Title: Assimilation of GPM-retrieved Ocean Surface Meteorology Data for Two Snowstorm Events during ICE-POP 2018

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We would like to thank the reviewers and the editor for their thorough analysis of our manuscript and for suggesting changes that will help our paper to have a better quality. We have reproduced the reviewer's comments below — our responses appear in blue. The line number in this response refers to the manuscript without track changes.

Reviewer #2 comments:

General assessment:

The manuscript describes the assimilation of GPM-retrieved near-surface variables into the WRF model for two snow-storm case studies during the ICE-POP 2018 research and forecast programs during the Olympic and Paralympic Winter Games. The impact of the assimilation of this data into a numerical weather prediction model and its impact for winter storm prediction is in general an interesting topic. However, from my perspective the publication of the present manuscript can only be considered after substantial revision.

We have enhanced introduction section (please see the last paragraph in Page 4 Line 113-134) and the data section (please see Line 153-166) in the updated manuscript. We have made substantial changes throughout the result section and the conclusion section. We have also added one more figure to discuss how the data assimilation modified the moisture convergence and moisture transport, hence influenced the precipitation production (please see Line 396-408 in Page 13).

Major comments:

• The conclusions drawn from the two experiments are partly self-evident and don‘t provide much useful insight. It is shown that the assimilation draws the analysis closer to the assimilated observations, however, unless there is a bug in the system, this is to be expected. Moreover the authors show and discuss over several paragraphs that the effect of the assimilation are lost quickly during the forecast by the atmospheric dynamics. This is also a normal effect in the dynamic atmosphere, so for the reader it is not so much of interest that it happens, but rather how long the desirable positive impact lasts. Also the conclusion that A-B (which is actually the increment added in the assimilation step, which is not mentioned in the manuscript) has the same pattern with respect to positive and negative values as O-B is self evident and can be inferred directly from the equations of the data assimilation algorithms.

Yes, it is true that a successful data assimilation will drive the analysis closer to the observation. But it is also true that a reasonable analysis cannot be created without fine tune the data assimilation system and the parameters. Also, the data impact on different weather systems can be case dependent (as confirmed by the two storms in this paper). Therefore, it is necessary to conduct these experiments to demonstrate how the satellite retrieved observation influences the initial condition and the forecast fields. Actually, there has been very little effort to assimilate these surface flux retrieved meteorology observations in modelling systems. It has not been an emphasis in the satellite-based flux community. Therefore, this is unique to test the assimilation of this dataset. In this paper, the focus is to demonstrate the data assimilation, investigate how long the data impact will last and by how much the data assimilation will influence forecast. These will be very useful in designing the data assimilation cycles in order to retain the information from the observations. A-B is the increment, this has been added in Line 318.

• I would suggest to rewrite and restructure the manuscript answering the following questions more clearly:

◦ What data is assimilated? Describe the GPM mission (I think it is not even mentioned that this is satellite-based...) and not only the retrieval of the variables

In this study, we assimilated the ocean surface meteorology data 2-m temperature, 2-m specific humidity, and 10-m wind speed retrieved from GPM microwave observations. This point is mentioned in the introduction section Line 111, and in the data section Line 178 and the experiment section 2.2 Line 275. More discussion of this dataset has been added in Line 114-121. The description of GPM mission was added in the data section Line 161-166.

◦ What is the effect on the analysis? Verification against independent variables would be desirable

Figs. 6 and 12 shows the data assimilation increment on surface temperature, relative humidity and wind speed for March and February case, respectively. In the updated version, Root Mean Square Difference over the model domain was calculated for these variables too (Page 11 Line 337-341). There are not much other observations over the ocean could be used for direct verification, except some buoy data, which has already been used for validation of the GPM retrieved ocean surface observation.

◦ What is the effect on the forecast (on different variables such as precipitation, temperature...), and how long does it last?

Fig. 7 shows the impact of the data on the analysis and the RMSD on 1-6h surface temperature, specific humidity, and wind speed forecast. The impact has a large decline in the first hour of integration for temperature, specific humidity, and wind speed, but a large portion of the impact could still be seen in the 6-h forecast. Figs. 8 and 13 shows the impact of the data on *temperature forecast. Figs. 9, 10, 13 shows the impact of the data on 1-h accumulated and 24-h accumulated precipitation fields.*

• Motivation: For an improved prediction of snowstorms probably upper air observations would be more useful than these surface observations. This does not mean it is not interesting to exploit this surface data for data assimilation, I just wonder whether the motivation could be formulated slightly broader, e.g. to find out which variables can be improved during these two case studies, and which not.

We have enhanced the introduction section by adding the following discussions on the motivation of the current research "There is a long heritage of assimilating ocean surface buoy measurements within a data assimilation framework, but there has been little effort focused on assimilating the surface retrievals. This is part due to a lack of a real-time availability of these estimates and partly due to the focus on radiance-based assimilation system. The latter are not particularly tuned for leveraging lower-layer information in microwave observations as the stand-alone efforts originating from the satellite-derived flux community. The ICE-POP 2018 campaign provided a unique opportunity with near-real time passive microwave estimates of surface meteorology and a heavily observed regional environment to test the potential impact of assimilating wide-spread observations of near-surface meteorology." (please see Page 4 Line 120-127).

• Figures: The manuscript contains many too many figures, and many of them do not contain conclusive information, thus can be omitted without loss of information

We have removed the previous Fig. 7, but do think the other 14 figures are necessary in order to effectively convey our conclusions from the two case studies. These figures show how and by how much the assimilation of this particular dataset influences the model initial condition and forecast for snowstorm events. These figures also show how consistent and different the data assimilation impacts are for the two different storms.

◦ The scatterplots in Figure 5 could be omitted

Figure 5 gives us the general statistics of the observational data and the distribution of O-B that have been assimilated into the model. We think it's important to keep Fig. 5 in the manuscript.

◦ Figure 7 and 8: Can be omitted, it only proves that the atmosphere is a dynamic (chaotic) system

Figure. 7 has been removed. Figure.7 (previous Figure. 8) shows how much data impact was produced in the analysis field, and how long and how much of the data impact would last in the forecast. We think Fig. 8 should be kept in the manuscript.

◦ Figure 9 can be omitted, Figure 10 is much more informative

We agree that, with the 24-h accumulated precipitation, Fig. 11 (previous Fig. 10) has more information than Fig. 9. But we prefer to keep Fig. 9 in the manuscript, we also added a new Fig. 10 for moisture convergence and moisture transport to explain the physical reason for the difference between Fig. 9b and 9c.

◦ Figure 11: a RMSE value would probably give more information than this Figure

We have provided the RMSE value in the updated manuscript (please see Page 12 Line 376-377).

◦ Table 2: It would be much more illustrative to have some visualization here… Time-series or bar plots for example.

It would be good to keep it as Table 2 considering that we already have 14 figures.

Minor comments

• L90: This sentence is self-evident. Should be omitted.

This sentence has been removed.

• L103: What kind of observations?

It's dropsonde data. We have added this point in the manuscript (please see Line 103).

• L109: ocean surface meteorology conditions is too unspecific here

We have revised this sentence as "In support of the International Collaborative Experiments for PyeongChang 2018 Olympic and Paralympic Winter Games (ICE-POP 2018) field campaign, ocean surface meteorology conditions (2-m temperature, 2-m specific humidity, and 10-m wind speed) were retrieved from the Global Precipitation Measurement (GPM) microwave observations from January to March 2018" (please see Line 111).

• L128-130: What kind of radars are the KMA radars, and what exactly is PIP

The KMA radars are S-band Doppler radars. This information has been added in the manuscript Line 149. The Precipitation Imaging Packages (PIP) is a video disdrometer made up of a single high-speed camera, continuously recording at 380 frames per second, and a halogen lamp to backlight the precipitation particles.

• L156-158: This last sentence of the paragraph is not clear to me

This sentence has been removed from the manuscript.

• Page 7: Which model domain and which resolution was actually used for the data assimilation experiments? Was the data assimilated into all three model runs?

The data were assimilated into all 3 domains.

• L223-225: Is xb the same as xguess?

Yes, we have modified Xguess as Xb for consistency.

• L27: What is pseudo relative humidity?

Pseudo relative humidity (PRH) is defined by the following equation:

$$PRH = \frac{r}{r_{sb}(T_b, P)}$$

where r is the mixing ratio, r_{sb} is the mixing ratio of a volume of air that is saturated with water vapor from background, which is affected by temperature from background T_b and atmospheric pressure P.

• L237: What kind are the satellite wind data? Atmospheric motion vectors? Or Scatterometer data?

It includes satellite derived wind reports from GOES satellites and scatterometer wind vector (according to https://www.emc.ncep.noaa.gov/mmb/data_processing/prepbufr.doc/table_1.htm and https://www.emc.ncep.noaa.gov/mmb/data_processing/Satellite_Historical_Documentation.htm#Sec.%20II).

• L251: What do you mean the data assimilation results are compared with WRF simulations? Don't you actually compare analyses and forecasts with observations?

Thank you so much for catching that. This sentence has now been revised as "In this section, WRF simulations with and without the assimilation of the GPM-derived ocean surface meteorology data were compared with the observations collected for the March 7-8 and February 27-28 snowstorm cases." (Please see Page 9 Line 287-289).

• L269: Isn't actually the humidity of the model higher than that of the observation, if you get a negative observation minus background?

Correct, the model background has a higher specific humidity than the observation. This has been corrected in the manuscript in Line 308.

• L291: A-B is actually the increment that is added to B in the assimilation step.

Correct, A – B is the increment in the assimilation step (added in Line 318. This sentence has been revised (Please see Page 11 Line 335-337).