated” in the second reanalysis. Please, rephrase to constrain appropriately the scope of the paper. Moreover, “Quantify the contribution” sounds ambiguous. The aim could be rephrased to “quantify the impact of four observation systems on the quality of precipitation simulation”. Finally, it would be appropriate writing that the impact of the data denial experiments is studied for the quality in the simulations and not on processes. Which might be understood from the title of the paper.

L85-91 - As explained in the general comments the general data denial procedure and the nomenclature have to be well introduced and explained in this section and kept during the remainder of the manuscript. More clarity is needed in the nomenclature.

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2.2 Observing System Experiment Description – Some relevant information is missing regarding the description of the observational data sets and its assimilation procedure. For example, for GNSS a short description of what is ZTD and its relationship to humidity would be desirable. For Wind profiles, some notions on what is the measurement technique is needed. The same applies for the Spanish radar. Overall more details on how these variables are assimilated is advised. For example, no information is given about the forward operators and specific prognostic variables for each of the 4 data observation systems.

Figure 1 – Add a legend of the observations shown in the figure. Change caption to show that also GNSS is used in the study even if the coverage is not shown in Figure 1.

Figure 2 – Adds no relevant information.

2.3 Validation protocol – The terminology is somewhat confusing. First of, the term validation should be used for observations vs. simulations comparisons, not for a comparison against the reference run REANA. Second, the authors should differentiate between a validation against dependent and independent observations. My suggestion would be starting the description of the validation protocol talking about the comparison against observations (dependent and independent) and afterwards about the evaluation of the impact against REANA and not otherwise.

L149-150 - The sentence “This data set represents the largest one in terms of total amount, even though it represents a small fraction of assimilated data (1.85%)” needs further explaining. Considering the information conveyed in Table 1, where the satellite are the most numerous assimilated observation, why is it here said that the GNSS data set is the largest in terms of amount of observations? And finally, why do they “represent only a small fraction of assimilated data”? Did these data not pass the quality control of the assimilation system? Are they rejected due to blacklisted reports? Please elaborate.

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L157-159 - Why is there a minimum in the correlation at 15 h? Were the measurements less accurate at that time of the day and therefore dismissed? Were there less observations assimilated? Is it due to any physical process with a diurnal cycle? The explanation should be mentioned in the manuscript even if just briefly.

L163-165 - This sentence should be reformulated. Why no impact can be seen in the analysis of RMS differences, but a small impact is present for FG RMS differences?

L165-167 - This information is not understandable. If the largest benefit of assimilating GNSS data occurs in the layer, between 600 hPa and 850 hPa how is it possible that “the slight benefit of assimilating reprocessed GNSS data appears between 700-850 hPa.”? The 700 hPa – 850 hPa layer is contained within the 600 hPa – 850 hPa. Overall, a clearer description of when the reprocessed data is used or not should be needed. Again, it would be advisable choosing one nomenclature for each data set and stick to it.

Figure 4 – No appreciable differences exist in the profiles of the AN runs for REANA, NOGNSS, OPERGNSS. Furthermore only small differences (0.05 g/kg) can be found for the FG runs. If there are no differences in the profile, why is the standard deviation of the integrated moisture so different (up to 1 mm) in Fig. 3? Also more information about the radiosondes used is needed. Where are they located? What is the temporal resolution?

Figure 5 – What is the explanation for the large bias in mean ZTD between the independent station Marfret-Niolon and the set of simulations? This is especially evident after 12 UTC. This would imply a wet bias in the REANA, NOGNSS and OPERGNSS simulations compared to the independent observations.

Figure 6 – Just as Fig. 5 and Fig.11 the validation against the Marfret-Niolon is too noisy to convey any conclusive information. Either quantify or change the graph to show real evidence of the good(bad) performance of the OPERGNSS and NOGNSS assimilations.

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Table 3 Shows Interesting information as GNSS observations have no wind information. How can the improvement in the wind description be explained? Is it a direct impact of the 3DVar assimilation? Is there any physical process explaining the improvement? If so, please mention it in the manuscript. Also reference to other publications if this effect had been addressed before.

Figure 8 - This result is very interesting. Especially striking is the loss of skill for OP-ERGNSS with larger daily precipitation. How can this be explained? Is this a result of the lower number of occurrences for heavier precipitation events? Please, add an explanation in the manuscript.

L205-207 - Indeed the comparison is noisy, with this graph is not possible to see which simulation has the largest (smallest) correlation. Please, add a table with the average values or present the differences against REANALYSIS, otherwise the statement “the correlation for the NOGNSS is lower and the standard deviations are in general higher for the NOGNSS” is unsupported. The same applies to Figures 5 and 6.

L231-236 – There is no evidence for the results conveyed of the NOLIDAR experiment. Please add the corresponding evidence.

L238-245 – This result is revealing. Why the improvement of the Spanish radars can only be observed over Spain? why is the impact so Local? does it depend on the assimilation system (3DVar)? How is the localization of the data assimilation working in your system? More information is needed on how the 3DVar system is implemented for these cases.

Technical corrections

Abstract (L13-14) –For clarity, add information of lead time (number of hours) considered as a “very short term forecast” and a “short term forecast”.

L16 - “Copyright Statement. TEXT”.

L21 - Correct the typo “...6 November 2012) in the north western ...”.

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L31 - Bad double parentheses style. It should read "(Application of Research to Operations at Mesoscale; Seity et al., 2011)".  
<https://guides.library.nymc.edu/c.php?g=567729&p=4609898>

L35 - Acronym of Innovative Observing and Data Assimilation Systems is missing.

L36 - Substitute "With a view of" by "with the aim of".

L36-39- Sentence is too long. Split it and rephrase "...due to a system upgrade in the middle of the SOP1). The second one included in addition a maximum of observations..."

L39 - Substitute "This latter" for "The latter".

L56 - The description of the paper's sections ("This paper is arranged as follows...") should start in a new paragraph.

L108-109. Sentence "...data is 75 m but for assimilation, data were thinned at 75 m below 2000 m up to 450 m above 5000 m." should read "...data is 75 m but for data assimilation above 5000 m the resolution was thinned to 450 m."

Table2 - Should be placed at the beginning of Section 2.2, outside the bullet point 2.2.4 as it shows the overview of all observation systems, not only on the Spanish radar.

L171 - English style is not correct because of double enclosure (see above).

L171-175 - Show the plots with the SEVIRI results in the supplementary material.

L176 - For the "various AROME-WMED ZTD analyses" use the designated nomenclature AN.

L177 - Please rewrite "is slightly and consistently higher".

Figure 4 - Instead of top and lower panel should show left and right.

L185 - It should read "This airborne radar was on board the Falcon 20 aircraft"

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Figure 5 caption - Write explicitly that the Marfret-Niolon observations are independent.

Line 195 - It is not clear to which data set the authors are referring to with the terms “re-processed data” and “real-time ones”, is the NOGNSS or OPERGNSS ?. Be consistent in the nomenclature of the data sets.

L201 - Instead of “IWC” it should be “IWV”.

L.209 - Instead of wind it should be “humidity” or “relative humidity”

Figure 11 - Instead of top (bottom) panel it should be left (right).

L230-236 - Indicate which Figure is being analysed when saying “The denial NOLIDAR experiment results are close to the reanalysis ones as these data represent very few additional data and are located over ocean where few observations are available for the comparison”.

L242 - It should read “This impact does not remain at longer . . .”

L250 - Should read “Catalonia”.

L251 - Rephrase sentence “on, in the evening the Italian Ligurian coast.”.

Figure 20 - has a bad quality, letters cannot be read and are blurry.

L304 – Substitute “Iberic Peninsula” for “Iberian Peninsula”

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2020-153>, 2020.

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